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## Essays

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### Preventing statistical errors in scientific journals

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#### Abstract

There is evidence of a high prevalence of statistical reporting errors in psychology and other scientific fields. These errors display a systematic preference for statistically significant results, distorting the scientific literature. There are several possible causes for this systematic error prevalence, with publication bias being the most prominent one. Journal editors could play an important role in preventing statistical errors in published literature. Concrete solutions entail encouraging sharing data and preregistration, and using the automated procedure “statcheck” to check manuscripts for errors.

#### Keywords

Statistical errors, publication bias, statcheck, data sharing

In a recent study<sup>1</sup>, we documented the prevalence of statistical reporting inconsistencies in more than 250,000  $p$ -values from eight major psychology journals, using the new R package “statcheck”<sup>2</sup>. The program *statcheck*: converts PDF and HTML articles to plain text files; extracts results of null hypothesis significance tests that are reported exactly according to APA style<sup>3</sup>; recomputes the  $p$ -value based on its accompanying test statistic and degrees of freedom, and checks if the reported  $p$ -value matches the recomputed  $p$ -value, taking rounding of the reported test statistic into account. We found that in half of the papers at least one  $p$ -value was inconsistent with the test statistic and degrees of freedom. In most of these cases, the reported  $p$ -value was only marginally different from the recomputed  $p$ -value. However, we also found that one in eight papers (12.5%) contained gross inconsistencies that may have affected the statistical conclusions: in those cases the reported  $p$ -value was significant, but the recomputed  $p$ -value was not, or vice versa. We found a higher prevalence of gross inconsistencies in  $p$ -values reported as significant, than  $p$ -values reported as non-significant, implying a systematic bias towards statistically significant findings.

This high prevalence of statistical errors in psychology papers is alarming, and there is evidence that this problem is not unique to psychology. Similar inconsistency rates have been found in, for instance, the medical sciences in general<sup>4</sup> and psychiatry in particular<sup>5</sup>. Even though small reporting errors might be inconsequential, wrongly reporting a  $p$ -value of 0.37 as 0.36 will probably not have serious effects, the apparent focus on significant results is worrying and can have far-reaching consequences. It may have added to the excess of (false) positive findings in science<sup>6,7</sup>. There are several explanations for this high error prevalence. First, most of the inconsistencies could have been caused by mere sloppiness. Especially in psychology this is easy to imagine,

since a single psychology paper on average already contains about ten statistical tests<sup>1</sup>. In the tangle of statistical output, it is imaginable that a  $p$ -value (or test statistic or degree of freedom) is copied incorrectly. Matters probably become worse because many researchers are not in the habit of double checking their own or their co-authors' analyses who sometimes do not even have access to the raw data in the first place<sup>8</sup>. However, sloppiness alone does not explain the apparent systematic preference for significant findings.

A possible explanation for the excess of  $p$ -values wrongly reported as significant is publication bias: significant results have a higher probability to be published than non-significant results<sup>9-11</sup>. It is imaginable that researchers just as often wrongly report a significant  $p$ -value as a non-significant  $p$ -value. However, because of publication bias, only the gross inconsistencies that wrongly present a  $p$ -value as significant are published, resulting in a systematic bias in favour of significant findings. Conversely, it is also possible that researchers *suspect* that their findings will not be published if they do not find a significant effect, and because of this, they more often wrongly round down a non-significant  $p$ -value to obtain a significant finding, than vice versa. This would be in line with the finding of John *et al*<sup>12</sup>, who found that 22% of a sample of over 2000 psychologists admitted to knowingly having rounded down a  $p$ -value to obtain significance, which would lead to an excess of false positive findings. Of course it could also just be the case that researchers unknowingly maintain double standards concerning the checking of their results: they would inspect their results with more scrutiny when the result is unexpectedly non-significant, but not when it is significant.

I believe journal editors can play an important role in preventing, detecting, and/or correcting statistical errors in scientific literature. There are several concrete steps that could be taken to improve the state of the published literature.

A possible solution to the problem of statistical reporting errors is to promote data sharing. In previous research it has been found that if researchers were unwilling to share data of a certain paper, there was a higher probability that the paper contained reporting errors, often concerning statistical significance<sup>13</sup>. This finding could illustrate that authors are aware of the inconsistencies in their paper and refuse to share their data out of fear of being exposed. An alternative explanation for this finding is that researchers who manage their data with more rigour both make fewer mistakes and archive their data better, which makes data sharing easier. In both cases the prevalence of reporting errors might decrease if journal editors were to encourage data sharing.

Besides the possibility that authors themselves may become more precise in reporting their results if they have to share their data, encouraging data sharing has more benefits. If authors would submit their data and analysis scripts alongside their manuscript, it would allow for so-called analytic review<sup>14</sup>. In analytic review, peer reviewers or statistical experts verify if the reported analyses and results are in line with the provided data and syntax. Not only will this encourage authors to manage their data more carefully in order for a third party to understand it, statistical errors that were overlooked at first have a higher probability of being detected before publication.

Editors could decide to make data sharing mandatory, taking into account certain exceptions concerning privacy etc (see eg the policy of *PLoS One*). Another option is to simply reward authors who share data. For instance, the journal *Psychological Science* awards badges to papers that are accompanied by open data and also awards badges for open materials and preregistered studies. Although at first sight these badges might seem trivial, they can be considered a quality seal and have inspired many researchers to share their data.

Of course, researchers could still conceal deliberate rounding errors towards significance by manipulating the raw data before submitting them. However, falsifying research data like this is explicit scientific fraud. Data from self-reports show that scientific fraud is much more uncommon than questionable research practices such as wrongly rounding a  $p$ -value<sup>12</sup>, so it seems implausible that encouraging data sharing will result in researchers hiding rounding errors by manipulating the raw data. In any case, there will always remain ways to commit fraud in science, but encouraging data sharing will definitely make it harder.

Another way to avoid reporting errors and to facilitate analytic review, is for editors of journals that adhere to APA reporting style to make use of *statcheck*<sup>2</sup>. As described above, *statcheck* is a package for the statistical software R<sup>15</sup> that can automatically scan articles, extract statistical results reported in APA style, and recompute  $p$ -values. Editors could make it standard practice to use *statcheck* to scan papers upon submission to check for statistical reporting inconsistencies. This takes almost no time; on average, *statcheck* can scan approximately 250 papers per minute. Since many journals already have an automatic plagiarism check, it is a small step add a check for reporting inconsistencies. Results that are flagged as problematic can then be corrected before publication. R and *statcheck* are both open source and freely available. For more information about *statcheck* and an extensive analysis of its validity, see our paper<sup>1</sup>. For instructions on how to install *statcheck*, see <http://mbnuijten.com/statcheck>.

The excess of results wrongly presented as significant is probably caused by publication bias. A promising way for editors to try to avoid publication bias is to encourage preregistration. Preregistration can take many forms, but in general the idea is that researchers write a detailed research (and analysis) plan *before* collecting the data.

This research plan is then “registered” somewhere online (eg in a repository for clinical trials such as <https://www.clinicaltrialsregister.eu>), or even submitted to a journal. In the latter case, the research plan is peer reviewed, and if the plan meets the standards of the journal, the researchers can receive an “in principle acceptance”, no matter what the results will be – given that they will adhere to the research plan (see eg the guidelines for registered reports in the journals *Cortex*, *Comprehensive Results in Social Psychology*, and *Perspectives on Psychological Science*). This way, the decision to publish a paper cannot be influenced by whether the results were significant or not, avoiding the selective publishing of  $p$ -values wrongly rounded down as compared to the ones wrongly rounded up. On top of that, it takes away an incentive for researchers to deliberately report a non-significant  $p$ -value as significant.

Besides side-stepping publication bias and avoiding systematic reporting errors, preregistration also solves the problem of HARKing: Hypothesizing After the Results are Known<sup>16</sup>. When researchers are HARKing, they first explore the data to find interesting patterns, and then present these findings as having been predicted from the start. If a researcher performs a lot of exploratory tests, he or she is bound to find at least one significant result purely by chance. Reporting only the tests that were significant leads to an excess of false positive findings. However, if the research plan and hypotheses are registered beforehand, there is a clear distinction between confirmatory and exploratory tests in the paper, which allows for a more reliable interpretation of the results<sup>17</sup>.

To conclude, there is evidence of a high prevalence of statistical reporting inconsistencies in scientific literature. Even though many of these inconsistencies are minor errors that are probably due to mere sloppiness, there is also a high prevalence of gross inconsistencies that may have affected the statistical conclusion, mainly in favour of statistical significance. Even though we can only speculate why there are more results wrongly presented as significant (deliberately rounding down, publication bias, less rigorous checks of findings in line with expectations, etc) it remains a worrying finding, reflecting a systematic preference for “success” and leading to an excess of false positive findings in the literature.

There are several concrete steps that journal editors can take in order to avoid or reduce the number of reporting errors. For instance, editors could encourage data sharing and preregistration, or use the program *statcheck* to automatically check for inconsistencies during the review process. Besides decreasing the prevalence of reporting errors, these measures also reduce publication bias, HARKing, and other questionable research practices.

Statistical reporting errors are not the only problem we are currently facing in science but at least it seems like one that is relatively easy to solve. I believe journal editors can play an important role in achieving change in the system, in order to decrease statistical errors slowly but steadily and improve scientific practice.

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## Independent editors – how secure is your position?

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Prior to the start of the millennium, scientific papers were published largely as hard copy, usually by editors working for particular organisation and societies, which either paid to have them published by firms such as Academic Press and Wiley, or more usually the journals were sent out to subscribers (usually institutional libraries rather than individuals) who paid for each one or for a "basket" of those in a similar field. Most publishers did not see any particular profit in providing this service for the scientific community. But all this was soon to be quickly superseded by electronic publishing, with almost no journals continuing to produce only hardcopy, and increasingly fewer offering both hardcopy and online versions, as had been the case with *Cell Biology International* in the last few years, for which I was Editor-in-Chief for 14 years before recently demitting office.

Around 2000-2001 when electronic online publishing began in earnest, there was a rapidly growing demand for publication, and the "author-pays" model took off. Authors were charged for papers that were accepted for publication, and the papers were immediately made accessible to readers free of charge once online. Although the converse method still remains available, ie the *reader* or the institution pays to download the full paper (only the abstract being free), this has become less and less common. Crucially, the author-pays system means that publishers get paid up-front.

Taking an overview of the present situation, publication costs are miniscule compared with the cost of hardcopy journals published before the 21<sup>st</sup> century. Unlike authors of novels and other publications, who sell their work or receive royalties, scientists and other academics do not get paid for their contribution to the literature; on the contrary, they pay for the privilege. And the costs can be hefty, with publishers charging thousands of pounds or euros per accepted article. The scientific paper has become a cash-cow, which is why there has been a burgeoning of new journals. Many less reputable outfits have jumped on the band-wagon – the predatory journals. Beall's list shows how prolific these have become as the entries are coming close to a thousand; see <http://scholarlyoa.com/publishers/>.

Now I come to the crux of this article, which concerns my own experience as an independent editor. When these changes in electronic publishing began, some publishers created "core" journals edited by their own staff. However, they also encouraged the creation of new journals that would be independently edited. This meant that someone not employed by the publisher would work hard to establish a journal, raise an impact factor and get a healthy submission rate of good articles. Anyone who has attempted this exercise will know how much time and effort is required to succeed. In my own case, I created two new journals,