Bramness et al. (1) published a very interesting report on the seasonality of suicide over the course of a 39-year period and concluded that there has been an overall decline in the “magnitude of seasonality” after accounting for the well-known springtime peak in temperate climates. The report has an imbalance between the methodological justifications for choosing generalized additive models over other statistical strategies and the actual study results, leaving too little numeric information for the readers to scrutinize. The estimated parameters for the seasonal and secular components have not been presented; there are only goodness-of-fit statistics and incomplete graphical information. What was the amplitude of the seasonal variation in the total period? Is that what the authors refer to as the “magnitude of seasonality,” or are they alluding to the statistical significance of the seasonal component? How much has it changed?

When we examine the results graphically, another potential problem in the definition of the models emerges: It seems that the amplitude of seasonal variations is proportional to the mean suicide counts. One cannot tell if this is the case because that has not been formally tested. If it is true, a multiplicative model would have been more appropriate (2). Because the magnitude of the seasonality declined after 1990, the alleged reduced seasonality could have been explained by this multiplicative factor.

Finally, when using absolute monthly counts as the response variable, the number of days in the month is an important potential source of confounding that Bramness et al. do not seem to have considered. Having 2 fewer days in a month (February) would theoretically result in suicide counts that were 7% lower. In fact, when visually inspecting the presented graphs (because no other source of information was available), one can see that February had the lower mean suicide counts, with an amplitude that was approximately 7%. It remains unclear whether the seasonal variation is a result of the so-called calendar effect (2). It would be very informative if the authors could respond to these questions and provide supplementary material as a resource for readers to fully appreciate the study results.

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REFERENCES


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THE AUTHORS REPLY

In his letter to the editor (1), Volpe questioned the statistical method of our recent paper (2), in which we showed a significant reduction in the seasonality of number of suicides over the past several decades. The author cited a website from Statistics New Zealand (3) that explained traditional seasonality time series models in layman’s terms. What this summary does not tell (but what any standard textbook on time series would note) is that such time series models usually assume normally distributed measurement data. However, monthly suicide counts are neither normally distributed nor measurement data. They are counts, and counts have their own statistical properties and corresponding models. This is naturally handled by the generalized additive model framework used in our study.

The normal distribution is widely applicable because of the central limit theorem. The normality assumption, however, does not always hold, and other modeling approaches must then be taken. For skewed data, a log-transformation is often suggested. For regression models with additive effects, back-transformation of models with log-transformed outcomes leads to multiplicative effects on the original scale. The author’s suggestion that our results might be due to using an additive rather than a multiplicative model is thus