Letters to Editor

Comparison of efficacy of ketamine versus thiopentone-assisted modified electroconvulsive therapy in major depression

Sir,

I thank Dr. Tak and Dr. Salvi for critically reviewing my publication. I would like to offer my comments on the methodological observations pointed out by them.

The study used Diagnostic and Statistical Manual of Mental Disorders, 4th Edition, Text Revision (DSM-IV TR) diagnostic criteria for major depressive disorder because the study was done between the year 2010 and 2013 when DSM-5 was not available.

Semi-structured clinical interview was used to make the diagnosis rather than a structured clinical interview like SCID-IV. While SCID-IV would increase the reliability and validity of the diagnosis, it is more useful in large multicenter trials and population-based epidemiological studies where the diagnosis is made by different researchers and the reliability and validity may be questionable. We, by design, preferred to avoid it as it requires training and needs to be purchased which would have added to the cost of the study which was a nonfunded one. In our study, the diagnosis was made by the researcher and cross-checked by a trained psychiatrist with >15 years of clinical experience in using ICD-10, DSM-III-R, and DSM-IV. The cases with doubt about diagnosis were not considered for the study. Hence, the diagnosis was quite reliable and valid.

We tried to keep the sample as homogenous as possible by including only 18–45-year-old adults in the sample. Middle-aged and elderly people were excluded as at this age many incipient organic factors also come into play. Our study population is not financially sufficient to undergo a radiological investigation to rule out such factors. Hence, we preferred to play safe and included an age band where organic factors are least likely besides ensuring uniformity. There is no evidence to doubt homogeneity of sample based on the inclusion of adults from 18 to 45 years of age to study major depression.

We appreciate the reader’s observation about the striking similarities in the similar age and domiciliary distribution of patients. We too were surprised by this finding. Even after auditing the method of study and cross-checking data, we concluded that this finding is because of chance factor only instead of any experimental error; hence, we decided to publish it highlighting the same.

We acknowledge that we did not estimate the sample size and chose a sample size of 60 patients as per our convenience and could not overcome the lacunae of the other small sampled studies. This was again due to the time constraints as this research was an essential part of the training curriculum of the first author which was to be submitted within prescribed time limits. To overcome this limitation, we used only those inferential statistics which are robust for small sample sizes. At the time, this study was conducted (year 2010–2013), there were no similar studies that had a larger sample size. Even to this date, to the best of our knowledge, there have only been a couple of studies on ketamine as electroconvulsive therapy (ECT) anesthetic in major depression that have used bigger sample sizes.

Finally, we chose 1 mg/kg dose for IV ketamine because we wanted to use ketamine as a sole anesthetic and this is an appropriate anesthetic dose. While studies with intravenous ketamine infusions have used subanesthetic doses of 0.5 mg/kg, we were not using ketamine as an adjunct to another anesthetic during the ECT procedure. There have been no guidelines or differential-dose research studies to guide what would be the most appropriate dose of ketamine for modified ECT for major depression.

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Conflicts of interest
There are no conflicts of interest.

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