The Impact of Environmental Regulation on Firm and Country Competitiveness:
A Meta-Analysis of the Porter Hypothesis*

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ABSTRACT

Since the early 1990s, the validity of the Porter Hypothesis has been the focus of intense research to establish whether well-designed environmental regulation may enhance – rather than reduce - competitiveness. However, little consensus exists on the extent to which environmental regulation might generate profitability enhancing innovation offsets. This paper reports on a meta-analysis of 103 publications which estimate the relationship between environmental regulation and firm or country-level productivity or competitiveness. We find considerable heterogeneity in both the sign and significance level of over 2,000 estimated "effect sizes" in these studies. A positive effect of environmental regulation is more likely at the state, region or country level, compared to facility, firm or industry level – although in both cases the most likely scenario is statistical insignificance. These findings are consistent with the strong version of the Porter Hypothesis whereby strict but flexible environmental regulations induce innovation and over time increase country-level competitiveness.

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I. Introduction

Conventional economic wisdom has long held that environmental protection is costly to firms required to comply with government regulations. By definition, adding a constraint to the production possibility set such as requiring less pollution from a firm that is otherwise profit maximizing can only reduce profitability (Palmer, Oates and Portney, 1995). Yet, a revisionist view emerged in the early 1990s, crystalized by Porter (1991) and Porter and van der Linde (1995) who posited that regulation could potentially enhance productivity and/or competitiveness by generating substantial "innovation offsets". What has since become known as the Porter Hypothesis suggests that environmental regulation may provide an incentive for firms to innovate, which can improve productivity and in some cases even result in lower costs. As Jaffe and Palmer (1997) noted, there are several forms that the Porter Hypothesis might take:

- *The 'weak' version*: environmental regulations will stimulate 'certain kinds' of innovation, for which the opportunity cost exceeds the direct benefits to the firm.

- *The 'strong' version*: these regulation-driven '"innovation offsets"' can often exceed the costs of regulatory compliance, resulting in net productivity gains.

- *The 'narrow' version*: well-designed (i.e. flexible, market-based) regulations give firms greater incentives to innovate and will have less adverse impact on productivity than prescriptive regulations.

Numerous studies have tested each of these hypotheses, often reaching different conclusions (Lankoski, 2010). For example, while most studies find that environmental regulation generally spurs innovation, there is significant disagreement over the strength of this signal and the nature of the resulting innovation (Johnstone et al., 2009; Hascic and Johnstone, 2011). The greatest conflict surrounds how environmental regulation affects competitiveness (normally measured through productivity). Most early studies concluded that it negatively affected productivity – often significantly (Palmer et al., 1995). The second decade of research has produced more mixed results. Several studies, for example, find that under certain circumstances, environmental regulation can have a positive effect on productivity (Berman and Bui, 2001; Lanoie et al., 2008).

The strong version of the Porter Hypothesis has been particularly controversial. This is unsurprising given that the notion that firms will ignore opportunities to improve financial performance until encouraged to do so by the imposition of government regulations directly contradicts the profit maximization assumption of neoclassical economics. Ambec et al. (2013) review a cross-section of these studies and posit a range of factors that may explain the conflicting results. These include: the variability of the regulatory approach; the firm's sector and market conditions; the environmental problem being addressed; the firm's governance and management approach; and the research methodology. They also note that more recent studies of the strong version of the Porter Hypothesis tend to find positive results compared to earlier negative findings.

Given the significant and broad reaching environmental, economic, and distributional impacts of environmental policy, it is important firstly to understand why the literature on the Porter Hypothesis reaches such divergent conclusions, and secondly why the balance of positive and negative findings on the relationship between environmental regulation and competitiveness appears to be shifting over time. To what extent is it due to methodological differences (e.g. how productivity or innovation is measured) or publication bias (i.e. researchers may work harder to 'prove' the Porter Hypothesis as time goes on)? To what extent does it stem from differences in regulatory approaches (e.g. flexible or market-based instruments), characteristics of the firm (e.g. the industry, or degree of competitiveness), or other factors (e.g. available technological substitutes)? Without a systematic empirical comparison of findings in the literature, it is difficult to assess the importance of these factors and ultimately to judge if and when the Porter Hypothesis is valid.[1]

In response to the lack of clarity surrounding the validity of the Porter Hypothesis and the significant public policy implications following from a comprehensive understanding of the conditions under which the hypothesis does and does not hold, this paper reports on the results of a systematic meta-analysis of 103 publications that empirically estimate the relationship between environmental regulation and measures of firm or country-level productivity or competitiveness.[2] The next section briefly describes the meta-analysis methodology and how it applies

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[1] Indeed, one of the recommendations of Ambec et al. (2013) is a call for a meta-analysis of previous research.
[2] Since the focus of this meta-analysis is the relationship between environmental regulation and firm or country-level productivity or competitiveness, we are directly considering only the 'strong' version of the Porter Hypothesis.

to our study. Section III summarizes our search methodology and how we determine

the sample of eligible empirical publications examining the relationship between

environmental regulation and competitiveness, while section IV describes the

variables coded. Section V summarizes our data set and provides an initial look at

the wide variance in study designs. Our findings are reported in section VI.

Concluding remarks and directions for future research are contained in Section VII.

II. Meta-Analysis

Meta-analysis is an empirical methodology that endeavors to aggregate the

findings of different studies investigating the same underlying relationship. In our

case, the underlying relationship is the impact of environmental regulation on

productivity and competitiveness. Analyses have reached different conclusions

using different data, research methodologies, measures of environmental regulation

and measures of productivity or competitiveness – but they all seek to explain the

same phenomenon.

In a meta-analysis, each study is treated as an individual observation. It is a

well-accepted methodology that has generated a significant literature and best

practice approaches in many disciplines including environmental economics. Nelson

and Kennedy (2009: 346) report on an analysis of 140 meta-analyses in

environmental economics, and explain:

> Briefly, the investigator collects a set of primary studies that contain a
> common empirical outcome, such as the long-run price elasticity of gasoline,
> the willingness-to-pay (WTP) for freshwater quality, or the influence of air
> pollution on property values. In contrast to controlled experiments in the
> natural sciences, the primary studies in economics employ different study
> designs, model specifications, and econometric techniques. In a meta-
> regression, the dependent variable is a common summary statistic or "effect-
> size," such as a regression coefficient for the price elasticity, a predicted value

for the WTP, or significance level of the air pollution coefficient. One or more values of this statistic are drawn from each primary study. It is of crucial importance that this dependent variable is measuring the same economic concept across primary studies. The moderator (independent) variables in the regression include characteristics of the primary data, study design, valuation method, sample size, model specification, econometric methods, and other "quality" variables such as place and date of publication.

There are several possible goals of a meta-analysis. In some cases, the analyst is interested in estimating a weighted average mean "effect size" which might be an elasticity or monetary value. In some cases, however, the only question is whether or not the sign of the underlying relationship is positive or negative (and/or statistically significant). In other cases, the goal is not to estimate one average effect, but instead, to attempt to explain the heterogeneity that is observed across studies.

A recent meta-analysis by Horváthová (2010) illustrates how this method can be useful in explaining conflicting studies. Horváthová analyzes the findings of 37 publications that test the linkage between environmental performance and financial performance. For example, she finds that the empirical method used matters – with simple correlations tending to result in negative findings while more sophisticated econometric models find a positive relationship. She also finds differences across countries and time.

III. Search Methodology

Typically, search procedures for meta-analyses focus on title, abstract and keywords in only one or a small number of primary databases. However, in many areas of empirical economics this approach is likely to be insufficient. Our search protocol is much more extensive in order to guarantee - to the greatest degree possible - that all relevant papers are identified (Nelson 2013).

We first conducted an extensive review of the literature, including the following sources:

- All empirical papers cited within known literature reviews including Jaffe et al. (1995), Copeland and Taylor (2004), Brunnermeier and Levinson (2004), Lankoski (2009), Iraldo et al. (2011) and Ambec et al. (2013).

- Citation searches using Google Scholar of the above literature reviews and the relevant empirical studies cited therein.

- Keyword searches in Google Scholar, EconLit, SSRN, AgEcon, and ProQuest Dissertations, using the combinations of (environment/al regulation, environment/al policy, climate change policy) with competitiveness, financial performance, economic performance, Tobin's q, ROI, profitability, R&D, innovation, patents, capital, productivity, and technology. Additional searches were conducted for the terms Porter Hypothesis and pollution haven hypothesis.

- Technical reports and working papers were searched from the following organizations: CESifo, US EPA , National Technical Information Service, Office of Technology Assessment, Congressional Budget Office, European Academy of Business in Society, Academy of Management's Organizations and the Natural Environment, Network for Business Sustainability, Resources for the Future, Swedish Foundation for Strategic Environmental Research, US Department of Agriculture (Economic Research Service), and national statistical agencies in Norway, Sweden, Netherlands and the UK.

- Personal emails were sent to 26 key authors requesting any relevant unpublished papers or empirical results. We received replies from 20 of these authors supplementing our original search or confirming that they did not have any additional research to consider; searches of the researcher websites of the remaining six authors were conducted to uncover any additional papers.

This process resulted in 1,055 publications being identified for screening (see Table 1). From that initial set, 649 were empirical studies that appeared to be related to the Porter Hypothesis, and thus required more detailed review. From that set of 649, each publication was reviewed for eligibility to determine if there were empirical findings related to the Porter Hypothesis that could be coded. We identified 14 ineligible "review articles," and 27 publications that focused on voluntary initiatives instead of mandatory government regulation. Of the remaining empirical studies that focused on mandatory regulation, 103 were eligible for final coding. To be eligible, the publication must include an empirical estimate of the impact of mandatory environmental regulation on (a) facility, firm, or industry performance, and/or (b) state, regional, or country-level competitiveness. In many cases, an earlier unpublished working paper was found along with a published version of the same paper. In those instances, each table of the working paper was compared to the published version. In 11 publications, one or more regression results in a working paper differed from the published version, and they were

treated as additional tables to the published version (and identified separately as being from a working paper).[3]

Our meta-analysis therefore focuses on the 103 publications (in addition to related working papers) listed in Appendix A, and 2,025 effect sizes (usually regression coefficients) that address firm or facility-level productivity or profitability or state, regional, or country-level competitiveness. Four publications (two of which were dissertations) analyzed more than one dataset independently and were treated as two separate publications in our analysis. Thus, while we empirically analyzed 103 distinct *publications* and their associated working papers, we consider our sample to be 107 *studies*. Each of the publication sources is listed in Appendix B.

The diversity of publication sources is striking. Of the 103 unique publications coded, 16 were unpublished working papers, 4 were dissertations and 2 were book chapters which are not associated with publication in an academic journal. Thus, we coded a total of 81 unique academic publications from 53 different journals. The most frequent journal was *Ecological Economics* (10 publications),

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[3] As normally dictated by best practice in the meta-analysis field (Stanley and Doucouliagos 2012), these additional model specifications and effect sizes were coded and included along with the effect sizes reported in the published paper. Results from a working paper differed from a published version in only 11 publications – and generally the results are only slightly different. (7 of the 11 working papers came from the recent time period, 2011-2015. The working papers come from a diverse set of institutions around the world, including smaller universities and research institutes.) Moreover, as discussed later in the paper, each effect size is weighted by the reciprocal of the number of effect sizes in a particular study; hence, inclusion of additional effect sizes from a working paper does not increase the weight given to that publication. In addition, some of our analysis is limited to the "preferred specification" from a publication – which would always exclude findings from a working paper when a published paper was available.

followed by *Environmental and Resource Economics* (5), *Journal of Productivity Analysis and Journal of Public Economics* (3 each). An additional 11 journals each had 2 publications (*Economics Letters, Energy Policy, Journal of Environmental Management, Journal of Political Economy, Kyklos, Land Economics, Public Administration Review, Research Policy, Resources and Energy Economics, Review of Economics and Statistics, and The World Economy*), while 39 journals had only one published article in our sample. Fields ranged widely from leading general interest economics journals to more specialized discipline and field journals in environmental economics, management, industrial ecology, public policy, development, and others.

There were 177 distinct authors among these 103 publications. Only 23 authors had more than one paper in our sample. The top authors with four papers each were Dietrich Earnhart and Dylan Rassier (each co-authoring the same four papers), Wayne Gray, and Shunsuke Managi. Valeria Costantini, Paul Lanoie and Ronald Shadbegian each had three publications. Appendix C contains a complete list of authors with more than one paper, their affiliation at the time of publication, and all of their coauthors.

The number of effect sizes reported in each publication ranged from as little as one to as many as 121.[4] The median number of effect sizes per publication was 10. As shown in Table 2, of the 107 studies we analyze in this paper, 70 focus on facility, firm or industry-level productivity or profitability (45 of which are at the

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[4] As discussed in Section V, we have dealt with the lack of statistical independence of multiple effect sizes within each study in two alternative ways. First, we weight each effect size by the reciprocal of the number of effect sizes in that particular study. Second, we choose a "preferred" effect size for each study.

firm or facility level and 25 at the industry level), while 37 focus on state, regional or country-level competitiveness. We exclude papers that focus primarily on the "pollution haven hypothesis" where the dependent variable is either foreign direct investment (FDI) or the geographic location of plant births and/or closures.[5] Instead, the 37 studies on state, regional or country-level competitiveness examine outcomes such as international trade flows, output measures, and productivity.

Following Lipsey and Wilson (2001), the coding protocol consists of two components – a coding form and a coding manual.[6] The coding manual contains comprehensive instructions directing the coder how to consistently code each variable of interest onto the coding form. We developed the coding manual guided by two strands of the academic literature. First, we followed best practice in the meta-analysis literature by including standard fields for publication and effect size information (e.g., publication name, year of publication, authors, data years, sample size and unit of observation). Second, we paid close attention to the Porter Hypothesis literature to determine which environmental regulation and effect size variables were likely to be important in explaining the wide divergence of published outcomes (e.g. environmental regulation type and measure, data source, target of environmental regulation). Following development of the draft coding protocol, a one-day 'expert panel' was convened to discuss improvements and refinements. The

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[5] Note that several papers in this study primarily focus on the pollution-haven hypothesis but also include outcome measures such as net employment or GDP (see, e.g., Fredriksson et al., 2003 in Appendix A). In those instances, we include the empirical results obtained using competitiveness outcome measures (net employment or GDP) from these studies; but exclude the effect sizes focusing on FDI or geographic plant births/closures.
[6] Available on request.

expert panel consisted of leading researchers in the fields of meta-analysis and/or environmental economics.[7] Following this meeting, the coding protocol was finalized based on feedback from the expert panel.

Following comprehensive coder training sessions, each publication was initially coded by a research assistant based at either Vanderbilt University or the University of Ottawa. Each coder was a current or former graduate student selected on the basis of his/her knowledge of environmental economics and econometrics. Following initial coding, the coding database was systematically checked by one of the lead researchers for consistency and coding errors.

Document searches took place beginning in the Fall of 2012, with updates through June 2014. Thus, the above search process should be considered complete as of the end of June 2014. However, during the collection of external data and in preparing the dataset during the summer of 2015, we conducted a systematic review of each paper to determine whether or not there were more recent versions available – either a revised working paper or a published version of a dissertation or working paper in our dataset. If a publication that was identified by our search process ending June 2014 had been updated or published as of the end of July 2015, the more recent version was included. Thus, while some of the papers are dated past mid-2014, according to the search criteria any new published or unpublished papers that first appeared after mid-2014 are excluded.

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[7] The expert panel members were Stewart Elgie, Anthony Heyes, Nick Johnstone, Paul Lanoie, Jon Nelson, David Popp, and David Wilson. They share no blame for any errors in design or execution – only thanks for their assistance and devoting their valuable time to this project.

IV. Variables Coded

A total of 20 variables were coded at the "publication level", including date, author(s), funding source(s), publication outlet, range of years for the dataset, sample size, unit of observation (e.g. facility, firm, industry, country), industrial sector(s), country(ies) in which the data are based, purpose of the study, whether Porter was mentioned in the text or abstract, and any sample restrictions placed on the data.

A total of 11 variables were coded for each dependent variable, including the variable measure (e.g. output, accounting profit, stock prices, or self-reported performance in the case of firms, or GDP, growth rates, employment, etc. in the case of countries), its format (level, log, binary, categorical, index), mean, and standard deviation. In addition, source and sampling details were coded, such as whether it was quantitative or qualitative official agency data, researcher, trade association or other survey data. In the case of survey data, information about the sampling strategy and response rate was also coded. Identical variables were collected for each environmental regulation being studied – although here the variable measures are replaced with the relevant descriptors such as pollution abatement and control expenditures, number of inspections, number of regulations, non-compliance fees, regulatory stringency, etc. In addition, each regulation variable was identified as being a technology standard, emission standard, emission tax, cap and trade, regulatory disclosure, etc. Finally, the regulatory target (air, GHG emissions, water, etc.) was identified.

Finally, 30 additional variables were coded for each effect size. When available, we coded the sign of the unstandardized regression coefficient and its size and standard error, t-statistic, p-value, the direction of the relationship between the environmental regulation variable and competitiveness, level of significance (1%, 5%, 10%, insignificant), number of observations, data structure (cross-section, time series, etc.), whether this was a subsample of the full study data, the econometric model (OLS, 2SLS, GLS, Tobit, etc.), whether there was any lag structure in the model, instrumental variables, and detailed information about the number and type of independent variables included in the model.

V. Data Description

This section briefly summarizes the data that was coded for our 107 studies.

Publication/Sample Characteristics

Table 3 summarizes the publication-level data. Some of these studies are based on data that are quite old – with two studies including data from 1958 and 16 studies including data from prior to 1976. About half of the publications include data from 1990 or earlier. Only 36 studies (34%) contain data with an earliest date of 1996 or later, with one paper whose earliest data was from 2009 and one from 2010. The median 'start date' was 1999. Not shown, the earliest 'end date' (the date at which a study's data ended) was 1975 with the latest being 2010 and the median 1999. Thus, there is a wide range of data and many studies with data ending in the early to mid-1990s.

The date of publication ranges from 1982 through 2015 with the median publication date being 2008. Only four studies were published prior to 1991, the

year in which Porter first published his claim that regulation might have a positive impact on competitiveness.[8] About two-thirds of the studies (69/107) mention the Porter Hypothesis or cite one of Michael Porter's relevant papers in their references. Facility, firm or industry-level studies were more likely to mention Porter (68% - 48/70) than state, regional or country-level studies (57% - 21/37).

Overall, about 34% (37 out of 107) of the studies in our sample are based solely on U.S. data, while 16% (17 out of 107) are based on countries in the OECD and another 23% from EU only countries. Only 12 studies are global and include countries that span these other categories. Aside from the 37 US-based studies, there were 36 other studies published using data from only one country: four from India, three each from Japan, the Netherlands, and Sweden, two from Canada, China, Germany, Mexico, Spain, Switzerland, Taiwan, and the U.K., and one each from Austria, Brazil, France, Korea, New Zealand, and Norway.[9]

One of the common critiques of the existing literature is the fact that most studies are cross-sectional, thus making any causal inferences problematic (see, e.g., Lankoski, 2010; and Ambec et al., 2013), as the Porter Hypothesis anticipates a lag between the time that regulations are implemented, firms innovate, and competitive outcomes are realized (Porter and van der Linde, 1995: 108). Furthermore, as Brunel and Levinson (2016) emphasize, simultaneity presents a significant challenge in studies seeking to address the impact of environmental regulation on

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[8] In all four studies, effect sizes were either negative or statistically insignificant; consistent with the prevailing view at the time that environmental regulation was detrimental to competitiveness.

[9] All but 11 of these single-country studies are included in the earlier categories. The remaining countries outside the EU, Europe, and the OECD include Brazil, China, India, Mexico and Taiwan.

competitiveness, since competitiveness may simultaneously contribute to regulatory stringency. Researchers typically seek to address the simultaneity of environmental regulations via natural experiments and the use of instrumental variables. The inclusion of instrumental variables also serves to address the possible endogeneity of regulation severity; since more stringent environmental regulation is politically more acceptable and hence more likely to be enacted when firms are prosperous, the estimated effect of environmental regulation on firm/facility competitiveness and profitability may be biased upward. We found only about 25% of studies (27 out of 107) included a lagged independent variable measure for regulation and 13% (14 out of 107) used instrumental variables. However, approximately 70% (73 out of 107) of studies had at least one specification based on a panel dataset.

Dependent Variable Measures

Among the 107 studies, over 250 different dependent variable (i.e., "outcome level") measures were coded (although some of these are the same measures on different samples that were coded separately in order to estimate elasticities). Of the 150 outcome level variables at the firm or industry level, about 100 used output or productivity as the measure, while 50 relied upon accounting or stock price measures including intangible asset values. While most of the regional, state, or country-level dependent variables were based on international trade flows, other measures included growth in GDP and employment levels.

Environmental Regulation Measures

One of the tenets of the Porter Hypothesis is that innovation offsets – and hence positive productivity or competitiveness - are more likely to be realized under flexible environmental regulations such as emission fees or marketable permits. Unfortunately, most of the studies utilize a broad measure of environmental regulation that does not permit such a fine distinction and comparison. In the set of 107 studies, the type of regulation can only be identified with adequate specificity to identify "command and control" (technology standards) regulation in seven papers, while more "flexible" measures (emission standards, taxes, cap and trade approaches, subsidies, etc.) were identified in 32 publications. Emission taxes were identified in 13 publications and cap and trade programs in four papers.

While some of the regulation measures were directly focused on the number or type of regulations, others were proxies that might be correlated with regulation but are often far from ideal measures (Brunel and Levinson 2016). The two most prevalent measures were pollution control expenditures (whether measured by operating expenses, investments or taxes) and perceived measures of regulatory stringency (whether objective such as the number of regulations or subjective analyses of researchers or survey respondents) – each representing about 45% of all effect sizes. The remaining measures which were used in the literature as proxies for environmental regulation were pollution emissions (5%), government environmental enforcement or compliance expenditures (3%), and government environmental R&D expenditures (2%), though each of these measures are considered problematic (Brunel and Levinson 2016).

In most cases, the regulatory target was either unspecified or across media (i.e. multiple targets). Individual targets included water quality (contained in 18 publications), air quality (15), greenhouse gas emissions (10), waste (6), and energy efficiency (3).

VI. Findings

A. Comparison of Effect Sizes

We start with the basic question underlying the strong version of the Porter Hypothesis – does environmental regulation increase productivity or competitiveness? Table 5 reports on the direction of the relationship between environmental regulation and these outcomes. In this table, we do not require statistically significant findings – instead, we simply record whether the relationship is positive or negative. The "equal weighted" rows represent the fraction of effect sizes that are negative or positive within each study. Thus, each study is given a weight of one but the directional relationship may be expressed as a fraction within each study. For example, if there are four effect sizes estimated in a study and only one was positive, that study would be weighted 25% positive and 75% negative.

Using this approach, about 54% of publications measuring facility, firm or industry-level impacts find a negative relationship, while 43% of those measuring state, regional or country-level competitiveness find a negative relationship. The "preferred specification" rows use only one effect size per publication (based on coder judgment taking into account any claims made by the authors as well as

goodness of fit, model specification, and sample size).[10] As shown, about 54% of studies find a negative relationship between environmental regulation and productivity or profitability at the facility, firm or industry level, while 46% find a negative relationship at the regional, state or country level. When combined, positive and negative findings are about equal. However, once disaggregated, there appears to be a higher chance of finding a negative finding at the facility, firm or industry level, and a higher chance of a positive finding at the state, regional or country level.

Table 6 reports on a similar comparison taking into account the significance level of the estimated relationship. Thus, in the equal-weighted case, 27% of facility, firm or industry-level studies find a negative and statistically significant relationship, and 27% have a positive and significant finding. However, at the state, regional or country level, while 12% find a negative and statistically significant relationship, 28% of findings show a positive and statistically significant relationship. Using the preferred specification, at the facility, firm, or industry-level, there is more likely to be a negative and statistically significant relationship (32% versus 28%) while at the state, regional or country level, we are more likely to find a positive and statistically significant relationship (32% versus 14%). In all cases, however, the largest category is "insignificant." Once again, we find more support for the Porter Hypothesis at the state, regional or country level than at the firm or industry level.

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[10] Note that in some cases, more than one environmental regulation variable is included in a regression equation, or a paper only reports on multiple subsamples. In those cases, for example, if the preferred specification included one positive and one negative coefficient, each would be given a weight of 50%.

In addition to examining the sign of the effect sizes, we calculated elasticity in cases where adequate information existed in the studies. For example, if the dependent variable and regulation variable were both measured in natural logs in a regression, we can directly impute the coefficient as an elasticity. Similarly, if one of the variables are continuous and not in log form, we can compute an elasticity if we have the respective mean for that variable in the regression sample. Overall, we were able to estimate elasticities in less than half of the studies, 46 out of 107. As shown in Table 2, out of the 2,025 effect sizes, we were able to estimate an elasticity in about one-third (647) of the cases. These elasticity estimates allow us to place study findings on a similar footing and provide more information about the magnitude and economic significance of any findings. They also allow us to test the statistical significance of effect sizes.

As shown in the first row of Table 7, excluding about 4% of cases with outliers (elasticity estimates greater than 5 in absolute value), the average elasticity estimated in these 46 studies was +.15 (when all elasticities within a study are used), and +.11 when only preferred specifications are used.[11] These estimates are calculated by weighting each effect size by the square root of the number of observations that were used in the model that estimated the particular effect size, in

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[11] In some cases, there were multiple elasticities within the preferred model specification. This might occur because there was either more than one environmental regulation variable in a regression model, or there were two different samples being used in a paper and both were determined to be preferred specifications. In those cases, the "within publication" preferred specification elasticities were given equal weight.

addition to weights based on the number of effect sizes within each study.[12]   The

95% confidence interval for the elasticity using all specifications is +0.08 to +0.22.

However, once the sample is restricted to preferred specifications, the confidence

interval no longer remains positive, ranging from -0.11 to +0.32.

Because the dependent variables are considerably different across studies,

Table 7 also reports elasticities for different dependent variable specifications. The

dependent variable measured competitiveness (e.g., GDP, trade flows, or

employment) in 16 publications, profit (e.g., accounting or stock prices, value

added) in 12 publications, and output measures of productivity in 18 publications.

Notably, the elasticity estimates (both positive and negative) are considerably

smaller and the 95% confidence interval is between -0.08 and +0.01 when the

dependent variable is a measure of firm profit compared to when it is a measure of

either competitiveness or productivity.

Forest plots provide a useful graphical summary of mean effect sizes and

corresponding confidence intervals in meta-analyses. Effect sizes are typically

shown with blocks corresponding to the proportional weight assigned to each study.

Figure 1 shows a Forest plot of partial correlations and corresponding standard

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[12] While it would be preferable to calculate a weight for the effect sizes based on the
standard error of each elasticity estimate (perhaps combined with the number of
effect sizes within each publication), the standard error was only available for 12
publications (and only in 9 publications with preferred specifications). Instead, to
maintain a sufficient sample size of studies, we use the model's sample size to
weight each elasticity. See Stanley and Doucouliagos (2012: 46). Further, because
the sample sizes for elasticities found in these 46 studies ranged from 47 to 151,908,
we used the square root of sample size for weighting purposes, since otherwise a
few studies would entirely dominate the estimates. Alternative specifications of
weights do not significantly alter the basic finding that using the preferred
specification, the estimated elasticity is close to zero and statistically insignificant.

errors for the set of preferred specifications in each study.[13] We were able to

estimate 1303 partial correlations in 73 studies (189 partial correlations and 70

studies using only the preferred specifications). The partial correlation of the

preferred specification is displayed together with its confidence interval

represented by a horizontal line. The area of the square represents the weight that

the study contributes to the meta-analysis. Interestingly, the studies that contribute

the most weight to the meta-analysis are clustered around an effect size of zero.

Furthermore, taking into account the standard errors that cross zero, a slight

majority of estimated effect sizes are zero. Overall, about 52% of estimated effect

sizes are positive. Restricting the analysis to preferred specifications, 50% of the

effect sizes are positive. The unweighted mean effect size is +0.013 (range -.53 to

+.55), and +0.021 (range -.53 to +.51) using the preferred specifications. Weighting

each effect size by its standard error (and number of effect sizes within each study),

we can calculate a 95% confidence interval around these estimates. For the full

sample, the estimated mean effect size is +0.007 with a confidence interval of

[+0.003, +0.011]. Using only preferred specifications, the mean effect size is +0.0025

with a confidence interval of [-0.009, +0.015]. Thus, we cannot reject the hypothesis

that the underlying effect size is zero.[14]

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[13] Unlike in the case of elasticities, we are able to use the preferred weights
(standard errors), which are available in a large proportion of cases where we also
are able to estimate partial correlations.

[14] However, as noted above, samples sizes varied considerably across studies; a fact
that might substantially reduce standard errors and thus increase the weight given
to any particular study. As a robustness check, we weighted each effect size by the
square root of its corresponding sample size. However, this does not change our
finding that we cannot reject the hypothesis of a zero average effect size.

Publication bias – the preference for statistically significant results – is a pervasive issue in the social sciences (Stanley 2005; Stanley 2013). Indeed, given the potential for research surrounding the impact of environmental policy on competitiveness to significantly impact public policy discourse, it is important to examine the presence (or absence) of publication bias in studies empirically investigating the Porter Hypothesis. Figure 2 (full sample) and Figure 3 (preferred specifications only) consider the presence of potential publication bias via a "funnel plot" for all computable partial correlations and corresponding standard errors. Funnel plots graphically indicate possible publication bias by plotting an estimated effect on the horizontal axis along with its precision on the vertical axis. We would expect studies with the largest sample size to be plotted near average, since these studies are assumed to be more reliable and therefore less dispersed, and smaller studies spread evenly on both sides, therefore creating a distribution which approximates a funnel shape.[15]   Following Sterne and Egger (2001), we use the standard error of the computable partial correlations as the measure of study size. Given that in the absence of heterogeneity or selection bias, 95 percent of the studies should lie within the funnel defined by the pseudo 95% confidence limits, these funnel plots indicate the potential presence of publication bias (Sterne and Harbord 2004). Using the statistical test suggested by Egger (1997), we reject symmetry in the full sample (p < .01), but cannot reject symmetry in the sample based on preferred specifications (p =.20). Figures 2 and 3 provide some additional insights into the nature of the published studies on the Porter Hypothesis. First,

[15] See also Lipsey and Wilson (2001, 142-3).

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there are large clusters around the zero line. In some meta-analyses, just the

opposite is found – indicating a bias towards not publishing "null" findings. That

does not appear to be the case in this literature. Second, there are a significant

number of effect sizes outside the 95% confidence limits – but they are mostly the

larger sample sizes, not an indication of small sample bias. Instead, this is an

indication of considerable heterogeneity in study findings – something that is clear

from the tables that follow.

B. Why Do Different Studies Obtain Contradictory Findings?

As shown above, there is considerable heterogeneity in the findings across

studies and no dominant pattern. We are equally likely to find a positive, significant

result as a negative, significant result – at least in the case of facility, firm or

industry-level studies. In the case of state, regional or country-level studies, we are

more likely to find a positive relationship – although we are even more likely to find

a statistically insignificant relationship. To help understand why this mixed pattern

emerges, we return to the theory and empirical evidence summarized in Ambec et

al. (2013).

Tables 8 and 9 report on bivariate comparisons of the effect sizes and key

variables of interest. Table 8 assumes an equal weight for each effect size within a

study (similar to Table 5), while Table 9 uses the preferred specification from each

study (similar to Table 6).[16] First, we separate out the effect sizes by the unit of

analysis. For example, as shown in Table 8, out of 70 publications where the unit of

analysis was a facility, firm, or industry and we could determine the sign of the

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[16] Since Tables 8 and 9 report simple bivariate comparisons, the reader is cautioned against drawing any causal inferences from these findings.

effect size, 46% were positive. Similarly, as shown in Table 9, when the preferred

specification is used, 46% are positive. However, once the significance level is

considered, there is a positive and significant effect in only 29% of cases and a

negative and significant effect in 27% of cases (with the remaining 44% finding a

statistically insignificant effect). Similar findings are shown in Table 9 using the

preferred specification.

As noted earlier, the Porter Hypothesis specifically mentions that flexible

regulations are more likely to yield positive innovation offsets. Unfortunately, the

form of regulation is rarely specified in published studies, oftentimes because the

measures are comprehensive and include all forms of regulation (e.g. pollution

abatement and control expenditure or perceived regulatory stringency). In fact, as

shown in Table 4, only 7 out of 107 studies described regulatory types that could be

considered "command and control" (mostly technology standards or bans), while 32

out of 107 studies described the regulation type as being "flexible" (e.g. emission

standards, emissions tax or cap and trade system). The small number of instances

where such distinctions could be made makes it particularly difficult to test this

important hypothesis. Indeed, as shown in Table 8 while about 50% of studies with

"flexible" regulations have positive results, so do those with command and control

regulations. However, when restricted to studies where statistical significance is

known and only one preferred specification is used (Table 9), 32% of effect sizes

with flexible regulations are positive and statistically significant, compared to only

7% of those with command and control regulations. Thus, we are more likely to find

a positive, statistically significant result when the study examines the impact of flexible regulation as opposed to command and control regulation.

In addition, we note that more flexible and market-based regulations are a relatively newer approach to environmental protection. Thus, one proxy for flexible regulation (especially in studies that rely upon "mixed" types of regulation) might be the time frame of the study data. As shown in Table 3, about 33% (36/107) of the publications are based on data whose earliest year was 1996 or later. Anecdotal evidence cited in Ambec et al. (2013) suggests that we are more likely to find positive and statistically significant results in these newer studies. However, the data reported on in Tables 8 and 9 are not consistent with this finding. One possible explanation is that because countries have constantly ratcheted up the stringency of regulation over time, the studies with newer data might be reflective of these more stringent regulations. Put differently, studies based on earlier data might be looking at low hanging fruit.

One interesting finding is that studies which mention Porter are less likely to find negative and significant results and more likely to find positive and significant results than studies that do not mention Porter. For example, as shown in Table 8, 59% of all effect sizes are positive when Porter is mentioned, and 37% are positive and statistically significant (compared to 17% that are negative and statistically significant). This result may point to the existence of reporting bias in the sense that studies directly investigating the Porter Hypothesis are more likely to find a positive and significant relationship between environmental regulation and competitiveness relative to studies with an alternative primary research question (Nelson 2011). On

the other hand, it might simply indicate that authors who find a positive and significant correlation between regulation and competitiveness are more likely to cite Porter as a way to rationalize their otherwise non-intuitive findings.

Table 9 also reports on a statistical test of the hypothesis that preferred effect sizes are equally likely to be positive as negative.[17] We can reject this hypothesis in only two such comparisons. First, when data is U.S. based, effect sizes are more likely to be negative (36% negative versus 64% positive, $p < .05$). Second, as suggested above, when Porter is mentioned, effect sizes are more likely to be positive (59% positive versus 41% negative, $p < .05$).

Finally, we note the concern in the literature about the possibility of endogeneity and simultaneity with respect to an economy's competitiveness and environmental regulatory stringency (see e.g., Ambec et al., 2013; Brunel and Levinson, 2016). For example, we expect higher demand for environmental protection in wealthier economies. Further, the Porter Hypothesis suggests there is a lag between the time of enactment of environmental regulation, subsequent innovation, and finally observable improvements in competitiveness. Thus, we analyzed the extent to which studies accounted for this lag structure as well as whether or not they attempted to explicitly account for simultaneity through an instrumental variables approach.

As shown in Table 8, only 25% (27 out of 107) of studies included a lagged regulation variable in their model; and 16% (17 out of 107) used instrumental

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[17] See Bushman and Wang (2009: 209-10). We conduct a nonparametric sign test of the hypothesis that the estimated effect size is greater than 0. Note that we have not conducted this test for Table 8 because there are many correlated effect sizes within each publication.

variables as a method to proxy for environmental regulation. The results are again mixed. In particular, we are more likely to find positive effect sizes when studies are based on lagged regulations; whereas negative effect sizes are more likely when using instrumental variables. For example, as shown in Table 9, when restricted to preferred specifications, about 51% of effect sizes based on lagged regulations are positive (and 49% are negative, p > .10); while 36% are positive and statistically significant compared to only 23% that are negative and statistically significant. On the other hand, when instrumental variables are used to proxy for environmental regulation, about 40% of effect sizes are positive (and 60% are negative, p > .10); while 37% are negative and statistically significant compared to only 16% that are positive and significant. Once again, however, about half of effect sizes are statistically insignificant. While this is a small sample, the findings are similar to the overall results – studies are just as likely to find positive as negative effect sizes, and are more likely to find statistically insignificant results than either positive or negative statistically significant results.

To further investigate the nature of heterogeneity across studies and to control for factors that might affect the sign of the estimated effect sizes, we estimated a series of random effects, weighted meta-regression models with dependent effect size estimates (Hedges, Tipton & Johnson, 2010). This method (robumeta in STATA) accounts for the fact that the variance estimates of within-study effect sizes are not only correlated but likely to be of similar precision due to the fact that effect sizes are likely to have similar sample sizes within study than

across study. In addition to accounting for within-study effect size correlations, individual effect sizes are weighted by their standard errors.

Table 10 reports on a series of meta-regressions where the dependent variable is defined as the partial correlations.[18] Model 1 includes only the "flexible regulation" variable, along with an indicator for studies that are conducted at the regional, state or country level (as opposed to firm for facility level). While we find no statistically significant coefficient for the effect of flexible regulation, the region, state or country level coefficient is positive and statistically significant (p < .01).

Model 2 adds several methodological control variables. First, we include an indicator for whether or not the regulation measure is a lagged variable, an important test of the Porter Hypothesis which predicts a lag from when a regulatory policy is first introduced to when it might induce innovation – ultimately leading to increased competitiveness. While the coefficient on lagged regulation is consistently positive, it is statistically insignificant. Second, as control variables, we include indicators for regression models that include an instrumental variable to account for endogeneity, panel data sets, and the preferred model within each publication. None of these methodological variables are statistically significant, and they do not affect the significance level or signs of the other variables in the model.

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[18] Note: we also attempted to estimate this model using elasticities as the effect sizes. While we had 589 elasticity estimates over 44 studies, there were only 91 observations over 13 studies with elasticity variance estimates. This is too small a sample to obtain estimates using robumeta.

Model 3 adds several control variables that examine the heterogeneity of the Porter Hypothesis studies by country, industry,[19] year, outcome, and regulatory measures. Once again, none of these control variables are statistically significant, and their inclusion does not affect the other variables in any meaningful way.

VII. Summary of Findings and Concluding Remarks

This paper reports on a systematic meta-analysis of 107 studies that empirically estimate the relationship between environmental regulation and measures of facility, firm, industry, state, regional or country-level productivity or competitiveness. These 107 studies yielded over 2,000 unique "effect sizes." We find

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[19] We have defined "most polluting industries" to include 2-digit SIC codes mining (10, 12, 14), petroleum (13, 29), pulp and paper (26), chemicals (28), iron and steel, non-ferrous metals (33), fabricated metals except machinery and transportation (34), and utilities and waste services (49). Samples that exclude all of these industries are considered "least polluting," with remaining studies being considered "mixed." Although there is no single definition of "pollution intensive" or "dirty" industries, numerous authors have utilized these distinctions in their analysis. Moreover, while there are some differences in definitions, there are also considerable similarities. Jaffe et al. (1995) define pollution-intensive industries based on their abatement cost. For example, while they report PACE as a percentage of total capital expenditures to be 7.5% overall, four industries had 11% or more – Paper and Allied Products, Chemical and Allied Products, Petroleum and Coal Products, and Primary Metal Industries (Jaffe et al., Table 6). Fabricated Metal Products was in their "moderate" category (4.35%), although other authors include this in the "pollution-intensive" sector (see e.g. List and Co, 2000). Similar approaches have been followed by Greenstone (2002) and Morgan and Condliffe (2009), the latter of which have a criterion that to be included in the pollution-intensive industry category, "they emit at least 6 percent of the total sector's emissions of the primary chemical precursors to ozone." Note that our definition of "least polluting" simply means that none of the firms in that category fall within the "most polluting" category.

considerable heterogeneity in both the sign and significance level of estimated effect sizes across studies. We test the strong form of the Porter Hypothesis – postulating that well designed environmental regulations may lead to innovation that in some cases results in net productivity gains and increased competitiveness. On average, over the 107 studies and 2,025 effect sizes, we find that the relationship between environmental regulation and productivity or competitiveness is about equally likely to be positive as it is negative. However, we also find considerable heterogeneity. These mixed findings have been documented in earlier literature reviews, and contribute to the oftentimes conflicting policy statements we observe about the impact of environmental regulation on firm or country competitiveness.

One relatively consistent finding in our data is that while the likelihood of finding a positive or negative relationship is about equal in the aggregate, we are more likely to find a negative relationship at the facility, firm or industry level and more likely to find a positive relationship at the state, regional or country level. For example, using the preferred specification (Table 9), at the facility, firm or industry-level, there is more likely to be a *negative* and statistically significant relationship (32% versus 28% positive) while at the state, regional or country level, we are more likely to find a *positive* and statistically significant relationship (32% versus 14% negative). In all cases, however, the largest category is "insignificant." To the extent the Porter Hypothesis as originally formulated focused on the competitiveness of

nations, this finding is particularly interesting and worthy of further exploration (Porter 1991).[20]

The evidence on whether "flexible" regulations such as emission standards, emission fees and cap-and-trade programs are any more or less likely to result in a positive and statistically significant relationship than other regulatory approaches is somewhat mixed. First, we note that differentiating between "flexible" and "command and control" regulation was only possible in about 30% of the studies (32 out of 107).[21] Moreover, true market-based regulations (i.e. emission taxes or cap and trade) were identified in only 17 studies. While about half of the studies with flexible regulations show positive results – so do those with command and control regulation. However, when restricted to findings where the level of statistical significance is known, flexible regulations are much more likely to exhibit positive and statistically significant results than command and control regulations.

Finally, studies that include a lagged regulatory variable are more likely to find positive and significant results. This is particularly important in the context of the Porter Hypothesis because it postulates that while there might be short-term costs associated with new regulations, firms will adjust and in the long-term become

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[20] Though a sizable literature investigating the Porter Hypothesis has focused on the impact of environmental regulation on industry and sectoral competitiveness, Porter's original 1991 paper discussed the impact of environmental standards on the competitiveness of American industry in international markets.
[21] One possible avenue for future research is to augment our data with independent assessments of the format of regulation in each study. This would require bringing information and/or expert opinion outside the studies themselves. For example, while a study of water pollution regulation in several European countries might not identify whether the form of regulation was command and control or flexible, legal experts in those countries might be able to classify them accordingly.

more competitive. Thus, a negative contemporaneous and positive long-term

relationship is to be expected.

Figure 1 - Forest Plot of Partial Correlations (preferred specification)

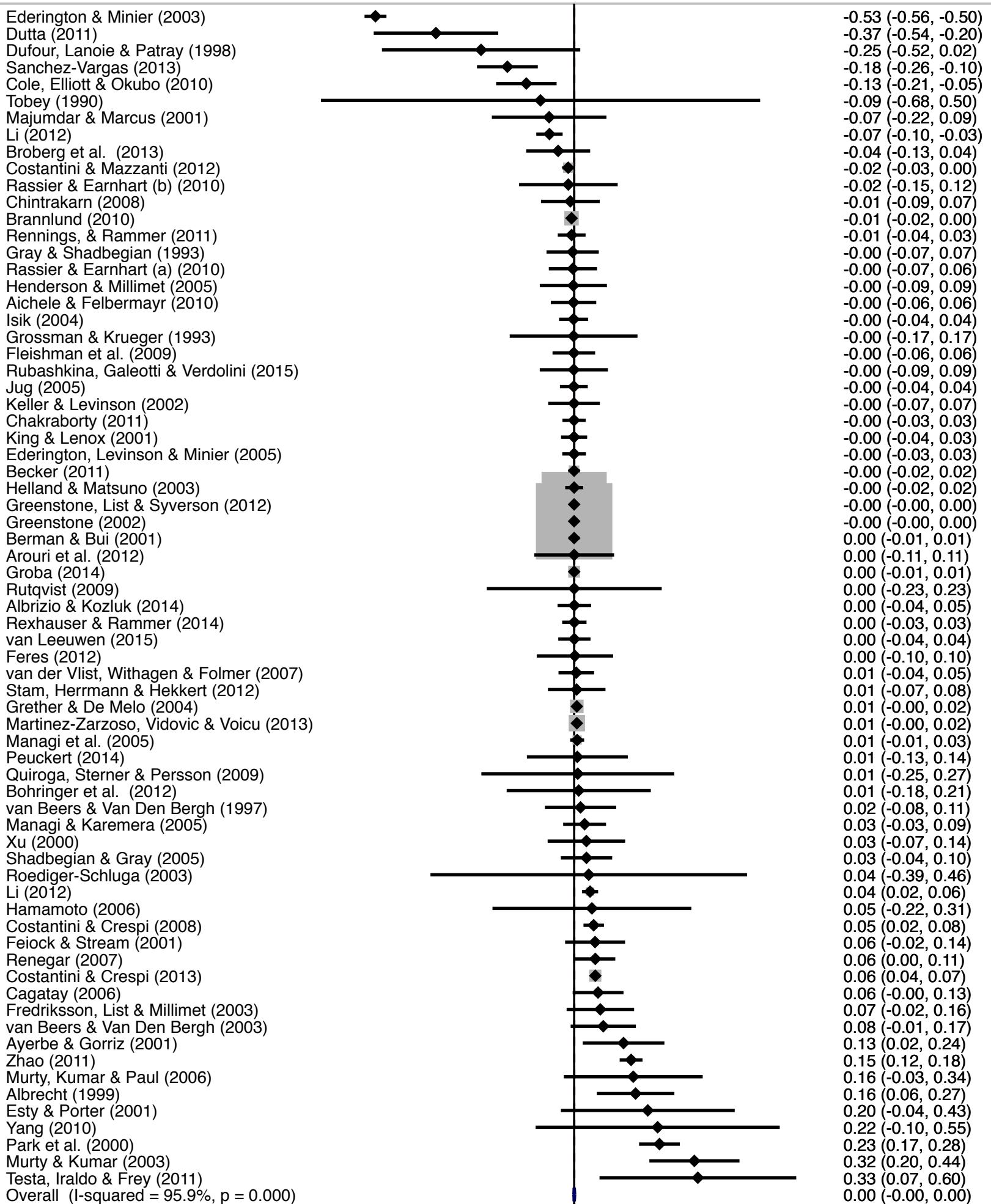| Study ID	ES (95% CI)
Ederington & Minier (2003)	-0.53 (-0.56, -0.50)
Dutta (2011)	-0.37 (-0.54, -0.20)
Dufour, Lanoie & Patray (1998)	-0.25 (-0.52, 0.02)
Sanchez-Vargas (2013)	-0.18 (-0.26, -0.10)
Cole, Elliott & Okubo (2010)	-0.13 (-0.21, -0.05)
Tobey (1990)	-0.09 (-0.68, 0.50)
Majumdar & Marcus (2001)	-0.07 (-0.22, 0.09)
Li (2012)	-0.07 (-0.10, -0.03)
Broberg et al. (2013)	-0.04 (-0.13, 0.04)
Costantini & Mazzanti (2012)	-0.02 (-0.03, 0.00)
Rassier & Earnhart (b) (2010)	-0.02 (-0.15, 0.12)
Chintrakarn (2008)	-0.01 (-0.09, 0.07)
Brannlund (2010)	-0.01 (-0.02, 0.00)
Rennings, & Rammer (2011)	-0.01 (-0.04, 0.03)
Gray & Shadbegian (1993)	-0.00 (-0.07, 0.07)
Rassier & Earnhart (a) (2010)	-0.00 (-0.07, 0.06)
Henderson & Millimet (2005)	-0.00 (-0.09, 0.09)
Aichele & Felbermayr (2010)	-0.00 (-0.06, 0.06)
Isik (2004)	-0.00 (-0.04, 0.04)
Grossman & Krueger (1993)	-0.00 (-0.17, 0.17)
Fleishman et al. (2009)	-0.00 (-0.06, 0.06)
Rubashkina, Galeotti & Verdolini (2015)	-0.00 (-0.09, 0.09)
Jug (2005)	-0.00 (-0.04, 0.04)
Keller & Levinson (2002)	-0.00 (-0.07, 0.07)
Chakraborty (2011)	-0.00 (-0.03, 0.03)
King & Lenox (2001)	-0.00 (-0.04, 0.03)
Ederington, Levinson & Minier (2005)	-0.00 (-0.03, 0.03)
Becker (2011)	-0.00 (-0.02, 0.02)
Helland & Matsuno (2003)	-0.00 (-0.02, 0.02)
Greenstone, List & Syverson (2012)	-0.00 (-0.00, 0.00)
Greenstone (2002)	-0.00 (-0.00, 0.00)
Berman & Bui (2001)	0.00 (-0.01, 0.01)
Arouri et al. (2012)	0.00 (-0.11, 0.11)
Groba (2014)	0.00 (-0.01, 0.01)
Rutqvist (2009)	0.00 (-0.23, 0.23)
Albrizio & Kozluk (2014)	0.00 (-0.04, 0.05)
Rexhauser & Rammer (2014)	0.00 (-0.03, 0.03)
van Leeuwen (2015)	0.00 (-0.04, 0.04)
Feres (2012)	0.00 (-0.10, 0.10)
van der Vlist, Withagen & Folmer (2007)	0.01 (-0.04, 0.05)
Stam, Herrmann & Hekkert (2012)	0.01 (-0.07, 0.08)
Grether & De Melo (2004)	0.01 (-0.00, 0.02)
Martinez-Zarzoso, Vidovic & Voicu (2013)	0.01 (-0.00, 0.02)
Managi et al. (2005)	0.01 (-0.01, 0.03)
Peuckert (2014)	0.01 (-0.13, 0.14)
Quiroga, Sterner & Persson (2009)	0.01 (-0.25, 0.27)
Bohringer et al. (2012)	0.01 (-0.18, 0.21)
van Beers & Van Den Bergh (1997)	0.02 (-0.08, 0.11)
Managi & Karemera (2005)	0.03 (-0.03, 0.09)
Xu (2000)	0.03 (-0.07, 0.14)
Shadbegian & Gray (2005)	0.03 (-0.04, 0.10)
Roediger-Schluga (2003)	0.04 (-0.39, 0.46)
Li (2012)	0.04 (0.02, 0.06)
Hamamoto (2006)	0.05 (-0.22, 0.31)
Costantini & Crespi (2008)	0.05 (0.02, 0.08)
Feiock & Stream (2001)	0.06 (-0.02, 0.14)
Renegar (2007)	0.06 (0.00, 0.11)
Costantini & Crespi (2013)	0.06 (0.04, 0.07)
Cagatay (2006)	0.06 (-0.00, 0.13)
Fredriksson, List & Millimet (2003)	0.07 (-0.02, 0.16)
van Beers & Van Den Bergh (2003)	0.08 (-0.01, 0.17)
Ayerbe & Gorriz (2001)	0.13 (0.02, 0.24)
Zhao (2011)	0.15 (0.12, 0.18)
Murty, Kumar & Paul (2006)	0.16 (-0.03, 0.34)
Albrecht (1999)	0.16 (0.06, 0.27)
Esty & Porter (2001)	0.20 (-0.04, 0.43)
Yang (2010)	0.22 (-0.10, 0.55)
Park et al. (2000)	0.23 (0.17, 0.28)
Murty & Kumar (2003)	0.32 (0.20, 0.44)
Testa, Iraldo & Frey (2011)	0.33 (0.07, 0.60)
Overall (I-squared = 95.9%, p = 0.000)	0.00 (-0.00, 0.00)

-.679   0   .679

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Figure 2
Funnel Plot of Partial Correlations
(full sample)

Figure 3
Funnel Plot of Partial Correlations
(preferred specifications only)

Funnel plot with pseudo 95% confidence limits

Table 1

Disposition of Research Publications During Search Process

	Number
Total papers reviewed	1,055
Empirical papers related to PH	649
- Not eligible: Literature reviews	14
- Not eligible: Voluntary programs	27
- Not eligible: Ineligible empirical methodology	165
- Not eligible: Other (no outcome measure related to PH)	339
- Eligible publications	103

Table 2
Number of Studies, Effect Sizes & Elasticity Estimates by Outcome Measures
(107 studies that include productivity or competitiveness measures)

	Studies		Studies with Elasticity Estimates	
	Number	# Effect Sizes	Number	# Elasticity Estimates
Facility/firm/ industry-level	70	1,059	33	313
- Facility/firm	(45)	(749)	(22)	(222)
- Industry	(25)	(310)	(11)	(101)
State/regional or country-level	37	966	13	334
Combined	107	2,025	47	647

Table 3
Publication-level Characteristics

	Facility, Firm or Industry	State, Regional or Country	Combined
Earliest Year of Data	Number of Publications		
Prior to 1976	11	5	16
1976-1980	8	5	13
1981-1985	5	3	8
1986-1990	11	5	16
1991-1995	12	6	18
1996-2000	12	11	23
2001-2005	9	2	11
2006+	2	-	2
	70	37	107
Year of Publication			
1980-1985	2	-	2
1986-1990	2	1	2
1991-1995	2	2	4
1996-2000	2	5	7
2001-2005	19	10	30
2006-2010	20	8	28
2011-2015	23	12	35
	70	37	107
Mention Porter?			
Yes	48	21	69
Country			
OECD only	10	7	17
Europe (including countries outside EU)	1	1	2
EU only	22	3	25
US only	23	14	37
Global	3	9	12
Other single country	9	2	11
Total	70	37	107
Funding Source			
NSF-type source	17	9	26
Business/commerce	5	2	7
Environmental	13	4	17
No funding mentioned	35	22	57
Total	70	37	107

Table 4
Environmental Regulation Characteristics

	Facility, Firm or Industry-level	State, Regional or Country-level	Combined
Type of Regulation			
Command and control	7	0	7
Flexible	25	7	32
- Emission tax	5	8	13
- Cap & trade	2	2	4
Media			
Air (other than GHG)	11	4	15
Greenhouse gas emissions	6	4	10
Energy efficiency	2	1	3
Waste	6	0	6
Water	14	4	18
Other (land, pesticides, etc.)	5	3	8
Unknown or more than 3	26	21	47
Total	70	37	107

Note: If a publication contains more than one type of regulation or one media, each one that appears is counted as being in the publication.

Table 5
Direction of the Relationship between Environmental Regulation and
Financial Outcomes

	Facility, Firm or Industry-level		State, Regional or Country-level		Combined	
All Specifications						
	Number	%	Number	%	Number	%
Negative	37.9	54%	15.9	43%	53.1	50%
Positive	32.1	46%	21.1	57%	53.9	50%
# publications	70	100%	37	100%	107	100%
Preferred Specifications						
	Number	%	Number	%	Number	%
Negative	37.5	54%	17.0	45.8%	54.5	52%
Positive	31.5	45%	20.0	54.2%	51.5	48%
# publications	69	100%	37	100%	106	100%

Table 6
Percent of Effect Sizes that are Significant & Direction Relationship between
Environmental Regulation & Financial Outcomes

	Firm or Facility-level Productivity or Profitability	State, Regional or Country-level Competitiveness	Combined
All Specifications			
Negative, Sig.	27%	12%	22%
Insignificant	46%	60%	51%
Positive, Sig.	27%	28%	27%
# publications	67	36	103
Preferred Specifications			
Negative, Sig.	32%	14%	26%
Insignificant	40%	54%	45%
Positive, Sig.	28%	32%	29%
# publications	65	36	101

Table 7
Estimated Elasticity of Competitiveness, Profit, and Productivity on Environmental Regulation

Dependent Variable	Elasticity Range	All Specifications		Preferred Specifications	
		# Pubs	Elast. [95% CI]	# Pubs	Elast. [95% CI]
Combined	-4.47, +4.38	46	+.15 [+.08,+.22]	41	+.11 [-.11, +.32]
- Competitiveness	-4.12, +4.38	16	+.20 [+.08,+.33]	13	+.23 [-.20, +.67]
- Profit	-2.32, +0.55	12	-.02 [-.05, +.01]	12	-.03 [-.08, +.01]
- Output/productivity	-4.47, +3.36	18	+.22 [+.07,+.37]	16	+.02 [-.45, +.49]

Note: The first row represents all dependent variable specifications. The second row restricts the sample to effect sizes where the dependent variable was a measure of competitiveness. The third row represents effect sizes that measure profits or firm valuation, while the last row represents effect sizes that measure output or productivity. Outliers (29 cases out of 647 with an elasticity of greater than 5 in absolute value) have been eliminated. In some cases, there were multiple elasticities within the preferred model specification. This might occur because there was either more than one environmental regulation variable in a regression model, or there were two different subsamples being used in a study, and both were determined to be preferred specifications. In cases with multiple "preferred specification" elasticities, all such estimates within that study were given equal weight. However, all effect sizes have also been weighted by the square root of the number of observations that were used in the model which estimated that particular effect size.

Table 8
Bivariate Comparisons
(Equally weighted effect sizes within each publication)

	Full sample		Significance level known			
	N	% Positive	N	% Negative Significant	% Insignif.	% Positive Significant
Level of Analysis						
- Facility, firm or industry	70	46%	70	27%	44%	29%
• Facility or firm	45	47	45	25	47	28
• Industry or sector	25	43	25	30	36	33
- State, region or country	37	57	37	12	59	29
Type of Regulation						
- Command & control	7	52	7	28	46	25
- Flexible	32	47	32	28	40	32
- Emission standard	15	33	15	31	49	20
- Emission tax	9	56	9	18	57	25
- Cap & trade	4	58	4	15	35	50
- Mixed/unknown	81	51	81	17	53	29
Regulatory Measure						
- Abatement costs	51	46	51	23	48	29
- Regulatory stringency	46	52	46	21	54	25
- Enforcement	13	52	13	16	58	26
- Government R&D	3	100	3	0	13	87
- Emissions as proxy	3	84	3	7	21	72
Other Controls						
- U.S. data	37	36	37	28	57	15
- Start year >=1996	36	57	36	23	49	28
- Porter mentioned	69	59	69	17	46	37
Modeling Strategy						
- Instrumental variables	17	37	17	35	49	16
-Lagged regulation variable	27	53	27	17	45	37

Table 9
Bivariate Comparisons
(One preferred specification per publication)

	Full sample		Significance level known			
	N	% Positive	N	% Negative Significant	% Insignif.	% Positive Significant
Level of Analysis						
- Facility, firm or industry	69	46%	65	32%	46%	28%
• Facility or firm	45	47	43	34	42	24
• Industry or sector	24	43	22	28	36	35
- State, region or country	37	54	36	14	54	32
Type of Regulation						
- Command & control	7	50	7	36	57	7
- Flexible	32	50	29	31	37	32
- Emission standard	15	36	13	33	52	15
- Emission tax	8	62	7	21	36	43
- Cap & trade	4	38	4	25	38	38
- Mixed/unknown	77	49	74	22	47	31
Regulatory Measure						
- Abatement costs	50	43	48	29	40	30
- Regulatory stringency	44	53	42	25	51	24
- Enforcement	11	54	10	20	47	33
- Government R&D	3	100	3	0	0	100
- Emissions as proxy	3	83	3	3	23	73
Other Controls						
- U.S. data	37	36**	34	36	51	13
- Start year >=1996	36	55	36	25	45	30
- Porter mentioned	69	59**	66	19	44	37
Modeling Strategy						
- Instrumental variables	17	40	17	37	47	16
- Lagged regulation variable	23	51	21	23	41	36

** p < .05, testing the hypothesis that p(positive) = p(negative)

Note: Although this table is restricted to one preferred specification per study, some specifications include more than one environmental regulation measure as an independent variable. Thus, for example, one regression equation might have two environmental regulation variables with one having a positive coefficient and one being negative. In some cases, there is also more than one "preferred" specification – for example, in a publication where only three subsamples are reported and no full sample estimates are made, all three subsamples might be considered preferred and be equally weighted.

Table 10: Meta-Regression
Dependent Variable: Partial Correlations

	Model 1	Model 2	Model 3
Region/state/country	.045***	.059***	.052***
Flexible regulation	-.006	-.011	-.018
Methodological Variables			
Lag regulation		.015	.020
Instrumental var.		-.069	-.075
Panel data		-.020	-.023
Preferred model		.010	-.05
Sample Characteristics			
US only			-.0006
OECD			.049
EU			.001
Start year >1996			.002
Most polluting			.027
Least polluting			.056
Outcome Measure			
Output			.011
Regulation Measure			
Cost			-.004
Stringency			.019
Enforcement			.029
Emissions proxy			.155
Constant	-.003	.022	-.014
N	1303	1303	1303

* $p < .10$; ** $p < .05$; *** $p < .01$

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Appendix A
103 Publications in Meta-Analysis

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Appendix B
Publication Sources for 103 Porter Hypothesis Papers

Source	Number
Working Papers	16
Ecological Economics	10
Environmental and Resource Economics	5
Dissertations	4
Journal of Productivity Analysis	3
Journal of Public Economics	3
Book Chapters	2
Economics Letters	2
Energy Policy	2
Journal of Environmental Management	2
Journal of Political Economy	2
Kyklos	2
Land Economics	2
Public Administration Review	2
Research Policy	2
Resource and energy economics	2
Review of Economics and Statistics	2
The World Economy	2
Academy of Management Journal	1
Agribusiness	1
Agricultural Economics	1
American Journal of Agricultural Economics	1
Applied Economics	1
Business strategy and the environment	1
Canadian Journal of Economics	1
Contemporary Economic Policy	1
Energy Procedia	1
Environment and Development Economics	1
Environmental Economics and Policy Studies	1
Environmental Innovation and Societal Transitions	1
European Journal of Law and Economics	1
Frontiers of Economics in China	1
Growth and Change	1
Industry and Innovation	1
Int'l Journal of Agricultural Resources, Governance and Ecology	1
International Journal of Development Planning Literature	1
International Journal of Global Environmental Issues	1
International Journal of Production Economics	1
International Journal of the Economics of Business	1

Journal of Applied Economics	1
Journal of Cleaner Production	1
Journal of Economic studies	1
Journal of Economics & Management Strategy	1
Journal of Environmental Economics and Management	1
Journal of Environmental Law & Practice	1
Journal of Evolutionary Economics	1
Journal of Industrial Ecology	1
Journal of Management and Governance	1
Journal of Regional Science	1
Journal of Regulatory Economics	1
Management Decision	1
Science Technology & Society	1
The American Economic Review	1
The Bell Journal of Economics	1
The International Trade Journal	1
The Journal of Industrial Economics	1

Appendix C
Authors with Multiple Papers in Meta-Analysis

Author	Affiliation at time of Publications	Co-Authors	# Pubs
Earnhart, Dietrich	Univ. of Kansas	Rassier, D.	4
Gray Wayne B.	Clark Univ.	Shadbegian RJ	4
Managi, Shunsuke	Tokyo Univ. of Agriculture & Tech.	Opaluch JJ;Jin D;Grigalunas TA;	4
Rassier, Dylan G.	U.S. Dept. of Commerce; PriceWaterhouseCoopers	Earnhart, D.	4
Costantini, Valeria	Università degli Studi Roma Tre (Italy)	Crespi, F.	3
Lanoie, Paul	École des Hautes Études Commerciales (Canada)	Dufour, C; Patry, M.; Lajeunesse; Laurent-Lucchetti, Johsntone, N; Ambec, S.	3
Shadbegian, Ronald	U.S. Bureau of Census; Univ. of Massachusetts	Gray, W.	3
Crespi, Francesco	Università degli Studi Roma Tre (Italy)	Costantini, V.	2
Ederington Josh	Univ. of Miami; Univ. of Kentucky	Levinson A;Minier J;	2
Greenstone, Michael	Univ. of Chicago; MIT	List, J.; Syverson, C.	2
Levinson, Arik	Georgetown Univ.	Keller, W.; Ederington, Josh; Minier, J.	2
List, John	Univ. of Chicago, Univ. of Maryland	Greenstone, M.; Syverson, C.	2
Lundgren, Tommy	Umeå University (Sweden)	Marklund, ; Brannlund, R.	2
Marklund, Per-Olov	Umeå University (Sweden)	Lundgren, P. Broberg, T; Samakovlis, E. Hammar, H.	2
Millimet, Daniel	Southern Methodist Univ.	Fredriksson, P.G.; List, John; Henderson, D.J.	2
Minier, Jenny	Univ. of Kentucky; Univ. of Miami	Ederington, J; Levinson, A.	2
Murty M. Narshima	Delhi Univ. (India)	Kumar S; Paul, M.	2
Patry, Michel	École des Hautes Études Commerciales (Canada)	Dufour, C; Lanoie, P; Lajeunesse, R.	2
Rammer, Christian	Centre for European Economic Research (ZEW), Germany	Rennings, K. Rexhauser, S.	2

Rennings Klaus	Centre for European Economic Research (ZEW), Germany	Rammer C; Ziegler, A.; Zwick, T.	2
van Beers, Cees	Leiden Univ. (Netherlands); Delft Univ. (Netherlands)	Van Den Bergh, Jeroen;	2
Van Den Bergh, Jeroen	Vrije Univ. (Netherlands)	van Beers, Cees	2
Ziegler, Andreas	Centre for European Economic Research (ZEW), Germany; Univ. of Kassel (Germany)	Bohringer, C; Moslener, U., Oberndorfer, U., Rennings, K. Zwick, T.	2