Rejected letters

SIR

Gupta raises an interesting issue. The editor of the BMJ forwards rejected letters onto authors. Gupta articulates the idea that is foremost in every letter-writer’s mind: will the idea in the letter be filched without giving the originator credit? The author to whom the letter is forwarded may say that he or she had thought of it already. Indeed Jung, with his ideas of group consciousness, has supplied the theoretical basis for such a defence. If the letters are forwarded to a limited private readership without prior printing, then disputes over precedents can only increase, as no one will be able to decide who was first past the post. Furthermore, a paper supporting a new idea is more likely to get printed than one whose new data merely confirms a previous publication.

The BMJ answers Gupta’s criticism by taking up his suggestion of circulating a questionnaire and collating the data. I was a recipient of the questionnaire. I was struck by its structure, which seemed solely to garner support for the already extant practice of circulation of rejected letters. It seemed that the questionnaire had no other or great target, such as improving the standard of letters. To this limited end, the BMJ had polled a list of rejected authors—a resource which was not available to the complainant. I can imagine they would not send on to him a list of addresses (although Gupta was an interested third party and they could have done so under their own analysis). However, he could have been invited to submit his own questionnaire in the envelope or contribute to theirs. Carried out as it was, the effect was that the jury had been addressed by one party to the dispute and so the balance of justice was tilted. This biased use of resources reminded me of the annual meeting of a large national company where the company resources are ranged against the insistent questioning of the small investor.

I would have asked the panel whether they objected to sending on manuscripts of their rejected articles to those workers interested in the field—in the interests of scientific debate, of course. If this question had appeared above one concerning the letters being sent on to interested parties, perhaps many more than the actual proportion of 30% might have disagreed, illustrating an old maxim that one should only believe statistics if one has had a chance to manipulate them oneself.

Craft and Smith say that one third disapproved of the practice and that a third of the respondents were worried that their ideas might be stolen. I must say that after this sizeable no vote, I am surprised that they even remotely consider it as support for continuing the practice. I would have said it was a damning indictment of a practice that they might have realised they should never have started.

The pilfering of ideas which have been passed round a limited circulation does occur. Nature aired it in 1989 under the title “Plagiarism charge casts shadow over peer review” which accurately sums up the facts of that case.

Having established a problem exists, one can try to assess if it occurs at a significant rate. Lock has written extensively on this and related subjects. His view is that it is more widespread than the few instances that are in the public domain. Ironically, he expressed the view in the very pages of the BMJ.

Craft points out that the text itself is copyright and therefore cannot be re-cycled. Certainly any patient data cannot be plagiarised and so this only leaves sending on the letter so that the ideas themselves can be acted on by a further recipient. This offering of another person’s ideas occurs without his permission. The original letter-writer could do this himself, if that is what he wanted. I was always surprised that the BMJ were unable to afford the cost of telling an author his letter was rejected (that is, his idea was not that good) and yet was all the time able to afford the cost of sending it on unknown to a third party, for further use if wished.

Now, I write to the editor of a journal with the expectation that my letter will be published if he thinks it good enough. I do not see that there is implicit acceptance that there is a second category of not good enough for the general reading public which is, however, good enough for a few of the editor’s appointees. The editor of the journal does not circulate a list of planned editorials so that other editors can avoid duplication of effort and resources, even though this may be of benefit for the general public, nor does Dr Craft publicise her scoops before she writes them. This illustrates, to my mind, how dead the concept of empathy has become: an editor is free with other people’s ideas but is more careful with his own and sees no inconsistency. A poll was necessary to find out how letter-writers felt because the BMJ was quite unable to perceive it empathically.

Do editors learn by experience? One final disturbing point is that a related issue was raised in 1993 in the BMJ when an author complained that a paper had been turned down and then an editorial on the same subject made the same points. The editor, in a compliant answer, stated that they could not guarantee that it would not happen again. So, three years later, the issue recurs—I feel solely because the issue was not adequately addressed by a monopoly supplier in the first place. I note that it was raised privately with the editor at the time but that there was no satisfactory resolution, which perhaps public scrutiny and accountability might have accomplished. I doubt this debate would...
have been reopened if the JME had not intervened. I congratulate the JME for taking the bull by the horns—and I congratulate the BMJ for changing its practice and ceasing to send rejected letters on to other authors.

References

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Thirst and hydration in palliative care

SIR

I write to correct an error in my recent paper in your journal, and to clarify and expand a point relating to the physiology of thirst.

Author's error: Introduction, line 11, page 147: for six papers, read four papers.

Physiology of thirst: The physiological control of thirst is extremely complex, and my knowledge of it rather rusty. In touching on the subject I have made a statement that is misleading. On page 148 of my paper in the section on "The question of thirst" I wrote: "Does severe dehydration suppress thirst in cancer patients as it does in the healthy elderly?" In fact it is probably not dehydration that suppresses thirst in the elderly, but suppression of thirst that predisposes to dehydration. Phillips et al showed reduced thirst during fluid deprivation in seven healthy elderly men, compared with seven healthy young men. The reason for this was not clear but the authors postulated diminished baroreceptor and volume receptor mediated thirst since levels of the peptide hormone vasopressin, which is linked with osmoreceptors, were not reduced. However, certain odd features in the study suggested that cognitive factors were involved, since thirst levels that were suppressed during fluid deprivation, rose during a subsequent "sham" intravenous infusion. Therefore the knowledge that one cannot have access to fluids, may lead to thirst suppression. The important point however is that the combination of dehydration and thirst suppression, whatever the mechanism, is potentially lethal, and could indeed lead to "an escalating spiral of decline".

In the context of a possible reduction in thirst perception in the dying it is of interest that loss of osmotic thirst has been reported in patients with multiple system atrophy. It is also of interest that opiates play a part in the control of vasopressin secretion, as may prostaglandins. Whether this alters thirst perception I do not know, but clearly morphine and other pain-killers used in palliative care could influence fluid-balance control in unpredictable ways.

References

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