Does Data Posting Increase Citations?

Don A. Moore and Andrew K. Rose*

Preliminary: Please Do Not Quote without Permission
Comments Welcome
Updated: September 2, 2018

Executive Summary
No

Abstract
This study estimates the effect of data sharing on the citations of academic articles. Out of more than 250 high-impact scientific journals, seventeen have adopted the requirement that data be posted publicly; these journals changed policy beginning in 2004 and continuing through 2016. We collect data on citations to empirical articles in these journals, and compare citations to articles published immediately after the policy change to citations for articles published immediately before it. We also compare citations for empirical articles in our 17 "treatment" journals to citations for theoretical articles in the same journals (which are unaffected by the data posting requirement), and citations to empirical articles in matched "control" journals which do not require data sharing. While there is heterogeneity across journals, we find no consistent evidence that data posting increases or even changes citation counts generally. Citations to empirical papers published in journals that require data sharing are insignificantly different from articles published either in the same journals before the requirement or in journals with no data sharing requirement.

Keywords: empirical; article; journal; policy; switch; shift; sharing; academic; scientific; impact.

Don A. Moore
+1 (510) 642-1059
dm@berkeley.edu
learnmoore.org

Andrew K. Rose
+1 (510) 642-6609
andrewkrose@berkeley.edu
faculty.haas.berkeley.edu/arose

* Moore is Lorraine Tyson Mitchell Chair in Leadership and Communication, and Rose is B.T. Rocca Jr. Chair in International Business, both at the Haas School of Business, Berkeley CA 94720-1900. For research assistance we thank Shreya Agarwal, Amelia Dev, Ekaterina Goncherova, Pranay Kumar, Sydney Mayes, Maya Shen, Cody Strohl, Mitchell Wong, and Winnie Yan. We also thank Paul Gertler, Miriam Green and Ross Levine for comments. Rose thanks the Central Bank of Barbados and the National University of Singapore for hospitality during the course of this research. A current version of the paper, the data set and key output are freely available at Rose's website. The data collection techniques, hypotheses, and empirical methods were pre-registered; the relevant documents are available at https://osf.io/pxdch/.

1. Introduction

Scientific claims are more persuasive than mere opinions because they are backed up by observable, reproducible facts. Scientists can increase the credibility of their claims by sharing the data on which they are based. The ease of posting data on the internet has lowered the cost of data sharing dramatically; accordingly, a small but growing number of scientific journals have recently started requiring that authors share their data by posting it publicly. However, this requirement remains more the exception than the rule, and few researchers post their data voluntarily when journals do not require them to do so (Tenopir et al., 2011). The implication is that there are costs to sharing data and few researchers believe that these costs are outweighed by the benefits. In this paper we ask the question "Is there a benefit to posting data, manifest in increased citations?" To our surprise, the answer is negative; journals that require data sharing do not seem to enjoy an increase in their impact, at least as measured by citations.

Motivation

Advocates of open science have argued that data posting should be standard practice (Miguel et al., 2014), but its implementation varies widely. Most academic journals and professional societies encourage researchers to share their data with colleagues, but these are often informal unenforced recommendations; only a small number of journals require it. As a result, data sharing is more the exception than the rule. Researchers give several reasons for their failure to post data. These reasons highlight various costs, including the potential for being scooped and the effort required to make data sets accessible. It is also plausible, however, that there are benefits, as demonstrated recently by Christensen, Dafoe and Miguel (2018). If research with posted data is more persuasive or believable, it might have greater impact. Researchers who post their data may acquire a reputational advantage. Moreover, data sets are often useful for analyses the original authors did not think or chose not to conduct; a paper may receive citations "indirectly" if other researchers use the posted data (Henneken & Accomazzi, 2011; Piwowar, Day, & Fridsma, 2007; Piwowar & Vision, 2013). For all these reasons, it seems likely that articles published with posted data may deliver increased citations compared to similar papers published without.

Our pre-registered research plan is available at: https://osf.io/pxdch/; our data and analysis output is available at http://faculty.haas.berkeley.edu/arose/RecRes.htm#Misc .

2: Data

Empirical Strategy

A simple comparison between citations to papers published with and without posted data is problematic. Perhaps authors who post their data are systematically different from those who do not, in that they choose to publish in different journals? We try to minimize this problem by focusing on journals which began to *require* data posting, which enables us to compare papers published immediately before and after posting became mandatory.[1] Using within-journal variation in citations enables us to keep the journal constant, focusing our attention on the effect of the change in data sharing policy. Accordingly, we first identify journals that have changed their policies to require data posting for published papers.

Next, we collect citation count data for empirical articles – that is, articles that analyze data – published immediately *after* a change in data posting policy, as well as analogous citation counts for articles published in the period immediately *before* the regime change. This enables us to compare the difference in citations for articles published in the same journal before and after a new data sharing requirement. Still, we were concerned about the possibility that some event in that particular field happened to influence both the change in journal policy and the change in citation rates. To account for this possibility, we collect comparable data for two natural comparison sets. First, we consider *theoretical* articles published in the same journals; since these do not use or have data, they should be unaffected by any change in data posting policy. Second, we match the (seventeen) "treatment" journals which required data posting to "*control*" journals which did not, and collect citation data for empirical articles published in these control journals.

Throughout, we use a mixture of standard statistical techniques to model citation counts for articles affected and unaffected by data posting requirements.

Identification of "Treatment" Journals

The first step in our analysis requires choosing the scientific journals which began to require the public posting of data. We began with the 250 scientific journals with the highest impact, as rated by the *Scimago Journal Rank Portal*. The Scimago journal rank indicator measures a scientific journal's impact much like a traditional impact factor, and attempts to standardize measures across fields to make impact ratings comparable, thus allowing us to examine journals from a wide variety of scientific fields (Guerrero-Bote & Moya-Anegón, 2012).[2]

For each of the 250 journals, we determined whether the journal's editorial policy included a requirement that authors of published papers share the data supporting the paper's main findings. Some journals have an editorial policy that requires authors to make a data availability statement (detailing whether or not data is available), but do not make data sharing a requirement; we do not count this as having a required data sharing policy. If data sharing policies were ambiguous (for example, if the policy read something like "we highly encourage authors to share data"), research assistants contacted the editorial staff of the journal for a clarification of the policy. If the editorial staff of a journal replied that data sharing was encouraged but not required, we again do not count that journal as having a required data sharing policy. In total, sixteen journals from the original list of 250 journals qualified as having a required data sharing policy. However, we drop one journal (*ACS Nano*) from inclusion in our treatment group since its data-posting policy has been in place since its first issue in 2007, so that it never experienced a switch in data-sharing requirement.

Additionally, research assistants attempted to identify any other journals with required data sharing policies that were not on the list of the top 250 journals. These web searches included the search phrases "academic journals with required data sharing policies" or "academic journals with required data posting policies." This identified two additional journals requiring data sharing, leaving us with seventeen "treatment" journals. The treatment journals and the dates when the data sharing requirement was implemented are tabulated in Appendix Table A1.

In the case of four treatment journals, there are plausible alternative dates for the shift in data sharing policy; these are usually associated with more stringent enforcement of a pre-existing policy. In such cases, we have both a preferred "default" date for the regime change and a plausible "alternate"; we check explicitly for robustness with respect to these dates in our analysis below.[3]

Identification of "Control" Journals

Citations to the *empirical* papers published in our (17) treatment journals are our main interest; we compare citations to papers published just *after* the policy change to citations for papers published just *before*. Citations to *theoretical* papers published in our treatment journals constitute one natural set of "control" observations. We also match our treatment journals to other journals that did *not* switch regime in the sample, as an alternative set of control observations.

Our objective is to identify control journals that do not require data posting, but that are otherwise as similar as possible to the treatment journals in terms of observable characteristics. Accordingly, we selected a set of matched journals that have never required data posting from the Scimago "Top 250" list, but are otherwise closely matched journals to our 17 focal journals. We matched treatment to control journals using the criteria used to create the SJR ranking and thus the list itself (these data are posted by Scimago on its website and include the journal's: h-index; total number of documents published; total references; total citations; country; and category). Using this information, we performed conventional propensity score matching, comparing each of the 17 journals to the other journals using the Scimago criteria. Each of the 17 treatment journals was then matched to a control journal within scientific discipline (broadly defined); some control journals were best matches to more than one treatment journal. Further details on the matching procedure are contained in an appendix.

Data Collection

For each journal, we wish to compare the number of citations for papers published before and after the enactment of the required data policy (the actual/matched date for treatment/control journals). We collected details for 200 empirical articles or two years' worth of papers (whichever is less) on either side of a policy change, for each journal. For each of these papers, research assistants then recorded the annual flow of new *Web of Science* citations received one, two, three, four, and five years post-publication. For articles published less than five years before the end of the sample (2016), we collected citations for as many years as available.

In order to determine which of the relevant articles from our journals reported empirical (as opposed to theoretical) research, we chiefly relied on Amazon Mechanical Turk workers ("MTurkers"). A survey instructed the MTurkers how to determine whether an article is empirical, before beginning to classify articles as empirical or not.[4] Two MTurkers reviewed each article, and reported whether the article is empirical in nature, as well as its DOI (digital object identifier) and the URL where they found the article. In the (88% of) cases where the answers of the two MTurkers matched, they each received a bonus and we accepted the answer. For the remaining articles, the discrepancy was resolved by research assistants trained to assess the article's empirical nature.

Data Summary

We are left with a data set of 4,403 empirical papers in our treatment journals using our default policy switch dates (4,491 when our alternate dates are used); each was published close to the date when the relevant treatment journals began to require data sharing. Since each of these papers can contribute up to five individual observations (one for citations in each of up to five years after publication), we end up with a data set of 19,679 observations (using default dates; 16,494 using alternate dates). Since most papers published in our treatment journals are empirical rather than theoretical, the analogous data set for theoretical papers published in the treatment journals has only 3,296 observations (using default dates; 2,812 using alternate dates). By way of contrast, the data set for empirical papers published in our matched control journals has 11,217 observations (using default dates; 9,384 using alternate dates).

3: Empirics

An Informal Peek at the Data

We begin with a simple examination of the data on citations. We divide our data into citations for three types of papers; a) empirical papers from our treatment journals, our chief interest; b) theoretical papers from our treatment journals; and c) empirical papers from our control journals. For each set of papers, we further split the data into citations received before and after the imposition of the data sharing policy.[5] Many of our papers receive no citations in any given year after publication but a few receive many; we thus examine the natural logarithm of (1+Citations) initially.

Descriptive statistics for the six sets of papers are provided in Table 1. For each set of papers, Table 1 provides the minimum, maximum, and mean value of log(1+Citations), as well as the 10^{th}, 50^{th} (median), and 90^{th} percentiles of the distribution. These citations include references made any time between the first and fifth years after publication, inclusive. Without exception, statistics for the data *before* the data sharing policy are similar to those corresponding *after* the policy change.

Figure 1 is a graphical analogue to the statistics of Table 1, providing histograms for each of our six sets of citations. Each of the three columns in Figure 1 represents a combination of journal (either treatment or control) and paper type (either empirical or theoretical); the top row presents the histograms for papers published before the policy change, while those below are the analogues for papers published afterwards. Like the descriptive statistics of Table 1, each histogram portrays the log of (1+Citations) received in any of the first through fifth years after publication. The histograms in the bottom row – representing citations earned *after* data sharing is required – are similar to those above, before the change in policy.

The evidence from Table 1 and Figure 1 is informal. Still, it is inconsistent with the notion that citations rise – let alone rise dramatically – with data sharing.

Papers generate citations over time, and the patterns of these citations over time could, in principle, differ with the requirement of data sharing. Since both Table 1 and Figure 1 lump together citations received at different horizons, Figure 2 takes a different tack. It provides three "event studies," one for each of our three sets of journals/papers. Consider the graph at the left of Figure 2, which portrays average citations for empirical papers published in our treatment journals. At the left of the graph, in the first year after publication, the mean flow of new citations (≈4.1) for articles published *before* the policy change is portrayed with a thin grey line; it is surrounded by a shaded +/- 2σ confidence interval. Average annual citations (and the corresponding confidence intervals) rise with time; the right side of the graph depicts the mean and +/- 2σ CI for new citations garnered five years after publication. The comparable average for papers published *after* the policy change is shown with a thick dashed line, and is always similar to the analogue for papers published *before* the change.

The middle graph in Figure 2 depicts the average for *theoretical* papers published in our treatment journals, while the graph on the right portrays empirical papers for our *control* journals. The latter two sets of data show that citations tend to increase after journal policy shifts towards required data sharing, and this shift verges on statistical significance. This is true for both theoretical papers published in our treatment journals, and empirical articles published in our control journals. That is, where the empirical papers published in our treatment journals do not receive more citations after data sharing is required, both theoretical papers published in the same journals and empirical papers published in our control journals do in fact receive marginally more citations. This implies that the requirement of data sharing might change the nature of publication, raising both the quality of theoretical submissions to treatment journals and the quality of empirical submissions to control journals.

For each of the first five years after publication, the divergence between average citations before and after the policy shift is small, neither substantively nor statistically significant. This impression is confirmed by the simple *t*-tests for differences in citation means, tabulated in Table 2. Most strikingly, there is no evidence that required data sharing *raises* citations counts. If anything, citation counts seem slightly lower after a data sharing policy, at least for the first few years after publication. The opposite is observed for both theoretical papers published in the treatment journals and the empirical papers published in our control journals, which tend to receive more citations after the policy shift. Many of these mean changes are statistically significant; we show below that such evidence fades once we controls are added for other influences.

All the work presented thus far is informal; journals and publication years are combined, and there are no controls for extraneous factors. Accordingly, we now turn to a more rigorous statistical analysis.

Model Specification and Estimation

We estimate the effect of data posting policies on citations using the following simple model:

$$Citations_{i,j,t+y} = \beta POLICY_{j,t} + \gamma \log(COAUTH)_{i,j} + \{\phi_j\} + \{\theta_t\} + u_{i,j,t} \qquad \text{for } y=1,\ldots,5 \qquad (1)$$

where:

- $Citations_{i,j,t+y}$ is the flow of new citations that article i in journal j published in year t receives in y years after publication,
- β is the coefficient of interest,
- POLICY is a binary variable that is one if journal j required data to be shared in year t and zero otherwise,
- γ is a nuisance coefficient,
- $\log(COAUTH)_i$ is the natural logarithm of the number of co-authors for article i,
- ϕ and θ are journal- and (publication-) year-specific fixed effects,
- u is an unexplained residual that accounts for the myriad reasons why citations vary.

Equation (1) estimates the coefficient of interest β, using a “within” panel estimator, since it compared citations to articles in a given journal (received y years after publication) immediately before and after the journal switches data sharing policy, after allowing for fixed effects to account for extraneous factors common to a journal or publication year. It asks the question "What is the effect of required data sharing on the flow of new citations to an article a certain number of years (y) after its publication?" Since we gather citation data for up to five years (if possible) after publication, we estimate (1) in five different cross-sections.

Equation (1) uses citations from each of the five years since publication separately. A different strategy is to pool the data for all years after publication, and estimate the effect of data sharing on citations. We pool across years since publication in two different ways. First, we constrain the effect of the policy shift to be the same – that is, an intercept shift – for all years:

$$Citations_{i,j,t+y} = \beta POLICY_{j,t} + \gamma \log(COAUTH)_{i,j} + \{\phi_j\} + \{\theta_t\} + u_{i,j,t} \qquad \text{for all } y \qquad (2)$$

We also let the effect of the policy change vary over years since publication by estimating:

$$Citations_{i,j,t+y} = \Sigma_y \beta_y POLICY_{j,t+y} + \gamma \log(COAUTH)_{i,j} + \{\phi_j\} + \{\theta_t\} + u_{i,j,t} \qquad \text{for all } y \qquad (3)$$

Where (2) forces the effect of the policy shift on citations to be the same for each year after publication, (3) allows the impact to vary by years since publication.

We estimate each of these three models in three ways. First, we estimate (1)-(3) with simple least squares (hereafter "LS"). However the regressand, citations, is far from normally distributed; citations are discrete rather than continuous, plenty of articles receive zero new citations in a given year, and no paper receives a negative number of citations. Accordingly, we also transform the regressand by taking the natural logarithm of one plus citations, and again estimate with LS. Finally, we estimate the model directly with Poisson regression to account explicitly for the count nature of the regressand. As it turns out, all three estimators deliver qualitatively similar results.

Benchmark Results

The results in Table 3 tabulate estimates of β from (1), along with its standard error, robust to clustering by journal, treating each of the (five) years since publication separately. There are nine different columns of results. The three at the left-hand side of Table 3 present results from estimating (1) on observations of empirical papers from our set of (17) treatment journals. The three columns in the middle are analogous but consider observations for theoretical papers from the treatment journals, while the three at the right use observations for empirical papers for our control journals. For each of the three different data sets, we present estimates when (1) is estimated: a) with LS using raw untransformed citations as the regressand; b) with LS, using as the regressand log(1+citations); and c) with Poisson on raw untransformed citations.

Results from our focal (empirical/treatment) data set are displayed on the left part of Table 3. The most striking result is the predominance of negative estimates of β; empirical papers published after a journal begins to require data sharing tend to receive *fewer* citations for the first three years after publication, compared with empirical papers published in the same journals immediately before the policy shift. Of the fifteen (=three estimators x five time horizon) point estimates, nine are negative. However, the effects are insubstantial and only one of the fifteen estimates is significantly different from zero at even the 5% confidence level. The finding of no statistically significant or substantive effect on citations also characterizes theoretical papers published in treatment journals (tabulated in the middle columns of Table 3) as well as empirical papers published in control journals (in the right-hand columns). Still, the most interesting result is that there are no strong indications of any *positive* effect of data sharing on citations to the empirical papers published in our treatment journals.[6]

The same message emerges when we pool the data across years since publication; Table 4 presents those estimates. Results of our key coefficient, β, estimated with (2) are tabulated in the top part of Table 4, while results from (3) appear in the bottom panel. Pooling the data also results in no consistent significant patterns across estimators. This is true of the empirical articles published in our treatment journals (results tabulated at the left), as well as the theoretical articles published in the treatment journals and the empirical articles published in our control journals (tabulated in the middle and right columns of Table 4 respectively). Still, the presence of absence remains the most striking feature of the key results at the left; there is no evidence that requiring data sharing boosts citations. Indeed, the data points, if anything, weakly in the opposite direction.

Thus far, our analysis has considered all citation levels similarly; our statistical analysis quantifies the effect of a formal data sharing policy on the level (or log) of citations. In Tables 5 and 6, we transform the regressand to consider whether data posting changes the odds of an empirical paper being either a “dog” with a small number of citations or a “hit” with a high number, if it is published in a treatment journal with a data sharing policy. In the extreme left-hand column, we consider a paper to be a dog if it receives no new citations at all. We create an appropriate binary regressand (one if the paper receives no new citations in a given year, zero if it receives any) and re-estimate (1)-(3), using probit to account for the binary nature of the dependent variable. Most of the estimates of β are positive, indicating that the probability of publishing a dog is higher after a data sharing policy is imposed, though not to a statistically significant effect degree. To check on the robustness of our definition of a dog, we use three different thresholds, defining a paper as a dog if it receives either: a) no new citations in a year; b) either no or one new citation; or c) less than five new citations. Similarly, we define hits as paper that receive: a) a flow of at least ten new citations in a year; b) at least twenty-five new citations; or c) at least fifty new citations. The results in Tables 5 and 6 indicate that the probability of a dog/hit being published after a switch in data policy tend to rise/fall, though rarely to any significant degree. Again, there is no evidence of increased citations following a data sharing policy.

To summarize, our benchmark results reveal no evidence that data posting raises citations.

Sensitivity Analysis

We now show that our results do not depend on a number of the key methodological choices we made in the results presented above.

Tables 3 and 4 use our default dates for the four treatment journals where we are uncertain about the exact date when data sharing was required. Appendix Tables A3 and A4 are analogues to Tables 3 and 4 but use our alternate dates. In the large, our results are unchanged; the alternate dates provide no evidence that articles published in journals that require data sharing receive more citations when the policy is in place. Indeed, the alternate dates provide, if anything, marginal evidence that required data posting seems to *reduce* citations slightly in the first year after publication.

Table 7 provides extensive further sensitivity analysis. We analyze the robustness of β estimates from (2), using the empirical/treatment observations, since these can be succinctly reported and are indicative of the general thrust of Tables 3 and 4. The top row of Table 7 is the default set of estimates, one each for LS regression on citations and log(1+citations), and a third for Poisson estimation of citations, all taken from Table 4. The default estimates show a negative but statistically insignificant effect of data sharing on citations for all three estimation techniques.

The first four rows of sensitive analysis in Table 7 change the specification of (2) by successively dropping: a) the control for the log of the number of co-authors (i.e., setting $\gamma=0$); b) the journal fixed effects (i.e., setting $\{\phi_j\}=0$); c) the publication year fixed effects (i.e., setting $\{\theta_t\}=0$); and d) both the journal and publication year fixed effects, which are replaced by interactions between journal and publication year fixed effects. None of this has much impact;

the estimates of β remain negative and statistically insignificantly different from zero for all three estimators.

The next seven rows of Table 7 estimate equation (2) for smaller observation sets. Successively, the rows drop: a) the first and b) last quarter of the journals by name; c) the first and d) last quarter of the sample by year; e) all Economics journals; f) all Political Science journals; and finally g) all Economics and Political Science journals. A row then replaces Poisson with Negative Binomial estimation. None of these changes to the sample alters the conclusion; the estimates of β remain small and insignificantly different from zero.

The final set of checks removes extreme values from the data set. First, three rows drop observations from the LS estimates with residuals that lie at least a) two; b) two and a half; and c) three standard deviations from zero. Then, four different sets of observations are dropped from the Poisson estimation, two sets each of small (0 and 0-1) and large (>14 and >49) flows of citations. Throughout essentially all the sensitivity analysis, the point estimates of β remain negative but statistically insignificant.

The estimates in Tables 3-4 are produced from separately handling three different data sets: a) empirical papers in treatment journals (our focus); b) theoretical papers in treatment journals; and c) empirical papers in control journals. In Appendix Tables A5 and A6, we pool the first (empirical/treatment) data set with both of the latter data sets in sequence. This amounts to a "difference-in-difference" estimator of β, since we are comparing, *ceteris paribus*, the effect of required data sharing before and after the policy shift, for papers published in journals that were either affected or unaffected by the new policy regime. Since we have two different sets of control data, we present two sets of estimates.   The results tabulated in Tables 3-4 lead one to suspect that combining the (usually insignificant and small) results from the empirical/treatment data with the (similarly insignificant and small) results from either control data set, would be expected to deliver pooled estimates akin to the empirical/treatment data. So it turns out; empirical articles affected by the policy shift experience somewhat lower citations than those articles unaffected by a policy shift (either because they do not use data or because they are published in journals that do not require data sharing). Appendix Tables A5 and A6 show that this effect is most pronounced immediately after publication but is rarely statistically significant, and is sensitive to the exact statistical technique and data comparison. Critically though, there is no evidence that authors of empirical articles published when data sharing is required are rewarded with higher citations.

The sensitivity analysis indicates that our results are robust, and do not stem from a minor assumption implicit in our methodology or a small subset of our data. Journals that require data posting do not seem to publish papers that are unusually highly cited.

Heterogeneity Across Journals

Where the sensitivity analysis of Table 7 combines data from all seventeen treatment journals, Table 8 presents individual results when β is estimated with (2) using data on empirical papers from a single journal.[7] There are twenty-one rows, one for each of the thirteen treatment journals with a clear date for the policy change, and two for the four journals with alternate dates. As in Tables 3, 4, and 7, we present three estimates of β, one each for LS on citations and log(1+citations), and a third for Poisson estimation of citations.

There is considerable heterogeneity in the effects across journals. For ten journals, the estimates are either statistically small or inconsistent across estimators. Indeed, β is insignificantly different from zero for all three estimators for seven of these ten journals. Four journals (*American Political Science Review, Journal of Political Economy, Methods in Ecology and Evolution*, and *Sociological Methods & Research*) show consistently *negative* effects of the data sharing policy on citations for all three estimators. *Development* shows consistently positive estimates of β using our default dates, but consistently negative estimates using our alternate dates. Only two journals (*American Economic Review* and *Journal of Labor Economics*) deliver consistently positive estimates of β at conventional significance levels.

The last finding – in particular, that citations rise by a statistically significant amount for the *American Economic Review* – is of particular interest, given the recent work by Christensen *et al*. (2018), who also find that citations rise consistently for articles published in the *AER* when authors share their data. The focus of Christensen *et al*. (2018) is slightly different from ours. Where we are interested in a switch *of formal journal policy* on citations, their interest lies in the effect on citations of *actual* data availability, which they measure and use on a paper by paper basis (they use the change in journal policy as an instrumental variable). Since actual data availability is costly to obtain, Christensen *et al*. collect and use data for articles published in two treatment (*American Economic Review* and *American Journal of Political Science*) and two control journals. Our wider set of seventeen treatment journals delivers weaker results than those of Christensen *et al*., which likely results from the breadth of our sample, given that our results for the *AER* echo their findings.[8] Critically, Christensen *et al*. conclude that they cannot exclude the possibility of a switch in type of submissions associated with regime switch. While we do not have evidence on this, it could explain our negative results.

4: Conclusion

When Robert Merton (1942) articulated the core values of science, the first two were universalism and communality. Universalism asserts that the validity of the scientific claim rests on the strength of the evidence, not the identity of the scientist. Communality asserts that science is "a product of social collaboration" in which openness is a bedrock principle: "Secrecy is the antithesis of this norm; full and open communication its enactment" (1942, p. 274). By adopting data-sharing requirements, journals are capitalizing on technological progress to help enforce full and open communication between scientists. Many have argued that sharing data is a scientist's obligation.

There are also personal benefits associated with posting data. Our original intent in this research was to quantify the benefit, in the form of additional citations, received by scientists forced to share their data through journal policy; we went in to this project expecting that there would be some measurable citation boost. We have found no evidence that such a benefit, in fact, exists. This naturally leads to the question of why we fail to replicate prior results showing a benefit to posting data.

The first possibility is that our hypothesis is false, and that posting data does not increase an article's impact. It could be that the discrepancy between our results and those of prior studies arises from differences in our methods. Those prior studies compared papers with and without posted data, independent of journals' requirements. Instead, the authors who chose to post their data did so voluntarily and not because the journal required them to do so. It could, of course, be possible that the authors of better papers in better journals are more likely to regard it as worthwhile to post their data. We attempted to control for these confounding factors by examining publications in the same journal in adjacent time periods. It is possible that these confounding factors explain prior findings, and that by controlling for them we make the result disappear.

It seems possible that the requirement of data sharing makes authors reluctant to publish their papers in journals that require it. That is, a data-sharing requirement might drive away excellent papers or authors who have choices about where they publish their work. If this were the case, we might see a reduction in high quality submissions to journals that require data posting and an increase in the quality of submissions at rival journals. We observe a glimmer of this result in the modest increase in citations to articles in our control journals post-policy. However, this evidence is not practically or statistically significant enough for it to qualify as persuasive. This possibility might also imply a reduction in the probability of a "hit" papers being published in journals requiring data posting. We observe no such result.

Another possibility is that any benefit of greater credibility conferred by posting data is offset by readers' increased ability to discover problems in the data analysis. Instead of adding to the persuasiveness of published work, data posting instead allows readers to "see how the sausage is made" and increases the chances that they find errors. This might imply a reduction in the rate of positive citations (though also at least a temporary *increase* in the rate of critical citations to such papers). While possible, we regard this hypothesis as unlikely. It is rare that anyone downloads posted data, even rarer that anyone goes to the trouble to re-analyze it, and rarer still that such re-analysis identifies errors significant enough to call the published results into question.

It may be that data sharing eliminates negative citations by making empirical claims more credible. That is, papers without posted data accrue more citations by other papers which criticize them or question their results. However, critical citations are relatively rare. Even rarer are critical citations whose concerns would be laid to rest by having access to the data. Thus, we are dubious that this explanation can account for our results.

The explanation that we regard as most likely is that journals are sufficiently lax in their enforcement of data-posting policies that any beneficial effects of data posting have been watered down by articles that do not in fact share their data, despite the fact that they are published in journals that require it. This explanation would reconcile our results with others showing that those articles with posted data enjoy more citations. We suspect it is not enough for journals to announce policies requiring authors to post their data. Without enforcement, authors will not go to the effort of organizing and posting their data. Indeed, the very fact that data posting is not already common practice underscores researchers' reticence to post their data voluntarily. Realization of the scientific ideal of full disclosure requires stronger incentives.

References

Benjamin, D. J., et al. (2017). Redefine statistical significance. *Nature Human Behaviour*.

Christensen, Garret, Allan Dafoe and Edward Miguel (2018) "Data Sharing and Citations: Causal Evidence from Economics and Political Science" unpublished.

Guerrero-Bote, V. P., & Moya-Anegón, F. (2012). A further step forward in measuring journals' scientific prestige: The SJR2 indicator. *Journal of Informetrics, 6*(4), 674–688. http://doi.org/10.1016/j.joi.2012.07.001.

Hamermesh, D. S. (2017). Citations in economics: Measurement, uses, and impacts. *Journal of Economic Literature.*

Henneken, E. A., & Accomazzi, A. (2011). Linking to data-effect on citation rates in astronomy. *arXiv preprint: 1111.3618*.

Merton, Robert K. 1942. "A Note on Science and Democracy." *Journal of Legal and Political Sociology* 1:115–26.

Miguel, E., Camerer, C., Casey, K., Cohen, J., Esterling, K. M., Gerber, A., Glennerster, R., et al. (2014). Promoting transparency in social science research. *Science, 343*(6166), 30–31.

Piwowar, H. A., & Vision, T. J. (2013). Data reuse and the open data citation advantage. *PeerJ, 1*, e175.

Piwowar, H. A., Day, R. S., & Fridsma, D. B. (2007). Sharing detailed research data is associated with increased citation rate. *PloS One, 2*(3), e308.

Tenopir, C., Allard, S., Douglass, K., Aydinoglu, A. U., Wu, L., Read, E., … Frame, M. (2011). Data sharing by scientists: practices and perceptions. *PloS One, 6*(6), e21101.

Table 1: Descriptive Statistics for log(1+Citations)

Journals	Papers	Before/After Policy Shift	Percentile					Mean
			Min	10	50	90	Max	
Treatment	Empirical	Before	0	0	1.6	2.8	6.3	1.6
Treatment	Empirical	After	0	0	1.6	3.0	6.4	1.6
Treatment	Theoretical	Before	0	0	.7	2.2	4.4	1.0
Treatment	Theoretical	After	0	0	.7	2.2	4.4	0.9
Control	Empirical	Before	0	.7	1.8	2.8	5.5	1.7
Control	Empirical	After	0	0	1.8	3.1	5.2	1.8

Table 2: Simple (t-) Tests for Equality of Citations before and after Regime Change

Years since Publication	Empirical Papers, Treatment Journals	Theoretical Papers, Treatment Journals	Empirical Papers, Control Journals
1	-.1 (.2)	.7* (.3)	1.5*** (.3)
2	-.4 (.3)	1.0* (.5)	1.7*** (.4)
3	-.5 (.4)	.5 (.5)	1.6** (.5)
4	.0 (.5)	-.5 (.5)	2.0*** (.5)
5	.1 (.5)	-.4 (.5)	2.3*** (.6)

Coefficients are point estimates for (average number of citations for articles published after regime switch) – (average number of citations for articles published before regime switch). Standard error recorded in parentheses; coefficients significantly different from zero at the .05/.01/.005 significance level marked by one/two/three asterisk(s).

Table 3: Effect of Policy Switch, treating years after publication separately

Years since Publication	Empirical Papers, Treatment Journals			Theoretical Papers, Treatment Journals			Empirical Papers, Control Journals		
Estimator	LS	LS	Poisson	LS	LS	Poisson	LS	LS	Poisson
Log(1+Cites)	N	Y	N	N	Y	N	N	Y	N
1	-.97 (.57)	-.11 (.06)	-.22* (.09)	.20 (.60)	-.00 (.08)	-.06 (.23)	-2.79 (2.21)	-.19* (.08)	-.36 (.21)
2	-.40 (.42)	-.03 (.05)	-.04 (.06)	.93 (.95)	-.04 (.14)	.12 (.18)	-1.28 (1.61)	-.02 (.07)	-.14 (.14)
3	-.17 (.57)	-.05 (.05)	-.01 (.07)	.27 (.83)	-.01 (.10)	-.04 (.15)	-1.82 (2.14)	-.04 (.07)	-.18 (.16)
4	.37 (.53)	.01 (.07)	.04 (.06)	-.19 (1.54)	-.03 (.19)	-.06 (.32)	-2.41 (3.02)	-.02 (.12)	-.23 (.19)
5	.18 (.66)	.00 (.06)	.02 (.07)	-.79 (1.18)	-.10 (.13)	-.17 (.24)	-2.27 (2.40)	-.05 (.09)	-.22 (.14)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with least squares/Poisson; each cell represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05 (.01) significance level marked by one (two) asterisk(s). Regressand is annual citations/article unless marked. Controls included but not recorded: intercept, fixed effects for journal and publication year, and log number of co-authors.

Table 4: Effect of Policy Switch, treating years after publication simultaneously

Years since Publication	Empirical Papers, Treatment Journals			Theoretical Papers, Treatment Journals			Empirical Papers, Control Journals		
Estimator	LS	LS	Poisson	LS	LS	Poisson	LS	LS	Poisson
Log(1+Cites)	N	Y	N	N	Y	N	N	Y	N
1-5	-.20 (.42)	-.03 (.05)	-.02 (.06)	.11 (.88)	-.04 (.11)	-.04 (.20)	-1.98 (2.15)	-.06 (.08)	-.21 (.16)
1	-.08 (.59)	-.00 (.07)	-.00 (.10)	.09 (.98)	-.01 (.14)	.03 (.22)	-2.14 (2.51)	-.05 (.08)	-.20 (.20)
2	-.17 (.23)	-.03 (.03)	-.02 (.03)	.18 (.48)	-.02 (.05)	.00 (.10)	-.93 (1.00)	-.03 (.04)	-.10 (.07)
3	-.14 (.16)	-.02 (.02)	-.02 (.02)	.10 (.27)	-.00 (.03)	-.01 (.06)	-.64 (.68)	-.02 (.02)	-.07 (.05)
4	.04 (.14)	.01 (.01)	.01 (.02)	.01 (.22)	-.01 (.03)	-.01 (.05)	-.45 (.54)	-.01 (.02)	-.05 (.04)
5	-.06 (.10)	-.01 (.01)	-.01 (.01)	-.06 (.17)	-.02 (.02)	-.03 (.04)	-.43 (.41)	-.02 (.02)	-.05 (.03)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with least square/Poisson; each column represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05/.01.005 significance level marked by one/two/three asterisk(s). Regressand is annual citations/article. Controls included but not recorded: intercepts for each year elapsed since publication, fixed effects for journal and publication year, and log number of co-authors.

Table 5: Effect of Policy Switch, treating years after publication separately, controlling for number of co-authors, empirical papers in treatment journals, dogs and hits

Years since Publication	0 Cites	0-1 Cites	0-4 Cites	>=10 Cites	>=25 Cites	>=50 Cites
1	.09 (.06)	.05 (.07)	.14 (.09)	-.34* (.16)	-.50*** (.15)	.12 (.16)
2	.00 (.08)	.00 (.08)	.02 (.06)	-.02 (.07)	-.19 (.17)	-.46* (.21)
3	.21*** (.06)	.14*** (.05)	-.05 (.10)	-.01 (.07)	-.19 (.20)	-.18 (.16)
4	-.06 (.17)	-.05 (.12)	-.06 (.06)	-.11 (.07)	.01 (.13)	-.23* (.09)
5	-.09 (.18)	-.05 (.09)	-.06 (.07)	.01 (.07)	-.22*** (.08)	-.20 (.12)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with probit; each cell represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05/.01.005 significance level marked by one/two/three asterisk(s). Regressand is 1 for specified citations, 0 otherwise. Controls included but not recorded: intercept, log number of co-authors, fixed effects for journal and publication year. Default journal regime shift dates.

Table 6: Effect of Policy Switch, treating years after publication simultaneously, controlling for number of co-authors, empirical papers in treatment journals, dogs and hits

Years since Publication	0 Cites	0-1 Cites	0-4 Cites	>=10 Cites	>=25 Cites	>=50 Cites
1-5	.04 (.07)	.02 (.04)	-.02 (.06)	-.07 (.06)	-.17 (.11)	-.22 (.12)
1	.01 (.07)	-.02 (.05)	-.08 (.10)	-.06 (.13)	-.34 (.18)	-.21 (.32)
2	.04 (.04)	.03 (.03)	.02 (.03)	-.01 (.04)	-.03 (.09)	-.22* (.09)
3	.05 (.03)	.02 (.02)	.00 (.02)	-.04 (.02)	-.07 (.05)	-.04 (.04)
4	-.02 (.02)	-.02 (.02)	-.02 (.02)	-.01 (.02)	-.02 (.03)	-.04 (.02)
5	.01 (.02)	.01 (.01)	.00 (.01)	-.02 (.01)	-.05*** (.02)	-.05 (.03)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with probit; each column represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05/.01.005 significance level marked by one/two/three asterisk(s). Regressand is 1 for specified citations, 0 otherwise; data set includes empirical papers from treatment journals. Controls included but not recorded: log number of co-authors, intercepts for each year elapsed since publication, fixed effects for journal and publication year. Default journal regime shift dates.

Table 7: Sensitivity Analysis

	LS	LS on log(1+Citations)	Poisson
Default	-.20 (.42)	-.03 (.05)	-.02 (.06)
Drop log number co-authors	-.17 (.42)	-.03 (.05)	-.02 (.06)
Drop journal fixed effects	-.62 (.48)	-.07 (.04)	-.08 (.07)
Drop publication year fixed effects	-.08 (.32)	.01 (.03)	-.01 (.05)
Substitute journal x year fixed effects	-.40 (.45)	-.06 (.05)	-.05 (.05)
Drop first quarter of journals by name	-.17 (.44)	-.04 (.05)	-.02 (.06)
Drop last quarter of journals by name	.12 (.73)	.03 (.05)	.03 (.11)
Drop first quarter of sample by year	-.15 (.44)	-.03 (.05)	-.01 (.06)
Drop last quarter of sample by year	-.16 (.45)	-.03 (.05)	-.02 (.06)
Drop Economics journals	.26 (.44)	-.01 (.06)	.03 (.06)
Drop Political Science journals	-.40 (.45)	-.05 (.05)	-.05 (.06)
Drop Economics and Political Science journals	-.05 (.38)	-.05 (.06)	-.01 (.04)
Negative Binomial			-.03 (.07)
Drop 2σ residual outliers	-.23 (.30)	-.02 (.03)	
Drop 2.5σ residual outliers	-.24 (.36)	-.03 (.04)	
Drop 3σ residual outliers	-.35 (.40)	-.03 (.05)	
Drop 0 citation observations			-.01 (.05)
Drop 0/1 citation observations			-.02 (.05)
Drop >14 citation observations			.01 (.03)
Drop >49 citation observations			-.05 (.06)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with least squares/Poisson; each cell represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05/.01.005 significance level marked by one/two/three asterisk(s). Regressand is annual citations/article except for middle column; data set includes empirical papers from treatment journals. Controls included but not recorded unless otherwise noted: log number of co-authors, intercepts for each year elapsed since publication, fixed effects for journal and publication year.

Table 8: Effect of Policy Switch, treating years after publication simultaneously, journal by journal

Journal (Default Regime Switch Dates)	**LS**	**LS, log(1+Cites)**	**Poisson**
American Economic Review	1.08*** (.37)	.19*** (.06)	.26*** (.09)
American Journal of Political Science (default)	-.42 (.47)	-.09 (.09)	-.09 (.10)
American Political Science Review	-1.58* (.75)	-.49* (.24)	-.76* (.31)
Bulletin of the American Meteorological Society (default)	-.36 (.79)	-.41*** (.13)	-1.24*** (.31)
Development (default)	1.05** (.40)	.12* (.06)	.19* (.08)
Ecological Applications	-.50 (.45)	-.09 (.09)	-.11 (.11)
Ecological Monographs	-.34 (1.05)	.02 (.11)	-.05 (.15)
Econometrica	1.07 (1.09)	.46*** (.16)	.22 (.20)
Gastroenterology	.53 (.87)	.06 (.06)	.05 (.09)
Journal of Labor Economics	2.35*** (.64)	.31* (.15)	.69*** (.18)
Journal of Political Economy	-8.69** (3.19)	-.42*** (.14)	-.86*** (.22)
Methods in Ecology and Evolution	-3.44*** (1.20)	-.40*** (.09)	-.63*** (.17)
PLoS Biology	.45 (.80)	-.04 (.09)	.05 (.10)
Political Analysis	-1.43 (1.60)	-.10 (.14)	-.27 (.32)
Proceedings of the National Academy of Sciences (default)	-.51 (.95)	-.13*** (.04)	-.06 (.10)
Review of Economics and Statistics	.60 (.33)	.05 (.05)	.16 (.08)
Sociological Methods & Research	-2.32*** (.75)	-.43* (.21)	-1.12*** (.38)
Alternate Regime Switch Dates			
American Journal of Political Science (alternate)	-.78 (1.22)	-.16 (.17)	-.16 (.25)
Bulletin of the American Meteorological Society (alternate)	1.81 (3.76)	-.02 (.32)	.17 (.33)
Development (alternate)	-1.32*** (.30)	-.30*** (.06)	-.40*** (.09)
Proceedings of the National Academy of Sciences (alternate)	-.43 (.82)	-.06 (.05)	-.04 (.08)

Coefficient on dummy variable (1 for any [1-5] year after data posting required, 0 otherwise) estimated with least square/Poisson; each cell represents a separate regression. Robust standard error recorded in parentheses; coefficients significantly different from zero at the .05/.01/.005 significance level marked by one/two/three asterisk(s). Regressand is annual citations/article unless marked otherwise; data set includes empirical papers from treatment journals. Controls included but not recorded: intercept, log number of co-authors, fixed effects for publication year and years since publication.

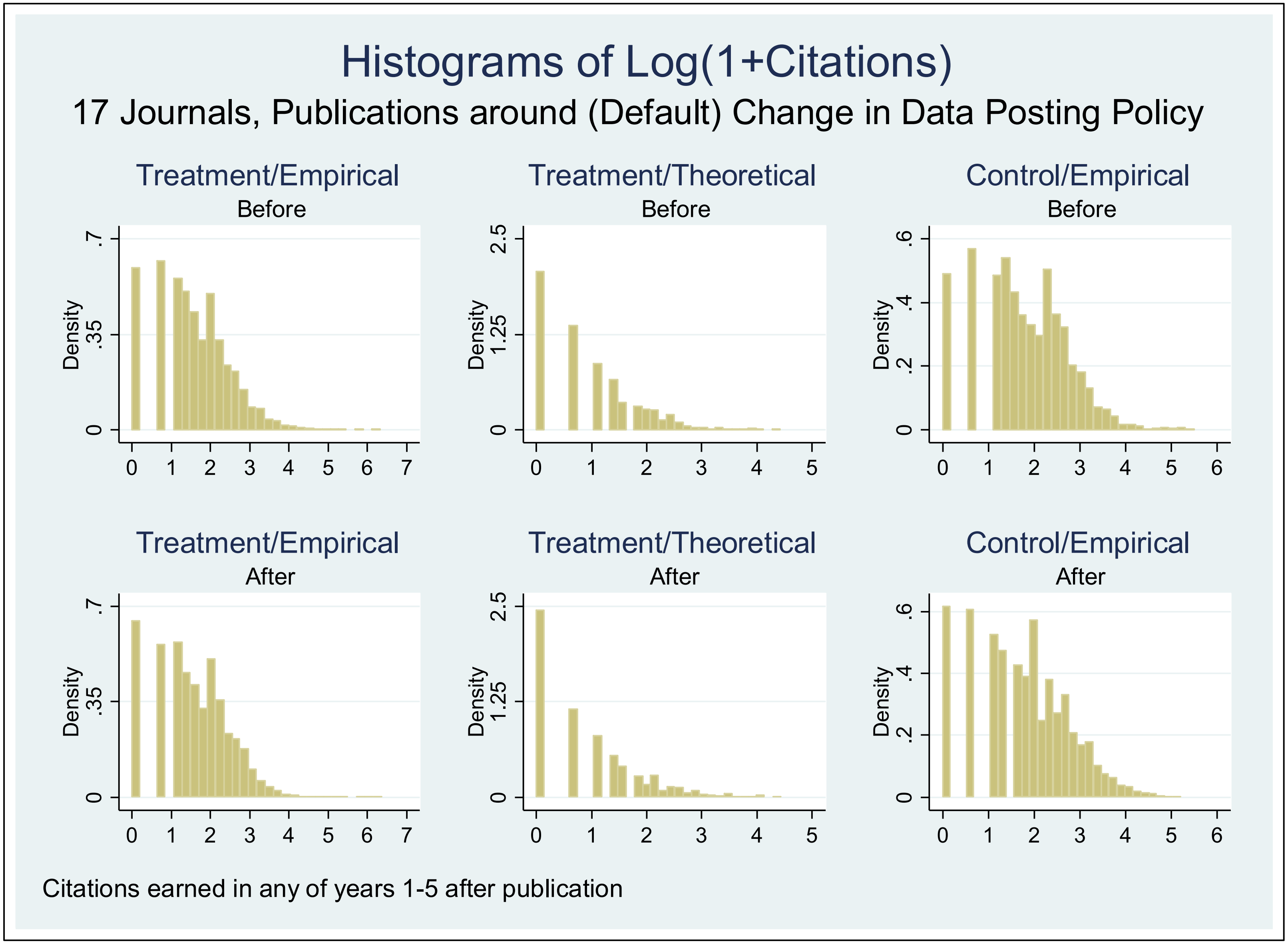
Histograms of Log(1+Citations)
17 Journals, Publications around (Default) Change in Data Posting Policy
Treatment/Empirical
Before
Treatment/Theoretical
Before
Control/Empirical
Before
Treatment/Empirical
After
Treatment/Theoretical
After
Control/Empirical
After
Density
0
.35
.7
1.25
2.5
.2
.4
.6
0 1 2 3 4 5 6 7
Citations earned in any of years 1-5 after publication

Figure 1

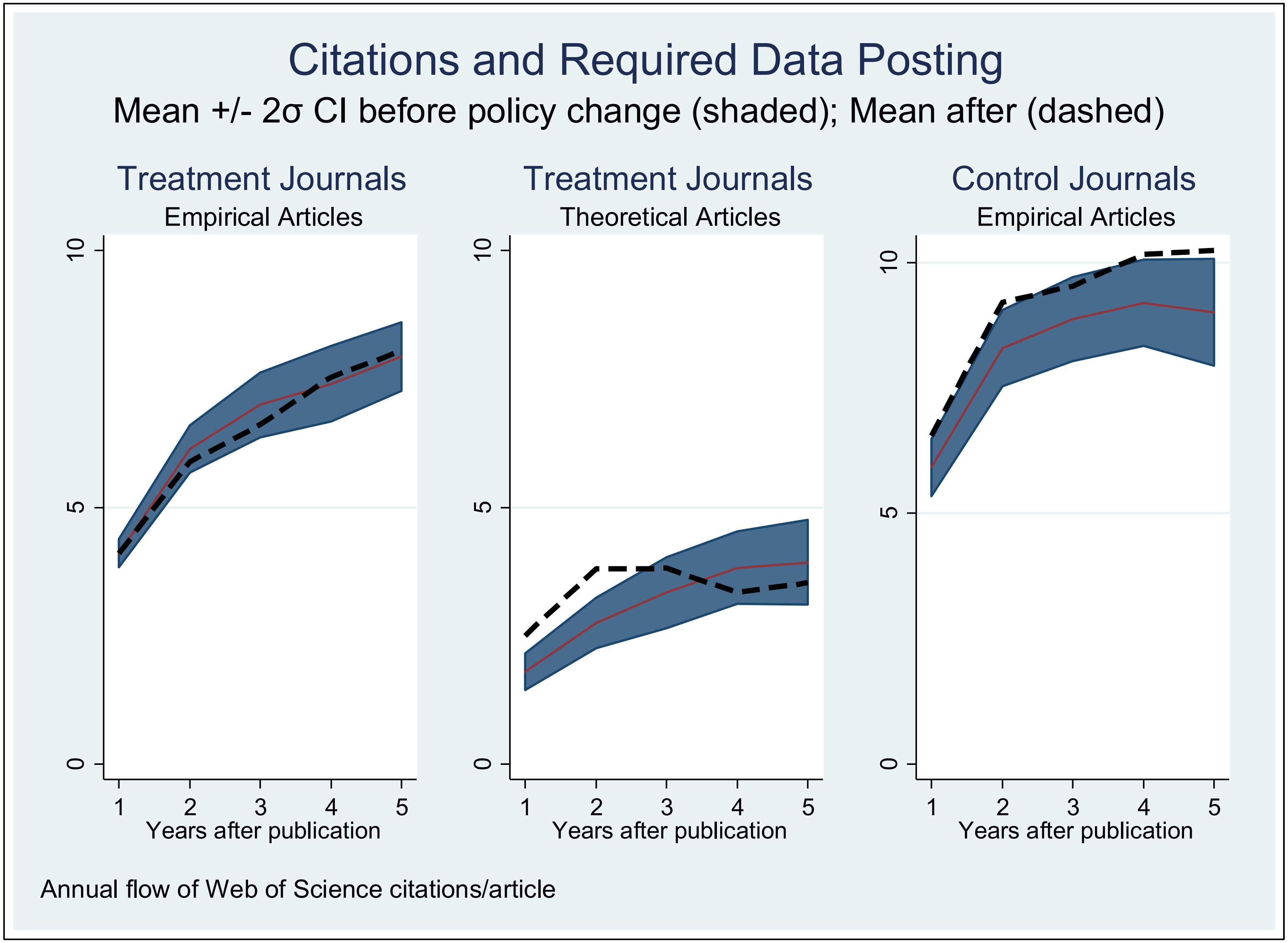
Citations and Required Data Posting
Mean +/- 2σ CI before policy change (shaded); Mean after (dashed)
Treatment Journals
Empirical Articles
Treatment Journals
Theoretical Articles
Control Journals
Empirical Articles
0
5
10
1
2
3
4
5
Years after publication
Annual flow of Web of Science citations/article

Figure 2

Appendix: Choosing the Control Journals

We chose our set of control journals using conventional propensity score matching. In particular, we downloaded the data set used by Scimago to create its rankings (freely available online from http://www.scimagojr.com/journalrank.php). This provides us with the six variables we use to match treatment to control journals: a) the journal's h-index; b) the total number of citable documents published in the last three years; c) citations/document over the last two years; d) references/document; e) the country where the journal was published; and f) the category of the journal. Since there are only two countries of relevance where our treatment journals were published, we created a binary variable for journals published in the UK, leaving American journals as the default. Since we only have a limited number of treatment journals, we also consolidated journal category into eight areas: Biology; Ecology; Economics; Medicine; Miscellaneous; Molecular Biology; Multidisciplinary; Nano technology; and Sociology and Political Science.

After creating the data set, we then created a binary variable, with unity for our treatment journals and zero for potential control journals and estimated a cross-sectional probit equation; the results are tabulated below. After estimating the probit model, we then matched each of the treatment journals to a single control journal, using the closest possible journal (by predicted probit score) within journal category. Sometimes this resulted in more than one treatment journal being matched to the same control journal. The control journals are tabulated in Appendix Table A1; the probit regression estimates are tabulated in Appendix Table A2.

Appendix Table A1: Treatment Journals with Default Dates for Formal Data Sharing Policy

Treatment Journal	Policy Change	Control Journal
American Economic Review	March 2005	Journal of Monetary Economics
American Journal of Political Science	July 2012	International Organization
American Political Science Review	March 2016	International Organization
BAMS	January 2002	Criminology
Development	February 2011	EMBO Journal
Ecological Applications	January 2014	Ecology Letters
Ecological Monographs	January 2011	Trends in Ecology and Evolution
Econometrica	January 2004	Journal of Monetary Economics
Gastroenterology	July 2007	PLoS Medicine
Journal of Labor Economics	February 2009	Journal of the European Economic Association
Journal of Political Economy	June 2005	Journal of Financial Economics
Methods in Ecology and Evolution	January 2014	Ecology Letters
PLoS Biology	March 2014	Developmental Cell
Political Analysis	January 2012	International Organization
PNAS	October 2005	Science
Review of Economics and Statistics	February 2012	Journal of Accounting Research
Sociological Methods & Research	November 2007	Journal of Public Admin. Research & Theory

Alternative Dates

American Journal of Political Science	March 2015
BAMS	December 2013
Development	February 2016
PNAS	April 2015

Note: BAMS is Bulletin of the American Meteorological Society; PNAS is Proceedings of the National Academy of Sciences of the United States of America

Appendix Table A2: Probit Model of Treatment Journal Characteristics

h-index	.006 (.003)
Citable Documents (last three years)	.0000 (.0001)
Citations/Document (last two years)	-.35 (.11)
References/Document	-.011 (.009)
UK Dummy	.69 (.39)
Pseudo R^2	.43

Regressand is binary variable; 1 for treatment journals, 0 for potential control journals. Estimated with probit; 229 observations; 41 failures completely determined. Estimated tabulated are probit coefficients with standard errors recorded in parentheses. Intercept and (8) journal area controls included but not recorded.

Appendix Table A3: Effect of Policy Switch, treating years after publication separately Alternate Policy Switch Dates

Years since Publication	Empirical Papers, Treatment Journals			Theoretical Papers, Treatment Journals			Empirical Papers, Control Journals		
Estimator	LS	LS	Poisson	LS	LS	Poisson	LS	LS	Poisson
Log(1+Cites)	N	Y	N	N	Y	N	N	Y	N
1	-1.04* (.39)	-.11* (.05)	-.15* (.07)	-.59 (1.15)	.00 (.09)	-.21 (.29)	-1.37 (.63)	-.17** (.05)	-.16** (.03)
2	-.32 (.41)	-.04 (.04)	-.03 (.05)	-.49 (1.26)	-.00 (.14)	-.12 (.23)	-.55 (1.21)	.05 (.07)	-.06 (.09)
3	-.26 (1.26)	.02 (.08)	.00 (.15)	-.38 (.94)	.01 (.13)	-.09 (.19)	.56 (.38)	.06 (.05)	.08* (.03)
4	-.95 (1.54)	.03 (.09)	-.08 (.16)	-1.92 (1.02)	-.19 (.18)	-.48* (21)	1.35 (.61)	.15 (.11)	.13 (.06)
5	-1.64 (1.88)	.04 (.12)	-.14 (.19)	-2.26* (1.00)	-.18 (.12)	-.50** (.14)	.68 (.60)	.05 (.10)	.03 (.08)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with least squares/Poisson; each cell represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05 (.01) significance level marked by one (two) asterisk(s). Regressand is annual citations/article unless marked. Controls included but not recorded: intercept, fixed effects for journal and publication year, and log number of co-authors.

Appendix Table A4: Effect of Policy Switch, treating years after publication simultaneously Alternate Policy Switch Dates

Years since Publication	Empirical Papers, Treatment Journals			Theoretical Papers, Treatment Journals			Empirical Papers, Control Journals		
Estimator	LS	LS	Poisson	LS	LS	Poisson	LS	LS	Poisson
Log(1+Cites)	N	Y	N	N	Y	N	N	Y	N
1-5	-.71 (.68)	-.03 (.05)	-.07 (.09)	-.99 (.84)	-.05 (.11)	-.24 (.17)	-.10 (.62)	.01 (.06)	-.03 (.06)
1	-.46 (.62)	-.02 (.05)	-.06 (.08)	-1.21 (1.00)	-.07 (.14)	-.21 (.24)	-.06 (.60)	-.03 (.06)	-.11 (.10)
2	-.33 (.31)	-.04 (.03)	-.04 (.04)	-.45 (.48)	-.02 (.05)	-.10 (.09)	-.19 (.49)	.02 (.04)	.00 (.03)
3	-.37 (.31)	-.02 (.02)	-.04 (.04)	-.31 (.24)	-.02 (.03)	-.09* (.04)	.05 (.16)	.02 (.02)	.01 (.01)
4	-.13 (.24)	.02 (.02)	-.00 (.03)	-.18 (.19)	.00 (.03)	-.05 (.05)	.07 (.16)	.01 (.02)	.01 (.02)
5	-.23 (.18)	.00 (.01)	-.02 (.02)	-.22 (.15)	-.01 (.02)	-.07 (.04)	-.09 (.09)	-.01 (.02)	-.02 (.01)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with least square/Poisson; each column represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05/.01.005 significance level marked by one/two/three asterisk(s). Regressand is annual citations/article. Controls included but not recorded: intercepts for each year elapsed since publication, fixed effects for journal and publication year, and log number of co-authors.

Appendix Table A5: Effect of Policy Switch, treating years after publication separately, difference in difference analysis

Comparison Set	Theoretical Papers, Treatment Journals			Empirical Papers, Control Journals		
Years since Publication						
Estimator	LS	LS	Poisson	LS	LS	Poisson
Log(1+Cites)	N	Y	N	N	Y	N
1	-.84 (.53)	-.10 (.05)	-.21* (.09)	-1.65 (.90)	-.14** (.05)	-.36 (.20)
2	-.26 (.39)	-.03 (.05)	-.03 (.06)	-.74 (.66)	-.03 (.04)	-.14 (.14)
3	-.13 (.51)	-.04 (.05)	-.01 (.07)	-.79 (.88)	-.04 (.04)	-.18 (.16)
4	.32 (.49)	.00 (.06)	.04 (.06)	-.51 (1.08)	-.00 (.06)	-.23 (.19)
5	.09 (.61)	-.01 (.05)	.01 (.07)	-.60 (.97)	-.01 (.05)	-.22 (.14)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with least squares/Poisson; each cell represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05 (.01) significance level marked by one (two) asterisk(s). Regressand is annual citations/article unless marked. Controls included but not recorded: a) intercept, fixed effects for b) journal and c) publication year, d) log number of co-authors, and interactions of a)-d) with comparison set dummy.

Appendix Table A6: Effect of Policy Switch, treating years after publication simultaneously, difference in difference analysis

Comparison Set	Theoretical Papers, Treatment Journals			Empirical Papers, Control Journals		
Years since Publication						
Estimator	LS	LS	Poisson	LS	LS	Poisson
Log(1+Cites)	N	Y	N	N	Y	N
1-5	-.17 (.38)	-.05 (.04)	-.03 (.05)	-.84 (.84)	-.04 (.04)	-.21 (.16)
1	-.07 (.52)	-.01 (.06)	.00 (.09)	-.82 (.98)	-.02 (.05)	-.09 (.12)
2	-.13 (.21)	-.04 (.02)	-.02 (.03)	-.45 (.40)	-.03 (.02)	-.06 (.04)
3	-.11 (.14)	-.02 (.02)	-.02 (.02)	-.32 (.28)	-.02 (.01)	-.04 (.03)
4	.03 (.12)	.00 (.01)	.01 (.02)	-.12 (.22)	-.00 (.01)	-.02 (.03)
5	-.06 (.08)	-.02* (.01)	-.01 (.01)	-.19 (.16)	-.01 (.01)	-.03 (.02)

Coefficient on dummy variable (1 for after data posting required, 0 otherwise) estimated with least square/Poisson; each column represents a separate regression. Robust standard error (clustered by journal) recorded in parentheses; coefficients significantly different from zero at the .05/.01.005 significance level marked by one/two/three asterisk(s). Regressand is annual citations/article. Controls included but not recorded: a) intercept, fixed effects for b) journal and c) publication year, d) log number of co-authors, e) years elapsed since publication and interactions of b)-e) with comparison set dummy.

Endnotes

[1] Unlike Christensen et al. (2018), we do not check that data for a given article is actually available, only that it is published in a journal which requires it to be available.

[2] Scimago is available at http://www.scimagojr.com/journalrank.php; in particular, we use the SJR indicator.

[3] For instance, in the case of *AJPS*, in July 2012 the editor announced a policy that authors should provide data on *Dataverse*. However, compliance was not checked or verified, and in March 2015 *AJPS* tightened replication requirements. *BAMS* required, in Jan 2002, the "Free and Open Exchange of Environmental Data" while in December 2013 it required "Full and Open Access to Data." In the case of *Development*, in February 2011 a policy (posted on the website) required authors to upload micro array data within six months of publication, and to upload gene sequence data at the time of submission. In February 2016, *Development* formed a partnership with Dryad which required authors to upload micro array and gene sequence data at article submission. *PNAS* made a data posting requirement in "Information to Authors" in October 2005, but tightened policy in April 2015 by a required data availability statement.

[4] The survey is available at https://berkeley.qualtrics.com/jfe/form/SV_0SoVnb1GTvoPgCF?Q_= .

[5] Control journals, by construction, do not experience any policy shift; we use the corresponding dates for the matched treatment journals.

[6] Our estimates of γ, the effect of the number of co-authors on citations, are consistently positive. We consider this to be a control rather than a coefficient of interest, but are reassured by its stability; this indicates to us that are methodology does not preclude a positive finding, should it be present in the data.

[7] With an intercept replacing the now-irrelevant journal fixed effects.

[8] There are also smaller differences in our methodologies: a) where we use a window around the regime switch date of +/2 years, or 200 articles (whatever's lower), they use +/- 4 years; b) we use Web of Science citations, where they use Scopus; and c) their default methodology uses interactive journal x year fixed effects instead of separate journal and year fixed effects.