Don't be paranoid, even when you find out that people are out to get you

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Scientific research is difficult and often depressingly Sisyphean. The pleasure we get from the rare decisive result is often obliterated by the knowledge that we have to write up the paper to get any credit for it. We start writing and then, after many revisions, submit the paper. Is there anything more ego-degrading than having the paper rejected? Why can't they see that, unlike others (that is, probably, the reviewers), we are attacking difficult problems, while those who go after low-hanging fruit seem to have their papers sail through? Why do they focus on trivial methodological issues or irrelevant controls, leaving the big ideas we propose to languish without any comment? To add insult to injury, all we get is a form letter from the editor saying that the reviewers did not like the paper — as if the editor is a secretary who pushes papers from one reviewer to another rather than somebody who is supposed to think independently. Of course, had we belonged to the right club, none of this would have happened. If you haven't had these thoughts at one time or another, you must be living on a different planet. All of us (editors are also authors) have suffered through these feelings. Not being an expert on the psychology of paranoia, I naively thought that it was rare. But, upon a cursory look in the literature, I found that ideas of persecution (in the absence of psychiatric illness) are very frequent; one study of college students showed that almost half of them have these symptoms.[1]

Aware of such thoughts, we, the editors, encourage authors to suggest reviewers to use in the peer review of their papers in the hope that they will choose sympathetic referees. An editor who looks at this 'preferred' list of reviewers quickly checks PubMed to see whether they are former coauthors and, more importantly, whether they actually know something about the field; after all, paranoia is not restricted to authors—we have it too! Like most other journals, *Kidney International* allows authors to exclude reviewers. The problem, however, is that too often those excluded are the most appropriate reviewers. There have been instances when one author excluded so many reviewers that the whole subfield was excluded. Regardless, we often accommodate these wishes; however, I hope it is understood that exclusion of reviewers is interpreted by us as a suggestion, not a mandate.

From a sociological point of view, paranoid ideation and ideas of persecution are often seen in those without power: immigrants, the marginalized, the weak, and those who feel that their fate is determined by others, and to malignant others at that.[2] It is a bit of exaggeration to say that authors belong in this category of powerless people; yet after submission of the manuscript there is no doubt that its fate has been handed over to others. Given the emphasis on publication in our academic reward system, it is not surprising that allowing others to determine the fate of something this important can generate feelings of persecution. Surprisingly, the demand to exclude 'hostile' reviewers is not as common as one would think.

I confess that I have often used excluded reviewers, and my impression was that their reviews were no harsher than those of preferred, presumably friendly reviewers. Over the past year or so, I conducted a mini-study whose results might be of interest. Those of you with an epidemiological bent should, however, refrain from calculating the power of my conclusions, since this analysis has all the hallmarks of papers we usually reject: it is retrospective, the controls are likely to be different from the experimental group, too few papers were examined, and so on. Further, given that reviewers were excluded, the editors were conscious of the fact that the authors felt they might be in conflict. Hence, we looked at these reviews differently. Despite all these caveats, I feel that this analysis resembles the best kind of case report in being anecdotal but telling in its content, and showing that, if there is an effect, it is too small to warrant a more comprehensive study.

Only about 8% of authors in the past year excluded reviewers. Does this suggest that we have become wiser than the average college student? The study by Ellett *et al.*[1] suggested that many college students who exhibited paranoid ideas had "poor self-esteem." This is probably why our paranoia is so much less frequent than theirs; nobody can accuse professors in medical schools (the majority of our authors) of having low self-esteem. I read the 24 papers of which we had received reviews from referees thought to be antagonistic. For each paper we had also asked at least one and sometimes more than one referee who was listed by the author as 'preferred.'

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Not original Overall, the data presented are not original, are mostly descriptive and do not provide new insight into the effect of x on yyy in this rat model.	Some data analysis also needs to be reworked and there is a significant methodological problem. Nevertheless, on balance I think that the paper should be published, but only after revision.
More studies are needed Additional control experiments are required and the authors need to more carefully point out the novel aspects of their study in view of the published data.	An interesting though somewhat limited observation with implications for understanding renal inflammation.
No mechanism presented While the experiments have been conducted carefully, the data do not advance the field significantly.	This paper is somewhat similar in scope to many papers which claimed x involvement in yyy. I think the mechanism linking x to yyy is still ill-defined. Clearly, more compelling data is required.
***In vivo* studies needed** The experiments are straightforward. However, the importance of this finding should be established *in vivo* by demonstrating that x expression in diabetic rats or mice is ameliorated by the administration of yyy and that this has some effect on the sclerosis observed in these animals.	The effect of x is quite interesting; however, the mechanism of these effects are not entirely clear. The authors must show how x regulates transcriptional control of yyy. Another major problem is how x protects renal injury in diabetic nephropathy *in vivo*. *In vivo* experiments are essential to get a definite conclusion.
Enemies stab you in the front First, the information is not novel. Second, the design of the study is faulty. Finally, the hypothesis suggested by the authors was not proved. This is another descriptive study of a well-known phenomenon of high mortality in x.	This is a well written straightforward study that should be of interest to clinicians; I did not find any major flaws or errors in data handling or analysis.
Friends stab you in the back This study is interesting and addresses a potentially important issue in renal pathophysiology. However, there are several issues that should be clarified to make the study more robust.	The paper, while original in addressing the role of x in yyy, lacks originality in providing any new information regarding x function in inflammatory disease. I am disappointed by the lack of attention paid by the authors regarding what the data mean.
Enemies cannot resist a gratuitous jab It is a logical extension of their previous work but again several important controls are not present. I like the fact that they admit that their previous paper had flaws as no one could reproduce their data.	The major finding of study is the protective effect of a new x analogue which induces protection from yyy. The delayed treatment data is very impressive such that it might be useful as a new treatment of yyy, yet the manuscript needs major revision.
Friends suspect hidden motives There are however several methodological and analytical flaws in the study (vide infra). Moreover, some findings remain unexplained. Altogether these limitations preclude publication.	The study is not adequately performed, because only samples from a previous trial were analyzed. Neither the choice of the study nor the used dialyzers are appropriate to prove an effect. A market-driven intention must be suspected.
Your enemies have read your papers The authors demonstrated previously that x was not present as such but as a conjugate. When learning this, we stopped publishing on x. The authors, however, continue to publish on it as if nothing had happened.	This is an excellent paper. With a little more explanation (as below) it should be a good contribution.

Of these 24 papers we had already accepted eight, and one was still awaiting the receipt of minor revisions before it was finally accepted. Hence, we accepted 38% (9/24) of the papers. We rejected eight on the basis of the recommendation of both reviewers. In addition, three were rejected because the preferred, but not the excluded, reviewers recommended that outcome. Another four were rejected because the excluded, but not the preferred, reviewers recommended so. These numbers are not very different from our general statistics on editorial decisions for the past year. We reject 60% of submissions without review. Of the reviewed papers (that is, the comparison group), we accept 45%, compared with the 38% of those with excluded reviewers.

Although this 'outcome analysis' demonstrates that there is no reason for paranoia, it is worthwhile to examine whether the tone of the reviews by the enemies differed from that of the reviews by the friends. Reviews can hurt, and even if the outcome is good, the wounds remain for a long time. Witness the embarrassing case of Rosalyn Yalow, who received the Nobel Prize for co-developing the radioimmunoassay: she displayed a rejection letter from the *Journal of Clinical Investigation* in her Nobel Lecture.[3] Reviewing papers, like science itself, is a human activity, and nobody is implying that referees are saints. But let us see what actually was written about the 24 papers under scrutiny. Although space does not permit showing the full comments of the reviewers, I have selected emblematic phrases. These were often sent to the editors and not meant to be shown to the authors; thus they probably reflect the true feelings of the reviewers more accurately than do the comments sent to the authors.

These reviewer comments are classified into several categories. In each category, the comments are verbatim, with minor changes to hide the subject matter; those by 'friendly' reviewers are green, and those by 'antagonistic' reviewers are red. I hope you will see that, more often than not, the responses say the same things, but often at different emotional temperatures.

Are these comments fair? That the acceptance rate of papers with selected reviewers is similar to that of papers with unselected reviewers suggests that the reviewers were always fair; or, that the Editorial Board made corrections for such reviewers if they were not. The most interesting observation to be made about these comments is that reviewing—being an aspect of human nature—is, alas, unpredictable.

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