Do Conflict of Interests Disclosures Work? Evidence from Citations in Medical Journals

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Abstract

Financial ties between drug companies and medical researchers are thought to bias results published in medical journals. To enable readers to account for such bias, most medical journals require authors to disclose potential conflicts of interest. For such policies to be effective, conflict disclosure must modify readers' beliefs. We therefore examine whether disclosure of financial ties with industry reduces article citations, indicating a discount. A challenge to estimating this effect is selection as drug companies may seek out higher quality authors as consultants or fund their studies, generating a positive correlation between disclosed conflicts and citations. Our analysis confirms this positive association. Including observable controls for article and author quality attenuates but does not eliminate this relation. To tease out whether other researchers discount articles with conflicts, we perform three tests. First, we show that the positive association is weaker for review articles, which are more susceptible to bias. Second, we examine article recommendations to family physicians by medical experts, who choose from articles that are a priori more homogenous in quality. Here, we find a significantly negative association between disclosure and expert recommendations, consistent with discounting. Third, we conduct an analysis within author and article, exploiting journal policy changes that result in conflict disclosure by an author. We examine the effect of this disclosure on citations to a previously published article by the same author. This analysis reveals a negative citation effect. Overall, we find evidence that disclosures negatively affect citations, consistent with the notion that other researchers discount articles with disclosed conflicts.

Keywords: Financial interests; Bias in medical research; Research and development; Disclosure regulation; Transparency

JEL classification: D83, D84, G18, K20, L51, M40, O31

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1. Introduction

Financial ties between drug companies and doctors are thought to bias doctors' judgments about the value of medical treatments. Physicians given free drug samples or attending company-sponsored symposia are more likely to prescribe sponsors' drugs (Orlowski and Wateska 1992; Engelberg et al. 2014). Similarly, research funded by drug companies is more likely to find favorable treatment effects (Bekelman et al. 2003; Sismondo 2008; Oostrom 2019). Potential reasons range from implicit bias to drug companies' control over research design or their suppression of adverse research results (Collier and Iheanacho 2002; Moore and Loewenstein 2004; Sage 2006).

One solution is to bar such financial ties altogether. Laws such as Stark I and II prohibit physicians from making referrals for diagnostic tests at labs to which they have a financial tie (Bethard 2004). Many medical schools bar medical product companies from sponsoring student events on their campuses (AMSA 2016). Prominent journals, such as The New England Journal of Medicine (NEJM), from 1990, and The Lancet, from 2004, refused publishing reviews and/or editorials by authors with relevant financial conflicts (Relman 1990; Drazen and Curfman 2002; James and Horton 2003). Interestingly, editors of both journal later relaxed their bans as they had difficulty finding qualified authors without conflicts (Drazen and Curfman 2002). Likewise, the U.S. Food and Drug Administration for a period barred doctors with ties to drug companies from serving on advisory committees making recommendations on drug approvals, but the policy has been revised because it was difficult to find physicians without a conflict (Pham-Kanter 2014).

A reason not to ban financial ties is that they can also provide benefits. About two thirds of the roughly $150 billion in U.S. medical research conducted each year is funded by drug companies (Research America 2017). The pharmaceutical industry is particularly important for

clinical research: in 2014 it funded 6,550 human trials as compared to 1,048 funded by the NIH (Cohn 2015). The free exchange of information between researchers and manufacturers could facilitate technological advances in medicine, and hence stigmatizing industry interaction could be detrimental to innovation (e.g., Epstein 2010; Stossel and Stell 2011). Financial ties to industry could also facilitate the adoption of new drugs: for instance, physicians with drug company ties were early adopters of calcium channel blockers for hypertension (Rosenbaum 2015).

An alternative to a ban on financial ties is the disclosure of such conflicts to relevant parties. Recently, the US adopted the Physician Payment Sunshine Act, which requires medical product companies to disclose all payments over USD 10 to physicians on a public website. Physicians are required to disclose to patients if they received a fee for referring their patients to a clinical trial (Sah et al. 2016). Nearly all medical journals require disclosure of sources of funding and other financial conflicts of interest by journal authors (Malani et al. 2015; Shawwa et al. 2016). The idea behind such disclosure regulation is to enable readers to interpret the findings in light of the conflicts. Moreover, to the extent that readers and other researchers view certain conflicts as problematic, they can discount articles with such conflicts, which in turn provides researchers with incentives to choose financial ties with drug companies judiciously. Therefore, disclosure regulation is often viewed as an alternative to a ban, as well as a compromise between the concerns about conflicts and the potential benefits of financial ties.[1]

[1] Although this paper focuses on conflict disclosures in medical research, there is a broader trend towards disclosure regulation in lieu of explicit rules for behavior in many areas (e.g., Fung et al., 2007; Leuz and Wysocki, 2016). But there are also examples combining the two policy tools. In U.S. elections, some rules limit outside contributions to political campaigns while others permit them but require that contributions be disclosed (Federal Election Commission 2015). U.K. corporate governance follows a comply-or-explain approach, setting standards for audit committees, remuneration committees and boards, from which a company may deviate, but then it has to provide an explanation, which in turn could trigger a market sanction (Cadbury Report, 1992). Similarly, U.S. law firms are barred from representing new clients whose interests may conflict with existing clients, unless the law firm discloses that conflict to both existing and new clients, and both clients decide to waive it (Patterson 1980).

It is still an open question whether conflict of interest disclosures work as intended. Our contribution is to examine how conflict of interest disclosures in scholarly articles in medical journals affect citations by other medical researchers. A necessary, though not sufficient,[2] condition for conflict disclosures to work is that they affect readers' beliefs and assessments of research. There is survey evidence that doctors view hypothetical articles more negatively if they disclose drug company funding (e.g., Kesselheim et al. 2012). But we need more systematic evidence on how people respond to actual conflict disclosures, rather than hypothetical ones. For example, Sah et al. (2016) find that conflict disclosure can lead patients to trust a source more.

We choose the medical research setting for several reasons. Industry funding and financial ties with drug companies are important and commonplace in medical research. Medical journals have introduced disclosure regimes for published articles, which has led to a significant increase in the frequency of conflict disclosures (e.g., Malani et al., 2015). Medical researchers likely have a sophisticated understanding of the various ties with drug companies, the relevant tradeoffs, as well as the disclosures. By using citations, we take a revealed-preference approach, inferring a potential "discount" for research with conflicts from citation behavior rather than a survey. Prior research used citations in other contexts to measure economic effects (Borjas and Doran 2012; Azoulay et al. 2013; Azoulay et al. 2015; Azoulay et al. 2017). Citations are a good metric as they are not only used as an indicator of article quality and information value, but also matter in the academic labor market (e.g., they are employed by universities in tenure decisions).

[2] It is clearly not sufficient that readers respond to disclosed conflicts for disclosure requirements to be welfare enhancing. Other important questions are whether the disclosures accurately reveal conflicts and how researchers adjust their behavior due to the disclosure requirement. For example, conflict disclosure may discourage certain research to avoid being seen as conflicted (Shaywitz and Stossel 2009), or it may morally license even more biased research (Cain et al. 2005). We do not examine whether revelation is accurate as we observe disclosed, not actual, conflicts. We also do not examine the effects of the disclosure regime on researchers' behavior or published research.

Given that financial ties with drug companies are thought to bias research, we expect other researchers to discount conflicted articles. Thus, disclosure of industry ties should reduce citations to an article. We test this relation using over 17,000 research and review articles in 7 medical journals during a 21 year period, from 1988 to 2008 – a period over which the disclosure of conflicts substantially increased as medical journals introduced conflict of interest disclosure policies. We hand-coded conflict of interest disclosures by each author of these articles, obtained data on citations to these articles and certain article characteristics from Thomson-Reuters, and scraped additional characteristics from PubMed, a web portal for medical articles.

A major challenge for our tests is that financial ties are subject to selection. Drug and medical device companies likely offer financial benefits, such as funding and advisory relationships, to higher quality researchers who in turn would garner greater citations irrespectively. Industry funding could also enable particularly important or novel studies (e.g., vaccine development for COVID-19). Such non-random assignment of financial ties could lead to a positive association between disclosures and citations, masking the predicted negative effect of disclosure. Consistent with positive selection, we find that articles with disclosed conflicts have over two and a half times as many citations than articles that disclose no conflicts.[3] As illustrated in Figure 1, this difference is persistent and, if anything, increasing over time.

We tackle selection in three ways. First, we add explicit controls for article quality. If selection were based solely on these variables, then this approach would allow us to determine if disclosures reduce citations. However, there are likely aspects of article quality that are observable to other (citing) researchers but not captured by our control variables. Consistent with this line of

[3] Alternatively, medical product companies may seek out authors who they perceive to be more susceptible to bias, which would likely exaggerate the predicted negative effect of disclosure on citations for the average article. In that sense, our analysis provides novel descriptive evidence on the selection of researchers by drug companies.

reasoning, we find that quality controls reduce the positive association between disclosure and citations, but they do not eliminate it. Next, we exploit differences in article types, namely original research and reviews. Review articles are often viewed as more susceptible to bias (e.g., Relman 1990; Hansen et al. 2019). Thus, if articles are discounted due to disclosed conflicts, this effect should be more pronounced for reviews and hence the positive association between disclosures and citations should be further attenuated for reviews. Our results are consistent with this conjecture, showing a negative interaction between reviews and disclosed conflicts.

Second, we analyze a setting, in which quality selection of articles with and without disclosed conflicts is minimal or at least muted. Specifically, we examine whether an article is recommended by research experts in a University of Chicago program called Priority Updates from the Research Literature (PURL). The purpose of the PURL program is to identify and disseminate important and relevant research studies to family physicians. A PURL recommendation is less susceptible to quality selection because the Program picks recommendations from a pool of articles, which have been pre-selected ("nominated") by experts, on the basis of quality and importance of findings. To the extent that the nomination process screens out lower quality articles (and authors) that would not have received financial support from drug companies, it reduces the positive selection effect from financial ties. Applying this strategy at the article level, we find that nominated articles with disclosed conflicts are less likely to be recommended as PURL by the Program. Thus, our findings in this setting are consistent with experts discounting articles with disclosed conflicts.

Third, in the spirit of Azoulay et al. (2015) and Azoulay et al. (2017),we return to citations and conduct a difference-in-differences analysis within-author for a given article, in which we can fix unobservable article quality and other characteristics. Specifically, we exploit the fact that many

medical journals introduced substantially tighter disclosure policies around 2001/2002. We examine how a first-time conflict disclosure by an author in one article published after the regime change at time $t + k$ affects citations to another article by the same author published at an earlier time t. We then compare this to the change in citations to an article published at time t by a matched control author who does not disclose a conflict in another article at time $t + k$. In this analysis, we find that conflict disclosure has a negative citation effect. Citations to the previously non-disclosing article decrease on average by up to 7 percent. This effect is substantially larger for review articles, consistent with our earlier findings for reviews. Additionally, we show that the negative effect persists over time and, if anything, becomes larger. It is robust to alternative matching algorithms to identify articles for treatment and control authors. The negative citation finding is remarkable as it relies on spillover effects from articles disclosing conflicts to previously published articles without such disclosures. In this sense, our strategy is conservative as conflict disclosures presumably have a larger discount effect on articles in which they are provided.

Our paper contributes to the literature in several ways. First, we provide novel evidence consistent with the notion that researchers (or readers) discount articles that disclose authors' financial ties with drug companies. Our evidence is based on actual citation behavior by well-informed readers, and hence our revealed-preference evidence complements prior work using survey experiments (e.g., Chaudhry et al. 2002; Schroter et al. 2004; Kesselheim et al. 2012). Our spillover results indicate that conflict disclosures modify other researchers' beliefs about the quality of articles and in doing so shed light on the necessary condition for conflict disclosures to be effective. This evidence is important, considering the widespread use of disclosure and transparency mandates to address concerns about conflict of interests (Fontanarosa and Bauchner 2017) as well as in public policy more generally (Fung et al. 2007).

Second, we more broadly contribute to the literature on conflict of interests. Such conflicts are deemed to be particularly important in medicine and medical research (e.g., Lexchin et al. 2003; Engelberg et al. 2014; Oostrom 2019), but they are also prevalent in regulatory and financial settings. For example, conflicts of interest frequently arise with financial analysts or rating agencies (e.g., Michaely and Womack 1999; Agrawal and Chen 2008; Sangiorgi and Spatt 2017). Consistent with evidence that conflicts can bias behavior, our study shows that, on average, researchers view conflicts negatively. Such evidence is important as it implies that conflicts are "priced" (have penalties), which is a necessary condition if disclosure regimes are to help in minimizing bias from conflicts of interest.

Third, our study provides descriptive evidence on the prevalence of financial ties with drug companies, which we show are subject to pervasive selection effects. While prior investigations analyze which research areas drug companies tend to fund (e.g., Acemoglu and Linn 2004; Budish et al. 2015), we are not aware of systematic evidence on which *researchers* drug companies fund. The matching of productive, high quality researchers with drug companies is one of the most striking and robust features of our data.

2. Conceptual Underpinnings and Research Design Challenges

For the purpose of this study, we take the evidence on research bias from industry conflicts as given and analyze discount medical research articles that disclose industry conflicts. For a disclosure policy to be effective and work as intended, disclosing a conflict of interest must modify readers' beliefs.[4] Given prior evidence that financial ties with the industry can positively bias research findings or the conclusions drawn from them, we expect readers to draw negative

[4] A potentially important "target audience" in this regard are referees. Interestingly, John et al. (2019) find no effect of conflict disclosures on referees in the field of emergency medicine.

inferences about the quality of an article and its findings from the disclosure of industry funding or other conflicts.

With such a discount or negative inference, researchers with conflicts should be reluctant to disclose their industry ties unless they are required to do so. That is, we should see little voluntary disclosure of conflicts. This prediction presumes that conflict disclosures provide only negative information about the quality of the article and its findings, and not also information about other quality attributes of an article or the quality of its authors. However, it seems plausible that industry funding could enable researchers to do better studies or that drug companies choose higher quality authors as their consultants and advisors. To the extent that these quality attributes are not observable or known to readers, the disclosed conflict itself can provide positive information and hence lead to positive inferences. In this case, the incentive to withhold information about conflicts changes. If in turn disclosure only led to positive inferences about article and author quality, then all authors would voluntarily disclose their conflicts. The latter is not what we observe in practice. For instance, it is inconsistent with the fact that most medical journals have adopted requirements to disclose conflicts of interest over the years and that authors' conflict disclosures increase substantially around the introduction of these policies (e.g., Malani et al., 2015). Thus, observed disclosure behavior suggests that at least for some authors the costs of disclosure outweigh the (incremental) quality signal from the conflict. The latter is plausible considering that readers have many other quality signals. Researchers know each other as well as the institutions at which the authors work. They see presentations at conferences and in workshops. Moreover, they are experts in their fields and trained to form beliefs about the quality of research from reading or reviewing articles. Thus, while it is possible that other researchers infer information from the conflict

disclosure, the incremental (quality) information could be relatively small. Authors then tradeoff this incremental signal against any discount that readers apply for conflicts of interest.

This discussion highlights that there are three relevant sets of information. One set is other information that is observable to readers, even in the absence of disclosure. This set includes positive or negative information about the authors from any other source, including the average propensity of authors to have industry conflicts, as well as information about the quality of the findings that is gleaned from the article itself. The second set is incremental positive information that the disclosure provides. The third set is orthogonal negative information due to the nature of the disclosed conflicts and their implications about bias in the research findings. If there is no incremental positive information, authors provide conflict disclosures only when they are required to do so (and such policies are enforced). With incremental positive information, there is a tradeoff between the effects of the second and third information set. As a result, we would expect some authors to disclose conflicts voluntarily, even in the absence of a mandate, and for them, readers' inferences should be on net positive. Thus, as long as the second information set is not empty, it is not obvious that readers draw on average negative inferences from conflict disclosures, even when a discount exists.

A key challenge for our analysis is therefore to account for article (and author) quality and the associated selection effects. Clearly, observed conflicts are not randomly assigned to authors and articles and hence one cannot simply measure the potential discount for conflicted articles by comparing readers' beliefs across articles with and without conflict disclosures. In addition, we face the empirical challenge that we do not observe all the quality attributes in the first information set that readers and other researchers have when they form beliefs about an article. Without

properly controlling for these quality attributes, we might not find a conflict discount even when it exists.

Finally, we note that we cannot use the introduction of mandated conflict disclosure at various medical journals as an instrument for observed conflicts. The reason is that the average change around the introduction of the mandate essentially reflects readers' updating about the prevalence of conflicts and hence could be positive and negative.[5]

3. Conflicts of Interests, Citations and Article Quality

In this section, we describe our data on journal articles, their characteristics and citations as well as the hand-collection of conflict of interest disclosures. We then report the results from baseline cross-sectional regression analyses that relate disclosed financial ties with drug companies with article citations, controlling for observable measures of article quality. We use citations by other researchers to measure readers' beliefs about article quality and conflicts of interests. We later design progressively stringent analyses to test whether readers discount articles that disclose conflicts of interest.

3.1. Data and Sample Selection

Our sample consists of articles from seven general-interest medical journals: American Journal of Medicine (AJM), Annual Review of Medicine (AR), British Medical Journal (BMJ), Journal of the American Medical Association (JAMA), Lancet, Mayo Clinic Proceedings (Mayo), and New England Journal of Medicine (NEJM). Aside from them providing us with a broad cross-section

[5] To see this, assume that in the pre-disclosure period readers cannot distinguish between articles with and without conflicts. Suppose further they know that on average 20% of the articles have conflicts and apply a commensurate discount to all articles. In the post period, readers learn that only 15% of the articles have industry conflicts and hence discount those but not the remaining 85%. The IV estimate in this case would be positive, reflecting the update from 20% to 15%, even though readers apply a discount.

of articles from all fields of medicine, we select these journals because of their disparate conflict disclosure policies, a source of variation we exploit in Section 5. Four of these journals are members of the International Committee of Medical Journal Editors (ICMJE). All but two journals (AR, Mayo) are listed by the ICMJE (www.icmje.org) as currently following the Uniform Requirements for Manuscripts established by the ICMJE. These requirements include submission guidelines and rules regarding the disclosure of conflicts, though journals can deviate from these requirements.

We purchased from Thomson Reuters a list of all items published in these seven journals during 1986-2010. To focus on items with a substantive impact on clinical medicine, we retained only those articles that Thomson Reuters categorized as a research or a review article and excluded, for example, editorials and news articles. Research articles are those that contain primary research; reviews are articles that synthesize research articles. Due to resource constraints, we randomly sampled a fourth of research articles published prior to 1999, and randomly sampled a fourth of all articles after 2003. We retained all articles between 1999 and 2003, as this time period contains critical changes in journal policies. In reviewing the sample, we found the categorization of research and review articles of Thompson Reuters to be overly inclusive, as it would fail to filter out extraneous articles, such as for instance images published in NEJM's *Images in Clinical Medicine* section, or humorous articles in BMJ's Christmas issue.[6] In order to retain only substantive articles, we implement a detailed data cleaning algorithm, described in the Data Appendix.

[6] Including extraneous articles would contaminate the observed association between citations and conflict of interest disclosures, as disclosures rarely accompany such articles, and they also receive scant citations.

Due to the computation of citation variables (described in the next section), we limit our sample period to articles published between 1988 and 2008. Our final article sample includes 18,843 articles. To obtain further article characteristics, we scraped records from PubMed, a free online database of life sciences maintained by the U.S. National Library of Medicine, and found data for 17,931 articles (95.2% of our sample). We extracted PubMed's classification of the article type (e.g., review, clinical trial, comparative study), and Medical Subject Headings (MeSH).

A team of undergraduate research assistants recorded the conflict of interest disclosures by authors who were listed in the byline or materially contributed to each article. A random subset of articles from each journal and year was checked by a separate research assistant to assess error rates and improve coding. Conflicts were categorized by the source of the conflict (industry, government, educational institution, other) and the nature of the relationship (defined below). In this study, we focus on authors' relationships with industry, i.e., commercial conflicts of interest. We interpreted ties with industry in a broad sense including various profit-oriented enterprises (e.g., drug manufacturers, suppliers of medical equipment, private policy consultants, and industry lobby groups). We do not include for-profit health care institutions such as hospitals, nursing homes, or insurance providers in our industry category.

3.2. Key Variables

For each publication in our article sample, we obtain from Thomson Reuters all citations from the time they were published until 2011. From these data we calculate 3-year citation counts.[7] Although an article may be published part way through a year, citation data only indicate the year of a citation to that article. Therefore, when we calculate 3-year citations, we count the year of

[7] The correlation between 1, 3 and 5-year citation counts is above 0.9. We use 3-year citations as a compromise. They can be calculated for more articles than 5-year counts but contain more information than 1-year counts.

publication plus the following three years.[8] To address the fact that articles have a citation life cycle and that the citation count is for three years plus up to 12 months, depending on the month of publication, we include indicators for the month in which an article is published in all regressions. While we have citation data starting in 1986, we begin the analysis in 1988 because we include author- and organization-related controls that rely on the number of citations garnered prior to publication. As our citation data extend only until 2011, we cannot calculate 3-year citations for articles from 2009 and 2010 and hence we end the analysis in 2008.[9]

Although it is possible for an article to cite another in order to criticize it, we count each citation positively. The reason for this choice is that negative citations appear to be rare in the medical literature and unlikely to affect overall citation counts. Our assessment is based on two tests. First, we carefully examined citations to an article that should have generated a relatively high rate of negative citations: the 2000 NEJM article that reported the VIGOR study, which was later criticized for masking severe cardiovascular side effects of Vioxx (Bombardier et al. 2000; Flapan 2004).[10] We find that only 3% of the citations to this article are negative. Second, we carefully examine citations to randomly selected articles from our sample that are severely conflicted. We find that only 2.6% of citations are negative, and rarely refer to conflicts.

That said, not all (positive) citations have equal value. Some citations are authors citing themselves (self-citations) and some are citations by articles in less prestigious journals. To address the former, we omit self-citations from our counts (e.g.,Azoulay et al. 2013; Azoulay et al. 2015). For the latter, we also conduct analyses using impact-factor weighted citation counts

[8] For example, if an article is published on July 1, 2000, we include all citations from 2000 (which includes about 6 months of possible citations) and all citations from 2001, 2002, and 2003.

[9] We obtain very similar results if we instead use 1-year citations and include articles in 2009 and 2010.

[10] Vioxx is an alternative to non-steroidal anti-inflammatory pain medication (NSAID). Vioxx's purported benefit was that it avoided the gastrointestinal pain associated with NSAIDs. However, it was later discovered that taking Vioxx was associated with greater risk of death from cardiovascular causes.

based on the citing journal's impact factor. Details are provided in the Data Appendix. Finally, we use the log of citations (plus one) to address the skewness in citation counts.

We classify articles by type: research or review. We employ data from Thomson Reuters, PubMed and the journal tables of contents to categorize articles as research or reviews. Details of the categorization are provided in the Data Appendix. We also code study type and subject matter. For 44.8% of research articles matched to PubMed records, PubMed assigned at least one study type, including clinical trial, comparative study, meta-analysis, case study and others. Assigned study types are non-exclusive, but have little overlap. For 84.5% of research articles and 81.4% of reviews matched to PubMed records, we are able to categorize articles to subject areas using category C (diseases) of the MeSH headings.

The primary variables of interest indicate disclosed conflicts. Disclosed financial relationships with the drug industry for a given author are assigned to the following categories: author or study received funding ("funding"); author or study received drugs or other medical equipment ("gave drug or materials"); author was an employee ("employee"); author was a consultant or advisor ("consultant"); author received a fee/honorarium for speaking, lectures or other non-specified reason ("honoraria/speaker"); author received an award ("award"); author owned equity or stock options pertaining to the study ("equity"); or author had some other type of relationship ("other"). An author is categorized as having a "drug COI" if he or she fell into any one of the categories above, or if a study-level conflict existed. To simplify the analyses and tables, we also aggregate conflicts into subcategories for some analyses (e.g., all individual-level conflicts such as consultant, honoraria/speaker, award, or equity). We code an article as having a conflict if any of its authors had a conflict. We also create article-level variables indicating specific types of conflicts if any of the authors had a conflict in the respective category.

Table 1 provides summary statistics on a subset of variables for each of the seven journals. As expected given the journal choice, the table shows substantial heterogeneity in the number of articles and also citations. Articles published in the two most prestigious journals in our sample, NEJM and JAMA, tend to have more disclosed conflicts, which could reflect both a positive association between article quality and conflicts and stricter disclosure requirements in those journals (e.g., Malani et al. 2015).

Panel A and Panel B of Table 2 provide summary statistics for the binary and continuous variables used in our analyses, respectively. Articles with conflict disclosures have more raw and adjusted citations. Articles with any disclosed conflicts appear to be of higher quality, when measured by authors' and their organizations' prior citations, having authors from top-50 medical schools, or articles' relative position in the journal's main section. These descriptive statistics already point to very strong positive selection, which can confound testing the prediction that conflict disclosures lead to a citation discount.

3.3. Cross-sectional Regression Analysis: Original Research Articles

Our initial research design focuses on original research articles, and entails a linear regression of 3-year citations ($CIT3_i$) to an article on a binary indicator for whether the article discloses conflicts of interest and various controls:

$$\ln(CIT3_i + 1) = \beta_1 DCOI_i + \gamma_j + \gamma_y + \gamma_m + \beta_2 X_i + \epsilon_i$$

We employ different measures for disclosure of conflicts ($DCOI_i$) at the article level, though our baseline model uses an indicator for whether any author disclosed any conflict. We include journal (γ_j), publication year (γ_y) and month (γ_m) fixed effects to address selection into journals, time trends in citations and conflicts, and imperfections in our measure of 3-year citations,

respectively. We cluster standard errors at the journal x year level (and by matching strata for the coarsened exact matched sample).

To mitigate positive selection in financial ties, we add a number of controls for article characteristics and quality. Author-related controls include the number of authors of the article, whether any of the authors is from a medical school that is ranked top-50 in the world, the number of citations garnered in the 3 years preceding the publication of the article by the authors of the article, as well as the corresponding prior 3 year citation count for the organizations from which they hail.[11] Article-related controls include the subject matter of the article, the type of study reported in the article (e.g., clinical trial or meta-analysis), the number of pages in the article, whether the article is in the main section of the journal, and the relative position of the article in the journal's main section.

In Table 3, Panel A, we report the results from the cross-sectional analysis using the disclosure of any industry conflict by any of the authors. The first column reports the basic association including only the fixed effects. The disclosure of an industry-related conflict by any author[12] is associated with a significantly higher number of citations (101%). Adding quality controls substantially attenuates this association, as predicted, but it remains statistically significant, showing that, even in the most stringent specification (Column 6), articles with disclosed conflicts have 33% more citations than articles without disclosure. Quality controls and article characteristics have the expected signs, though some, such as hailing from a top-50 medical school,

[11] To preserve as much of the sample as possible, we slightly modify this variable for the first two years in our data (1988 and 1989). We describe this procedure in the Data Appendix.

[12] Replacing the disclosure of a conflict by *any* author with the disclosure that *all* authors have at least one conflict does not materially change our results.

become insignificant as other controls are added.[13] Table 3, Column 6, includes an indicator for other conflict disclosures unrelated to industry (e.g., government funding). The coefficient on this indicator is small and insignificant, and controlling for such conflicts does not alter our conclusions for industry-related conflicts, which appear to be different in nature. Matching articles on observable covariates first and then performing the regression analysis, as we do in the last 3 rows of Panel A, does not materially affect our conclusions or inferences.

To explore whether the association with citations differs depending on the substance of the disclosure, we also estimate regressions that include indicators for specific types of conflicts. In Panel B of Table 3, specifications (1) through (6) match those in Panel A, but we report only the coefficients for the disclosure variables. We find that obtaining industry funding is associated with 21% higher citations, even after controlling for various characteristics, including the study type and medical subject matter. Being an employee is also persistently associated with 28% higher citations even with all controls. Other individual conflicts, such as being a consultant, having equity in a drug company, still show positive coefficients, but they become relatively small and statistically insignificant when all controls are added and/or we match articles on observables first. Interestingly, in non-tabulated results, we obtain consistently negative and often statistically significant coefficients when all authors disclose that they are drug company employees.[14] These findings for severely conflicted articles as well as the coefficient magnitudes for less obvious and more personal conflicts are consistent with the notion that these articles are discounted more in that the positive selection effect is attenuated further (or no longer dominates).

[13] As a general rule, the indicator for top 50 medical school affiliation tends to become statistically insignificant once the past 3-year of citation count for author institutions is added to a regression, suggesting that past citations garnered by authors belonging to a given institution are a superior proxy for institutional quality in our context.

[14] Similarly, when all authors report at least one individual-level conflict, the coefficient on disclosure is no longer positive and significant.

Overall, we find that quality controls reduce but do not eliminate the positive association between disclosures and citations. There are at least three possible explanations: (i) other researchers do not view disclosures as evidence of bias; (ii) their citations do not account for conflicts even when there is bias; or (iii) we have insufficient controls for article quality (or positive selection). Our subsequent analyses attempt to distinguish the last explanation from the first two by employing different strategies to tackle selection.

3.4. Cross-sectional Regression Analysis: Original Research versus Reviews

Up to this point, we excluded review articles from the analysis. Reviews are often viewed as more susceptible to bias as surveying the literature and describing the existing studies involves judgment and choices (e.g., Relman 1990; Hansen et al. 2019). For this reason, several medical journals, such as NEJM and Lancet, adopted a policy of not publishing reviews or editorials by authors with financial conflicts.[15] All else equal, it is therefore plausible that reviews with industry conflicts are discounted more than research articles. A potentially countervailing force is editorial policy. Editors could set a different bar for reviews and accept reviews only when authors have fewer or less severe industry conflicts. Moreover, reviews are often solicited by the editors and hence editors could ask for reviews from authors with fewer financial ties. These selection effects would likely work against the aforementioned prediction. Thus, not seeing a material difference between original research and reviews would be difficult to interpret. A more negative effect for reviews, however, would be consistent with stronger discounting.

In Table 4, we report results from regression analyses adding review articles to the sample and then introducing an indicator for reviews as well as an interaction between reviews and conflict

[15] Interestingly, both journals later abandoned their strict policies as it was difficult to find qualified authors (Relman 1990; Drazen and Curfman 2002; James and Horton 2003), which is again consistent with a positive selection effect for articles with conflicts.

disclosure. We again report the indicator for any industry conflict by any of the authors. Consistent with the notion that reviews with industry conflicts are discounted relative to research disclosing a conflict, we find a negative interaction effect. The interaction effect is also robustly significant when we first match articles with and without conflicts on the control variables.[16] We view this evidence as descriptive but consistent with the interpretation that other researchers cite reviews that disclose industry conflicts less.

4. Using Expert Recommendations to Mitigate Selection on Quality

In this section, we turn to a setting in which positive selection of financial ties is arguably less of an issue. We examine the association between expert recommendations and disclosed financial ties with the drug industry, essentially replacing 3-year citations as the dependent variable with a variable indicating whether or not an article has been recommended by a team of experts. The expert recommendations emerge from a program at the University of Chicago's Department of Family Medicine and are called Priority Updates from the Research Literature (PURLs). The PURL setting mitigates selection on article (and author) quality because PURL experts nominate based on quality, and nominations are highly selective.[17] The nomination process likely eliminates lower quality articles, leaving articles whose authors could obtain industry funding or establish financial ties with industry if they wanted to. The process also indirectly controls for aspects of article quality that are unobservable to us.

4.1. Data, Sample and Summary Statistics

The goal of the PURL Program is to select relevant, scientifically valid and implementable

[16] Specifically, we match on the control variables using coarsened exact matching (creating four bins for each control variable). The controls we matched on in each column correspond to the controls included in the analogous columns of Panel A in Table 3.

[17] Between July 2007 and August 2009, the PURL nominators saw 44,114 medical articles and nominated 283, i.e., less than 1% of them (Rowland and Sharma, 2011).

articles from the research literature that are relevant to primary care and family medicine and then to provide a synopsis to practicing physicians. The program has been publishing its synopses, or PURLs, since July 2007. The PURL Program has three phases. First, a published article is nominated for review by a group of family physicians who actively survey the primary clinical research literature. Occasionally, articles can be recommended before they are published, e.g., if they report the results of an important clinical trial that is of importance to primary care regardless of what the trial finds. But generally speaking, articles have already gone through peer review by the time they are nominated. Second, the PURL team of experts makes a decision whether to review the article or not. Third, if the program reviews the article, it conducts a critical appraisal and then decides whether to recommend the article as a PURL, to recommend the article as an "important reference," which is still an endorsement of the article, or to drop it. Nominated articles are evaluated and scored based on the following PURL criteria: the article is scientifically valid, relevant to family medicine, applicable in a medical care setting, immediately implementable, clinically meaningful, and leads to a change in current practice.[18]

Articles enter our database once they are nominated. There are on average 34 days between when an article is published and when it is nominated[19] and on average 24 days from an article's nomination to the Program's decision about whether to recommend the article. The Program recommends roughly one to three PURLs per week. While conflicts of interest are not an explicit criterion of the PURL process, the Program's reviewers observe nominated articles' conflict of interest disclosures. Thus, it is meaningful to ask whether the PURL team of experts is less likely

[18] See Rowland and Sharma (2011).

[19] Since nominations can occur before print publication, we allow the number of days until nomination to take on negative values. The 34-day average reported above is calculated *including* these negative values. Conditional on the nomination taking place after print publication (the case for 295 observations out of 410 articles for which we observe the date of nomination), it takes an average of 54 days until the article is nominated.

to choose articles with financial ties to the drug industry.

We use a comprehensive dataset of all nominations to the PURL Program from January 2007 to December 2012. We also obtain data on certain characteristics of nominated articles from the PURL Program's own data base, from a PubMed scrape and from Thomson Reuters on citations. Our sample includes 448 articles nominated for review by the PURL Program. Among these, 316 articles are dropped, 79 are recommended as PURLs, and 53 are recommended as important references. Table 5 provides summary statistics on the sample articles and their characteristics. Notably, we find that the fraction of PURL nominations with disclosed industry conflicts is higher (49%) than for articles in our baseline analysis (19%). This fact is consistent with our conjecture that the PURL sample is positively selected on quality.

4.2. Analysis

Our design for the regression analysis of the PURL sample is similar to the design of our citation analysis:

$$PURL_i = \beta_1 DCOI_i + \gamma_s + \gamma_j + \beta_2 X_i + \epsilon_i$$

Our sample includes only articles nominated for recommendation by PURL. Our dependent variable is binary, taking a value of 1 if an article is either recommended as a PURL or as an important reference, and 0 if dropped.[20] γ_s and γ_j are study type and journal fixed effects, respectively. Due to the smaller sample size relative to the citation analysis in the previous section, we coarsen these fixed effects. Study type fixed effects include an indicator for randomized

[20] We combine the PURL and important reference categories because only 12% of articles are labelled important references. Thus, we have little variation to exploit the differences between the two categories. The results are similar, albeit statistically weaker, if we eliminate important references, which is to be expected considering the small number of PURLs.

controlled studies, an indicator for meta analyses, and a third indicator for all other study types.[21] Journal-fixed effects are coarsened based on journal prestige and include dummy variables for four journal groups.[22] In an attempt to compensate for the coarsening of journal fixed effects, in some specifications we include the impact factor of the journal in which the article appeared as an additional control for journal quality. As in the citation analysis, other variables in X_i include controls for the authors being in a top 50 medical school, and the number of pages and authors. We also control for the time from publication to PURL nomination and the number of citations to the article up to the year of its nomination, as both variables could capture article quality. For ease of interpretation, we present results from a linear probability model, but we find very similar results using a logit model. We draw inferences based on heteroscedasticity robust standard errors.

Table 6, Panel A presents regression results using our disclosure variables indicating when *any* author reports any industry conflict. We also present results with separate indicators for drug-industry funding and individual level conflicts (such as being a drug company employee or consultant). In Panel B, we focus on more heavily conflicted articles and code an article as having a conflict when *all* authors disclose at least one drug industry conflict as well as separate indicators for articles in which all authors have drug-company funding and individual level conflicts. For the separate conflict indicators, we test for joint significance of the two coefficients as authors could have both funding and personal-level conflicts. At the bottom of each panel, we present results from specifications in which we control for observables through coarsened exact matching

[21] In this formulation, the three study type indicators are defined to be mutually exclusive.

[22] We group journals into four groups: top general interest journals (170 articles), other general interest journals (35 articles), top field journals (96 articles), and field journals (147 articles).

instead of multiple regression.[23] In each column, we match conflicted and non-conflicted articles on observables that are included as controls in the regression presented at the top of the panel.

Unlike the citation analysis, we obtain negative coefficients on the conflict disclosure variables in almost all specifications. Our primary finding is that disclosures by any author are associated with a 7 to 10% lower probability of being recommended by the PURL Program after nomination. The disclosure coefficient is statistically significant once we introduce the controls for article characteristics and quality (though most quality controls do not come in as statistically significant).[24] The results for the matched sample are even stronger and the disclosure effect is slightly larger (8 to 15.5%). We do not see a negative association for disclosures of non-industry-related conflicts (such as NIH funding) and controlling for such conflicts, if anything, strengthens our findings. Breaking out the disclosure coefficient by type of conflict does not alter the results and their interpretation. The two disclosure coefficients are both negative and jointly significant in all specifications with the full set of controls. Interestingly, the coefficient magnitudes appear to be slightly larger for individual-level conflicts, i.e., for drug company employees, consultants or authors with speaker fees (though we are cautious about this interpretation).

In Panel B, examining the effects for more heavily conflicted articles, the effects tend to be stronger in that disclosures by all authors are associated with a 12 to 16.4% lower probability of a

[23] In the coarsened exact matching procedure, we coarsen continuous control variables (page length, number of authors, journal impact factor, number of days from publication to nomination, citations from publication to nomination) into quartile groups. We do not further coarsen the three study type dummy variables or the four journal fixed effect groups. Conflicted articles are matched to non-conflicted articles that belong to the same study type and journal grouping, as well as the same quartile of each of the continuous control variables included in the specification.

[24] The severely reduced explanatory power of the control variables in the PURL context relative to the citations analysis bolsters the interpretation of the nomination process as a fine filter for article quality. Interestingly, the only control variable with explanatory power in the regressions is the number of days that elapse from publication until nomination. Our interpretation is that this variable is likely to implicitly reflect the confidence of the nominating expert. Quick nominations are likely to be "no-brainer" decisions, whereas a long time until nomination might signify increased indifference toward the article.

PURL program recommendation. Again, the coefficient magnitudes tend to be the largest for individual-level conflicts, though the low incidence rates of articles for which all authors have individual conflicts substantially lowers powers and so the coefficients are statistically insignificant despite their magnitudes. The two conflict disclosure variables, however, are jointly significant in all specifications.

Overall, the analysis of the PURL sample suggests that experts use the disclosed conflicts to update their beliefs about an article and supports the notion that articles disclosing financial ties with the drug industry are discounted relative to articles without such disclosure. Using expert recommendations among PURL-nominated articles mitigates concerns about selection on unobservables relative to the citation analysis in Section 3. But we hasten to add that it is still possible that certain PURL criteria are negatively correlated with the presence of industry ties, which in turn could drive the observed association in the Table 6 analysis.[25] Given this possibility, we conduct an additional citation-based analysis for which we will be able to hold the article and hence the author team fixed.

5. Within-Author Analysis of Citations and Conflicts of Interest

In this section, we employ an alternative strategy to address selection in financial ties: We estimate citation effects within author using previously published articles. Ideally, we would like to run the following experiment. We take a sample of articles without conflict disclosures and, at some point after publication, randomly assign and reveal for a subset of articles that the authors

[25] We note, however, that a priori it would seem more likely that several of the PURL criteria, such as relevance to medical practice and being clinically meaningful, should be positively correlated with the drug industry providing funding or other financial support to the authors.

have a conflict. In this experiment, the change in citations for articles disclosing a conflict relative to article without conflicts measures the disclosure effect.

Although we cannot run this experiment, we can come close to many features of this experiment. Specifically, we can exploit the fact that many top medical journals substantially changed their conflict disclosure policies around 2001/2002 (Davidoff et al. 2001; DeAngelis et al. 2001; Smith 2001). Thus, publications in these journals likely reveal author conflicts that were previously not disclosed in their prior work.[26] Moreover, the new disclosures are the result of the journal's policy, which should be exogenous to any given author. We can therefore use these new disclosures to study the citation effects on articles that were previously published and did not reveal a conflict. To illustrate, we use an author's revelation of a conflict in JAMA in 2002 and then analyze how this disclosure relates to citations of an article the same author published in Heart in 2000. If the disclosure of industry ties in the JAMA article leads other researchers to update their beliefs about the disclosing author and her work in the hypothesized way, we expect citations of the Heart article to decrease after the revelation in 2002. Such spillovers, if they exist, are likely stronger when the two articles by the same author are closer in time, as it is then more plausible that the conflict itself is not new but already existed at the time of the Heart article. At the same time, close proximity of the two articles implies a short time to measure pre-disclosure citations of the Heart article, which makes it difficult to estimate changes in citations. Moreover, articles typically have a non-linear citation path over time. To address these challenges, we match treatment and control authors with two articles each, one published at time *t* without disclosures and another published at a later date, say *t+2*. The treated author discloses a conflict in the later

[26] See Malani et al. (2015) for evidence that conflict disclosures increase significantly around these changes in journals' disclosure policies.

article but the control author does not. We then examine the difference in differences in citations between the time *t* articles.

5.1. Data and Sample Selection

We begin by selecting candidate authors of articles from our earlier baseline sample (Table 1). Authors qualified as potential treatment authors if they published an article in NEJM, JAMA, BMJ or Lancet between 2002 and 2003 (post period) in which they disclosed a conflict. Authors qualified as potential control authors if they did the same but did not disclose a conflict in the post period. Among these candidates, we randomly selected 531 of the authors as potential treatment authors and 360 as potential control authors.[27] We then obtain Thomson Reuters data on all medical journal articles published by the selected authors between 1999 and July 2001.[28] As researchers could have (voluntarily) disclosed conflicts in their other work, the above selection procedure does not ensure that the post-period disclosure by the candidate treatment authors is indeed new and that the candidate control authors do not disclose conflicts in prior work.

In order to make sure that the assignment to treatment and control is accurate and the disclosures are indeed new, we check authors' disclosures in prior years. Specifically, for each author, we check their disclosures in their best-placed article each year, i.e., the article that placed in the journal with the highest impact factor amongst all the journals in which the author published that year. Our theory is that if authors disclose a conflict, they are most likely to do so in their

[27] Specifically, due to the time-intensive process of screening and cleaning the data, we collected the data in two rounds. In a first round, we selected 450 treatment and 125 control authors. In another round, we selected another 81 (235) treatment (control) authors to increase the overall sample size and to achieve a greater balance between treatment and control authors. For the purpose of the analyses, we aggregate the data from both rounds. We oversample treatment authors as it is more likely that we lose treatment authors during the subsequent cleaning process.

[28] We omitted August–December 2001, which is the time-period during which the ICMJE announced major changes in disclosure policy, which then went into effect at different times in 2001 for different journals. We omit a period that includes all policy start dates for the four treatment journals plus a small time buffer for implementation.

best-placed article because higher impact factor journals tend to have more rigorous disclosure requirements (Blum et al. 2009; Malani et al. 2015). If the best placed article in the pre period for an author provides a conflict disclosure, we drop the author from the sample. After these checks, we are left with about 184 treatment authors and 185 control authors. For each article written by this final set of authors between 1999 and July 2001 (pre period), we scrape PubMed for article characteristics on which we could match articles and obtain annual citation data from Thomson Reuters. After this exercise, we have 2,910 articles across the treatment and control author groups.

5.2. Difference-in-Differences Analysis

We conduct a difference in differences analysis in log citation counts for a pre-period article by a treatment author matched to a pre-period article by a control author. The first difference is citations before and after the treatment author discloses in a separate, post period article that she has a conflict. The second difference is across the treatment and control authors. This setup is comparable to Azoulay et al. (2017), which analyzes the drop in citations of prior work of authors that need to retract a more recent article. Specifically, we estimate the following regression using a coarsened exact matched sample:

$$\ln(CIT_{it} + 1) = \beta_1 Treated_r \; x \; Post_{r,t} \; + \gamma_t + \delta_i + \epsilon_{it}$$

where i indexes an article, t indexes the year in event-time and r indexes an author. *CIT* is the yearly citation count for a given article adjusted for self-citations.[29] *Treated* is a binary variable that indicates whether the author published an article with a drug COI in the post period. However, it is critical for our analyses that articles of treatment and control authors are well matched and

[29] For the cross-sectional regressions in Section 3, we weight citations by impact factor. However, in this analysis, we are interested in the changes of citations for a given article over time. Thus, we do not weight citations by journal impact factors. The latter change over time and hence citations patterns might be changing over time for reasons unrelated to COI disclosure.

satisfy the parallel-trends assumption. Therefore, in all specifications, we perform an exact matching on study type (e.g., clinical trial, comparative study, etc.), publication year and main author (i.e., whether the focal author is the first or last author). In some specifications, we enhance our matching by using coarsened exact matching for the impact factor of the journal, in which the pre-period article is published (binned into quartiles) as well as publication month (binned into quartiles). The latter is a further attempt to line up the citation trajectories of treatment and control articles in time.

Depending on the exact specification, we define the binary indicator *Post* in two different ways. In the first case, we set *Post* to the value of '1' beginning in 2003 as treatment authors disclose their first conflict in the year 2003 at the latest. However, this definition does not take into account an author's first actual disclosure and, thus, might be overly conservative for treatment authors with an earlier conflict. Therefore, in our preferred specification, we set *Post* to '1' starting in the year in which an author actually discloses their first conflict during the post period (i.e., starting in 2002 or 2003). We identify this specific year by checking NEJM, JAMA, BMJ, Lancet or the author's best-placed article in a given year for a disclosed conflict during the post period. Relatedly, for control authors, we use the first post-period publication in one of those journals as the respective author-specific "non-disclosure" date. However, with this *Post* definition (and to satisfy the parallel-trends assumption for the citation trajectories), we have to ensure that treatment and control articles are aligned in time so that they are "shocked" at roughly the same time in their life cycle by a later conflict disclosure (or lack thereof).[30] Therefore, we add the

[30] As we match treatment and control articles based on their publication year, we always ensure that treatment and control articles are at the same stage in their citation path for the other definition of *Post* (>= 2003).

number of months until the publication of the post-period "shock" article (binned into quartiles) as another variable to the coarsened exact matching procedure.

Finally, we include fixed effects γ_t for each year (measured in event time relative to *Post*). We also include article fixed effects δ_i and omit all article quality variables as they are time invariant and collinear with δ_i. We cluster standard errors by the corresponding matching strata.

Panel A of Table 7 presents regression estimates for each of the three matching algorithms using our two different *Post* definitions. For example, in Column 1, we find that citations to pre-period articles of treated authors decline significantly by about 4.6% after 2003 relative to the citations of matched control articles. In Figure 2, we expand this analysis by using a similar specification, but plotting out the estimated citation differences over time. This figure shows the coefficients of the interacted year dummies and *Treat* before and after the shock year 2003. In the pre-period, we do not find any significant differences and, if anything, an upward trend in the citation difference between treatment and control articles. After the shock year 2003, the coefficients on the interacted post-disclosure year dummies become negative. These effects are increasing over time, indicating that the citation effects are persistent, even many years after publication. In Column 4-6, we then use our preferred *Post* definition. Although we find largely consistent results, our coefficient estimates are slightly larger with negative effects on citations ranging from 6 to 7%.

In Panel B of Table 7, we again exploit that review articles are likely more susceptible to bias and hence should be more responsive. We conduct a sub-sample analysis and estimate the effects separately for review articles. We expect a larger citation penalty for reviews after an author subsequently discloses a conflict in another study. In line with this expectation, we find substantially stronger effects for review articles compared to research articles. For example,

Columns 5 and 6 suggest that the negative effect for reviews is about three times larger (15% versus 5%). This finding is consistent with our earlier results for reviews in Section 3.4.

Overall, the results are consistent with negative spillovers from the disclosure of an industry conflict in one article on citations to another, earlier article by the same author. These findings support the notion that citing researchers discount articles by authors having industry ties. Our documented spillovers are also in line with the results of Azoulay et al. (2017) for retractions. In closing, we highlight that this analysis provides a conservative test of the prediction that disclosed conflicts reduce citations because it requires readers to associate disclosures in one article to other articles by the same author – a larger cognitive effort for readers.

6. Conclusion

Financial ties between drug companies and medical researchers are generally thought to bias results in articles published in medical journals. In order to enable readers to account for such bias, most medical journals now require authors to disclose potential conflicts of interest. For disclosures to be effective, they must modify readers' beliefs. We examine whether the disclosure of financial ties with drug companies reduces citations to an article, suggesting that readers discount conflicted articles. A challenge to estimating this effect is selection: e.g., drug companies may seek out higher quality authors and fund their studies or hire them as consultants, generating a positive correlation between disclosed conflicts and citations. Our analysis confirms this positive association. Including observable controls for quality attenuates but does not eliminate the relation. To tease out whether readers discount articles with conflicts, we perform three tests. First, we show that the positive association is weaker for review articles, which are more susceptible to bias. Second, we use recommendations by medical experts to family practice doctors that, for institutional reasons, are less susceptible to selection. This approach yields a significantly negative

association between disclosure and expert recommendations, consistent with discounting. Third, we conduct an analysis within author and article, exploiting journal policy changes resulting in conflict disclosure in a new article and examine the effect of this disclosure on citations to previously published articles. This analysis reveals a negative citation effect from articles disclosing a conflict, in particular for previously published review articles. Overall, we find evidence that disclosures negatively affect readers' citation behavior, consistent with the notion that other researchers discount articles with disclosed conflicts.

An important caveat for our study is that citations measure beliefs among a subset of readers: those who write articles. On the one hand, readers who also produce scholarship may be more informed about the information content of disclosures. They may be more sensitive to the fact that companies seek out quality researchers when attempting to create ties or that such ties generate subtle biases in researchers. On the other, non-researchers may respond differently to conflict disclosures than researchers. For example, they may know less about authors' reputations and place more weight on disclosures. Alternatively, they may be relatively unresponsive to disclosures because they do not understand what ties mean in terms of selection or bias. Thus, we need more research on the important question of how readers respond to disclosed conflicts of interest.

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Data Appendix

A. Calculating Past Citations

We need to calculate citation counts from the data we obtained from Thomson Reuters, i.e., citations to articles in our seven selected journals for the period 1986-2010. Although in our analyses we only focus on articles from 1988-2008, we still need to shorten the window over which we average past citations for our control variables in 1988 and 1989. Specifically, to calculate past citations for articles published in 1988, we only take into account citations between 1986 and 1987 to articles published by the same author/organization in 1986 (in our parlance, these are 1-year citations to articles published in 1986). For past citations to articles published in 1989, we average the 1-year citations to articles their authors/institutions published in 1986 and 1987. For each year 1990 onward, we average 3 years of past 1-year citations (e.g. for 1990, it would be 1-year citations to articles published in 1986, 1987, and 1988). Note that throughout, the past citation control variable is generated such that there is no overlap between the time period over which the dependent variable citations are counted and over which past citations are tallied.

B. Defining Article Type

We assembled our sample of articles in two data collection waves. In the first wave, we used Thomson Reuter's categorization of articles as research or review to compose an initial list of sample articles published from 1986 to 2003. However, our hand-checking of this categorization revealed it was over-inclusive (e.g., humorous articles in BMJ's Christmas issue were categorized as research). Therefore, we refined Thompson's categorization of article type using data journal tables of contents and information from PubMed and journal tables of contents as follows. First, we dropped the BMJ Christmas issue from the sample. Second, we matched articles to journals' tables of contents and the categorization provided therein. We defined as research articles the

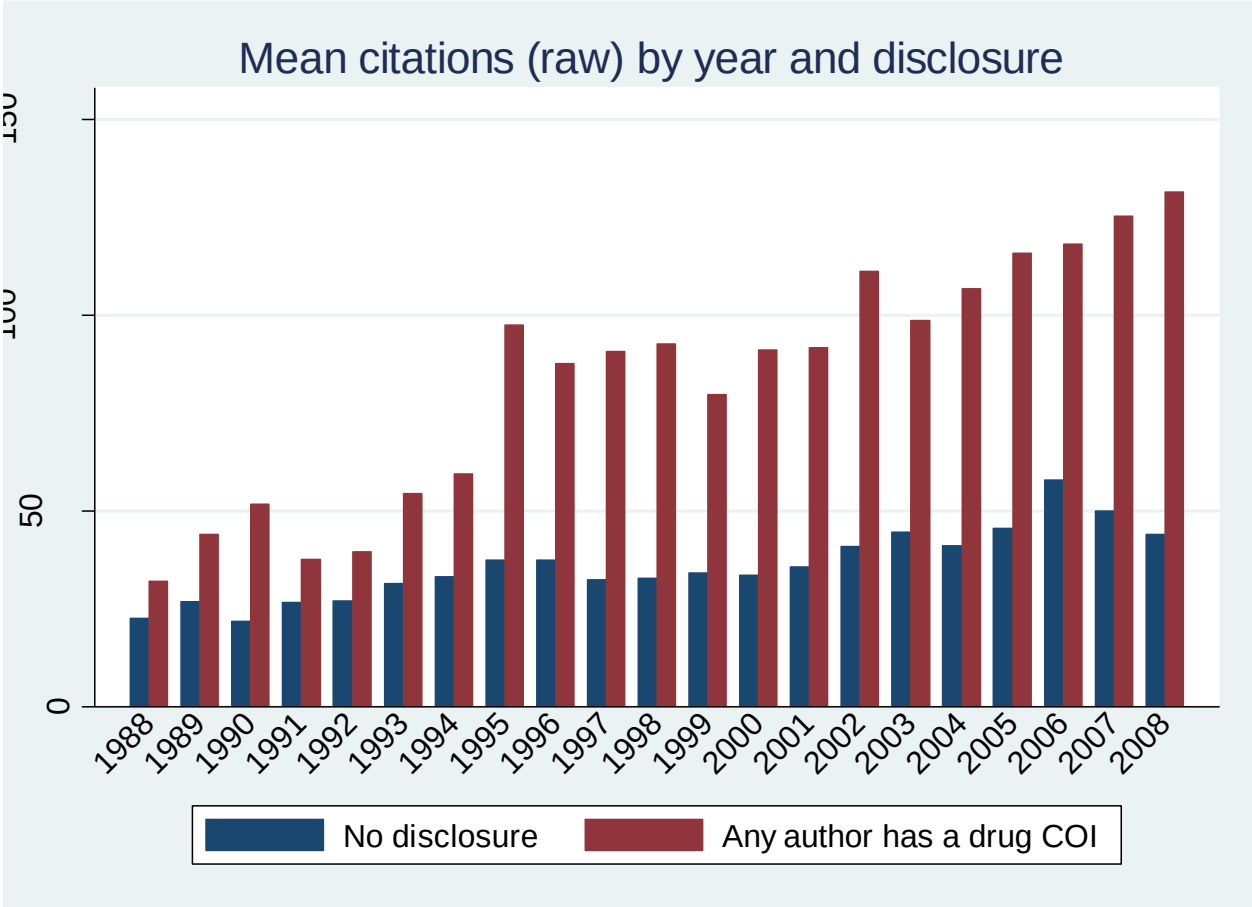
10,905 articles which appeared in AJM's Clinical Studies; BMJ's Papers, Research Articles, Clinical Research, and Papers and Short Reports; JAMA's Original Contributions, Brief Reports, and Preliminary Communications; Lancet's Articles, Early Reports, and Research Letters; Mayo's Articles and Original Articles; and NEJM's Original Articles sections. We defined as reviews the 1,172 articles which appeared in AR or in AJM's Reviews, JAMA's Reviews, Lancet's Review Articles, Mayo's Reviews and Subject Reviews, and NEJM's Review Articles sections. Third, we reclassified 2,589 more articles as reviews because PubMed classified them as "review". Fourth, we re-categorized the 274 articles our methodology labeled as both research and review as solely reviews. Finally, we dropped 39 articles our methodology labels as research but Thompson's labels as review.

In the second data collection wave, we collected information on research and review articles published from 2004 to 2010. In this wave, we constructed the sample based on the categorization algorithm designed in the first wave. In this way, we collected conflict disclosure data on 1,553 research articles. These are articles not categorized as review by PubMed or Thompson, published in: AJM's Clinical Studies; BMJ's Papers, and Research; JAMA's Original Contributions, Brief Reports, and Preliminary Communications; Lancet's Articles, and Research Letters; Mayo's Original Articles; and NEJM's Original Articles sections. We also collected disclosure information on 849 review articles. Of these articles, 195 were published in AJM's Reviews, JAMA's Reviews, Mayo's Reviews, or NEJM's Review Articles sections. Among the rest of the 654 articles categorized as review, 652 are classified by PubMed as review. The remaining 2 articles are in the sample on account of being classified as review by Thompson.

C. Defining Study Type

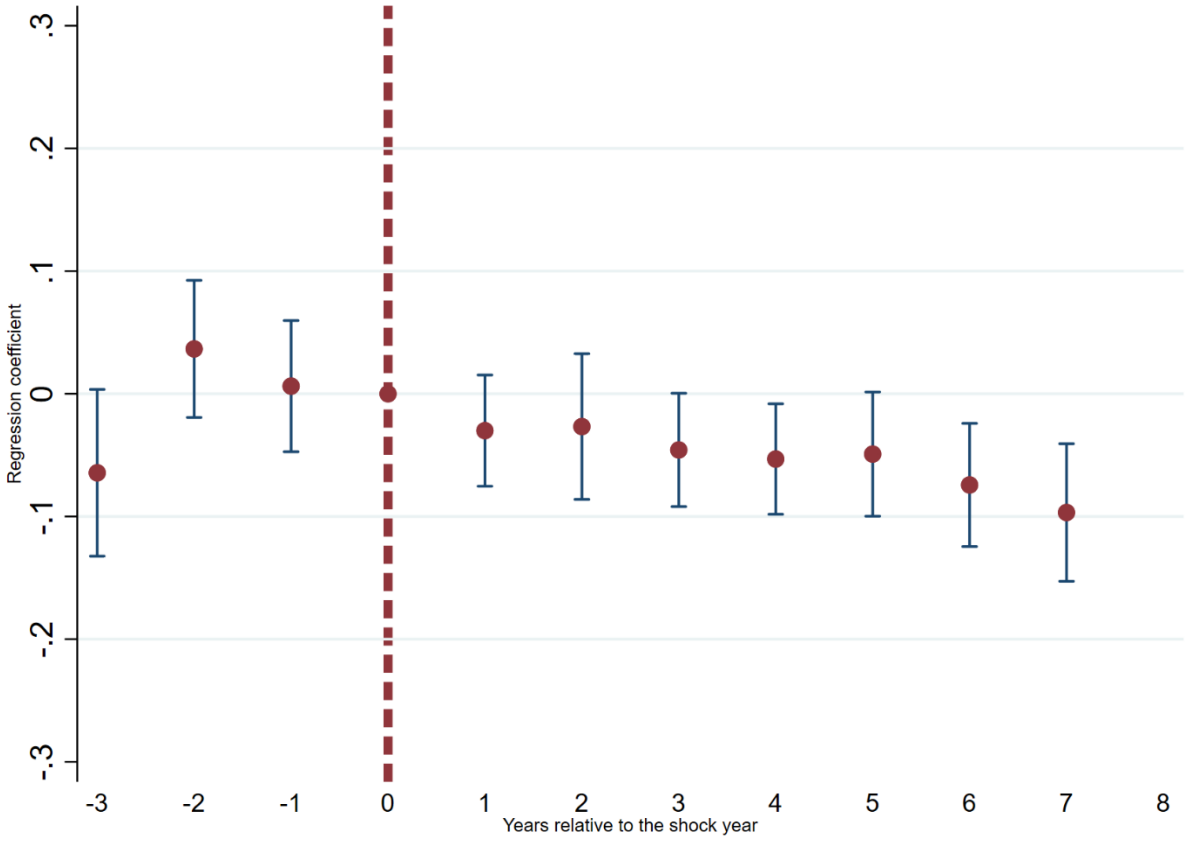
When PubMed assigned an article to multiple study types, we eliminated overlap by prioritizing certain classifications. A clinical trial was not permitted to be any other type. A comparative study was not permitted to be a meta-analysis or a case study. There was no overlap between the meta-analysis and case-study categories.

Figure 1: Average 3-year citations to articles with and without conflict disclosures



The figure plots the average 3-year citations to articles over our sample years depending on whether any of the article's authors disclose a drug-industry related conflict of interest. We calculate 3-year citations by summing the number of citations across the three calendar years following the article's publication, plus any citations received in the year the article was published.

Figure 2: Citation differences between articles with subsequent disclosure and matched control articles



The figure plots estimated citations differences between treatment articles, for which an author subsequently discloses a conflict of interest in another article, and matched control articles without subsequent disclosure. The figure shows the coefficient estimates for interactions between year dummies and an indicator variable for treated authors. All coefficient estimates are relative to the omitted shock year 2003 (t=0) and the matching procedure follows Column 1 of Table 7.

Table 1: Summary statistics by journal

Journal article variables:	**AMJ**	**AR**	**BMJ**	**JAMA**	**Lancet**	**Mayo**	**NEJM**
Number of articles	1614	657	4062	2373	4256	1173	3002
Citations (adjusted for impact, self cit.)	42	80	54	242	172	30	432
# pages	7	14	4	7	5	8	8
# authors	4	2	4	7	6	4	7
# 3-year pre-citations	50	53	46	104	53	31	68
# organizations' 3-year pre-citations	3603	5216	1414	5013	1948	3960	4174
Conflict of Interest variables:	**AMJ**	**AR**	**BMJ**	**JAMA**	**Lancet**	**Mayo**	**NEJM**
Any author has a drug COI	0.14	0.05	0.13	0.29	0.21	0.11	0.26
Funding	0.10	0.02	0.08	0.20	0.14	0.08	0.20
Gave drug or materials	0.01	0.00	0.02	0.05	0.04	0.01	0.03
Employee	0.05	0.03	0.02	0.09	0.05	0.05	0.10
Consultant	0.01	0.01	0.03	0.12	0.04	0.03	0.11
Honoraria/speaker	0.01	0.00	0.04	0.11	0.03	0.02	0.08
Award	0.01	0.00	0.00	0.01	0.01	0.00	0.01
Equity	0.00	0.00	0.00	0.05	0.01	0.01	0.04
Other	0.00	0.00	0.00	0.01	0.01	0.00	0.00

This panel present descriptive data on 17,137 articles from seven general interest medical journals published during 1988-2008. The sample comprises original research and reviews. Other articles are not included in the sample. *Citations (adjusted for impact, self cit.)* are citations to an article, weighted by the impact factor of the citing journal, and adjusted for self-citations by ignoring citations by articles that share at least one author with the cited article. *# pages* is the page length of the article; note that for 74 articles, page length is missing. *# authors* is the number of authors listed in the article's byline. *# 3-year pre-citations* is the mean number of impact-weighted, self-citation adjusted, citations garnered by the authors of the article in the three years preceding the publication of the article. *# organizations' 3-year pre-citations* is the mean number of impact-weighted, self-citations adjusted, citations garnered by the organizations with which the article's authors are affiliated over the preceding three years. Due to resource constraints, for both *# 3-year citations* and *# organizations' 3-year citations*, we only count citations by articles that appeared in one of the seven journals in our sample. The Conflict of Interest variables panel presents the proportion of articles published in a given journal that have at least one author with the specified conflict. *Any author has a drug COI* is a summary indicator for all drug company related conflicts. *Funding* denotes whether the article or any of its authors received funding or grants from drug companies, including (for authors this includes disclosed funding for previous projects). *Gave drug or materials* indicates if the study, or any of its authors, received pharmaceuticals, biomaterials, or any other in-kind support. *Employee* indicates if any of the article's authors designate a drug company affiliation in the byline. *Consultant* indicates if any of the article's authors disclosed having consulted for a drug company. *Honoraria/speaker* indicates if any of the article's authors disclosed having received payment or honoraria for speaking engagements or other (potentially unspecified) reasons such as conference travel. (Since authors might not always specify speaking engagement as a source of honoraria, some unspecified honoraria might in fact be more accurately characterized as *Speaker*, but we are unable to tell.) *Award* indicates if any of the article's authors disclosed having received any scholarships, endowed chairs, or awards sponsored by drug companies. *Equity* indicates if any of the article's authors disclosed an ownership stake in a drug company, including stock options. *Other* indicates if any of the article's authors disclosed a drug company related conflict not covered by the above categories.

Table 2: Summary statistics by article

Panel A: Binary variables and citation counts

		Binary variable mean			Mean of adjusted citations	
		All	Articles with:		Articles with:	
Binary variables	**Obs.**	**articles**	**Any conflict**	**No conflict**	**Any conflict**	**No conflict**
Conflicts						
Any author has a drug COI	17,137	0.19	1.00	-	365	129
Drug funding/materials	17,137	0.15	0.79	-	392	135
Individual level drug COI	17,137	0.11	0.60	-	435	140
Any author has a non-drug COI	17,137	0.77	0.83	0.75	190	119
Non-drug funding	17,137	0.46	0.54	0.44	222	133
Individual level non-drug COI	17,137	0.57	0.64	0.56	186	157
Top 50 (world) institution	17,137	0.26	0.30	0.25	217	159
Review	17,137	0.34	0.19	0.38	128	197
Main section article	17,137	0.55	0.72	0.51	220	116
Study type (from PubMed)						
Clinical trial	17,137	0.17	0.42	0.11	299	148
Comparative study	17,137	0.10	0.17	0.09	258	164
Multicenter study	17,137	0.10	0.30	0.06	429	144
Letter	17,137	0.07	0.03	0.08	55	182
Case study	17,137	0.05	0.02	0.05	79	178
Meta-analysis	17,137	0.02	0.03	0.02	309	171

This panel presents summary statistics for binary variables (Column 3-5) and citation counts (Column 6-7) on the article-level. In particular, Column 3 presents unconditional means for the binary variables used in our analysis across all articles, and Column 4-5 shows means depending on whether authors of an article disclose any drug company related conflict of interest. In Column 6 and 7, the mean of article citations (weighted by citing journal impact and adjusted for self-citations), conditional on the binary conflict of interest disclosure and article characteristic variables listed on the first column. *Any author has a drug COI* is an indicator for whether any of the article's authors disclosed any type of drug company related conflict of interest. *Drug funding/materials* is an indicator for whether the article or any of its authors received any source of funding, materials, or in-kind support from drug companies. *Individual level drug COI* is an indicator for whether any of the article's authors disclosed any non-study specific conflict (such as *Employee, Consultant, Award, Speaker, Honoraria,* or *Equity*). *Any author has a non-drug COI* indicates whether any of the article's employees disclosed a non-drug company related potential conflict of interest (we interpret this category broadly to include physicians who specify a hospital affiliation in the article's byline). *Non-drug funding COI* is an indicator for whether the authors disclosed having received funding or material support from non-drug company sources (government, nonprofit, or university). *Individual level non-drug COI* indicates whether any authors disclosed a non-drug company related conflict specific to their person *(Employee, Consultant,* etc.). *Top 50 (world) institution* indicates whether any of the authors were affiliated with a top-50 medical school, as ranked by The QS World University Rankings in 2011. *Review article* is an indicator for "review" articles as opposed to "original research." This categorization relies on several inputs; see manuscript and appendix for details. *Main section article* is an indicator for whether the article appeared in the given journal's most prominent section for publishing research findings (for example NEJM's "Original Articles" or JAMA's "Original Investigation"; see appendix for details). *Study type* is the article's "publication type", scraped from the PubMed online database. While publication types assigned by PubMed are non-exclusive, PubMed does not necessarily assign publication type to all articles. (Note that our definition of *Review article* is more expansive than PubMed's *Review* designation using also information from Thomson Reuters.) For articles, for which we were unable to match to PubMed records, we assign "0" to all publication types.

Table 2: Summary statistics by article

Panel B: Continuous variables

							Mean, given:	
	Obs.	**Mean**	**SD**	**p25**	**p50**	**p75**	**Any conflict**	**No conflict**
Citations (raw)	17,137	46	85	8	21	51	93	35
Citations (adjusted for self cit.)	17,137	42	80	7	18	46	85	32
Citations (adjusted for impact, self cit.)	17,137	174	359	19	64	187	365	129
# pages	17,063	6	3	4	6	8	7	6
# authors	17,137	5	5	2	4	7	8	5
# 3-year pre-citations	17,137	59	169	0	11	50	88	52
# organization's 3-year pre-citations	17,137	3054	4574	393	1416	3996	3280	3001
Relative position	17,137	27	36	0	0	50	41	24

This panel presents summary statistics for continuous variables used in our quantitative analysis. For each variable, we present the number of non-missing observations, the unconditional mean, standard deviation, 25th, 50th, and 75th percentile. The rightmost two columns present the conditional mean of each variable, depending on whether any of the article's authors disclose any drug company related conflict of interest. *Citations (raw)* is the combined number of citations an article in our sample receives during the year it is published and the three calendar years after publication. In *Citations (adjusted for self cit.)*, we subtract from the raw count citations received from articles that share at least one author with the cited article. In *Citations (adjusted for impact, self cit.)* we also weigh citing articles by the impact factor of the journal in which they are published. Relative position is a measure of the relative position in which the article appeared in the journal's main section. If the article appeared outside of the main section, it is equal to 0; within the main section, the middle article is given a value of 50, the lead article a value of 100, and others linearly in between. For description of the other variables, see the caption for Table 1.

Table 3: Regression analysis of citations and conflict disclosures controlling for article characteristics

Panel A: Conflict of interest by any author

	(1)	(2)	(3)	(4)	(5)	(6)
Any author has a drug COI	0.700***	0.516***	0.489***	0.361***	0.281***	0.283***
	[0.050]	[0.030]	[0.027]	[0.026]	[0.027]	[0.027]
Top 50 (world) institution		0.075***	0.082***	0.077***	0.031	0.031
		[0.027]	[0.027]	[0.025]	[0.025]	[0.025]
Log(# 3-year pre-citations + 1)		0.109***	0.096***	0.085***	0.059***	0.058***
		[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
Relative position		0.844***	0.820***	0.757***	0.551***	0.552***
		[0.048]	[0.047]	[0.046]	[0.038]	[0.038]
Main section		0.433***	0.428***	0.047	-0.095*	-0.095*
		[0.120]	[0.120]	[0.102]	[0.051]	[0.051]
Log(# organizations' 3-year citations + 1)					0.018***	0.018***
					[0.006]	[0.006]
Log(# pages)					0.740***	0.737***
					[0.043]	[0.043]
Log(# authors)					0.255***	0.251***
					[0.026]	[0.027]
Any non-drug COI						0.050*
						[0.030]
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	11,293	11,293	11,293	11,293	11,245	11,245
R-squared	0.413	0.494	0.510	0.533	0.567	0.567

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Any author has a drug COI		0.528***	0.495***	0.399***	0.306***	0.297***
		[0.025]	[0.028]	[0.030]	[0.034]	[0.034]
Observations		11,293	11,293	10,987	9,123	8,397
R-squared		0.416	0.447	0.459	0.463	0.468

Each observation represents a journal article. The estimation sample includes 11,293 articles classified as original research published in six general interest medical journals between 1988 and 2008. The dependent variable is the log transformation of 3-year citations, weighted by the impact factor of the citing journal, and adjusted for author self-citations. We add a 1 to all citation counts. For variable definitions, see caption for Table 1. Study type fixed effects include dummy variables for the six PubMed study types listed in Panel A of Table 2. Medical subject fixed effects include dummy variables for the two-digit subcategories within the "C-Diseases" top category in the National Library of Medicine's Medical Subject Headings (MeSH). There are over twenty two-digit subcategories, including, among others, Neoplasms (C04) and Cardiovascular Diseases (C14). Standard errors for the regular OLS specifications, reported in the top panel, are clustered at the journal-year level, and reported in brackets. Standard errors for the matching specifications, reported in the bottom panel, are clustered by matching strata, and reported in brackets. Coarsened-exact matching is performed on the covariates in the corresponding regressions (continuous variables are coarsened into quartiles) except medical subject, journal, year and month fixed effects, which are kept as fixed effects in the regressions on matched articles. */**/*** indicate significance at the 10/5/1% level.

Table 3: Regression analysis of citations and conflict disclosures controlling for article characteristics

Panel B: Individual conflicts of interest by any author

	(1)	(2)	(3)	(4)	(5)	(6)
Funding	0.522***	0.342***	0.329***	0.238***	0.186***	0.190***
	[0.056]	[0.034]	[0.033]	[0.032]	[0.031]	[0.031]
Gave drug or materials	0.355***	0.256***	0.225***	0.114**	0.049	0.043
	[0.071]	[0.055]	[0.052]	[0.050]	[0.049]	[0.048]
Employee	0.366***	0.353***	0.338***	0.257***	0.231***	0.249***
	[0.049]	[0.044]	[0.043]	[0.041]	[0.039]	[0.040]
Consultant	0.164**	0.115	0.119*	0.133**	0.133**	0.139**
	[0.076]	[0.071]	[0.067]	[0.062]	[0.064]	[0.064]
Award	0.369***	0.231**	0.216**	0.214**	0.195*	0.194*
	[0.105]	[0.100]	[0.092]	[0.099]	[0.099]	[0.099]
Honoraria/speaker	0.094	0.068	0.031	0.022	-0.007	-0.000
	[0.058]	[0.049]	[0.050]	[0.045]	[0.047]	[0.047]
Equity	0.002	0.010	0.035	0.064	0.014	0.014
	[0.074]	[0.071]	[0.067]	[0.064]	[0.062]	[0.062]
Other	0.213	0.017	0.035	0.052	0.039	0.034
	[0.165]	[0.156]	[0.153]	[0.150]	[0.142]	[0.143]
Control Variables (as in Panel A)	N	Y	Y	Y	Y	Y
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	11,293	11,293	11,293	11,293	11,245	11,245
R-squared	0.415	0.495	0.510	0.533	0.568	0.568

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Funding		0.388***	0.364***	0.288***	0.227***	0.210***
		[0.032]	[0.030]	[0.036]	[0.039]	[0.042]
Gave drug or materials		0.245***	0.211***	0.149**	0.130**	0.160**
		[0.067]	[0.054]	[0.059]	[0.059]	[0.064]
Employee		0.301***	0.298***	0.251***	0.218***	0.212***
		[0.057]	[0.057]	[0.047]	[0.054]	[0.064]
Consultant		0.173***	0.175***	0.163***	0.115*	0.091
		[0.052]	[0.048]	[0.054]	[0.061]	[0.070]
Award		0.261*	0.258*	0.193*	0.079	0.150
		[0.144]	[0.149]	[0.115]	[0.120]	[0.138]
Honoraria/speaker		0.092	0.034	0.053	0.045	0.086
		[0.062]	[0.065]	[0.058]	[0.071]	[0.072]
Equity		0.032	0.063	0.112	0.121	0.075
		[0.084]	[0.081]	[0.083]	[0.077]	[0.098]
Other		0.184	0.197	0.201	0.190	0.103
		[0.205]	[0.208]	[0.202]	[0.173]	[0.209]
Observations		11,293	11,293	10,987	9,123	6,747
R-squared		0.420	0.449	0.462	0.466	0.469

Each observation represents a journal article. The estimation sample includes articles classified as original research published in six general interest medical journals between 1988 and 2008. The dependent variable is the log transformation of 3-year citations (plus 1), weighted by the impact factor of the citing journal, and adjusted for author self-citations. Control variables include the page length and number of authors of the article, whether any authors are affiliated with a top 50 medical school, whether the article appeared in the journal's main section, the article's position within the main section, as well as the average citations of the article's authors (and the institutions they are affiliated with) from the past 3-years (introduced in the same order as in Panel A of Table 3). For variable definitions, see caption for Table 1. Study type fixed effects include dummy variables for the six PubMed study types listed in Table 2. Medical subject fixed effects include dummy variables for the two-digit subcategories within the "C-Diseases" top category in the National Library of Medicine's Medical Subject Headings (MeSH). There are over twenty two-digit subcategories, including, among others, Neoplasms (C04) and Cardiovascular Diseases (C14). Standard errors for the regular OLS specifications, reported in the top panel, are clustered at the journal-year level, and reported in brackets. Standard errors for the matching specifications, reported in the top panel, are clustered by matching strata, and reported in brackets. See description in Panel A for details on matching. */**/*** indicate significance at the 10/5/1% level.

Table 4: Regression analysis of citations and conflict disclosures for original research vs. reviews

	(1)	(2)	(3)	(4)	(5)	(6)
Any author has a drug COI	0.702***	0.511***	0.484***	0.357***	0.358***	0.361***
	[0.053]	[0.029]	[0.026]	[0.025]	[0.025]	[0.025]
Review	-0.084	0.612***	0.670***	0.218*	0.002	0.084
	[0.063]	[0.141]	[0.145]	[0.125]	[0.138]	[0.136]
Review x any author has a drug COI	-0.278***	-0.165***	-0.142**	-0.045	-0.039	-0.053
	[0.088]	[0.062]	[0.061]	[0.060]	[0.061]	[0.059]
Wald test: Any author has a drug COI + Review interaction = 0						
Test statistic	37.93	39.30	38.19	31.85	32.59	31.78
p value	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Control Variables (as in Panel A of Table 3)	N	Y	Y	Y	Y	Y
Control Variables x Review	N	Y	Y	Y	Y	Y
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject X Review	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	17,135	17,135	17,135	17,135	17,135	17,135
R-squared	0.408	0.476	0.490	0.510	0.513	0.516

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Any author has a drug COI	0.719**	0.554***	0.521***	0.424***	0.418***	0.399***
	[0.026]	[0.038]	[0.038]	[0.039]	[0.035]	[0.033]
Review x any author has a drug COI	-0.364**	-0.353**	-0.304*	-0.274**	-0.287***	-0.286***
	[0.017]	[0.171]	[0.152]	[0.115]	[0.086]	[0.079]
Wald test: Any author has a drug COI + Review interaction = 0						
Test statistic	69.52	2.007	3.093	2.968	3.222	2.878
p value	0.076	0.164	0.086	0.086	0.073	0.090
Observations	17,135	17,108	17,108	16,673	16,086	15,581
R-squared	0.400	0.404	0.430	0.446	0.449	0.449

Each observation represents a journal article. The estimation sample includes articles classified as original research or review published in seven general interest medical journals between 1988 and 2008. The dependent variable is the log transformation of 3-year citations (plus 1), weighted by the impact factor of the citing journal, and adjusted for author self-citations. Control variables include the page length and number of authors of the article, whether any authors are affiliated with a top 50 medical school, whether the article appeared in the journal's main section, the article's position within the main section, as well as the average citations of the article's authors (and the institutions they are affiliated with) from the past 3-years (introduced in the same order as in Panel A of Table 3). In addition, the conflict of interest variable is interacted with a review dummy, as are the following controls: authors' affiliation with a top 50 medical school, authors' and their institutions' citations from the past 3 years, article's main section status and its relative position in the main section, and medical subject fixed effects (specifications 3-6). For variable definitions, see caption for Table 1. Study type fixed effects include dummy variables for the six PubMed study types listed in Table 2. Medical subject fixed effects include dummy variables for the two-digit subcategories within the "C-Diseases" top category in the National Library of Medicine's Medical Subject Headings (MeSH). There are over twenty two-digit subcategories, including, among others, Neoplasms (C04) and Cardiovascular Diseases (C14). Standard errors for the regular OLS specifications, reported in the top panel, are clustered at the journal-year level, and reported in brackets. Standard errors for the matching specifications, reported in the bottom panel, are clustered by matching strata, and reported in brackets. . Coarsened-exact matching is performed on the covariates in the corresponding regressions (continuous variables are coarsened into quartiles) including Review, but except medical subject, journal, year and month fixed effects, which are kept as fixed effects in the regressions on matched articles */**/*** indicate significance at the 10/5/1% level.

Table 5: Summary statistics for expert recommendations (PURL Program)

	N	Mean	SD	p25	p50	p75
Any author has a drug COI	448	0.493	0.501	0	0	1
Drug funding/materials	448	0.420	0.494	0	0	1
Any author has an individual level drug COI	448	0.382	0.486	0	0	1
All authors have drug COIs	448	0.313	0.464	0	0	1
All authors received drug funding/materials	448	0.292	0.455	0	0	1
All authors have individual drug COIs	448	0.036	0.186	0	0	0
Log(# authors)	448	1.899	0.606	1.386	1.792	2.303
Log(# pages)	448	2.188	0.539	1.946	2.079	2.398
Log(Impact Factor)	448	2.423	0.952	1.686	2.314	3.389
Log(days from Publication to Nomination)	410	5.957	0.358	5.905	5.932	6.009
Log(Citations up to Nomination)	410	1.204	1.112	0.000	1.099	1.946
Top 50 (world) school	448	0.161	0.368	0	0	0

This table shows descriptive statistics for variables used in the expert recommendation sample. *Nomination* is a binary indicator taking on the value of "1" if an article that had been nominated to be a PURL (Priority Update from the Research Literature, published in the Journal of Family Practice) either becomes a "PURL" or an "Important Reference"; if the nominated article does not become a PURL or just an Important Reference, the dependent variable is "0". Articles in our sample were nominated between 2007 and 2012. *Days from Publication to Nomination* is the length of time (in days) it took from the article's publication until it was nominated by physicians to be a PURL. *Citations up to Nomination* is the number of citations the article received until it was nominated. Other variables are as defined in Table 1.

Table 6: Regression analysis of expert recommendations and conflict disclosures

Panel A: Conflict of interest by any author

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Indicator for nominated article voted PURL or "Important Reference"					
Any author has a drug COI	-0.074	-0.096*	-0.097*			
	[0.047]	[0.050]	[0.050]			
Drug funding/materials				-0.014	-0.040	-0.042
				[0.060]	[0.064]	[0.064]
Individual level drug COI				-0.078	-0.089	-0.085
				[0.057]	[0.062]	[0.062]
Any author has a non-drug COI			0.055			
			[0.077]			
Non-drug funding						0.010
						[0.050]
Individual level non-drug COI						0.071
						[0.049]
Top 50 (world) institution		0.003	0.002		-0.000	0.001
		[0.062]	[0.062]		[0.063]	[0.063]
Log(# pages)		0.094	0.093		0.102*	0.095
		[0.058]	[0.057]		[0.058]	[0.059]
Log(# authors)		-0.029	-0.031		-0.023	-0.036
		[0.047]	[0.047]		[0.047]	[0.049]
Log(Impact Factor)		-0.017	-0.018		-0.016	-0.009
		[0.051]	[0.051]		[0.051]	[0.051]
Log(Days from Publication to Nomination)		-0.111***	-0.107***		-0.116***	-0.107***
		[0.040]	[0.041]		[0.039]	[0.041]
Log(Citations up to Nomination)		-0.001	-0.001		-0.001	-0.001
		[0.023]	[0.023]		[0.023]	[0.023]
p value from F-Test: Drug-funding COI=0 & Individual level drug COI=0				0.172	0.068	0.072
Study Type FE	Y	Y	Y	Y	Y	Y
Journal FE	Y	Y	Y	Y	Y	Y
Observations	448	410	410	448	410	410
R-squared	0.015	0.034	0.035	0.018	0.037	0.042

Equivalent specifications with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Any author has a drug COI	-0.084**	-0.128**	-0.155***			
	[0.036]	[0.053]	[0.043]			
Drug funding/materials				0.014	-0.000	0.047
				[0.049]	[0.070]	[0.093]
Individual level drug COI				-0.114***	-0.141**	-0.239***
				[0.034]	[0.059]	[0.077]
p value from F-Test: Drug-funding COI=0 & Individual level drug COI=0				0.003	0.029	0.011
Observations	445	278	248	445	278	163
R-squared	0.008	0.018	0.027	0.012	0.021	0.048

The table presents results from OLS regressions where the dependent variable is an indicator taking on the value of "1" if an article that had been nominated to be a PURL (Priority Update from the Research Literature, published in the Journal of Family Practice) either becomes a "PURL" or an "Important Reference", and zero otherwise. Articles in our sample were nominated between 2007 and 2012. Days from Publication to Nomination and Citations up to Nomination are defined in Table 5. Other variables are as defined in Table 1. For OLS specifications in top panel, robust standard errors are reported in brackets. For the matching specifications, standard errors are clustered by matching strata and reported in brackets. Coarsened-exact matching is performed on the covariates in the corresponding regressions, including fixed effects (continuous variables are coarsened into quartiles). . */**/*** indicate significance at the 10/5/1% level.

Table 6: Regression analysis of expert recommendations and conflict disclosures (cont.)

Panel B: Conflict of interest by all authors

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Indicator for nominated article voted PURL or "Important Reference"					
All authors have a drug COI	-0.119**	-0.140***	-0.142***			
	[0.049]	[0.051]	[0.052]			
Drug-funding/materials				-0.092*	-0.109**	-0.113**
				[0.051]	[0.054]	[0.054]
Individual level drug COI				-0.044	-0.163	-0.170
				[0.112]	[0.109]	[0.110]
All authors have a non-drug COI			-0.011			
			[0.049]			
Non-drug funding						0.001
						[0.048]
Individual level non-drug COI						-0.073
						[0.060]
Top 50 (world) institution		0.006	0.006		0.003	-0.003
		[0.063]	[0.063]		[0.062]	[0.063]
Log(# pages)		0.092	0.092		0.095*	0.097*
		[0.056]	[0.056]		[0.056]	[0.057]
Log(# authors)		-0.024	-0.024		-0.029	-0.038
		[0.046]	[0.046]		[0.047]	[0.047]
Log(Impact Factor)		-0.019	-0.019		-0.019	-0.019
		[0.051]	[0.051]		[0.051]	[0.051]
Log(days from Publication to Nomination)		-0.111***	-0.112***		-0.111***	-0.121***
		[0.038]	[0.038]		[0.039]	[0.039]
Log(Citations up to Nomination)		-0.004	-0.004		-0.004	-0.004
		[0.023]	[0.023]		[0.023]	[0.023]
p value from F-Test: Drug-funding COI=0 & Individual level drug COI=0				0.195	0.055	0.047
Study Type FE	Y	Y	Y	Y	Y	Y
Journal FE	Y	Y	Y	Y	Y	Y
Observations	448	410	410	448	410	410
R-squared	0.023	0.042	0.042	0.018	0.039	0.042
Equivalent specifications with matching						
All authors have a drug COI	-0.104***	-0.137***	-0.164***			
	[0.017]	[0.045]	[0.052]			
Drug-funding COI				-0.075***	-0.099**	-0.133*
				[0.023]	[0.048]	[0.067]
All authors have individual level drug COI				-0.054	-0.155	-0.160
				[0.121]	[0.124]	[0.152]
p value from F-Test: Drug-funding COI=0 & Individual level drug COI=0				0.004	0.042	0.114
Observations	445	278	217	445	278	174
R-squared	0.011	0.019	0.027	0.006	0.014	0.025

The table presents results from OLS regressions where the dependent variable is an indicator taking on the value of "1" if an article that had been nominated to be a PURL (Priority Update from the Research Literature, published in the Journal of Family Practice) either becomes a "PURL" or an "Important Reference", and zero otherwise. Articles in our sample were nominated between 2007 and 2012. Days from Publication to Nomination and Citations up to Nomination are defined in Table 5. Other variables are as defined in Table 1. For the OLS specifications in top panel, robust standard errors are reported in brackets. For the matching specifications, standard errors are clustered by matching strata and reported in brackets. See panel A for details on matching. */**/*** indicate significance at the 10/5/1% level.

Table 7: Regression results from within-author analysis of matched treatment and control articles

Panel A: Difference-in-differences analysis of yearly citations

	Matching on:					
	(1)	(2)	(3)	(4)	(5)	(6)
	Study type, Publication year, Main author	Study type, Publication year, Main author, Impact factor	Study type, Publication year, Main author Impact factor, Publication month	Study type, Publication year, Main author, Months to shock	Study type, Publication year, Impact factor, Main author, Months to shock	Study type, Publication year, Impact factor, Main author, Publication month, Months to shock
	Post starting in 2003			*Post* starting in author-specific "Shock" year		
Treated x Post	-0.045**	-0.039**	-0.037*	-0.066**	-0.069**	-0.070**
	[0.018]	[0.019]	[0.020]	[0.030]	[0.027]	[0.027]
Fixed Effects						
Article FE	Y	Y	Y	Y	Y	Y
Event Year FE	Y	Y	Y	Y	Y	Y
# CEM Strata	36	84	127	90	171	190
Observations	30,851	30,105	29,572	30,103	29,148	28,938
R-squared	0.751	0.738	0.737	0.738	0.724	0.723

Panel B: Review articles versus other studies

	Matching on:					
	Study type, Publication year, Main author, Months to shock (Model 4)		Study type, Publication year, Impact factor, Main author, Months to shock (Model 5)		Study type, Publication year, Impact factor, Main author, Publication month, Months to shock (Model 6)	
	Article Type		Article Type		Article Type	
	Review	Non-Review	Review	Non-Review	Review	Non-Review
Treated x Post	-0.175**	-0.044	-0.158**	-0.051*	-0.161**	-0.052*
	[0.076]	[0.032]	[0.074]	[0.028]	[0.074]	[0.028]
Fixed Effects						
Article FE	Y	Y	Y	Y	Y	Y
Event Year FE	Y	Y	Y	Y	Y	Y
# CEM Strata	16	74	30	141	33	157
Observations	4,811	25,292	4,665	24,483	4,625	24,313
R-squared	0.732	0.738	0.706	0.725	0.707	0.724

The table shows the difference-in-differences analysis of yearly citations between treatment articles (*Treated*), for which an author subsequently discloses a conflict of interest in another article, and matched control articles for authors without subsequent disclosure. The unit of observation is article-(event-)year and the underlying sample comprises up to 2,910 articles by 184 treatment and 185 control authors. The dependent variable is the log transformation of yearly citations (plus 1), adjusted for author self-citations. In Panel A, two different definitions for *Post* are used. In Column 1 to 3, *Post* is coded as "1" starting in 2003 (and "0" otherwise); in Column 4 to 6, *Post* is coded as "1" starting in either 2002 or 2003 depending on when a conflict of interest was disclosed by a treated author or when a top four article without a conflict was published by a control author (and "0" otherwise). All specifications include article and event-year fixed effects (i.e., fixed effects for years relative to *Post*). Treatment and control articles are matched using coarsened exact matching. The matching covariates are indicated in the respective column headers. All conflicted and non-conflicted articles are exact matched on study type, publication year and main author. If applicable, coarsened exact matching is applied to impact factor (4 bins of equal width), months to shock (<12, 12-24, 24-36, >36) and publication month (<4, 4-7, 7-10,>10). *Study type* is defined as in Table 1, i.e. the article's "publication type", scraped from the PubMed online database. *Publication year* is the year in which the article was published. *Main author* indicates whether the focal author is listed as the first or last author on a publication. *Impact factor* of the journal is obtained from Thomson-Reuters or directly scraped from a journal's webpage. *Publication month* is the month in which the article was published. *Months to shock* is the number of months between the dates when the article was published and when a conflict of interest was disclosed in another article by the same author published in a top four or top-impact factor journal (for control articles, it is the number of months between the article and a later top four article in which no conflict is disclosed). Panel B extends the analyses of Column 4 to 6 of Panel A by reporting the results for two different sub-samples (articles with *Study type* "Review" versus all other articles). Standard errors are clustered by the corresponding matching strata. */**/*** indicate significance at the 10/5/1% level

Online appendix tables

Table A3a. Baseline regressions with quality controls, using all author COI and aggregated conflicts; original research articles only.

	(1)	(2)	(3)	(4)	(5)	(6)
All authors have a drug COI	0.706***	0.506***	0.477***	0.327***	0.260***	0.260***
	[0.056]	[0.033]	[0.031]	[0.030]	[0.029]	[0.030]
Top 50 (world) institution		0.081***	0.088***	0.081***	0.035	0.035
		[0.028]	[0.028]	[0.026]	[0.026]	[0.026]
Log(# 3-year citations + 1)		0.111***	0.098***	0.087***	0.060***	0.060***
		[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
Relative position		0.850***	0.827***	0.762***	0.553***	0.553***
		[0.049]	[0.048]	[0.046]	[0.039]	[0.039]
Main section		0.433***	0.428***	0.045	-0.097*	-0.097*
		[0.121]	[0.121]	[0.101]	[0.051]	[0.051]
Log(# organizations' 3-year citations + 1)					0.018***	0.018***
					[0.006]	[0.006]
Log(# pages)					0.746***	0.745***
					[0.043]	[0.043]
Log(# authors)					0.263***	0.263***
					[0.026]	[0.026]
All authors have non-drug COI						0.004
						[0.023]
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	11,293	11,293	11,293	11,293	11,245	11,245
R-squared	0.408	0.491	0.507	0.530	0.566	0.566

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
All authors have a drug COI		0.528***	0.488***	0.347***	0.281***	0.247***
		[0.029]	[0.029]	[0.030]	[0.038]	[0.040]
Observations		11,293	11,293	10,888	8,582	7,451
R-squared		0.417	0.448	0.460	0.470	0.454

This table extends Panel A of Table 3. We replace the two COI-related variables *Any author has a drug COI* and *Any non-drug COI* with *All authors have a drug COI* and *All authors have non-drug COI*, respectively. For the remaining definitions see the notes to Panel A of Table 3.

Table A3b. Baseline regressions with quality controls, using any author COI and broad COI categories; original research articles.

	(1)	(2)	(3)	(4)	(5)	(6)
Individual level drug COI	0.412***	0.358***	0.340***	0.291***	0.250***	0.262***
	[0.040]	[0.037]	[0.038]	[0.036]	[0.036]	[0.036]
Drug-funding COI	0.549***	0.366***	0.347***	0.240***	0.179***	0.181***
	[0.058]	[0.034]	[0.033]	[0.031]	[0.029]	[0.029]
Top 50 (world) institution		0.078***	0.085***	0.078***	0.031	0.025
		[0.027]	[0.027]	[0.025]	[0.025]	[0.025]
Log(# 3-year citations + 1)		0.107***	0.095***	0.084***	0.058***	0.058***
		[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
Relative position		0.837***	0.813***	0.755***	0.550***	0.550***
		[0.049]	[0.048]	[0.046]	[0.039]	[0.039]
Main section		0.437***	0.432***	0.044	-0.096*	-0.093*
		[0.122]	[0.122]	[0.102]	[0.051]	[0.051]
Log(# organizations' 3-year citations + 1)					0.018***	0.017***
					[0.006]	[0.006]
Log(# pages)					0.742***	0.734***
					[0.043]	[0.043]
Log(# authors)					0.251***	0.246***
					[0.027]	[0.027]
Non-drug funding COI						0.052**
						[0.026]
Individual level non-drug COI						-0.007
						[0.022]
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	11,293	11,293	11,293	11,293	11,245	11,245
R-squared	0.415	0.495	0.511	0.534	0.568	0.568

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Individual level drug COI		0.349***	0.333***	0.298***	0.229***	0.207***
		[0.041]	[0.040]	[0.039]	[0.042]	[0.048]
Drug-funding COI		0.409***	0.379***	0.301***	0.242***	0.238***
		[0.046]	[0.043]	[0.035]	[0.038]	[0.039]
Observations		11,293	11,293	10,987	9,123	6,747
R-squared		0.419	0.449	0.461	0.465	0.469

This table extends Panel A of Table 3. We divide the two COI-related variables into individual-level and drug-funding related COIs. For the remaining definitions see the notes to Panel A of Table 3.

Table A3c. Baseline regressions with quality controls, using all author COI and broad COI categories; original research articles.

	(1)	(2)	(3)	(4)	(5)	(6)
Individual level drug COI	0.218	0.259**	0.271**	0.152	0.249*	0.257**
	[0.156]	[0.117]	[0.115]	[0.125]	[0.128]	[0.128]
Drug-funding COI	0.708***	0.502***	0.472***	0.328***	0.255***	0.259***
	[0.055]	[0.033]	[0.031]	[0.031]	[0.030]	[0.030]
Top 50 (world) institution		0.081***	0.088***	0.082***	0.034	0.033
		[0.028]	[0.028]	[0.026]	[0.026]	[0.026]
Log(# 3-year citations + 1)		0.111***	0.098***	0.087***	0.060***	0.060***
		[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
Relative position		0.850***	0.827***	0.761***	0.552***	0.552***
		[0.049]	[0.048]	[0.046]	[0.039]	[0.039]
Main section		0.432***	0.427***	0.043	-0.097*	-0.097*
		[0.121]	[0.121]	[0.102]	[0.051]	[0.051]
Log(# organizations' 3-year citations + 1)					0.018***	0.018***
					[0.006]	[0.006]
Log(# pages)					0.745***	0.741***
					[0.043]	[0.043]
Log(# authors)					0.265***	0.264***
					[0.026]	[0.026]
Non-drug funding COI						0.027
						[0.025]
Individual level non-drug COI						0.019
						[0.028]
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	11,293	11,293	11,293	11,293	11,245	11,245
R-squared	0.408	0.491	0.507	0.531	0.566	0.566

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Individual level drug COI		0.162	0.188	0.131	-0.069	-0.034
		[0.200]	[0.186]	[0.149]	[0.199]	[0.190]
Drug-funding COI		0.532***	0.491***	0.352***	0.295***	0.256***
		[0.041]	[0.039]	[0.031]	[0.037]	[0.041]
Observations		11,293	11,293	10,888	8,582	6,353
R-squared		0.417	0.448	0.460	0.470	0.448

This table extends Table A3b. We require that *all authors* share (instead of any author has) the respective COI. For the remaining definitions see the notes to A3b.

Table A3d. Baseline regressions with quality controls, using all authors COI and detailed COI categories; original research articles.

	(1)	(2)	(3)	(4)	(5)	(6)
Funding	0.695***	0.491***	0.466***	0.332***	0.272***	0.278***
	[0.051]	[0.030]	[0.029]	[0.029]	[0.030]	[0.030]
Gave drug or materials	0.431***	0.305***	0.270***	0.136**	0.066	0.063
	[0.070]	[0.057]	[0.054]	[0.054]	[0.051]	[0.051]
Employee	-0.786*	-0.188	-0.145	-0.485	-0.387	-0.375
	[0.413]	[0.320]	[0.319]	[0.364]	[0.387]	[0.387]
Consultant	0.345	0.083	0.010	0.097	0.157	0.157
	[0.388]	[0.332]	[0.335]	[0.327]	[0.333]	[0.334]
Award	0.530**	0.443***	0.419***	0.230**	0.387**	0.380**
	[0.231]	[0.086]	[0.086]	[0.110]	[0.186]	[0.187]
Honoraria/speaker	0.189	0.093	0.135	0.024	0.109	0.109
	[0.328]	[0.301]	[0.307]	[0.290]	[0.282]	[0.281]
Equity	-	-	-	-	-	-
	-	-	-	-	-	-
Other	0.529*	0.253	0.184	0.185	0.199	0.201
	[0.301]	[0.290]	[0.278]	[0.282]	[0.265]	[0.266]
Control Variables (as in Panel A of Table 3)	N	Y	Y	Y	Y	Y
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	11,293	11,293	11,293	11,293	11,245	11,245
R-squared	0.408	0.490	0.506	0.530	0.566	0.566

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Funding		0.535***	0.495***	0.364***	0.304***	0.248***
		[0.024]	[0.026]	[0.034]	[0.038]	[0.044]
Gave drug or materials		0.294***	0.257***	0.156***	0.137**	0.166***
		[0.066]	[0.055]	[0.059]	[0.061]	[0.061]
Employee		-0.871	-0.802	-0.775**	-1.009***	-1.170**
		[0.666]	[0.692]	[0.385]	[0.385]	[0.454]
Consultant		0.339	0.186	0.119	0.085	-0.555
		[0.422]	[0.403]	[0.390]	[0.497]	[0.593]
Award		0.411	0.396	0.020	-0.003	-0.004
		[0.338]	[0.342]	[0.170]	[0.185]	[0.284]
Honoraria/speaker		0.115	0.200	0.154	-0.067	0.341
		[0.437]	[0.437]	[0.452]	[0.505]	[0.281]
Equity		-	-	-	-	-
		-	-	-	-	-
Other		0.402	0.247	0.143	0.155	0.144
		[0.364]	[0.325]	[0.256]	[0.265]	[0.249]
Observations		11,293	11,293	10,888	8,582	6,353
R-squared		0.418	0.448	0.461	0.471	0.449

This table extends Panel B of Table 3. Instead of using any author COI definitions, we require that all authors share the respective COI. For the remaining definitions see the notes to Panel B of Table 3.

Table A3e. Baseline regressions with quality controls, using any author COI and detailed COI categories; original research and review articles.

	(1)	(2)	(3)	(4)	(5)	(6)
Funding	0.519***	0.335***	0.322***	0.233***	0.230***	0.240***
	[0.057]	[0.034]	[0.033]	[0.032]	[0.031]	[0.031]
Gave drug or materials	0.345***	0.256***	0.227***	0.123**	0.120**	0.096*
	[0.070]	[0.055]	[0.052]	[0.051]	[0.051]	[0.050]
Employee	0.357***	0.339***	0.322***	0.240***	0.250***	0.300***
	[0.050]	[0.045]	[0.044]	[0.042]	[0.042]	[0.042]
Consultant	0.169**	0.109	0.111	0.129**	0.133**	0.151**
	[0.074]	[0.071]	[0.068]	[0.062]	[0.062]	[0.061]
Award	0.369***	0.228**	0.214**	0.215**	0.208**	0.193*
	[0.107]	[0.101]	[0.093]	[0.100]	[0.100]	[0.101]
Honoraria/speaker	0.114**	0.088*	0.053	0.042	0.043	0.062
	[0.058]	[0.051]	[0.053]	[0.047]	[0.047]	[0.047]
Equity	0.004	0.001	0.027	0.058	0.058	0.057
	[0.073]	[0.071]	[0.067]	[0.064]	[0.064]	[0.063]
Other	0.205	0.019	0.043	0.065	0.058	0.037
	[0.165]	[0.157]	[0.155]	[0.151]	[0.149]	[0.151]
Control Variables (as in Panel A of Table 3)	N	Y	Y	Y	Y	Y
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject x review	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	17,135	17,135	17,135	17,135	17,135	17,135
R-squared	0.410	0.477	0.491	0.510	0.513	0.520

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Funding	0.537**	0.408***	0.386***	0.311***	0.298***	0.286***
	[0.017]	[0.038]	[0.035]	[0.041]	[0.040]	[0.041]
Gave drug or materials	0.361***	0.252***	0.222***	0.163***	0.165***	0.178***
	[0.001]	[0.065]	[0.054]	[0.060]	[0.056]	[0.060]
Employee	0.370***	0.310***	0.300***	0.254***	0.248***	0.213***
	[0.005]	[0.056]	[0.057]	[0.049]	[0.050]	[0.057]
Consultant	0.168**	0.178***	0.179***	0.163***	0.181***	0.141**
	[0.010]	[0.053]	[0.050]	[0.054]	[0.056]	[0.058]
Award	0.380**	0.275*	0.271*	0.205*	0.201*	0.212*
	[0.012]	[0.140]	[0.143]	[0.115]	[0.116]	[0.121]
Honoraria/speaker	0.104	0.100	0.047	0.063	0.059	0.152**
	[0.021]	[0.060]	[0.063]	[0.059]	[0.066]	[0.061]
Equity	-0.001	0.031	0.063	0.104	0.089	0.076
	[0.004]	[0.082]	[0.080]	[0.082]	[0.083]	[0.087]
Other	0.207**	0.174	0.197	0.197	0.303*	0.223
	[0.012]	[0.203]	[0.208]	[0.203]	[0.171]	[0.180]
Observations	17,135	17,108	17,108	16,673	16,086	14,547
R-squared	0.402	0.407	0.432	0.449	0.451	0.454

This table extends Panel B of Table 3. We add review articles to the sample. In the matching procedure, we include Review as an additional covariate. For the remaining definitions see the notes to Panel B of Table 3.

Table A3f. Baseline regressions with quality controls, using all authors COI and detailed COI categories; original research and review articles.

	(1)	(2)	(3)	(4)	(5)	(6)
Funding	0.692***	0.481***	0.455***	0.322***	0.322***	0.337***
	[0.053]	[0.029]	[0.029]	[0.029]	[0.029]	[0.029]
Gave drug or materials	0.420***	0.303***	0.270***	0.142**	0.139**	0.122**
	[0.070]	[0.056]	[0.054]	[0.055]	[0.055]	[0.054]
Employee	-0.780*	-0.173	-0.149	-0.539	-0.453	-0.448
	[0.404]	[0.315]	[0.319]	[0.374]	[0.386]	[0.383]
Consultant	0.303	0.030	-0.041	0.054	0.055	0.095
	[0.392]	[0.348]	[0.351]	[0.346]	[0.342]	[0.345]
Award	0.507**	0.438***	0.412***	0.240**	0.251*	0.344**
	[0.229]	[0.093]	[0.097]	[0.121]	[0.137]	[0.140]
Honoraria/speaker	0.224	0.132	0.179	0.068	0.072	0.078
	[0.331]	[0.308]	[0.313]	[0.297]	[0.297]	[0.300]
Equity	0.524	0.578*	0.532	0.529	0.437	0.488
	[0.322]	[0.339]	[0.370]	[0.362]	[0.349]	[0.332]
Other	0.550*	0.275	0.219	0.225	0.216	0.225
	[0.306]	[0.295]	[0.285]	[0.287]	[0.287]	[0.282]
Control Variables (as in Panel A of Table 3)	N	Y	Y	Y	Y	Y
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject x review	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	17,135	17,135	17,135	17,135	17,135	17,135
R-squared	0.404	0.473	0.488	0.508	0.511	0.516

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Funding	0.709**	0.560***	0.521***	0.397***	0.387***	0.335***
	[0.019]	[0.034]	[0.034]	[0.041]	[0.036]	[0.041]
Gave drug or materials	0.437***	0.307***	0.273***	0.178***	0.185***	0.175***
	[0.003]	[0.065]	[0.056]	[0.062]	[0.059]	[0.061]
Employee	-0.775**	-0.853	-0.799	-0.770**	-0.817**	-0.833**
	[0.034]	[0.663]	[0.687]	[0.385]	[0.395]	[0.389]
Consultant	0.331**	0.328	0.193	0.118	0.134	0.178
	[0.025]	[0.416]	[0.395]	[0.390]	[0.400]	[0.473]
Award	0.526**	0.410	0.390	0.028	0.021	-0.048
	[0.014]	[0.327]	[0.328]	[0.157]	[0.170]	[0.189]
Honoraria/speaker	0.208*	0.138	0.227	0.200	0.194	0.060
	[0.027]	[0.433]	[0.430]	[0.452]	[0.445]	[0.493]
Equity	0.533**	0.541*	0.454	0.295	0.299	0.329
	[0.017]	[0.275]	[0.333]	[0.448]	[0.442]	[0.356]
Other	0.547**	0.425	0.291	0.197	0.214	0.260
	[0.023]	[0.366]	[0.331]	[0.268]	[0.263]	[0.258]
Observations	17,135	17,035	17,035	16,398	15,644	13,821
R-squared	0.397	0.410	0.436	0.457	0.458	0.427

This table extends Table A3d. We add review articles to the sample. In the matching procedure, we include Review as an additional covariate. For the remaining definitions see the notes to Panel B of Table 3.

Table A4a. Baseline regressions with quality controls, using all author COI and aggregated conflicts; original research and review articles.

	(1)	(2)	(3)	(4)	(5)	(6)
All authors have a drug COI	0.704***	0.499***	0.469***	0.320***	0.320***	0.326***
	[0.058]	[0.032]	[0.030]	[0.029]	[0.029]	[0.030]
Review	-0.092	0.609***	0.671***	0.218*	0.004	0.099
	[0.061]	[0.140]	[0.144]	[0.122]	[0.135]	[0.134]
Review x all authors have a drug COI	-0.438***	-0.252***	-0.220***	-0.082	-0.060	-0.067
	[0.092]	[0.070]	[0.068]	[0.066]	[0.066]	[0.067]
Wald test: All authors have a drug COI + Review interaction = 0						
Test statistic	10.40	12.75	13.65	13.30	15.70	15.98
p value	0.002	<0.001	<0.001	<0.001	<0.001	<0.001
Control Variables (as in Panel A of Table 3)	N	Y	Y	Y	Y	Y
Control Variables x Review	N	Y	Y	Y	Y	Y
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject x review	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	17,135	17,135	17,135	17,135	17,135	17,135
R-squared	0.404	0.473	0.488	0.507	0.510	0.512

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
All authors have a drug COI	0.721**	0.555***	0.516***	0.383***	0.375***	0.331***
	(0.022)	(0.038)	(0.037)	(0.040)	(0.036)	(0.040)
Review x all authors have a drug COI	-0.531**	-0.504***	-0.428***	-0.399***	-0.405***	-0.395***
	(0.017)	(0.135)	(0.113)	(0.083)	(0.082)	(0.072)
Wald test: All authors have a drug COI + Review interaction = 0						
Test statistic	24.00	0.192	0.822	0.079	0.159	0.889
p value	0.128	0.664	0.371	0.780	0.691	0.346
Observations	17,135	17,035	17,035	16,398	15,644	14,819
R-squared	0.397	0.409	0.435	0.456	0.456	0.433

This table extends Table 4. Instead of using *any author* COI definitions, we require that *all authors* share the respective COI. For the remaining definitions see the notes to Table 4.

Table A4b. Baseline regressions with quality controls, using any author COI and broad COI categories; original research and review articles.

	(1)	(2)	(3)	(4)	(5)	(6)
Individual level drug COI	0.421***	0.355***	0.337***	0.287***	0.294***	0.331***
	[0.038]	[0.036]	[0.037]	[0.035]	[0.035]	[0.035]
Drug funding/materials	0.545***	0.360***	0.341***	0.237***	0.235***	0.240***
	[0.059]	[0.034]	[0.033]	[0.031]	[0.031]	[0.030]
Review	-0.089	0.608***	0.667***	0.209*	-0.011	0.356***
	[0.063]	[0.142]	[0.147]	[0.124]	[0.138]	[0.131]
Review x individual level drug COI	-0.050	-0.028	-0.006	0.027	0.048	0.013
	[0.077]	[0.070]	[0.070]	[0.068]	[0.068]	[0.068]
Review x drug funding/materials	-0.287***	-0.209***	-0.204**	-0.126	-0.150*	-0.212**
	[0.096]	[0.080]	[0.079]	[0.080]	[0.081]	[0.082]
Wald test: Individual level drug COI + Review interaction = 0						
Test statistic	8.337	3.779	3.047	2.127	1.189	0.125
p value	0.004	0.054	0.083	0.147	0.277	0.725
Wald test: Drug funding/materials + Review interaction = 0						
Test statistic	30.75	31.05	32.07	29.79	35.56	38.48
p value	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Control Variables (as in Panel A of Table 3)	N	Y	Y	Y	Y	Y
Control Variables x Review	N	Y	Y	Y	Y	Y
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject x review	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	17,135	17,135	17,135	17,135	17,135	17,135
R-squared	0.410	0.477	0.491	0.510	0.513	0.520

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Individual level drug COI	0.425**	0.366***	0.346***	0.306***	0.308***	0.278***
	[0.023]	[0.042]	[0.041]	[0.041]	[0.040]	[0.042]
Drug funding/materials	0.562**	0.427***	0.400***	0.323***	0.315***	0.314***
	[0.015]	[0.048]	[0.045]	[0.040]	[0.038]	[0.038]
Review x individual level drug COI	-0.109*	-0.161	-0.128	-0.126	-0.151	-0.125
	[0.014]	[0.136]	[0.123]	[0.103]	[0.104]	[0.093]
Review x drug funding/materials	-0.339***	-0.288***	-0.266***	-0.247***	-0.234***	-0.235***
	[0.004]	[0.086]	[0.074]	[0.076]	[0.087]	[0.085]
Wald test: Individual level drug COI + Review interaction = 0						
Test statistic	75.80	2.892	4.240	4.475	2.733	3.472
p value	0.073	0.096	0.046	0.036	0.099	0.063
Wald test: Drug funding/materials + Review interaction = 0						
Test statistic	423.3	4.303	5.005	1.651	1.110	1.108
p value	0.031	0.044	0.031	0.200	0.293	0.293
Observations	17,135	17,108	17,108	16,673	16,086	14,547
R-squared	0.402	0.407	0.432	0.448	0.451	0.454

This table extends Table 4. We divide the two COI-related variables into individual-level and drug-funding related COIs. For the remaining definitions see the notes to Table 4.

Table A4c. Baseline regressions with quality controls, using all author COI and broad COI categories; original research and review articles.

	(1)	(2)	(3)	(4)	(5)	(6)
Individual level drug COI	0.223	0.258**	0.270**	0.149	0.177	0.215*
	[0.154]	[0.117]	[0.116]	[0.128]	[0.128]	[0.130]
Drug funding/materials	0.705***	0.494***	0.464***	0.320***	0.319***	0.326***
	[0.057]	[0.032]	[0.030]	[0.030]	[0.030]	[0.031]
Review	-0.089	0.613***	0.674***	0.221*	0.012	0.403***
	[0.061]	[0.140]	[0.144]	[0.123]	[0.136]	[0.137]
Review x individual level drug COI	-0.275	-0.257*	-0.251*	-0.113	-0.093	-0.088
	[0.177]	[0.146]	[0.140]	[0.152]	[0.154]	[0.155]
Review x drug funding/materials	-0.368***	-0.232**	-0.207**	-0.085	-0.107	-0.216**
	[0.103]	[0.093]	[0.092]	[0.097]	[0.098]	[0.100]
Wald test: Individual level drug COI + Review interaction = 0						
Test statistic	11.82	8.286	8.031	6.729	5.264	1.367
p value	<0.001	0.005	0.005	0.011	0.023	0.244
Wald test: Drug funding/materials + Review interaction = 0						
Test statistic	0.282	<0.001	0.053	0.188	1.019	2.394
p value	0.596	0.992	0.818	0.665	0.315	0.124
Control Variables (as in Panel A of Table 3)	N	Y	Y	Y	Y	Y
Control Variables x Review	N	Y	Y	Y	Y	Y
Fixed effects						
Study type	N	N	N	Y	Y	Y
Medical subject x review	N	N	Y	Y	Y	Y
Journal	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
Month	Y	Y	Y	Y	Y	Y
Observations	17,135	17,135	17,135	17,135	17,135	17,135
R-squared	0.404	0.473	0.488	0.507	0.510	0.515

Equivalent specification with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Individual level drug COI	0.225**	0.177	0.208	0.157	0.105	0.114
	[0.015]	[0.196]	[0.185]	[0.152]	[0.163]	[0.174]
Drug funding/materials	0.721**	0.557***	0.517***	0.385***	0.381***	0.330***
	[0.018]	[0.045]	[0.043]	[0.040]	[0.036]	[0.040]
Review x individual level drug COI	-0.332**	-0.375	-0.371	-0.390**	-0.358*	-0.393*
	[0.021]	[0.230]	[0.227]	[0.177]	[0.206]	[0.210]
Review x drug funding/materials	-0.434***	-0.372***	-0.310**	-0.262**	-0.261***	-0.257***
	[0.003]	[0.121]	[0.119]	[0.112]	[0.083]	[0.098]
Wald test: Individual level drug COI + Review interaction = 0						
Test statistic	8.686	4.306	2.484	6.268	3.915	5.728
p value	0.208	0.045	0.124	0.013	0.049	0.017
Wald test: Drug funding/materials + Review interaction = 0						
Test statistic	346.1	3.345	4.277	1.911	3.103	0.652
p value	0.034	0.076	0.046	0.168	0.079	0.420
Observations	17,135	17,035	17,035	16,398	15,644	13,821
R-squared	0.397	0.409	0.435	0.456	0.457	0.426

This table extends Table A4b. We require that *any author* has (instead of all authors share) the respective COI. For the remaining definitions see the notes to .Table A4b.

Table A6a. PURL OLS regressions with article quality controls, using any author COI; "Important References" dropped.

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	PURL indicator (dropping "Important Reference")					
Any author has a drug COI	-0.056	-0.067	-0.066			
	[0.044]	[0.047]	[0.047]			
Drug funding/materials				-0.019	-0.014	-0.015
				[0.055]	[0.059]	[0.059]
Individual level drug COI				-0.048	-0.072	-0.076
				[0.052]	[0.057]	[0.057]
Any author has a non-drug COI			-0.045			
			[0.073]			
Non-drug funding						-0.051
						[0.046]
Individual level non-drug COI						-0.006
						[0.046]
Top 50 (world) institution		-0.012	-0.011		-0.016	-0.014
		[0.058]	[0.058]		[0.059]	[0.059]
Log(# pages)		0.056	0.057		0.063	0.066
		[0.052]	[0.052]		[0.052]	[0.053]
Log(# authors)		-0.065	-0.063		-0.062	-0.058
		[0.045]	[0.045]		[0.046]	[0.046]
Log(Impact Factor)		0.013	0.014		0.011	0.014
		[0.044]	[0.044]		[0.044]	[0.044]
Log(Days from Publication to Nomination)		-0.097**	-0.100**		-0.101**	-0.101**
		[0.046]	[0.046]		[0.045]	[0.044]
Log(Citations up to Nomination)		0.034	0.034		0.034	0.035
		[0.021]	[0.022]		[0.021]	[0.021]
p value from F-test: Drug-funding COI=0 & Individual level drug COI=0				0.382	0.224	0.188
Study Type FE	Y	Y	Y	Y	Y	Y
Journal FE	Y	Y	Y	Y	Y	Y
Observations	395	364	364	395	364	364
R-squared	0.035	0.055	0.056	0.036	0.057	0.061

Equivalent specifications with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Any author has a drug COI	-0.050	-0.085	-0.065			
	[0.049]	[0.074]	[0.061]			
Drug-funding COI				0.022	0.040	0.098
				[0.030]	[0.067]	[0.084]
Individual level drug COI				-0.075*	-0.123*	-0.245***
				[0.037]	[0.068]	[0.081]
p value from F-Test: Drug-funding COI=0 & Individual level drug COI=0				0.033	0.193	0.014
Observations	392	243	218	392	243	141
R-squared	0.004	0.010	0.006	0.006	0.014	0.048

This table extends Panel A of Table 6. We drop 53 articles that were recommended as "Important References" from the analyses. For the remaining definitions see the notes to Panel A of Table 6.

Table A6b. PURL logit regressions with article quality controls, using any author COI.

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Indicator for nominated article voted PURL or "Important Reference"					
Any author has a drug COI	-0.360	-0.478*	-0.482*			
	[0.231]	[0.250]	[0.251]			
Drug funding/materials				-0.071	-0.201	-0.224
				[0.287]	[0.312]	[0.314]
Individual level drug COI				-0.384	-0.445	-0.434
				[0.280]	[0.306]	[0.308]
Any author has a non-drug COI			0.313			
			[0.429]			
Non-drug funding						0.046
						[0.249]
Individual level non-drug COI						0.383
						[0.251]
Top 50 (world) institution		0.014	0.006		-0.000	-0.003
		[0.303]	[0.303]		[0.308]	[0.310]
Log(# pages)		0.464	0.461		0.506*	0.481
		[0.296]	[0.295]		[0.301]	[0.304]
Log(# authors)		-0.141	-0.152		-0.112	-0.181
		[0.236]	[0.238]		[0.239]	[0.247]
Log(Impact Factor)		-0.075	-0.081		-0.068	-0.033
		[0.254]	[0.255]		[0.255]	[0.253]
Log(Days from Publication to Nomination)		-0.552*	-0.531*		-0.583**	-0.532*
		[0.286]	[0.284]		[0.287]	[0.288]
Log(Citations up to Nomination)		-0.005	-0.008		-0.004	-0.004
		[0.113]	[0.113]		[0.114]	[0.115]
p value from F-Test: Drug-funding COI=0 & Individual level drug COI=0				0.181	0.073	0.074
Study Type FE	Y	Y	Y	Y	Y	Y
Journal FE	Y	Y	Y	Y	Y	Y
Observations	448	410	410	448	410	410

Equivalent specifications with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Any author has a drug COI	-0.389**	-0.570**	-0.696***			
	[0.159]	[0.233]	[0.200]			
Drug funding/materials				0.064	-0.001	0.215
				[0.230]	[0.317]	[0.428]
Individual level drug COI				-0.541***	-0.644**	-1.074***
				[0.182]	[0.288]	[0.393]
p value from F-Test: Drug-funding COI=0 & Individual level drug COI=0				<0.001	0.039	0.021
Observations	445	278	248	445	278	163

This table extends Panel A of Table 6 and relies on logit (instead of OLS) regressions.

Table A7c. PURL logit regressions with article quality controls, using any author COI; "Important References" dropped.

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	PURL indicator (dropping "Important Reference")					
Any author has a drug COI	-0.362	-0.448	-0.446			
	[0.284]	[0.302]	[0.302]			
Drug funding/materials				-0.124	-0.105	-0.120
				[0.342]	[0.363]	[0.362]
Individual level drug COI				-0.306	-0.475	-0.505
				[0.329]	[0.349]	[0.350]
Any author has a non-drug COI			-0.282			
			[0.458]			
Non-drug funding						-0.326
						[0.290]
Individual level non-drug COI						-0.006
						[0.296]
Top 50 (world) institution		-0.078	-0.073		-0.103	-0.084
		[0.386]	[0.385]		[0.395]	[0.397]
Log(# pages)		0.345	0.349		0.387	0.403
		[0.303]	[0.305]		[0.305]	[0.314]
Log(# authors)		-0.433	-0.421		-0.414	-0.397
		[0.300]	[0.299]		[0.302]	[0.307]
Log(Impact Factor)		0.118	0.124		0.111	0.124
		[0.293]	[0.293]		[0.294]	[0.297]
Log(Days from Publication to Nomination)		-0.507*	-0.519*		-0.540**	-0.528**
		[0.263]	[0.267]		[0.258]	[0.262]
Log(Citations up to Nomination)		0.223*	0.229*		0.229*	0.240*
		[0.132]	[0.134]		[0.133]	[0.136]
p value from F-test: Drug-funding COI=0 & Individual level drug COI=0				0.382	0.201	0.162
Study Type FE	Y	Y	Y	Y	Y	Y
Journal FE	Y	Y	Y	Y	Y	Y
Observations	395	364	364	395	364	364

Equivalent specifications with matching

	(1)	(2)	(3)	(4)	(5)	(6)
Any author has a drug COI	-0.292	-0.454	-0.354			
	[0.296]	[0.386]	[0.312]			
Drug funding/materials				0.125	0.218	0.516
				[0.167]	[0.364]	[0.436]
Individual level drug COI				-0.446*	-0.680*	-1.282**
				[0.266]	[0.402]	[0.499]
p value from F-Test: Drug-funding COI=0 & Individual level drug COI=0				0.043	0.230	0.027
Observations	392	243	218	392	243	141

This table extends Panel A of Table 6. It relies on logit (instead of OLS) regressions and we drop 53 articles that were recommended as "Important References" from the analyses.