Letters to the Editor


In a recent article, Polakowski et al. (1) presented interesting results related to the association between monovalent H1N1 influenza vaccination and risk of Guillain-Barré syndrome. They found an incidence rate ratio of 2.41 (95% confidence interval: 1.14, 5.11). In their sensitivity analysis, which used a stricter definition of Guillain-Barré syndrome (exclusion of 3 out of 29 cases), the corresponding incidence rate ratio was 1.97 (95% confidence interval: 0.90, 4.34). The authors noted that the nonsignificance was “likely due to the exclusion of additional cases and corresponding decrease in power” (1, p. 970), but in their abstract they stated, “[H]owever, additional results that used a stricter case definition (Brighton level 1 or 2) were not statistically significant” (1, p. 962). The use of “however” in this sentence reveals that the statistical nonsignificance of their sensitivity analysis makes a difference to the authors. Don’t we all agree that the sensitivity analysis provided more or less the same results as the main analysis? Is there really anybody who would claim that an incidence rate ratio of 1.97 (less precise) and an incidence rate ratio of 2.41 (more precise) produce qualitatively different interpretations?

A count in 2000 of over 300 warnings on the limitations of null hypothesis significance testing (2) was followed a year later by compilation of a list of 402 references (3), among which we found 89 in biomedical publications. Despite the many cautions, significance testing of the null hypothesis remains one of the most prevalent, misused, and abused statistical procedures in the biomedical literature (4).

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REFERENCES

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Editor’s note: In accordance with Journal policy, Polakowski et al. were asked whether they wished to respond to this letter, but they chose not to do so.

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RE: “EXAMINATION OF HOW NEIGHBORHOOD DEFINITION INFLUENCES MEASUREMENTS OF YOUTHS’ ACCESS TO TOBACCO RETAILERS: A METHODOLOGICAL NOTE ON SPATIAL MISCLASSIFICATION”

In a recent article, Duncan et al. (1) compared tobacco retailer density and proximity measures computed within 2 administrative and 4 egocentric neighborhood definitions and precisely identified which measures significantly differed across neighborhood definitions. This comparative analysis led the authors to conclude, rightly so, that how one defines “neighborhood” may considerably influence neighborhood-level exposure measures. These findings echo the importance of both zoning and spatial scale, which has often been underlined in the health literature (2–4) since Openshaw first defined the Modifiable Areal Unit Problem in 1984 (5). The authors went on to conclude that “whenever possible, egocentric neighborhood definitions should be used” and that “the use of larger administrative neighborhood definitions can bias exposure estimates for proximity” (1). However, the authors should not have extended their findings, which concerned the difference between neighborhood resource accessibility measures, to a judgment on the most relevant spatial units to use in neighborhood and health research. We believe their conclusion stems from the following 2 assumptions that are pervasive in the literature