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Authors' reply: First, Drs Huthwaite & Stanley point out that a major concern is the likelihood of confounding in this scenario. In our previous research,[1] we examined government statistics on suicide in the 47 prefectures in Japan. The overall yearly suicide rate in Japan was 25 per 100 000 population in 1999. Pearson's correlation was used to calculate correlations of suicide rate with latitude, longitude, yearly mean temperature, yearly total sunshine, yearly mean individual income, and yearly unemployment rate in the 47 prefectures, although lithium levels were not measured in the study. There was a significant correlation with suicide rate for yearly total sunshine, yearly mean temperature, latitude, and yearly mean individual income. By using multiple regression analysis, yearly total sunshine was the only individual variable to predict significant variance in suicide rate. Taking these findings into consideration, we did not use yearly mean individual income or yearly unemployment rate.[2] Also, yearly total sunshine was similar between the 18 municipalities of Oita prefecture so we did not use this. Most importantly, only 18 municipalities prevented us from conducting further analyses including confounding factors. We are now planning to perform a large study to consider confounding factors.

Second, they state that the potential reasons behind the difference in lithium levels in the drinking water samples in the different municipalities are also not explained and ask how valid it is then to use the mean value to represent the lithium exposure in that area. Lithium levels of drinking water supplies were measured at 26 locations in Oita city and at 53 locations in the other municipalities. The reason for the large difference in lithium levels is unknown, but Oita prefecture may have different geological features between the 18 municipalities and such differences may bring about large differences in lithium levels, although this thought is speculative. Also, instead of the mean value, we used the median value for the analysis and similar results were obtained.

Third, Huthwaite & Stanley question the duration of exposure to a specific level of lithium in the drinking water, and where people source most of their drinking water and the use of bottled water. In Japan, most people drink tap water although a small portion of people drink bottled water. Therefore, it is meaningful to measure lithium levels in tap water supplies. Moreover, the duration of exposure to a specific level of lithium is unknown, but if the residents continue to live at the same place, then their age may be associated with the duration.

Finally, we agree that in the context of the short report it is difficult to fully assess the suitability of the analysis methods used. Nonetheless, we emphasise that although short reports are not in themselves conclusive, they can provide new findings which lead to comprehensive research to establish a definite conclusion. We would like readers to read short reports with this in mind, so that they are not misled.

Although Schrauzer & Shrestha emphasise that their data were partitioned in accordance with accepted methods of statistical trend analysis, in their report[3] they said only that the 27 Texas counties were classified into high, medium, and low groups according to the lithium content in the municipal water supplies. There was no explanation of how to divide the high (range 70–160 μg/l), medium (13–60 μg/l) and low (0–12 μg/l) groups. To avoid the suspicion of an arbitrary division, they should have fully described their method in their full paper. In addition, their results were adjusted only by population density and annual income.

Dawson *et al*[4] also investigated suicide rates and lithium in drinking water, classifying lithium levels as high ($\geqslant$70 μg/l) or low ($\leqslant$11 μg/l). This division might have derived from their previous study, in which they reported that the lithium levels were clustered into four groups ($\leqslant$11, 11–29.9, 30.0–69.9 and $\geqslant$70 μg/l), which would provide about equal distribution of the measured values at consistent increments.[5] Their results were adjusted by population density, the distance to the nearest state hospitals and rainfall.[4]

Taking the nature of these partitions of lithium levels[3–5] into consideration, our method of investigating the association between suicide rates and lithium in drinking water[2] is more valid. We used lithium levels as a continuous variable and applied weighted least squares regression analysis adjusted for the size of each population. In any case, as Huthwaite & Stanley pointed out, confounding factors were not sufficiently investigated by Schrauzer & Shrestha[3], Dawson *et al*[4] or us.[2] Therefore, beneficial effects of low levels of lithium on human behaviour has not been confirmed and further studies are clearly required.

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