

## Correspondence

EDITED BY LOUISE HOWARD

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### Authorship of clinical trial reports

I was most interested to read the report by Baldwin *et al* (1999), particularly as I am listed as a co-author, although I had never seen the text before and did not know it was being submitted for publication! How did such a situation arise and how can its repetition be avoided in future?

In many countries, particularly the USA, it is editorial policy to obtain consent to submission that includes *all* of those who participated directly in the work described; often to the extent of obtaining individual signatures. This is to be applauded. Working at the Feighner Research Institute in London, I personally treated 27 patients who were included in the above trial and informed the company concerned, on more than one occasion, that I wished to see the text of any manuscript submitted for publication prior to the event. But I received no reply.

Individual investigators should be named with their centres, rather than lumped together in an unspecified ‘study group’. It would also be helpful, in the context of any statistical conclusions reached, if the number of cases contributed from each individual centre were to be recorded in published reports.

**Baldwin, D., Bobes, J., Stein, D. J., et al (1999)**  
Paroxetine in social phobia/social anxiety disorder. Randomised, double-blind, placebo-controlled study. *British Journal of Psychiatry*, **175**, 120–126.

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**Authors' reply:** We are grateful to Dr Wheatley for his comments on the publication of the treatment study with paroxetine, and would like to acknowledge publicly the contribution made by Dr Wheatley and his colleagues within the Feighner Research

Institute. We thank all those principal investigators, their research teams and the patients who kindly took part in this investigation.

It is difficult to acknowledge all the clinicians who participate in large multi-centre treatment studies, and naturally some investigators will be disappointed when their contribution is not credited as much as that of their colleagues. Ideally, publication plans should be discussed at meetings with potential investigators prior to the start of a study, and then reviewed during the course of the investigation. This is now the policy at SmithKline Beecham, but this study with paroxetine was started before the policy was in place. Due to a change in personnel within SmithKline Beecham, the communication between the sponsoring company and all the participating investigators has been less than optimal, for which we apologise.

In the future, SmithKline Beecham will try to ensure that all the participating investigators are happy with the publication plan before the start of any treatment study. However, the question of contribution to full authorship will remain problematical, for collaborative study groups and journals alike.

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**Editor's response:** The *Journal* does enforce a policy of obtaining the signatures of all authors before accepting a manuscript for publication. In this instance, confusion has arisen because of uncertainty over the authorship status of members of a study group. This issue has now been clarified in our ‘Instructions to authors’ as follows:

The *Journal* does not consider to be authors people thanked in the Acknowledgements or listed as members of a study group on whose behalf a paper is submitted, but whose names do not appear as authors on the title page of the manuscript, or whose signed agreement to the manuscript's submission has not been obtained. It is the responsibility of the corresponding author to ensure that authorship is agreed among the study's workers, contributors of additional data and other interested parties, before submission of the manuscript.

Full text of these Instructions is located on the Royal College of Psychiatrists' web-site ([http://www.rcpsych.ac.uk/pub/bjp\\_ita.htm](http://www.rcpsych.ac.uk/pub/bjp_ita.htm)) and is next scheduled for paper publication in the July issue of the *Journal*.

### Psychological model of post-stroke major depression

Gainotti *et al* (1999) concluded that post-stroke major depression may fit with a psychological model rather than with a neurological model based on their findings that post-stroke patients with major depression displayed more reactive symptoms (emotionalism, catastrophic reaction, anxiety) and fewer endogenous symptoms than patients with endogenous depression.

There are some concerns regarding the conception and the methodology of the study. The major drawback in the methodology is the bias in selecting the control group. It is not surprising that patients with endogenous depression will have more endogenous symptoms than patients with post-stroke major depression, as major depression can be diagnosed using DSM–III–R (American Psychiatric Association, 1987) operational criteria in the absence of endogenous or melancholic symptoms. Further, it is also expected that affective symptoms related to brain damage, such as emotionalism and catastrophic reaction, will be more prevalent in post-stroke major depression than in patients with endogenous depression.

Gainotti *et al* seek to create an impression that there is no association between endogenous depression and psychological stressors, and that post-stroke major depression with symptoms such as anxiety and hyperemotionalism are the representation of a psychological reaction to stressful situations. The available literature fails to support the validity of dichotomous endogenous/reactive and endogenous/non-endogenous classifications (Farmer & McGuffin, 1989). Further, emotionalism observed in post-stroke patients is often