same-year was 1.0. The simulation was repeated 20 times. The average same-year odds ratio for z_3 was 1.01 (s.d. 0.29), as would be expected. In contrast, the average same-year odds ratio for z_1 was 5.46 (s.d. 1.60), reflecting the spurious increase.

While the z_3 approach does not generate an artificially large odds ratio, it has no advantage over the one used by Kessler *et al*, with respect to the indeterminate temporal order between same-year events. In the US National Comorbidity Survey, as in other studies with similar diagnostic interviews, the time of onset of disorders was recorded as subjects' age *in years*, a method that results in ties. Survival analysis with time-dependent covariates deals with chronologically ordered events, with the dependent variable occurring after the independent variable(s). The approach using z_3 also assumes a time order where none can be determined. Indeed, no statistical approach can correct for the lack of information regarding which disorder preceded the other. An unbiased approach to modelling tied data is to censor cases with same-year onsets just prior to the year in which the two events occur.

Kessler, R. C., Nelson, C. B., McGonagle, K. A., *et al* (1996) Comorbidity of DSM-III-R major depressive disorder in the general population: results from the US National Comorbidity Survey. *British Journal of Psychiatry*, **168** (supplement 30), 17–30.

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Author's reply: Professors Peterson & Breslau are incorrect in thinking that the cross-sectional odds ratios reported by Kessler *et al* (1996) are artefactually inflated. Their confusion is due to a lack of clarity in our paper about the specification in Model 2. This model used the predictors described by Peterson & Breslau as z_3 and z_4, not z_1 and z_2. As Peterson & Breslau note, there is no bias in this approach. We regret the confusion and appreciate this opportunity for clarification.

Peterson & Breslau also stated that bias in estimating the effect of z_4 can be avoided by censoring same-year onset cases. This, too, is incorrect. The fact that none of the excluded person-years has a prior history of the predictor will lead to bias in the estimated effect of z_4. However, a slightly different approach will yield an unbiased estimate: to censor all person-years with a value of one on z_3.

It is noteworthy that the estimated effect of z_4 in this unbiased approach is equivalent to the estimate in our Model 2, as both methods compare z_4 person-years with the other non-z_3 person-years. The censoring method does this by excluding the z_3 person-years, while Model 2 does it by introducing a control variable for the z_3 person-years. We prefer Model 2 for two reasons. First, unlike the censoring method, it allows the magnitude of the lagged and cross-sectional odds ratio to be compared directly. Second, in multivariate models, where more than one time-varying predictor is considered at a time, it allows direct comparison of nested models, which is impossible in the censoring approach due to the fact that the number of excluded person-years varies with the number of predictors.

Finally, it might be useful to comment on the concern raised by Peterson & Breslau that the cross-sectional logit associated with z_3 does not have a clear causal interpretation. This is correct. In the absence of confounding variables, the cross-sectional association between a predictor and an outcome can be due either to an effect of predictor on outcome, an effect of outcome on predictor, or both. It is possible to use the method of instrumental variables to obtain separate estimates of these two effects if an appropriate instrument variable exists (Angrist *et al*, 1996). However, this method yields inconsistent estimates unless the instrument variable is strongly related to the outcome (Bound, *et al* 1995). This is seldom the case. As a result, it is usually preferable to estimate the cross-sectional effect of predictor on outcome using a recursive specification. This is what we did in our report. This coefficient can generally be interpreted as an upper-bound estimate of the short-term effect of predictor on outcome when one has reason to believe that the reciprocal effects between predictor and outcome have the same sign, making it a useful statistic despite the fact that it is biased.

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One hundred years ago

The Lunacy Act, 1890, and its amendments

Your correspondent, "Formerly a County Asylum Superintendent," I am glad to see, suggests as a remedy for the present evils the *abolition of lunacy certificates*. I have long maintained that the lunacy certificate should be abolished as constituting a grievous interference with the liberty of the subject in obtaining medical treatment. That a person suffering from disease of the brain must obtain, or have obtained, the authorisation of a magistrate before going to a sanatorium for treatment would be grotesquely absurd if it did not cause such serious evil. The public would rebel at once if the lawyers demanded that a man with a

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