

that, in 2006, produced a most unsatisfactory document strongly arguing against any legislation in this area.<sup>3</sup> I very much hope that the College will withdraw this statement and take the only position that is appropriate in circumstances when, as is the case here, opinion is sharply divided, namely one of neutrality.

#### Declaration of interest

P.G. is a member of Healthcare Professionals for Assisted Dying, [www.hpad.org.uk](http://www.hpad.org.uk)

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**Authors' reply:** With opinion sharply divided we wonder whether it is possible to address the issue of assisted suicide without a charge of bias, and for this reason we thought it was better to be explicit about our position in relation to a change in the law. No editorial limited to 1500 words will satisfy our correspondents' criticisms that other evidence was overlooked. Matthew Hotopf was indeed involved with the Royal College of Psychiatrist's response to Lord Joffe's Bill.

We agree with Professor Graham that terminology is important. However, we do not think the distinction between assisted suicide and assisted dying is clear. The use of 'assisted dying' is problematic, we suggest, as it may be confused with the work of palliative care, a system of care specifically designed to assist people at the end of their lives, to do all to maintain dignity in dying and not to 'strive officiously to keep alive'.

Any change in the law is likely to involve drawing a distinction where assisting suicide is lawful as opposed to one where it is not. If one sets aside the legitimate moral question as to whether a doctor should ever assist in suicide, the issue comes down to devising a set of safeguards. Most, we think, would agree that freedom from coercion is important, although there may be debate about how to define this. Psychiatry may have only a limited role to play in such an assessment. The other main safeguards which tend to be proposed relate to the presence of suffering, mental capacity and consistency of wishes, and are areas we think psychiatrists bring expertise and might be expected to be agents in a new legislation.

The proponents of a change in the law might argue that a specific group can be defined in whom a law could safely be applied, whose request is valid and whose suffering is authentic. In terms of suffering, in an era where the voices and views of patients with psychiatric disorder are, thankfully, increasingly given due weight, we do not think it is tenable to suggest that patients with psychiatric disorder can so readily be distinguished from the rest of the population. If one makes 'unbearable suffering' a condition of assistance, but does not think that people with chronic mental disorders should have access to such assistance, then we suggest one has to answer the 'why not?' question. The suffering of patients with chronic mental disorders may be quite as unbearable or more so than that of a patient with cancer. As Dr Curtice's letter points out,<sup>1</sup> this is a live issue.

The issues of mental capacity in relation to suicidal behaviour are complex, as the case of Kerry Woolterton (a woman with an

emotionally unstable personality disorder whose death by suicide was not prevented on the basis that she had mental capacity) indicates.<sup>2</sup> The complexity is added to by the high frequency of depression<sup>3</sup> and cognitive impairments<sup>4</sup> in patients with advanced disease. We suspect that mental capacity assessments in this context are unlikely to be value neutral.

Our clinical experience of working with patients with advanced disease suggests to us that there is considerable commonality between the patients we see in emergency departments who have harmed themselves and patients receiving palliative care who have persistent suicidal ideas. No matter how apparently understandable their desires, in our experience there is nearly always a high degree of ambivalence, and we have seen many patients whose strong suicidal ideas have reversed with support provided by palliative care services.

In Oregon, the Death with Dignity Act became law before psychiatrists had had an opportunity to fully consider the implications of their role in the process. In a time when assisted suicide is being discussed in depth but not practised we have this opportunity. We hoped our article would encourage psychiatrists to grapple with the complexity of the arguments and consider how they might respond personally and professionally to the patient who asks for assistance to end their life.

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### Lithium concentrations in drinking water

Kapusta *et al* claim that they provide conclusive evidence that lithium concentrations in drinking water are inversely correlated with suicide rates. This claim is apparently based on the estimate of a negative association between the average level of lithium in drinking water and average district suicide mortality at a marginally significant level ( $P=0.022$ ) of an ecological study, males and females combined, in 99 Austrian districts. However, this claim can be challenged as there are limitations of the ecological model used to analyse the study.

First, it is well known that suicide mortality is associated with social demographic factors such as gender, age, area poverty and economic issues.<sup>1</sup> Such factors are largely variable across regions and hence constitute major heterogeneity in health outcomes such as suicide rate. Failing to take into account those risk factors will most likely lead to biased results. The authors were aware of this deficiency, but could not properly compensate for it for two reasons: (a) an ecological regression model with only 99 data-points can only include a few covariates; and (b) their model was incapable of incorporating variables at levels lower than district.

Second, weighted least square (WLS) regression analysis was used in the study to examine the possible association between

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.<sup>2</sup> 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.<sup>3</sup>

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.<sup>4</sup> 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.<sup>2,3</sup> 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](http://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,<sup>4</sup> 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,<sup>1</sup> it was our aim to stay close to their