same-year was 1.0. The simulation was repeated 20 times. The average same-year odds ratio for \( z_1 \) was 1.01 (s.d. 0.29), as would be expected. In contrast, the average same-year odds ratio for \( z_2 \) was 5.46 (s.d. 1.60), reflecting the spurious increase.

While the \( z_1 \) 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 or studies with similar diagnostic interviews, occurring after the independent variable(s).

Survival analysis with time-dependent covariates deals with chronologically or ordered events, with the dependent variable occurring after the independent variable(s). The approach using \( z_1 \) 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.

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 \( z_1 \) 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 \( z_1 \) 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
broken leg should be haled before a magistrate prior to being taken to a hospital. The broken leg can be seen, but unfortunately the broken brain is not so visible.

If there is danger of asylums admitting or retaining sane persons these institutions should be so supervised as to render this impossible, and this supervision should be accomplished by supervisors appointed and paid by the State. If the medical profession possessed any solidarity it would not tolerate for a moment the present position of matters. The duty of signing certificates and of making reports in regard to these sick persons is not only thrust upon medical men by law, but these laws are conceived and directed against the profession very much as if against the habitual criminal class.

The result of 'all this insane legislation', as it may appropriately be called, is that to avoid certification, or from fear of it, patients are kept from appropriate treatment, with the consequence that homicides and suicides are rife, while cases that might have recovered become chronically insane. A second result is that asylum medical officers have half their time occupied in filling up endless forms, reports, statements, &c., many of them utterly absurd and useless, but one at least, the periodical certification form, positively prejudicial, having often resulted in the discharge of dangerous lunatics. The proportion of medical officers in asylums is wofully small, and with the ever-increasing addition to the always heavy duties of administration little time or energy is left them for the treatment of their patients. The Lunacy Laws, indeed, are probably the most efficient cause in preventing that popular subject, "the increase of insanity," becoming "the decrease of insanity." The disease is clear, but what is its cause and what the remedy. The cause is patent and obvious. The Lord Chancellor is at the head of the lunacy system. Hence the anomaly of a Lunacy Commission, entrusted with the supervision of the treatment of insanity throughout the kingdom, being mainly composed of barristers. This ridiculous state of affairs is about comparable to what should obtain if the Lord Chancellor were supreme over the Admiralty, when our captains of ships would spend all their time in forms and correspondence in regard to why Jack had had his grog stopped. For many years past I have taken part in representations, petitions, and deputations to successive Lord Chancellors in regard to lunacy legislation, and I am fully convinced that this is waste of energy. The remedy for the present wretched state of affairs is to approach Parliament directly, with a view to have the Lord Chancellor's jurisdiction only over the property of the insane, but to place lunatics for their care and treatment under the charge of a Minister of Public Health. I would suggest the formation of an association to promote this object, and should be glad to hear from others interested in, and willing to work to, this end.

REFERENCE

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