Letters to the Editor

THREE AUTHORS REPLY

We appreciate Dr. Behrendt’s comments (1) on our statistical methodology and conclusions (2). In this 7-year cohort study, 408 participants died. The mean annual mortality
rate was 1.2 percent for total participants, 408/4,958, and 3.06 percent for those aged 65 years or more, 263/1,227 (2). Several points needed discussion before deciding which statistical methodology was appropriate. First, death itself might have been a possible competing risk factor for the incidence of diabetes, especially in the younger participants, but it would have been decreased in the older participants because death is an endpoint of natural life and the event of interest, especially for those who died when aged 75 years or more. For this group, the annual incidence of diabetes was 1.09, less than the 1.63 for those aged 65 years or more and 1.18 for those aged less than 65 years. Second, about 10 percent of the deceased, 41/408 of all participants and 23/263 of those aged 65 years or more, had new-onset type 2 diabetes before death. These patients with new-onset type 2 diabetes had been listed with the event of interest and did not signify a competing risk. Although 24.7 percent of the participants in the current study were aged 65 years or more, the effect of the competing risk factor for the event of interest might be negligible because of a low incidence, about 1 percent for annual incident diabetes, and a low annual death rate, about 1.2 percent of total participants and 3.06 percent of those aged 65 years or more. Therefore, the statistical method used in the current study is still acceptable, and whether the suggested statistical methodology used for diseases with high mortality rates (3, 4) is appropriate and convenient needs further study.

In the current study, the risk of developing diabetes for persons with hepatitis C virus (HCV) infection increased with decreased age when compared with age-group counterparts without HCV infection (2). Dr. Behrendt said, “a likelier interpretation [that the association between HCV and diabetes is significant only in persons below the age of 60 or 65 years] is that the finding is an artifact of survival bias against older persons with advanced HCV disease, who may have been disproportionately removed by death both before and during the study” (1, p. 751). However, the result is plausible and is not a survival bias because the elderly have many competing risk factors contributing to the development of diabetes, such as age-related insulin resistance, decreased insulin secretion, comorbidities, multiple drugs, decreased physical activity, and adiposity (5). These competing risk factors are probably lacking in the younger participants with HCV infection, which might exert its influence on the development of diabetes through its effect on insulin resistance or insulin secretion (6). Therefore, the relative risk of HCV infection in the development of type 2 diabetes in the elderly is not as significant as in younger persons.

Otherwise, the result for those with HCV infection and overweight or obesity showed an additive effect on the development of diabetes, but not a synergetic effect, because there was no interaction between HCV infection and obesity as suggested.

ACKNOWLEDGMENTS

Conflict of interest: none declared.

REFERENCES