

## Response to the Comments on “Effect of a Video-Assisted Teaching Program About ECT on Knowledge and Attitude of Caregivers of Patients With Major Mental Illness”

We thank the authors of the above comments for their interest in and time spent reading our published article<sup>1</sup> and raising certain issues to which we would like to respond. One of the issues raised was regarding the choice of the article for sample-size calculation. Though we accept that it is ideal to choose a study employing the same methodology, when there is no precisely comparable study in the literature, basing the calculation on studies that are comparable in terms of methodology and outcomes is an accepted practice. In the study conducted by Kheiri et al.,<sup>2</sup> the effect of focused lectures and booklet-based education on knowledge and attitude towards electroconvulsive therapy (ECT) was assessed among Iranian nurses and patient relatives. Both the studies involved a similar study population (relatives of a patient with major mental illness), adopted a similar methodology (targeted educational intervention), and assessed similar outcomes (knowledge and attitude towards ECT). We powered the sample to detect a minimum difference of 2.5 units

before and after the intervention. This value reflected the combined judgment of the investigators, and it referred to the change in total scores.<sup>1</sup>

We do agree that there is a probability of selection bias in our study. Nevertheless, the baseline knowledge and attitude scores were still on the lower side for the included subjects; this was notwithstanding the routine pre-ECT counselling provided to the patient relatives before obtaining consent. This counselling involves a simple explanation about how brief seizures will be intentionally triggered following application of small electrical currents to the brain while under anesthesia and preprocedural instructions to the patient’s caregivers about keeping the patient nil per oral from the previous night. However, this is not a structured explanation and does not involve the use of audio-visual aids. This study’s findings have helped us identify the lacunae in our clinical practice, and the video-assisted intervention used in this study is now increasingly used in our clinical setup. The baseline level of knowledge of the caregivers about ECT, to which the pre-ECT discussion would have contributed, is presented in **Table 1**<sup>2</sup>; most items showed a significant difference after the intervention. The majority of the sample was employed (82.5%) and educated at or above the higher secondary school level (52.5%); 30% of the sample had received only primary schooling, while 17.5% were educated till high school. The study

results may, therefore, be generalized to such a population only. We did not formally assess the socioeconomic status of the participants.

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## Comments on “Barriers to Impact Factor Growth in Two Major Psychiatry Journals in India”

“Barriers to Impact Factor Growth in Two Major Psychiatry Journals in India” is

a very well-articulated and meticulously thought out article. But it gets the readers thinking whether they are doing justice to their hard work by sending their research to be published in *Indian Journal of Psychiatry*, *Indian Journal of Psychological Medicine*, or the likes of it with a fairly low impact factor (IF). This got us on a fact-finding quest about how much does the IF really matter?

A recent publication has noted that the correlation between the IF of an open access journal and the citations its articles get in the future is poor and insignificant.<sup>2</sup> Most Indian psychiatric researches are unfunded barring a few from big institutes. Hence, the concept of publishing in a journal with a high IF is not a major priority to the average

Indian researcher, considering the long wait times and high rejection rates in these high IF journals.

Our intention is to raise the question of a journal's impact. Should journals' impact, and thereby, prestige, be based on whether their articles are read a lot or cited the most?

The IF of a journal is calculated based on how much its articles have been cited, which makes one accept that the researchers have read and been inspired by the article. But what about the rest of the profession that actually renders clinical service? While calculating IF, shouldn't we also take into account whether the articles of the journal have reached them? Shouldn't they be the arbitrators of value, because they are simply in larger numbers and provide service for the patients, thereby impacting more people? Shouldn't whatever source that influences them have the highest prestige?

Citations do not necessarily translate to impact and readership. The best example is the Clinical Practice Guidelines routinely published by the Indian Psychiatric Society in the *Indian Journal of Psychiatry*. They are widely read and help in clinical decision-making across the country, especially among postgraduates, but have one of the least citations.

We want to point out alternatives like Altmetric, which is updated in real-time and gives a better insight into the readership and access of articles. Altmetric works by quantifying the attention an article receives digitally. Then their algorithm produces a weighted score to reflect the relative reach of each source in the form of a bright multicolored donut with a number in the center (which you may have noticed while browsing through this journal's "Most Read" section). This is a good measure for analyzing what articles have captured the interest of the general public and stimulated discussion.<sup>3</sup> The best example of that would be an article on the use of Ashwagandha in stress, by Chandrasekhar et al., from this very journal has only 56

citations (web of science metrics) as of date but an Altmetric score of 924, which makes it among the top 5% of all research outputs scored by Altmetric.<sup>4</sup>

Moreover, in our opinion, in this publish or perish era, even clinicians attached to a teaching hospital who are not as much enthusiastic about research have to come up with new and interesting topics to increase their chances of publication, to remain relevant or get promoted; thus, to expect continuity in research under such circumstances is unrealistic.

The authors have drawn a general conclusion that Indian researchers do not value the importance of local research, by comparing the citations from only two journals. We believe that a simple yet more robust way to assess that would be to compare national and international literature citations in an article. On a closer look at the Sarma et al.<sup>5</sup> article on burnout in Indian psychiatrists mentioned by the authors, out of the 23 references in the study, only 14 (61%) talk about burnout in physicians, of which 2 (7%) are studies from the Indian context. One of the probable reasons for not mentioning the study by Bhutani et al. could be that it uses a different questionnaire as compared to their study (they did cite two local studies using the same questionnaire).<sup>6</sup> The point being, various factors influence the selection of a paper and its citation-worthiness, which is a purely subjective phenomenon.

We agree with the authors that citing local research is a good practice and should be encouraged. The IF is merely a journal's citation popularity among authors and does not reflect the journal's quality. Our energies should be spent on furthering the quality and consumption of research in the country.

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