Is there a Replicability Crisis on the Horizon for Environmental and Resource Economics?

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Environmental and resource economists pride themselves on the credibility of their empirical research. However, in other areas of the social and behavioral sciences, the credibility of empirical research is increasingly being debated by scholars. At the core of this debate are critiques of common research practices and of the professional incentives that encourage these practices. Widespread features of empirical science, such as low statistical power and selective reporting of data or results, are allegedly contributing to a "replicability crisis." We report *that these features are prevalent in the environmental and resource economics literature, and we argue that the discipline needs to take them more seriously. To help reduce their prevalence, we suggest changes in the norms and practices of funders, editors, peer reviewers, and authors.*

Keywords: replication crisis, reproducibility crisis, p-hacking, questionable research practices, exaggeration bias

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I. Introduction

The design of effective environmental policies and programs relies on credible scientific knowledge. Yet whether scientists have the right incentives to produce such knowledge is being questioned by a growing number of scholars (Baker 2016). Their concerns are often grouped under the umbrella term of a "replicability crisis," which posits that many empirical results in the scientific literature are false and would fail to replicate, were such replications more common. At the heart of the replicability crisis is a mix of questionable research practices and publication biases, particularly biases against studies that replicate prior studies or yield results that underwhelm (e.g., "null" results) or are contrary to prior studies.

Given that economics publications routinely inform public policy, a replicability crisis has important practical implications. Practitioners and policymakers may adopt or scale up interventions whose true impacts are different from those reported in published studies. For example, in a government's cost-benefit analysis of a regulation that aims to reduce air pollution, economists must first determine the likely impact of the regulation on emissions. To do so, they often turn to peer-reviewed publications that assess the impacts of analogous regulations. If the estimates in these publications are systematically exaggerated, the cost-benefit analysis will exaggerate the benefits from the new regulations. Were such problems widespread, a replicability crisis could subsequently create a wider crisis of confidence in the value of economics research for environmental policymaking.

To evaluate whether environmental and resource economists, and the practitioners who rely on their research, ought to be more concerned about a potential replicability crisis, we report results from a survey on research practices among environmental and resource economists and results from an empirical analysis of publications in top environmental and resource economics journal.

First, the empirical designs used by environmental and resource economists are often substantially underpowered. This feature leads to unreliable estimated effect sizes and, when combined with publication biases against statistically insignificant results, exaggerated effect sizes. Second, we report evidence of selective reporting of statistically significant results in the literature. Third, most published studies engage in multiple hypothesis testing with no efforts to inhibit false discoveries, a practice that weakens the credibility of the conclusions in these publications. Finally, substantial fractions of survey respondents reported engaging in what other scholars have labeled "questionable research practices," including selecting which results and research hypotheses to present only after the results are known.

To some scholars, these features of the environmental and resource economics literature would imply that the literature's published empirical results are, on average, unreliable and exaggerated (similar arguments have been made about science more generally). Although we do not believe this degree of pessimism is warranted, we do believe there are problems to which our discipline needs to attend, and we recommend that funders, editors and peer reviewers change the norms and incentives that environmental and resource economists face.

II. Sample Selection and Statistical Methods

We searched four journals for relevant studies: *The American Economic Review (AER)*, *Environmental and Resource Economics (ERE)*, *The Journal of the Association of Environmental and Resource Economics (JAERE)*, and *The Journal of Environmental Economics and Management (JEEM)*. To be selected, a study had to be published in the period 2015-2018, use regression analyses, and report the coefficient estimates and their standard errors. Three hundred and seven publications reporting over 21,000 estimates met our selection criteria.

With these studies, we constructed a data set that comprises only the main estimates reported and discussed in the articles. To do so, we follow Brodeur et al. (2016)'s definition of "main estimates," which excludes estimates of constants and control variables that are not discussed in the articles, as well estimates from summary statistic tables, placebo tests, or analyses in appendices.

To characterize the statistical power of these studies, we followed the approach of Ioannidis et al. (2017). After standardizing the reported estimates to make them comparable (as partial correlation coefficients), we calculated a plausible estimate of the true effect size that these studies are seeking to estimate. We then assessed the statistical power of the study designs to detect this effect size.

To assess the evidence for selective reporting of statistically significant results, we followed the approach of Brodeur et al. (2016). We look for an unexpected distribution of test statistics right around the conventional cut-off level for declaring an estimate to be "statistically significant" (p=0.05).

For more details on methods, the list of included studies, and links to the data and analysis code, see the online Materials and Methods (M&M).

III. Underpowered Designs and Exaggeration Bias

To yield policy-relevant conclusions, empirical studies need adequate statistical power. The statistical power of a study design is the probability of rejecting a null hypothesis when it is false. Often the null hypothesis in environmental economics is that a relationship between two variables is equal to a specific value, like zero. For example, in a study that estimates the average effect of a regulation on $PM_{2.5}$ emissions, the null hypothesis may be that the regulation had zero effect on emissions. If that hypothesis were, in truth, false, a study design with high statistical power would have a good chance of rejecting that hypothesis.

In other words, study designs with high power yield lower rates of false negatives (Type II errors), and thus they are more likely to detect a policy-relevant relationship between two variables, should one exist. Study designs with high power also yield more precise effect estimates (i.e., less uncertainty about the true magnitude of the effect), and thus they place tighter constraints on the range of potential magnitudes for the relationship. In policy design, magnitudes matter.

Another implication of these ideas is that, in underpowered designs, the sample distribution of estimated effects is highly variable – in other words, if the study design is repeated many times with new data, the estimated effects from each analysis would vary a lot. For example, consider an underpowered design that aims to estimate the average effect of a regulation on $PM_{2.5}$ emissions, where the true effect is a decrease in emissions of 7%. In practice, this design will yield a range of estimates that imply anything from a large reduction to a modest-sized increase in pollution. A high-powered design is much more likely to generate estimates within a narrow range around 7%.

In other words, underpowered designs tend to yield more extreme estimates than designs with high statistical power. Thus, in comparison to estimates from highly powered designs, estimates in underpowered designs have a higher probability of being much larger in magnitude than the true effects and of being the wrong sign (Button et al. 2013; Gelman and Carlin 2014).

Such variance is not necessarily a problem if replications are common and all estimated effects are published – we would be able to see the range of estimates published and calculate a weighted average estimate. However, there is a well-known bias against publishing replications and studies with statistically insignificant estimates (De Long and Lang 1992; Doucouliagos and Stanley 2013).

This bias, when mixed with underpowered designs, can lead to pervasive exaggerated effect sizes in the published literature. In other words, for an estimate in an underpowered design to be declared "statistically significant," it must be much larger than the true effect. So if most studies are underpowered, and studies are more likely to be published when they declare results that are "statistically significant," the published effect sizes will be, on average, exaggerated (and sometimes, the wrong sign).

Recognizing the limitations of pooling standardized estimates from models used for different purposes, we estimate that the median power of empirical designs in environmental and resource economics is 33% (i.e., a false negative rate of 67%)[1]. In most social sciences, adequate power is conventionally accepted to be 80%. We estimate that the designs are inadequately powered for nearly two out of three of the estimated parameters (Fig. 1; see also Fig. S1 in M&M). Similar values were reported for the economics discipline more generally (Ioannidis et al., 2017).

In other words, environmental and resource economics research designs in our sample are generally underpowered. This conclusion about power is tied to our assumption about the true effect size that the studies are attempting to estimate, an

[1] In this analysis, we approximate, in a single number, the magnitude of effect sizes that environmental and resource economists aim to estimate in their designs. To do so, we pool data from various models and lines of research inquiry. The true effect size in any study is, however, unknown and likely varies across contexts. Our conclusions about power would change with plausible changes in our assumption about the true effect size. To see how our conclusions, see Figures S2A and S2B and associated text.

assumption based on our analyses of the published literature (see M&M). As Ioannidis et al. concluded about economics in general, we conclude that the average study in environmental and natural resource economics aims to estimate a relatively small effect amidst considerable noise. Because the true effect size of a study is unknown, our power calculations are approximations. Nevertheless, our conclusion that research designs are generally underpowered holds for a range of plausible true effect sizes (see Fig. S2 in M&M).

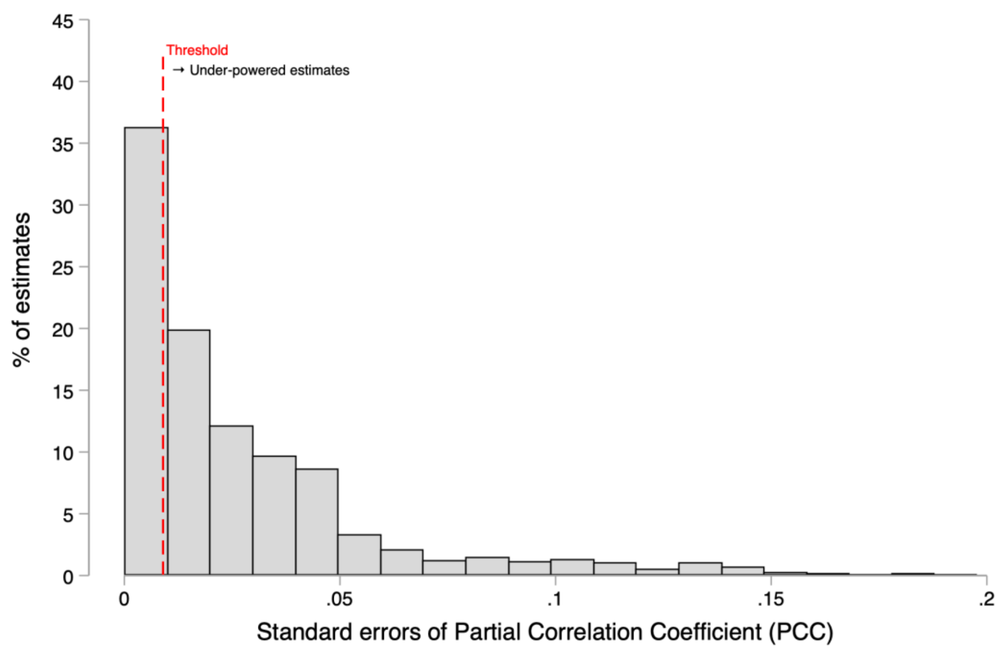

Figure 1: Percentage of estimates that are adequately powered in 307 empirical environmental and resource economics studies published between 2015-2018. Histogram shows the distribution of standard errors of the standardized effect sizes (PCCs). Bins on the horizontal axis are of width 0.01. The vertical dashed line is the threshold value for adequate power (80%). Estimates to the left of the threshold are adequately powered (33%), while those to the right are under-powered (67%).

Despite low power, however, most of the publications report statistically significant results. As noted above, if most studies are underpowered, and studies are more likely to be published when they declare results that are "statistically significant," the published effect sizes will be, on average, exaggerated. Following the approach of Ioannidis et al., we calculate the degree of "exaggeration bias" in the environmental and resource economics literature. This approach develops a weighted average of estimates from the adequately powered studies in our sample and assumes that this weighted average represents the true effect size that the studies seek to estimate. The difference between this estimate of the true effect size and the average standardized effect size reported in all studies in our sample yields an estimate of the "exaggeration bias" in this literature. We estimate that 56% of the reported estimates are exaggerated by a factor of two or more and 35% are exaggerated by a factor of four or more (Fig. S3 in M&M).

IV. Selective Reporting of Statistical Significance

A mix of underpowered designs and publication bias is not the only reason a scientific literature may contain false or exaggerated results. Selective reporting or misreporting of statistical significance, often done in reaction to fears about publication bias against "statistically insignificant results," can also contribute. More specifically, scholars may take actions in the pursuit of test statistics that pass conventional thresholds for statistical significance. Such actions may include selective reporting of statistically significant results after: (a) an analysis of multiple treatment or outcome variables; (b) the use of multiple identification strategies or regression specifications; and (c) the use of multiple sample selection rules, data trimming or exclusion rules, or endogenous data collection stopping rules ("Stop when $p<0.05$"). These actions are often given pejorative labels, such as "p-hacking" or "researcher degrees of freedom," but they are not necessarily deliberately done to mislead the reader. They may, for example, be considered reasonable steps to ensure coherence and cogency in the final publication. Nevertheless, when they are widespread, they can lead to an evidence base that misleads rather than informs.

For example, consider our prior example of a study that estimates the average effect of an environmental regulation on $PM_{2.5}$ emissions, but let us assume the true effect of the regulation is essentially zero. The first analysis yields an estimate of a 10% reduction, but with a large 95% confidence interval that includes zero, as well as small increases in pollution and large decreases in pollution (i.e., $p>0.05$). The evidence is consistent with a range of effect sizes and does not pin down the magnitude in a way that would be appealing to scientific and policy-making audiences, who typically demand less (sampling) uncertainty. One might thus try a different regression specification, or decide that some observations are too large, too small, or otherwise too unusual to include in the analysis. Some of these actions might yield an estimate with less uncertainty (i.e., $p<0.05$). Nothing is wrong with such exploratory analyses, if they are all reported. The danger lies when we convince ourselves that the estimates with the least uncertainty are the only ones we ought to report.

One hallmark of such actions is an unusual pattern in the distribution of test statistics: specifically, a double-humped distribution in which there is a plateau or decline on the lower side of the conventional threshold for statistical significance. In Fig. 2, we present the distribution of z-statistics for the estimates, where 1.96 is the conventional value for statistical significance. Under a range of assumptions, one would normally expect a smoothly declining function as one moves from low z-statistic values to high values (Brodeur et al., 2016). However, when researchers are confronted with test statistics that are just below the conventional critical value for declaring "statistical significance," we might expect selective reporting that would yield a shortage of such test statistic values (i.e., a shortage of z-statistics between 1.2 and 1.65, which correspond to p-values between 0.25 and 0.10, and a bump starting around 2, which corresponds to p-values just below 0.05).

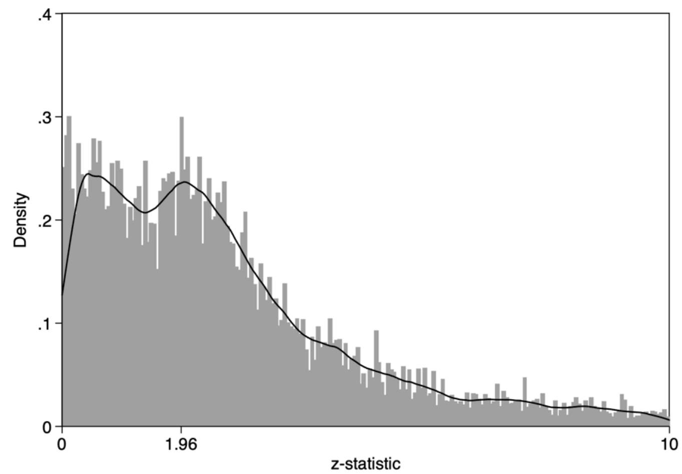

Figure 2: Distribution of *z* statistics for tests reported in 307 empirical environmental and resource economics studies published between 2015-2018. Black line corresponds to kernel density estimates using an Epanechnikov kernel function with 0.2 bandwidth. The p-values were first "de-rounded" to address variation in reporting precision across articles (i.e., number of decimal places). The values were then weighted by the inverse of the number of estimates presented in the same table multiplied by the inverse of the number of tables in the article. This weighting addresses variation in the number of tests reported across articles (see M&M for details, and for figures without these adjustments).

The unusual dip before 1.96 is consistent with "researcher degrees of freedom" (or editor's degrees of freedom) (see also Fig. S4 in M&M). Selective reporting of test statistics might not have major effects on overall inferences if study designs were properly powered and there were no publication biases against replications and null results. Yet, as reported in the prior section (and in our survey), these attributes do not appear common in the environmental and resource economics literature. In such a context, selective reporting can yield empirical results that may be misleading in ways that have practical implications for the design of policies and programs.

V. Multiple Comparisons and False Discoveries

Even when all analyses are reported, the results can mislead readers (and authors). Scholars often conduct multiple statistical tests, such as tests related to multiple average treatment effects, followed by tests related to multiple conditional average treatment effects, a.k.a., subgroup effects. Consider our running example of an environmental regulation aimed at reducing $PM_{2.5}$ emissions. In practice, an economist may estimate the average effect of the regulation on multiple pollutants (e.g., NO_x and $PM_{2.5}$), and then the average effects on these pollutants conditional on characteristics of the regulatory context (e.g., liberal government-controlled states vs conservative government-controlled states).

Multiple hypothesis testing gives rise to a statistical issue that is often called the multiple comparisons problem (Hochberg and Benjamini 1990). Multiple comparisons are a problem because the probability of (wrongly) observing at least one statistically significant result across a set of related tests increases with each additional test.[2] If we were to conduct twenty null hypothesis tests with a false positive error rate of 5%, we would expect at least one incorrect rejection even if all the null hypotheses were true. In our running example of the environmental regulation that aims to reduce PM_2, one might conduct multiple tests and conclude, "We find that the regulation has no effect on PM_2 and CO_2 emissions, but does

[2] The multiple comparison problem is well captured in this cartoon: https://xkcd.com/882/.

reduces NO_x in liberal-controlled states, where we would expect adequate enforcement of the environmental regulation." That conclusion might be warranted, but unless the authors took actions to reduce the likelihood of a false discovery, the conclusion may be misleading. The potential for misleading results becomes worse when only some of the test results – usually the statistically significant ones - are reported in the final publications (Section III).

Solutions to the multiple comparison problem are two-fold: (1) report all comparisons (all results), differentiating confirmatory from exploratory analyses and using online appendices when necessary to avoid disrupting the flow of the main text; and (2) adopt one or more approaches that have been developed to mitigate the probability of false discoveries. Two of the more popular approaches are (a) maintaining the family-wise false positive (Type 1) error rate across the suite of tests and (b) controlling the false discovery rate to a pre-specified level. In our review, we found that 63% of the studies conducted multiple hypothesis testing, but less than 2% of those studies adopted one of these approaches to mitigating the multiple comparisons problem (Table S1 in M&M).

VI. HaRKing and other Questionable Research Practices

To shed more light on research practices among environmental and resource economists, we invited members of the Association of Environmental and Resource Economists and the European Association of Environmental and Resource

Economists to complete a survey. The survey was designed to be short, with four Yes-No questions, two supplemental Yes-No questions, and two demographic questions (job position, gender). The Yes-No questions ask whether the respondents had engaged in research practices that other scholars have labeled "questionable" (John et al. 2012; Martinson et al. 2005). If respondents indicated that they engaged in the practice, they were asked, "Do you think your decision was justified?" (*Yes*, *No*, or *Possibly*) and were given an opportunity to provide further commentary.

Unlike data fabrication and other forms of fraud, questionable research practices (QRPs) are not necessarily improper. Yet they do offer researchers many "degrees of freedom" to select what readers see in the final publication, and thus they can contribute to the problems described in the previous three sections. For example, one of the potentially most damaging QRPs is Hypothesizing after Results are Known, or HaRKing. HaRKing is the practice of constructing post hoc hypotheses and reporting them as a priori ones, or conversely, failing to report a priori hypotheses that are not supported by the results (Kerr 1998). In other words, HaRKing involves the process of adding or removing hypotheses to fit the results *without acknowledging this process*. As with all QRPs in our survey, the most problematic attribute is the last four words of the prior sentence: "without acknowledging the process." QRPs, at their core, suffer from a lack of transparency, and thus can retard the advancement of science.

TABLE 1—RESEARCH PRACTICES IN PUBLICATIONS AMONG ENVIRONMENTAL AND RESOURCE ECONOMISTS

	% reporting "Yes"[*]	*% reporting "Decision was justified"*	*% reporting "Decision was possibly justified"*	***N***
I reported only a subset of the dependent variables that I explored in my analysis	50%	85%	14%	401
I reported only a subset of the analyses or experiments that I conducted	78%	84%	15%	389
I modified my original hypothesis to better match my empirical results	25%	71%	26%	381
I excluded or recategorized data after looking at the effect of doing so	29%	76%	21%	375
When conducting data analysis, I have chosen regressors after looking at the results.	66%	78%	20%	316
I have chosen not to submit to a journal a study with a null result[˥]	36%	N/A	N/A	316
Demographics of Respondents	Faculty 75%	Post-docs 7%	Female 30%	

[*] *The last two questions were supplemental and thus fewer respondents replied to them. For the complete questions and link to data, see M&M.*

[˥] *We do not label the last question as a QRP and thus do not report statistics on justifications. A "null result" was defined in the survey as "a precisely estimated small or zero effect, or an effect that is not statistically significant." Respondents could answer "Yes," "No," or "No, I have never produced a null result study." Nearly one in five respondents reported never having produced a study with a null result.*

The survey responses in Table 1 are intended to be suggestive. Among the limitations of our survey is our inability to assess the frequency of the practices. We do not claim that our sample is representative or that respondents were telling the truth. We suspect that the responses mark lower bounds on the prevalence of these practices for two reasons: (a) the invitation email indicated the survey was on research practices, and thus respondents may have been, on average, more interested in and knowledgeable about the topic than the average member; and (b) although the survey was anonymous and respondents were encouraged to tell the truth, self-reports of QRPs may under-report the true prevalence of these practices, particularly for practices that are well-known to be questionable, such as excluding data after seeing the results of such exclusion (see John et al. for empirical evidence of such under-reporting).

Despite the potential for under-reporting, the self-admission rates were high. Among the respondents who answered all four QRP questions, 92% admitted to at least one QRP. The proportions answering "Yes" are higher for faculty respondents. Most respondents who admitted to engaging in a practice also defended the practice as justified and, by and large, we found the justifications to be plausible (see data link in M&M). Nevertheless, this freedom to choose which variables, analyses and data readers will see– or whether readers will see anything at all (see "null" result question) - can contribute to a replicability crisis, particularly when combined with incentives for generating results that will please editors and peers.

In sum, the responses to our survey suggest there may be substantial unpublished data and analyses on the hard drives of environmental and resource economists. Whether knowledge of those data and analyses would change how we would interpret the empirical literature in environmental and resources economics is an open question.

VII. Discussion

Concerns about selective reporting and publishing are not new in economics (Leamer and Leonard 1983), and it is beyond the scope of our essay to delineate actions to address these concerns in environmental and resource economics. Nevertheless, we make six recommendations and refer readers to other sources for more details (e.g., Christensen and Miguel 2018; Rossi 1987; Palm-Forster et al. 2019):

1. *Emphasize designs and questions more and results less*. Editors, funders, peer reviewers, and authors need to acknowledge that, in most cases, we are seeking to estimate small, but potentially important, effects among noisy data. Thus we should not expect, or require, air-tight perfection in our empirical studies. When unrealistic outcomes are expected, rational agents will seek to deliver the veneer of such outcomes through hidden actions. In this vein, we should evaluate designs and questions, not results. We ought to abolish conventional statistical significance cut-offs and their associated asterisks (encourage reporting of confidence intervals and power for different effect sizes instead).[3] We should encourage authors to differentiate exploratory and confirmatory analyses, and

[3] Other motivations for eliminating the emphasis on statistical significance include the ways in which this focus masks more important issues about the magnitude of effects (Ziliak and McCloskey 2008).

not punish them for exploratory analyses that yield hypotheses that cannot currently be tested with easily available data.

2. *Foster a culture of constructive criticism*. Scholars often argue that science is self-correcting. For that argument to be valid, we need a culture of criticism and commentary. Yet in JAERE, for example, we could find no published comments in the last five years. Without a culture in which we willingly expose ourselves to scrutiny, false or exaggerated effect estimates will persist. Criticism, of course, should be done carefully, constructively, and in a manner that acknowledges that we are all fallible. It also ought to be open to the possibility of benign explanations for any issues discovered.
3. *Encourage and reward pre-registration, particularly for observational studies for which pre-registrations are rare*. Although pre-registration is no panacea, and depends on researchers being truthful about the "pre-" aspect of their pre-registration, it has been shown to greatly reduce the frequency of large, statistically significant effect estimates in the "predicted" direction (Kaplan and Irvin 2015; another option, which only addresses publication bias, adds noise to data prior to submission, so that the manuscript must be evaluated without knowledge of the actual results; MacCoun and Perlmutter 2015).
4. *Encourage and reward replications of influential, innovative or controversial empirical studies*. By "replication," we mean reproducing an analysis and examining its robustness to reasonable changes in the original design, as well

as repeating a design, experimental or observational, using new data that may include alternative treatment or outcome constructs. Under prevailing incentives, such replications have low rewards and high costs, in terms of both time and reputation. Not surprisingly, Duvendack et al. (2017), reviewing a set of “top 50” economics journals since the late 1960s, found only 188 replication studies; only 16 journals published more than three. We need to change those incentives. To avoid creating perverse incentives, journals ought to publish the replications regardless of whether they confirm, qualify, or disconfirm the original study. New journal review processes, in which a pre-registered report of the replication is “conditionally accepted” (conditional on the authors following the pre-registered protocol), may help with ensuring that high-quality replications are done and published.

5. *Encourage authors to report everything and avoid punishing them for transparency (see (1) above!).* Prior to publication, authors should be required to post, in a sanctioned repository, data sets and code files that have been confirmed to run and reproduce the results in the manuscript, as well as results that may have been generated but not reported in the manuscript because of space constraints or other reasons. These data and code files should also include data cleaning and variable construction steps (with comments that describe how the data were cleaned or constructed outside of the code file).

6. *Raise awareness*. Researchers need to better self-monitor the decisions they make in data preparation, analysis, and reporting. Are we making decisions that enhance the quality of the research, or just the likelihood of publication? Are we being sufficiently transparent in our manuscripts? Did we account for multiple comparisons? What is the power of our design to detect modest effect sizes (e.g., ~0.10 standard deviation)? If it is low, will we make the reader aware of this deficiency?

We have no doubt that most environmental and resource economists are motivated to conduct high-quality, scientific research. Like other economists, they have strived to eliminate biases in their empirical analyses. Nevertheless, many aspects of the research process are hidden from peer reviewers and readers. This hidden action is problematic when researchers face incentives to produce unblemished results that are significant in both the policy and statistical senses. We hope our essay encourages debate on actions our field can take to ensure the highest quality of its empirical research and thereby avoid a replicability crisis that would weaken our ability to influence science and policy in the future.

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Online Materials and Methods

Ferraro & Shukla, Is there a Replicability Crisis on the Horizon for Environmental and Resource Economics?

In addition to describing the statistical methods used in this paper, we describe the data collection procedures and the survey instrument. We also present results using our full sample of estimates and contrast them to the results using only the "main" estimates. The full list of studies included in our analyses, as well as the data and code used to do the power calculations, p-value analyses, and survey data analysis can be found at: https://osf.io/ckhjs/. In the uploaded survey data, we also include the open-ended justification text provided by respondents.

I. Study Selection Criteria

a. We searched four journals for relevant empirical studies: *The American Economic Review* (*AER;* not from *AER Papers & Proceedings*), *Environmental and Resource Economics (ERE)*, *The Journal of the Association of Environmental and Resource Economics (JAERE)*, and *The Journal of Environmental Economics and Management (JEEM)*.

b. To be selected, a study had to be published in the period 2015-2018, use regression analyses to estimate coefficients of policy-relevant variables, and report the estimates and their standard errors. We deliberately chose recent publications to make it less likely that we would miss any dramatic recent changes in how scholars conduct their research, changes that could have resulted from greater awareness of the issues we raise in our study.

c. All studies from JAERE, ERE and JEEM that meet the above criteria are included, including articles from special issues. To select relevant studies from AER, we also required them to have at least one of the following *Journal of Economic Literature* (JEL) codes:

JEL Code	Subfield Description
F18	Trade and Environment
F64	Economic Impacts of Globalization on Environment
H23	Environmental Taxes and Subsidies (Note: if using H23, search for the keyword "environment". Only if "environment" is present, then include the study)
N50 - N57	Agriculture, Natural Resources, Environment, and Extractive Industries
O13	Economic Development, Natural Resources, Energy and Environment (Note: if using O13, search for the keyword "environment", "energy" and "natural resources". If at least one of them is present, then include the study)
Q2 - Q4	Renewable Resources and Conservation; Nonrenewable Resources and Conservation; Energy
Q15	Land Ownership and Tenure, Land Reform, Land Use, Irrigation, Agriculture and Environment
Q50 - Q59	Environmental Economics

II. Data Collection Guidelines

After we selected articles that met our criteria, we used the following guidelines for data collection:

a. We collected all estimates that are reported by the authors (significant or otherwise). We did not include values from summary statistics tables, balance tables, placebo tests, or falsification tests.

b. We inputted data exactly as they appear in the published version of the study (i.e., no matter how many decimal places were reported).
c. Following Brodeur et al. (2016), we defined the "main variables" as variables that contribute towards answering the main research question. To make this determination, we looked at the purpose of each table and how the authors interpret the variables. If the authors discuss and interpret the variables in the text related to the table, we included the variable in our "main" data set. In this data set, we did not include estimated coefficients of constants or control variables. These estimates are, however, included in the "full sample" of estimates. Variables used to explore the "determinants of […]" were considered "main variables". Variables included in robustness checks where the central variables of interest in the paper are being investigated were also included as "main variables." If we could not determine whether a variable was a "main variable," we labeled it a "main variable."
d. We did not collect data from appendices.

From 307 studies, we collected a total of 30,939 estimates, of which 21,137 were designated as "main" estimates.

III. Note on Statistical Methods

a. Addressing the variation in reported decimal places in published studies (de-rounding)

Individual authors and journals have varied preferences and rules about rounding test statistics in publications. To statistically address this difference in reporting, we follow Brodeur et al. (2016) to de-round and reconstruct the estimates in our data. For example, if an estimate is reported to be 0.02, the true value would lie in the interval [0.015, 0.025). Using a uniform distribution, we randomly draw a value in this interval to reconstruct our test statistic.

b. Estimating Underpowered Studies and Exaggeration Bias

Statistical power is broadly defined as the probability of correctly rejecting the null hypothesis. Power calculations are conditional on some assumption of the size of the effect that the researchers are seeking to estimate. Thus, power may also be expressed in form of the minimum detectable effect (MDE). Following Bloom (1995), the MDE of a study design is the smallest effect that, if true, has an X% chance of producing an impact estimate that is statistically significant at the Y% level. X is the level of statistical power (denoted as $(1-\beta)$ and commonly set to 80%) and Y is the level of statistical significance (denoted as α and commonly set to 5%). The MDE can be written in terms of the standard error (Djimeu and Houndolo, 2016):

$$MDE = \left(t_{1-\frac{\alpha}{2}} + t_{1-\beta}\right) e \qquad (1)$$

where MDE is the minimum detectable effect, t_f is the t-distribution with f degrees of freedom e is the standard error of the estimated effect. Using conventional values of $\alpha = 0.05$ and $\beta = 20\%$ (for power of 80%) in (1) yields:

$$MDE = (1.96 + 0.84)\ e$$
$$MDE = 2.8\ e \qquad (2)$$

Using (2) and an estimate of the MDE, or "true" effect, we could check whether a study has adequate power by comparing the standard error of its estimate to the value MDE/2.8. If the estimate's standard error is smaller than this threshold value, then the study is adequately powered. To approximate the "true" effect in equation (2), Ioannidis et al. (2017) use a weighted average of reported effect sizes. The weights used are the estimates' precision (inverse of the variance), so that estimates with higher precision (low variance) get a larger weight.

The estimates (coefficients) across individual studies differ in the variables and functional form used, measurement scale and other factors. Comparing estimates across studies requires converting the estimates to a unitless measure with a common scale across studies. Meta-analyses typically use the partial correlation coefficient (PCC), calculated as (Havranek et al. 2016):

$$PCC = \frac{t}{\sqrt{t^2 + df}} \tag{3}$$

where t is the associated t-statistic of the estimate and df is the degrees of freedom. The standard error of the PCC can be estimated using (*ibid.*):

$$SEpcc = \frac{PCC}{t} \tag{4}$$

Using the absolute values of PCC, we can calculate the weighted average PCC using weighted least square fixed effects (referred to as WLS-FE). We let this value serve as an estimate of the true effect. We recognize that each empirical study in environmental and natural resource economics seeks to estimate a different effect, whose true value may vary across studies. The point of this exercise is approximate a reasonable value for the expected effect size that environmental and resource economists are seeking to estimate. We also explore how our conclusions change as this expected effect size changes.

We then compare the values of $SEpcc$ to the threshold value i.e., the estimated true effect (given by WLS-FE) divided by 2.8. Most published studies do not provide the information required to calculate the degrees of freedom (df) for each model. In the absence of this information, we approximate df using the sample size N. Thus, our power calculations yield conservative lower bounds of the power of the designs to estimate the target parameters.

Comparing the standard errors to the estimated threshold level using the sample of main estimates indicates that 67 percent of the estimates are underpowered. Using the full sample, we find that 65 percent of the estimates are underpowered. Figures S1A and S1B show the distribution of the standard errors of the PCC estimates along with the threshold values.

We compute the median power for our sample of estimates following Stanley et al. (2018). The median power is calculated as one minus the cumulative normal probability of the difference between 1.96 and the absolute value of the WLS-FE estimate divided by the median standard error. The median power for the sample of main estimates is 32.87% and for the full sample is 39.81%.

Figure S1A: Histogram shows the distribution of the standard error of the PCC estimates in the sample of main estimates. The dashed line indicates the threshold value for adequate power based on WLS-FE estimate. The y-axis shows the percentage of estimates in each bin. Estimates to the left of the threshold are adequately powered (33% of estimates), while those to the right (67% of estimates) are under-powered.

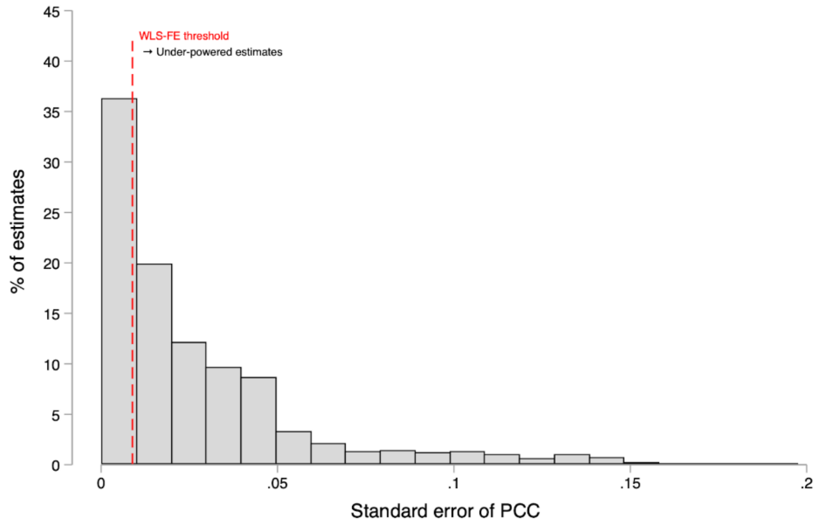
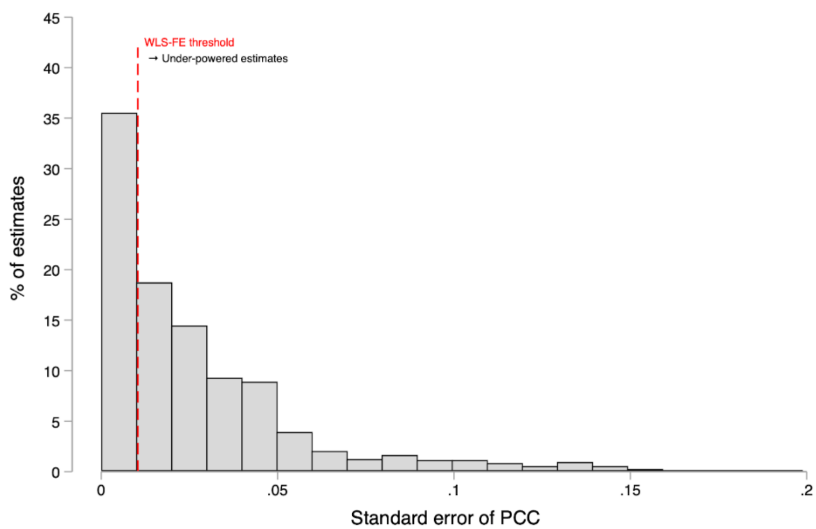


Figure S1B: Histogram shows the distribution of the standard error of the PCC estimates in the full sample. The dashed line indicates the threshold value for adequate power based on WLS-FE estimate. The y-axis shows the percentage of estimates in each bin. Estimates to the left of the threshold are adequately powered (35% of estimates), while those to the right (65% of estimates) are under-powered.



In comparison Ioannidis et al. (2017), who conduct a similar analysis for economics studies more generally, we report a lower proportion of underpowered estimates. There are four differences between our study and their study. First, they study a much broader range of sub-disciplines in economics and they calculate a different WLS-FE estimate for groups of studies nested in different meta-analyses, whereas we study only a single sub-discipline and use all the estimates in that sub-discipline. Second, we take a more conservative approach by using sample size N as a proxy for degrees of freedom (df). We do so because the information required to reconstruct the degrees of freedom is not available in most reported estimates (Ioannidis et al. obtain their estimates from meta-analyses that only use studies for which the *df* can be calculated). This difference results in upward bias of the partial correlation coefficient. As a result, the WLS-FE estimate and subsequently, the threshold for underpowered estimates is higher. Third, we only use peer-reviewed, published studies in our analysis. Ioannidis et al. (2017) data set constitutes "both published and unpublished meta-analysis, as well as several that were not published in economics journals but dealt with an economics relevant topic". It is not clear if the studies within their sample of meta-studies included unpublished studies, which may not have gone through the rigorous peer-review process and may have a higher share of underpowered estimates. Finally, we do not know whether the individual studies within their sample of meta-studies use absolute values of PCC, without which the value of WLS-FE would be lower and imply a higher number of underpowered studies.[1] Nevertheless, the average of the median power estimates reported in Ioannidis et al. (32.5%) is similar to our estimate of median power (32.9%). Furthermore, their median WLS-FE estimate (0.031) and their weighted average of effect sizes (0.016) are similar to our WLS-FE estimate (0.025 for main specifications; 0.030 for full sample).

In main estimate data set, 67 percent were designated as under-powered, based on our original estimate of the WLS-FE PCC value of 0.025. Of course, the true effect size in each study context is unknown and likely to vary across contexts. As pointed out by a reviewer, if there were a negative correlation between the power of a study design and the true effect size in the published literature, then our overall estimate of the true effect size for environmental and resource economics would be too small. In other words, if well-powered studies tend to be associated with contexts in which the true effect size is small, then we will overweight studies for which the true effect size is small, making it look like the studies in the literature are, overall, less statistically powerful than they really are. We know of no reason why such a correlation were to exist, but we cannot disconfirm it either.

Given that the true effect size is not known, we also explore how our conclusions change with changes in the assumed true effect size. For a range of "true effect" values, we computed the share of PCC estimates whose standard error is greater than the threshold value based on hypothetical true effect sizes divided by 2.8. Figure S2A shows the change in the percentage of under-powered estimates as the size of the true effect varies, using the sample of main estimates. We undertake a similar exercise for the full sample as well and plot these results in Figure S2B. Our range goes up to PCC= 0.20, which we believe is an upper bound on what a reasonable expected effect size would look like in economics (in terms of standard deviations of the outcome variable, such an effect size would be analogous to an effect size of roughly 0.5 SD).

[1] Like Doucouliagos (2011), we used absolute values of PCC for interpreting effect sizes. We were unable to obtain the data used in the Ioannidis et al. study to verify the values that the authors used in their analysis.

Figure S2A: Share of under-powered estimates under hypothetical values of the true effect size (Main estimates only)

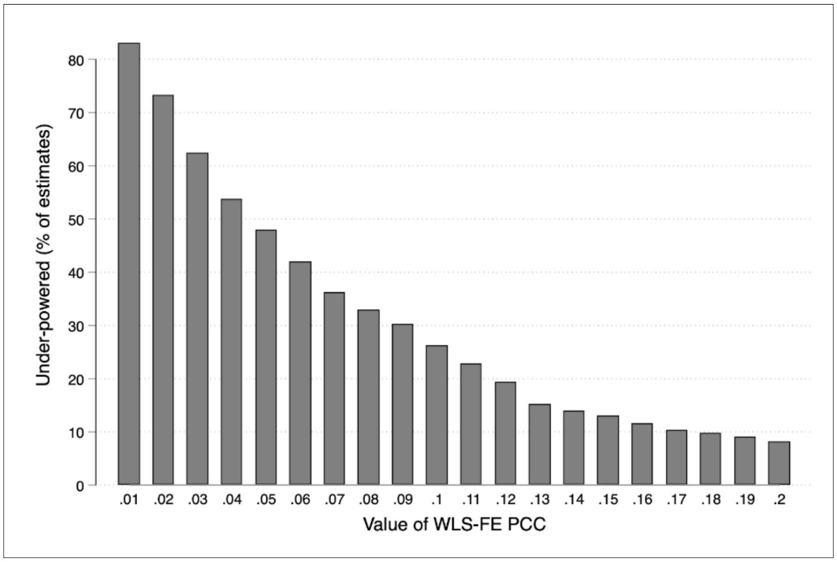
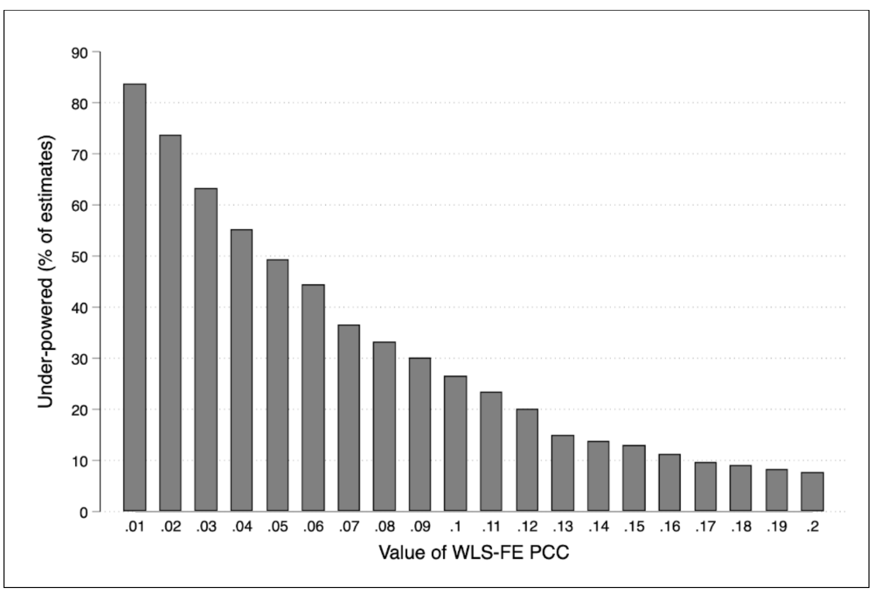


Figure S2B: Share of under-powered estimates under hypothetical values of the true effect size (Full sample)



Calculating the Exaggeration Bias

We re-estimated the WLS-FE using only the sub-sample of estimates that are adequately powered. This weighted average of estimates from the adequately powered estimators (WAAP) serves as a conservative benchmark for the 'true' effect (Ioannidis et al., 2017). We then compare this conservative, corrected meta-average (WAAP) to the average of all PCC estimates. Following Ioannidis et al (ibid.), the difference between the average PCC relative to WAAP is a conservative estimate of the "exaggeration bias" in research. Figure S3A and S3B show the distribution of the inflation across the main estimate only sample and full sample respectively.

In Figure S3A, 18.2 percent of the estimates are exaggerated by a factor of 2, 20.62 percent are exaggerated between 2 to 4 times, and 35.36 percent are exaggerated by a factor of 4 or more.

In Figure S3B, 19.3 percent of the estimates are exaggerated by a factor of 2, 22.1 percent are exaggerated between 2 to 4 times, and 29.1 percent are exaggerated by a factor of 4 or more.

Figure S3A: Distribution of the size of the PCC estimates relative to the WAAP (main estimates only)

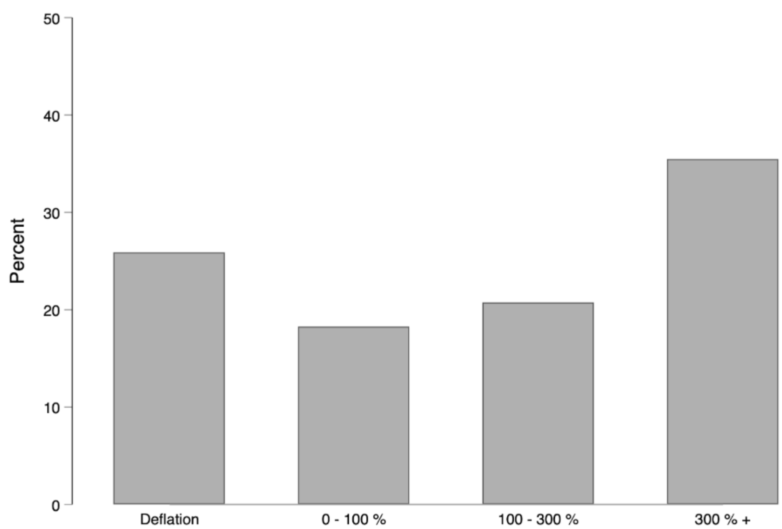
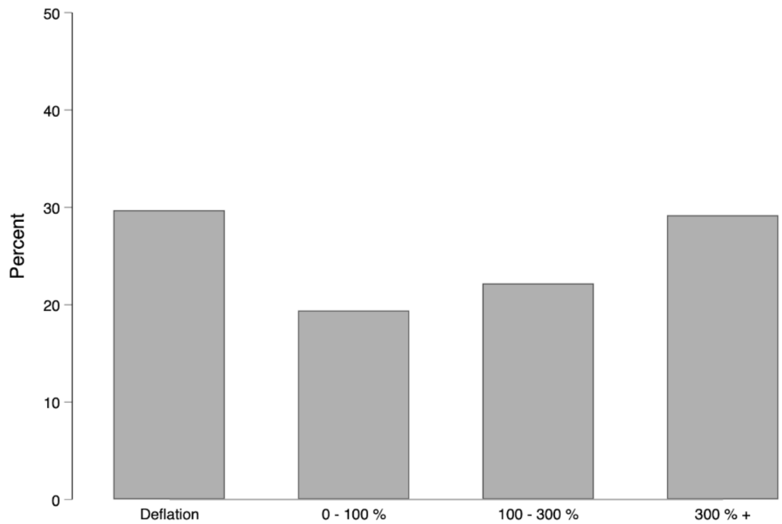


Figure S3B: Distribution of the size of the PCC estimates relative to the WAAP (full sample)



c. Distribution of p-values

Similar to Brodeur et al. (2016), we plot the distribution of t-statistics for estimates in our sample. As Brodeur et al. (2016) note, the t-statistic can be approximated as a standard normal distribution (z-statistic) asymptotically under the null hypothesis.

In the Figure S4, we plot the distribution of the unweighted and weighted z-statistics. The weights account for the number of estimates reported per article and per table in each article. In this way, the weights ensure that all articles contribute equally to the distribution regardless of the number of estimates the authors choose to report, and tables within the same article are assigned equal weights. To de-round and reconstruct the estimates in our data, using a uniform distribution, we randomly draw from the range of values that could have led to the reported estimate when rounded up (Brodeur et al., 2016) (see IIIa above).

We suspect an analogous problem of selective reporting can also arise when scholars use instrumental variable (IV) designs to estimate causal effects. Despite warnings (Stock and Yogo, 2005), many editors, peer reviewers, and authors follow a rule of thumb (Staiger and Stock, 1997) for evaluating whether an IV is "weak": if the first stage F-statistic is less than 10, the IV is deemed "weak" and authors must take steps to address potential bias in their IV estimator (or abandon it). As a result of this norm, researchers may be tempted to make data and modeling decisions that increase the F-statistic to just above 10. In our sample of studies, there were 60 F-statistics reported for the first stages of IV designs, and the modal whole number value was just above 10 (25% were between 10 and 15). However, without a clear idea of what the distribution in the absence of selective reporting should look like, drawing any conclusions from the observed distribution is difficult.

Figure S4A. Distribution of *z* – statistics (main estimates only). Black lines correspond to kernel density estimates using an Epanechnikov kernel function with 0.2 bandwidth. (A) the distribution of raw *z*-statistics; (B) the unweighted and de-rounded distribution of z-statistics (C) de-rounded and weighted using the inverse of the number of tests presented in the same article (D) de-rounded and weighted using the inverse of the number of tests presented in the same table (or result) multiplied by the inverse of the number of tables in the article.

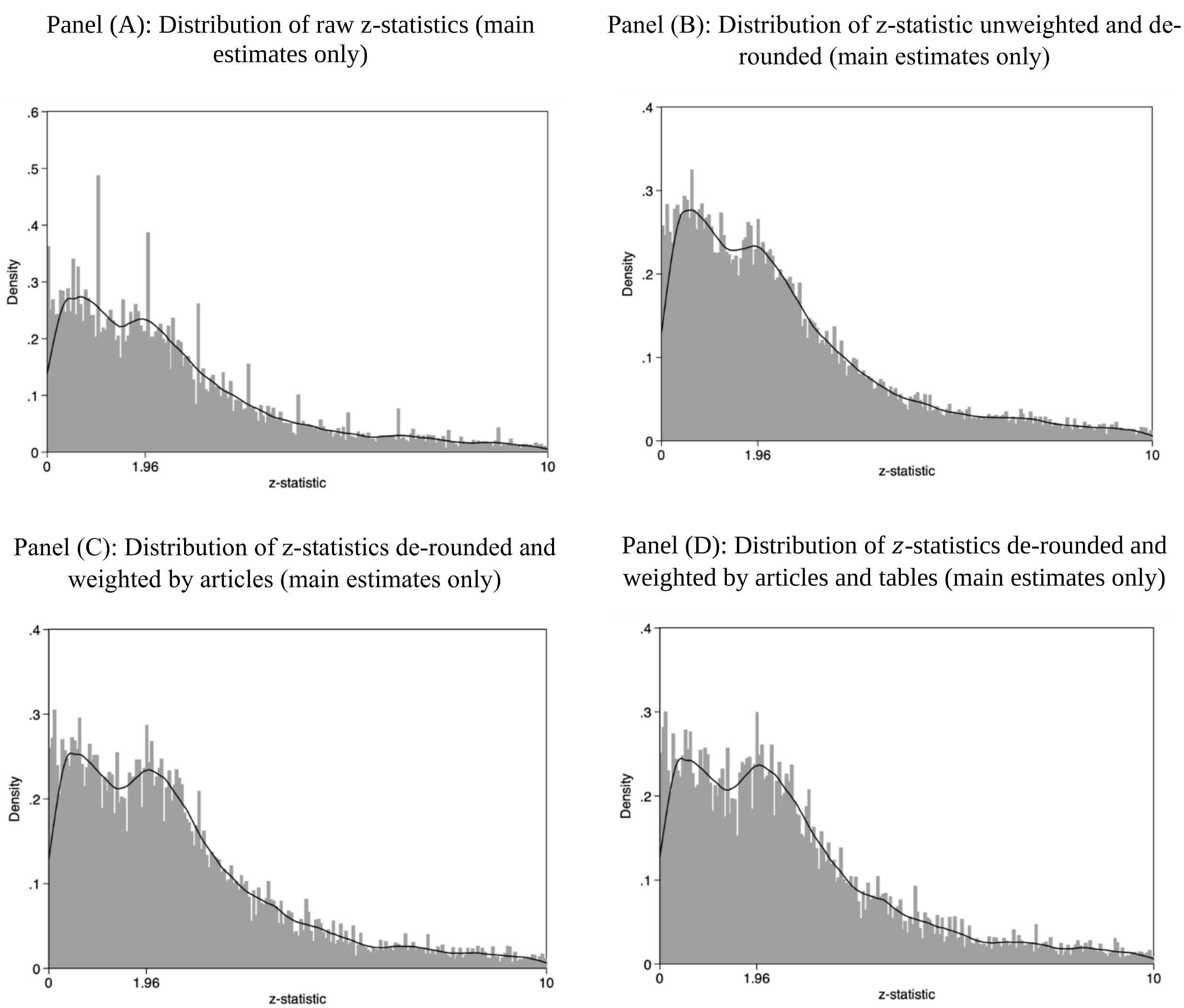
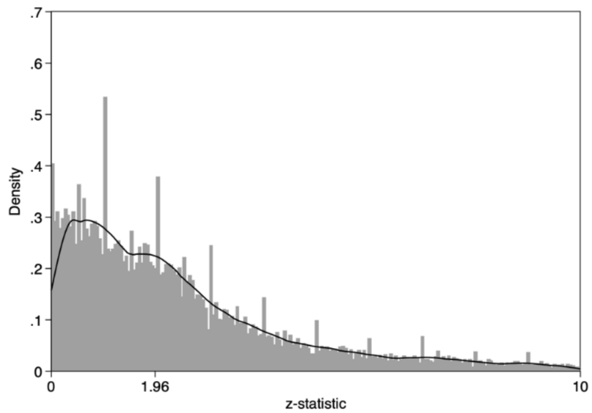
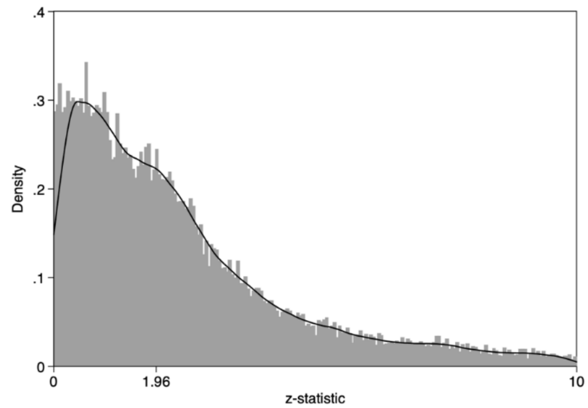


Figure S4B. Distribution of *z* – statistics (full sample). Black lines correspond to kernel density estimates using an Epanechnikov kernel function with 0.2 bandwidth. (A) the distribution of raw *z*-statistics; (B) the unweighted and de-rounded distribution of z-statistics (C) de-rounded and weighted using the inverse of the number of tests presented in the same article (D) de-rounded and weighted using the inverse of the number of tests presented in the same table (or result) multiplied by the inverse of the number of tables in the article.

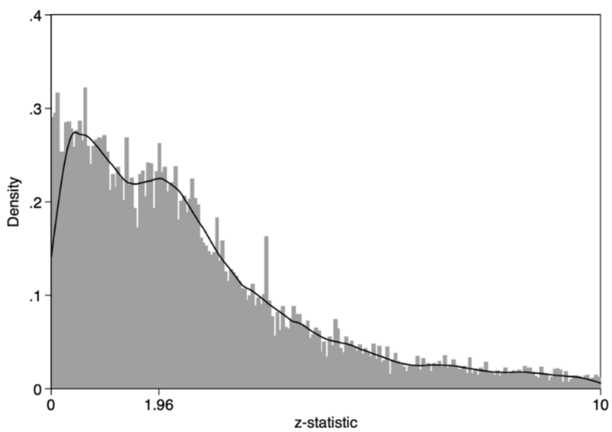
Panel (A): Distribution of raw z-statistics (full sample)



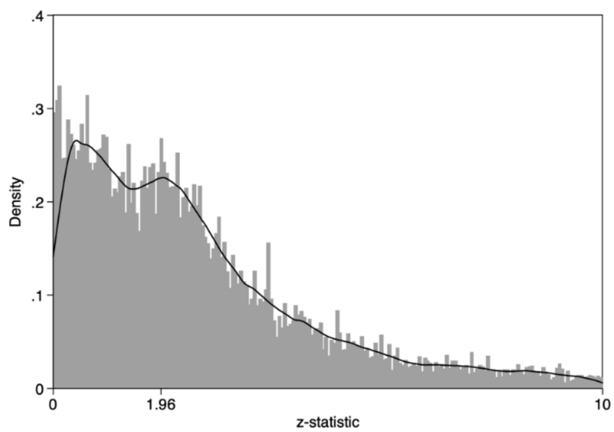
Panel (B): Distribution of z-statistic unweighted and de-rounded (full sample)



Panel (C): Distribution of z-statistics de-rounded and weighted by articles (full sample)



Panel (D): Distribution of *z*-statistics de-rounded and weighted by articles and tables (full sample)



d. Multiple Hypothesis Testing and False Discoveries

To examine whether an empirical study is conducting multiple hypothesis testing, we look at whether the authors investigated multiple outcomes associated with one causal variable or multiple causes associated with one outcome, or conducted analyses of sub-group effects or heterogeneous treatment effects. If they did do such analyses, we then used keyword search to see whether the authors have taken any steps to correct for family-wise error rate or false discovery rate. We searched on: false discovery rate, family wise error rate, Benjamini-Hochberg, Benjamini-Yekutieli, Bonferroni, per-comparison error rate, and Dunnett's test. Table S1 shows the number of studies that conducted multiple comparisons and those that took corrective steps.

In the text of our original manuscript, we had noted that maintaining the family-wise Type 1 error rate or controlling the false discover rate are approaches used by scholars applying frequentist modes of inference. Neither approach is required when using Bayesian modes of inference. Thus, in Table S1, we also included the number of studies using Bayesian modes of inference. Nevertheless, we wish to emphasize that Bayesian inference relies on incorporating *all* results into the formulation of posterior probabilities (i.e., all comparisons must be included). Simply applying Bayesian methods to a subset of the data analyses that one has conducted does not address any of the concerns about mistaken inferences. We cut these points from the main text because we believed they would only resonate with a small proportion of our intended audience.

TABLE S1— MULTIPLE COMPARISONS IN THE ENVIRONMENTAL AND RESOURCE ECONOMICS LITERATURE

Total studies	307
Studies with multiple comparisons	193 (62.86%)
Studies that seek to maintain the family-wise Type I error rate across the comparisons	3*
Studies that seek to control the false discovery rate across the comparisons	1*
Studies that use Bayesian, rather than frequentist, inference	0

** Two of the studies that addressed multiple comparisons were published in AER, one in JEEM, and one in JAERE. For full list of studies, see M&M.*

e. AERE/EAERE Member Survey

After the Institutional Review Board of Johns Hopkins University approved the survey, the secretariats of AERE and EAERE sent invitation emails to their members on behalf of Paul Ferraro. The EARE invitations were sent on 21 December 2019, and the AERE invitations were sent ten days later. Both groups received reminder messages in mid-January, in which the survey deadline was extended from 15 January to 25 January 2020.

The AERE Secretariat reported sending the invitation email to 977 people, of which 28 bounced back and 475 were opened. The EAERE Secretariat reported sending the invitation email to 1,126 people, of which 11 bounced back and 502 were opened (EAERE appears to drop non-renewed members from its database on 1 January, and thus the reminder email may have only gone to less than half the original number of recipients). The secretariats could only approximate the number of overlapping members between AERE and EAERE (i.e., individuals who received two invitations), and their approximations differ. We use EAERE'S estimate (86) because their members are incentivized to report joint membership to receive a discounted membership. We believe the actual number is higher. Based on these assumptions, we estimate a response rate of a little higher than 20% as a fraction of all recipients of the email invitation, and 43% as a fraction of all recipients who opened the invitation email.

Our respondent pool comprises current AERE or EAERE members who "have published at least one empirical paper in a journal, or as a dissertation chapter, book chapter, online working paper or report," and agreed to participate in the survey. Respondents were asked about practices conducted in one or more "published studies." For the survey purposes, a "published study" was defined as including "book chapters, dissertations, online working papers and reports, and peer-reviewed publications." The respondent pool of 401 researchers is comprised of faculty (75%), postdocs (7%), PhD students (8%) and staff in governmental or non-governmental organization (10%). About 85% of respondents answered all four questions on QRPs. The recruitment email and full survey instrument are below.

Recruitment Email and Survey of Research Practices for AERE and EAERE Members

RECRUITMENT EMAIL [EAERE email was identical with the exception of "EAERE" instead of "AERE"]

Subject: 4-question survey on AERE member research practices

Please find below a message sent to AERE members on behalf of Paul Ferraro, Johns Hopkins University.

Dear AERE members,
I would greatly appreciate it if you would be able to spare 3 or fewer minutes to answer four questions about research practices and two demographic questions. Just click on the link from any device:
http://jhubusiness.qualtrics.com/jfe/form/SV_5iGtsZZGJxQsixf

The link will be active until January 15, but if you click it now, you'll be DONE!
FYI, I aim to use the answers in a broad essay about research practices in environmental and resource economics for The Review of Environmental Economics and Policy. Thank you in advance for your assistance. I greatly appreciate it.

Paul Ferraro
Bloomberg Distinguished Professor
pferraro@jhu.edu
www.pauljferraro.com

p.s., apologies for cross-posting to joint AERE-EAERE members – you only need to do the survey once!

Survey on Empirical Research Practices

<u>All responses to this survey are anonymous.</u> This survey will ask you questions about empirical research practices. It should take fewer than 3 minutes to complete. If you would like more information, please contact Dr. Pallavi Shukla (pshukla4@jhu.edu).

Thank you,

Paul J. Ferraro and Pallavi Shukla

Johns Hopkins University

If you have published at least one empirical paper in a journal, or as a dissertation chapter, book chapter, online working paper or report, and agree to participate, please click "I agree to participate." By completing this survey, you consent to be in this research study. Your participation is voluntary and you can stop at any time.

○ I agree to participate

○ I do not agree to participate

Please answer the following questions truthfully and to the best of your knowledge. All replies are anonymous; we cannot tie a name to an answer. If you wish to change your answers, you can go back using the arrow buttons below. You also have the option of explaining your answers.

<u>A 'published study' includes book chapters, dissertations, online working papers and reports, and peer-reviewed publications.</u>

1. In a published study, I reported only a subset of the dependent variables that I explored in my analysis.

    ○ Yes

    ○ No

1. A. (OPTIONAL) Do you think your decision was justified? Feel free to elaborate in the comment box below.

- Yes
- No
- Possibly
- Comments (optional; please do not write names or institutions) ___________________________

2. In a published study, I reported only a subset of the analyses or experiments that I conducted (e.g., only some specifications, identification strategies, subgroup analyses, experimental sessions)

- Yes
- No

2. A. (OPTIONAL) Do you think your decision was justified? Feel free to elaborate in the comment box below.

- Yes
- No
- Possibly
- Comments (optional; please do not write names or institutions) ___________________________

3. In a published study, I modified my original hypothesis to better match my empirical results.

- Yes
- No

3. A. (OPTIONAL) Do you think your decision was justified? Feel free to elaborate in the comment box below.

- Yes
- No
- Possibly
- Comments (optional; please do not write names or institutions) ___________________________

4. In a published study, I excluded or re-categorized data after looking at the impact of doing so.

- Yes
- No

4. A. (OPTIONAL) Do you think your decision was justified? Feel free to elaborate in the comment box below.

- Yes
- No
- Possibly
- Comments (optional; please do not write names or institutions) ___________________________

5. Demographic questions to help us describe the respondent pool.
   What is your position? (Check one)

- Faculty
- Postdoc
- PhD student
- Staff in governmental or non-governmental organization
- None of the above

6. Gender

◯ Male

◯ Female

◯ Other

Thank you for your time. Would you be willing to spend another 2 or fewer minutes answering two more questions?

◯ Yes

◯ No

7. In a published study, when conducting data analysis, I have chosen regressors (control variables) after looking at the results.

◯ Yes

◯ No

7. A. (OPTIONAL) Do you think your decision was justified? Feel free to elaborate in the comment box below.

- Yes
- No
- Possibly
- Comments (optional; please do not write names or institutions) ____________________________

8. I have chosen not to submit to a journal a study with a null result ("null result" means a precisely estimated small or zero effect, or an effect that is not statistically significant).

◯ Yes

◯ No

8. A. (OPTIONAL) Do you think your decision was justified? Feel free to elaborate in the comment box below.

- Yes
- No
- Possibly
- Comments (optional; please do not write names or institutions) ___________________________

Please click the SUBMIT button below to submit your answers. Remember, you can go back to a previous page if you wish to edit any of your answers. Thank you for your time. We greatly appreciate it!

- SUBMIT

List of Publications Included in this Study

Abatayo, A., & Lynham, J. (2016). Endogenous vs. exogenous regulations in the commons. *Journal of Environmental Economics and Management, 76*, 51-66.

Ahlvik, L., & Iho, A. (2018, 11 1). Optimal geoengineering experiments. *Journal of Environmental Economics and Management, 92*, 148-168.

Alberini, A., Bareit, M., Filippini, M., & Martinez-Cruz, A. (2018). The impact of emissions-based taxes on the retirement of used and inefficient vehicles: The case of Switzerland. *Journal of Environmental Economics and Management*, *88*, 234-258.

Albouy, D., Graf, W., Kellogg, R., & Wolff, H. (2016). Climate Amenities, Climate Change, and American Quality of Life. *Journal of the Association of Environmental and Resource Economists, 3*(1), 205-246.

Albrizio, S., Kozluk, T., & Zipperer, V. (2017). Environmental policies and productivity growth: Evidence across industries and firms. *Journal of Environmental Economics and Management, 81*, 209-226.

Aldy, J., & Pizer, W. (2015). The Competitiveness Impacts of Climate Change Mitigation Policies. *Journal of the Association of Environmental and Resource Economists, 2*(4), 565-595.

Alesina, A., Stantcheva, S., & Teso, E. (2018). Intergenerational Mobility and Preferences for Redistribution. *American Economic Review, 108*(2), 521-554.

Allcott, H., & Taubinsky, D. (2015). Evaluating Behaviorally Motivated Policy: Experimental Evidence from the Lightbulb Market †. *American Economic Review, 105*(8), 2501-2538.

Allcott, H., Collard-Wexler, A., & O'Connell, S. (2016). How do electricity shortages affect industry? Evidence from India. *American Economic Review, 106*(3), 587-624.

Almer, C., Laurent-Lucchetti, J., & Oechslin, M. (2017). Water scarcity and rioting: Disaggregated evidence from Sub-Saharan Africa. *Journal of Environmental Economics and Management, 86*, 193-209.

Alolayan, M., Evans, J., & Hammitt, J. (2017). Valuing Mortality Risk in Kuwait: Stated-Preference with a New Consistency Test. *Environmental and Resource Economics, 66*, 629-646.

Amore, M., & Bennedsen, M. (2016). Corporate governance and green innovation. *Journal of Environmental Economics and Management, 75*, 54-72.

Andersen, D. (2016). Credit Constraints, Technology Upgrading, and the Environment. *Journal of the Association of Environmental and Resource Economists, 3*(2), 283-319.

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