Review of: "Purchasing and sourcing of e-cigarettes among youth in Scotland and England following Scotland's implementation of an e-cigarette retail register and prohibition of e-cigarette sales to under-18s"

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I thank the authors for the opportunity of reviewing this manuscript. Examining possible effects of nicotine and tobacco control policies is an important task. The manuscript is clear and concise, and it is positive to see that the authors have pre-registered their analyses.

However, there are two major issues that I hope that authors will address. The first is mentioned and discussed by the authors, but maybe taken too lightly, namely the issue of too few respondents aged 16-17 in the Scottish sample. One example is the results in Table 3: "In Scotland, the proportion of past-12-month vapers aged 16-17 who reported purchasing an e-cigarette product in the past 12 months was 21% in 2017 and 50% in 2018". This was based on 33 persons in 2017 and 32 in 2018. When broken down by place of purchase the numbers were often below 10. In my opinion, the cell sizes are simply too small to report differences between such groups and conduct statistical tests. This is a major problem because it is directly addresses the research aims.

There isn't an obvious solution to this problem. The authors could leave out even more of the pre-registered analyses and focus on differences between the Scottish and English sample, all age groups combined, and without place of purchase. Another solution is to model the probabilities of purchasing an e-cigarette product in the past 12 months / being refused sale of an e-cigarette product in the past 12 months / sourcing an e-cigarette product in the past 30 days using a continuous measure of age, and calculate the predicted probabilities or average marginal effects (e.g. dy/dx) between groups at different ages. The results from such an analysis (maybe in the form of a graph) could replace Tables 2-5.

The second major issue is related to the analyses (and also the first major issue). In the analysis-part, the authors write: "Unadjusted and adjusted (with predictors and covariates entered simultaneously into models) logistic regressions using weighted data were used to assess associations between each outcome and country, year, age group, and country*year and, in a subsequent model, country*year*age group". As far as I can see the results are shown as adjusted odds ratios, based on the three (six) following models:

logistic purchase/refused/sourcing country## year age_group

logistic purchase refused/sourcing country## year## age_group

The authors then write “Interactions were further examined by contrasting marginal estimates.” And under Table 4/5 “Associations (AOR, 95% CI, p) are based on marginal effects”. What does this mean, how are the odds ratios based on marginal effects? Do the authors mean that the AOR are based on model estimates?

As I commented above, another (and in my view better) solution is to calculate predicted probabilities or marginal effects from the models above. In addition, the authors should also consider dropping the p<.05 test criteria, both because .05 is an arbitrary value and because the relatively large number of tests conducted may lead to a multiple comparisons problem.