Letters to the Editor

Cohort effects explain the increase in autism diagnosis among children born from 1992 to 2003 in California
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Keyes et al. recently explored incidence of autism diagnosis in sequential cohorts of all individuals born in California from 1992 to 2003. They linked autism diagnoses from a case registration database to birth records to calculate number of diagnoses and time at risk. Using a constraint-based approach to age-period-cohort analysis, they concluded that incidence of autism diagnosis in California exhibits a robust and linear positive cohort effect.

Although incidence of autism diagnosis has increased across successive birth cohorts, I find the firm conclusion that this is due to birth cohort effects questionable. The authors’ analytic approach relies on placing constraints on the age-period-cohort terms in order to make the coefficients estimable, justification for this being a paper by Mason et al. What they omit to say is that a subsequent paper in the same journal discredited the method, concluding that ‘Although a constraint of the type described by Mason et al. seems trivial, in fact it is exquisitely precise and has effects that are multiplied so that even a slight inconsistency between the constraint and reality, or small measurement errors, can have very large effects on estimates’.

The Keyes paper includes a figure showing incidence of autism diagnosis increasing across successive birth cohorts, consistently with their model. However, these data can be reconfigured to similarly show a regular increase across successive periods (Figure 1). This resembles ‘drift’, as described by Clayton and Schifflers, in which there is a linear increase or decrease across successive periods/birth cohorts. In the case of drift, the trend can be equally accounted for by cohort or period effects, and these two cannot be empirically untangled. Given lack of information on the modelling process and procedure for choice of constraint, I am left wondering whether the authors may have inadvertently chosen their constraint to fit in with their prior theoretical orientation. Possibly they may have been influenced by the patterns in their Figure 1, which is of limited interpretation as the three effects of age, period and cohort are heavily confounded.

Possible misspecification of the trend in diagnosis as a cohort effect is of more than academic interest, as it risks erroneously discarding effects which may in fact be of importance in accounting for the trend, and determining whether it is likely to continue. I am not sure about the authors’ substantive conclusions. On the one hand, they dismiss period effects, saying ‘broad environmental factors that have equal effects across age cannot explain the increase in autism diagnosis’. On the other hand, their analysis is ‘consistent with changes in diagnostic practice and heightened awareness’, both of which are features of the sociocultural environment and bear across the age range considered here. Although the distinction between diagnosis in low-functioning and high-functioning individuals may offer some clues, I wonder whether further analysis of these data would leave both birth cohort and period-based explanations open. It would be interesting to see this further explored.
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References


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Authors’ Response to: Cohort effects explain the increase in autism diagnosis among children born from 1992 to 2003 in California

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It is absolutely true that the choice of modelling strategy in age-period-cohort analysis does potentially impact on the results, and that analysts cannot be guided by two-dimensional figures alone due to the inability to control all three factors simultaneously.1,2 It is not the case, however, that the Clayton and Schifflers method is in all cases superior to constraint-based methods. Each method used for age-period-cohort analysis requires certain unverifiable assumptions about the underlying pattern of age, period and cohort effects, including those estimating ‘drift’ parameters before period and cohort. Rather than applying the same age-period-cohort model regardless of the outcome under investigation, we believe—and have argued1–3—that the choice of model should be explicitly guided by theory as well as descriptive data patterns. In the case of autism diagnoses, there is substantial evidence that diagnoses are more common among 3- and 4-year-old children than older children, thus a simple constraint that diagnosis is constant after the age of 8 years allowed us to parsimoniously model the data without assuming that period and cohort effects are nonlinear variation from overall drift. We note that both period and cohort effects of strong magnitude were documented in our data using the constraint-based approach. Dr Spiers’s suggestion that both period- and cohort-based explanations for these data should remain open is consistent with the evidence we presented.4

References


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