



Comment on: “Diabetes incidence and projections from prevalence surveys in Samoa over 1978–2013”

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With great interest we read the recently published article about prevalence and incidence of diabetes in Samoa by Lin et al. (2017).

The authors used a sequence of cross-sections to first estimate the prevalence and then derived the incidence from these prevalence data. For this, they used a method published by Styblo et al. (1969). We thank the authors for pointing to this interesting work from the field of tuberculosis research. However, we have a concern about using this method in the context of diabetes. In virtually all populations we have seen that people with diabetes have a higher mortality than the people without diabetes having the same sex and age. Thus, there is an excess mortality that must be considered in the incidence estimation, which, however, is not taken into account in the method by Styblo et al. (1969).

To demonstrate the magnitude of the possible error when ignoring the mortality, we run a short simulation, which calculates the prevalence of diabetes in a hypothetical population from known incidence and mortality rates. The incidence is taken from Table 1 in Lin et al. (2017), the general mortality is chosen to follow a Gompertz law (Vaupel and Yashin 1985) and the hazard ratio for the mortality rates stems from Denmark (Carstensen et al. 2008). Of course, the simulation is hypothetical, but

all the input data are realistic for diabetes. After calculating the prevalence in this example, we apply the method of Styblo et al. (1969) to calculate the incidence. By this, we mimic the approach by Lin et al. (2017). Then, we compared the derived incidence with the true incidence and the results of the method described in Brinks and Landwehr (2015), which has also been cited in Lin et al. (2017). The results are shown in Fig. 1.

From Fig. 1 we see that the method by Styblo et al. (1969) (circles) systematically underestimates the true incidence. The reason lies in the fact that the method of Styblo et al. (1969) ignores the impact of mortality, which leads to biased results. The maximum relative error in the age range 25–64 years is 39%.

For comparison, the method by Brinks and Landwehr (2015) leads to an incidence estimate that is visually indistinguishable from the true incidence in this simulation (crosses in Fig. 1). The maximum relative error in this case is below 0.1%.

As stated correctly in the article by Lin et al. (2017), there is a need for information about the mortality when applying a method to derive incidence rates from prevalence data. However, it is not true that compartment models need cause-specific mortality rates. The illness-death compartment model underlying the method of Brinks and Landwehr (2015) only needs the difference in mortality between the diabetic and non-diabetic population (Brinks and Landwehr 2015). Causes of death are irrelevant.

For us, it seems that the authors do not have mortality data available (which indeed can be difficult to obtain) and try to overcome the problem of missing mortality data using an inappropriate method that completely ignores mortality. We do not think that this is appropriate, which we have demonstrated in an example about diabetes.

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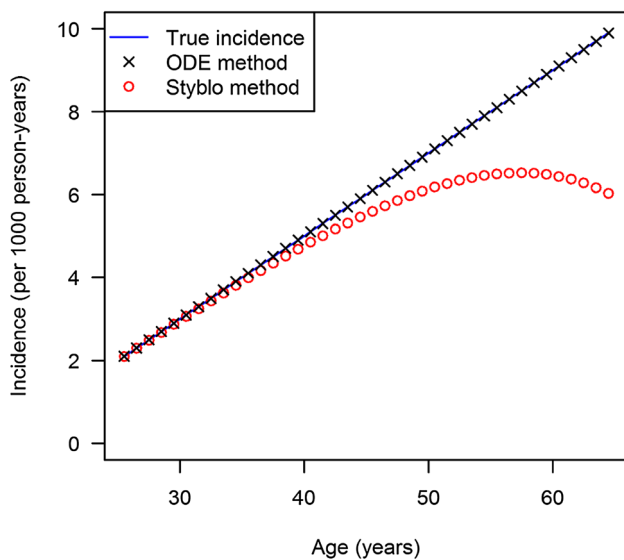


Fig. 1 Comparison of age-specific incidence rates based on the ordinary differential equation (ODE) and the Styblo method. The true incidence used to set up the simulation is depicted as solid straight line. The estimates based on the method of Styblo et al. (1969) and Brinks and Landwehr (2015) are shown as *circles* and *crosses*, respectively

As additional minor flaws in the article by Lin et al. (2017), we note that the formula for deriving the cumulative risk from the cumulative incidence is wrong in the competing risk situation, which applies here. Moreover, formula (3) derived from the Styblo method in the Supplement of Morrell et al. (2016) implies a division by zero if there is no secular trend in the risk of contracting diabetes (i.e., $s = 0$).

Given the flaws in the article by Lin et al. (2017), which possibly may lead to an enormous bias in the incidence estimates, we ask ourselves to what extent the conclusions drawn in the paper (Lin et al. 2017) and a related paper (Morrell et al. 2016) are really valid.

Compliance with ethical standards

Ethical statement This letter to the editor does not involve research involving human participants or animals.

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