understandable. After a visual inspection, the author further suggested that the variation in the data might be proportional to the mean and that this should have been tested. However, there is no need for either visual inspection or formal testing of whether the amplitude is proportional to the mean. It is. This is a fundamental property of the Poisson distribution (4), the key statistical distribution for count data. The above issues are naturally included in Poisson regression and are explicitly stated in all of the equations in our article (2). Details are outlined in the generalized additive model book that we cited (5).

Because the variance increases with an increasing mean, the question of what the overall amplitude of the seasonal variation in the total period is consequently cannot be answered with a fixed number. As the mean suicide counts changes throughout the period (Figure 1 in our original paper (2)), so does the amplitude. Presenting the amplitude for each year is not an alternative, because dividing the observation period into yearly units introduces bias of unknown direction and magnitude, as pointed out in the discussion in our paper. Because there is no way to present a continuously changing number as a fixed number, we chose to present the amplitude graphically for both the overall seasonal pattern and the pattern for 4 select years (Figure 2A and 2B, respectively, in our original paper (2)).

Volpe was also dissatisfied with the fact that the estimated parameters for the seasonal and secular components were not presented. In our model, the seasonal component is made up of 24 unimodal sine and cosine functions, each with their own frequency and amplitude. However, although neither of the individual trigonometric functions has a meaningful interpretation here, their sum does (6). We presented this sum in several figures (2).

Interestingly, Volpe shrugs off our presentation as being only goodness-of-fit statistics and incomplete graphical information. Akaike’s information criterion (7) is, however, not merely a goodness-of-fit statistic; it is a measure of the relative quality of statistical models. In our data, a model in which we assumed no change in the seasonal component performed so poorly compared with models in which we assumed a change that it had essentially no support in the data. Previous attempts to assess whether the seasonality in monthly suicide rates has decreased have been inconclusive because of lack of formal statistical testing. It is a paradox that the author is dissatisfied with us for actually designing such a test and reporting its results, rather than presenting the readers with numbers. In sum, the author of the letter to the editor wants more numbers, but we believe that the numbers for which he asked are not relevant.

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REFERENCES


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We enthusiastically reviewed the valuable results of the recently published study by Stevens et al. (1) concerning the association of weight cycling with cancer. These results, which were consistent with those from several studies and contradictory to those of some others, showed that weight cycling was not associated with cancer.

In addition to the exceptionally meticulous discussion proposed by the authors, 2 points can contribute to the interpretation of these results. First and foremost, the authors decided to exclude from the analyses all prevalent cancer cases up to 1994. This means that all of the participants could have been weight cyclers anytime in their lives, whereas cancer was only possible for them after approximately 50 years of age. The authors pointed out that they excluded these prevalent cancer cases to eliminate bias from weight changes caused by cancer, but according to the definition proposed in their study, only intentional weight changes were considered to be weight cycling. Accordingly, excluding almost half of the cancer cases in
the younger age group might have resulted in selection bias. Because the number of excluded cancer patients roughly matched the number included in the analyses (approximately 22,000 vs. 25,000 patients), the probable bias could be significant.

Furthermore, we reviewed some studies (2, 3) with results contrary to those of the study by Stevens et al. and found that in those studies, there were wider age ranges for participants (18–79 years and 40–79 years, respectively, vs. 50–74 years in Stevens et al.). Therefore, the differences between the results of these studies and those of the study by Stevens et al. might have resulted from the interaction of age (younger or older than 50 years, for instance) and weight cycling. Consequently, we think that limiting the participants of the study to individuals older than 50 years, for instance) and weight cycling. Furthermore, including prevalent cases in these prevalent cancer cases as incident outcomes in our analysis of purposeful weight loss of 10 or more pounds and the regain of 10 or more pounds that had been previously lost. Because the regain portion of the cycle could be affected by cancer development, data from the first 2 years of follow-up were excluded. This resulted in the exclusion of 4,954 participants, some of whom were likely diagnosed with cancer in this interval. The second, much larger group of cancer cases (n = 22,863) were excluded because they had a history of cancer at the time of enrollment into the study. It would have been inappropriate to have included these prevalent cancer cases as incident outcomes in our analysis because they occurred before the start of study follow-up and potentially before the period during which reported weight cycling occurred. Furthermore, including prevalent cases in the analysis would preclude conclusions about a temporal relationship between weight cycling and subsequent cancer risk because weight cycling might have occurred as a result of the cancer diagnosis. It also would have been inappropriate to include prevalent cancer cases in our analysis because they would not have been at risk of our outcome, which was defined as a first diagnosis of cancer. Thus, we feel that exclusion of prevalent cancer cases from our analysis is consistent with sound epidemiologic practices and did not bias our findings.

Vardanjani et al. (2) also suggested that the association of weight cycling with cancer risk should have been studied in a cohort with a broader age range. We were precluded from examining this association among individuals younger than 50 years of age because only approximately 1% of participants in the Cancer Prevention Study-II Nutrition Cohort were below this age at enrollment in 1992–1993 (3). We agree that it would be worthwhile to specifically examine the association between weight cycling and cancer risk among individuals younger than 50 years of age using other study populations.

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THE AUTHORS REPLY

In a recent analysis of data from the Cancer Prevention Study-II Nutrition Cohort, we found that weight cycling, which was reported at enrollment into the study in 1992–1993, was not associated with subsequent risk of a first cancer diagnosis (1). Vardanjani et al. (2) suggested that the exclusion of a number of cancer cases might have biased our results. These cases were excluded for 2 reasons. First, weight cycles were defined by the combination of purposeful weight loss of 10 or more pounds and the regain of 10 or more pounds that had been previously lost. Because the regain portion of the cycle could be affected by cancer development, data from the first 2 years of follow-up were excluded. This resulted in the exclusion of 4,954 participants, some of whom were likely diagnosed with cancer in this interval. The second, much larger group of cancer cases (n = 22,863) were excluded because they had a history of cancer at the time of enrollment into the study. It would have been inappropriate to have included these prevalent cancer cases as incident outcomes in our analysis because they occurred before the start of study follow-up and potentially before the period during which reported weight cycling occurred. Furthermore, including prevalent cases in the analysis would preclude conclusions about a temporal relationship between weight cycling and subsequent cancer risk because weight cycling might have occurred as a result of the cancer diagnosis. It also would have been inappropriate to include prevalent cancer cases in our analysis because they would not have been at risk of our outcome, which was defined as a first diagnosis of cancer. Thus, we feel that exclusion of prevalent cancer cases from our analysis is consistent with sound epidemiologic practices and did not bias our findings.

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