

lithium level in drinking water and district suicide mortality. The authors were careful to perform sensitivity analyses to examine the impact of extreme values on the outcome, and log-transformed many independent variables, as WLS is known to be sensitive both to extreme values and to distribution of variables. However, one most important aspect about the WLS analysis which seems not to be articulated in the paper is that in the model estimate of WLS analysis much depends on the choice of weighting variable. A different weighting would produce different estimates, in particular standard error of estimates. It is not clear what weighting variable the authors used in their analysis. Was it population size of district or variance of suicide mortality or something else? Was sensitivity analysis carried out on different weighting variables? Would the significant finding still be present if different weighting variables were used? What would be a better weight for this data-set? There seems a black box of uncertainty in interpreting the results.

Third, it is well known that ecological analysis is subject to the ecological fallacy, namely, association from the ecological model at area level may overestimate the population association that would be established by individual-level analysis.² Although not every ecological analysis necessarily presents such drawbacks, this study has not shown justification for not having such a problem. A negative correlation between suicide standardised mortality rate (SMR) and some area poverty measures such as unemployment rate and population density were not supported by individual-level analysis.³

Finally, since both district data on lithium concentrations and suicide mortality are available for up to 5 years for the period 2005–2009, the study could have obtained findings with more statistical power than the current findings if multilevel Poisson models for repeated measures within region were used for analysing SMR data.⁴ To organise data as years ($i = 1-5$) nested within district ($j = 99$), such a model will have many more data-points (maximum 495) so that important variables such as age and gender in some type of aggregated form, such as percentage of female and percentage of old people per district, could be included in the analysis without overfitting the model. In addition, the increasing trend of suicide mortality over time and variability of the SMR between districts and over time can be disentangled in the model. Although this model still cannot provide evidence on causal relationships based on aggregated data, it can overcome some limitations in the method used in the study. The core finding of this study as currently presented cannot be supported unless further analyses by means of more advanced multilevel models also yield the same finding.

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methods thus allowing for comparison with our results. Using weighted least squares (WLS) regression in ecological studies is a recognised method.^{2,3} By incorporating previous criticism, we extended the WLS model of Oghami *et al* by implementing further covariates and tested for stability of the hypothesis. As stated originally, weighting by population per district (number of inhabitants per district), was chosen.

In order to clarify the uncertainty raised by Yang, we recalculated the lithium estimates ($R^2 = 0.38$; $\beta = -0.24$; $t = -2.33$; $P = 0.022$) from the multivariate WLS model from Table 2: (a) without log-transformation of variables and (b) with additional weighting variables. Using non-transformed covariates, the estimates for lithium levels in the multivariate model were: $R^2 = 0.35$; $\beta = -0.25$; $t = -2.71$; $P = 0.008$. Weighting for the variance of suicide mortality produced a similar result for lithium levels ($R^2 = 0.41$; $\beta = -0.35$; $t = -3.40$; $P = 0.001$) and weighting for the variance of lithium levels even improved the estimates ($R^2 = 0.76$; $\beta = -0.55$; $t = -7.17$; $P = 2.9 \times 10^{-10}$), which further supports our hypothesis.

Concerning the issue of ecological fallacy, we rephrase a part of our discussion: it is clear that our study design cannot prove cause and the results are not applicable to individual cases. Our statement that we provide conclusive evidence, that lithium concentrations in drinking water are inversely correlated with suicide rates, is far away from any ecological fallacy. It would have been unacceptable to state that drinking lithium-containing water will reduce an individual's risk for suicide. Such suggestions could only be justified after double-blind placebo-controlled randomised trials with evidence level 1 (Grade A recommendation) according to the Oxford Centre for Evidence-based Medicine (www.cebm.net). Such trials would be desirable after the presentation of our replicated ecological evidence which can be classified as level 2c evidence and thus only justify a Grade B recommendation.

A recalculation of the model by means of a multilevel Poisson model with repeated measures would indeed be interesting and would further challenge the hypothesis. As previously applied by us,⁴ a hierarchical Bayesian model incorporating the neighbourhood structure to estimate the effects of variables on suicide mortality would be even more appropriate and will be applied in the context of a future study, which will take additional variables into account.

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Suicide as protest against social suffering in the Arab world

Since ancient times there has been a difference between suicide (an act of self-destruction) and self-immolation which, although self-destructive, has a sacrificial connotation. Self-immolation is associated with terrible physical pain (burning alive) and with the idea of courage. In modern times it has been used, among

Authors' reply: In attempting to replicate the findings of Oghami and colleagues,¹ it was our aim to stay close to their