

Letters to the Editor

Diabetic nephropathy

Sir

I read with great interest the article by Afifi and colleagues [1] in the Eastern Mediterranean health journal and I appreciate the authors' work. However, I would like to make a few comments because of the increasing prevalence of diabetes and its complications in the Arab world. Diabetic renal disease is a common complication and is the most prevalent cause of end-stage renal disease (ESRD) in the Western world [2]. According to the ESRD programme in the United States, the number of existing patients with terminal renal insufficiency caused by diabetes more than tripled between 1990 and 2001 [3].

The first question to pose here is: was this a 6-year study? The title possibly suggests it is a cohort study. However, it seems from the study design, tables and the results that it is a data review of an ambiguously selected sample of ESRD patients enrolled in the Egyptian renal data system. Both the sampling technique and the rationale behind the sample size are unclear. Moreover, in a cohort study we trace a risk factor to get an outcome, either prospectively or retrospectively. Here, the authors already know the outcome, i.e. ESRD, and the risk factor, diabetes, and the rates shown by the data are not in line with what cohort studies are supposed to reveal.

Moreover the results seem to be contradictory. The authors say that "the mean

age of patients with diabetic nephropathy was higher than that of patients with other causes of ESRD in the years [they] studied". They then say that "mortality among diabetic patients with ESRD was higher than in patients with ESRD from other causes". If we want to know how long patients with ESRD due to diabetic nephropathy live in comparison to ESRD due to other causes, we must evaluate the interval between 2 events: the diagnosis of ESRD, whatever its cause, and death. In fact, the 2 events of interest – diagnosis and death – may not occur for all the study participants during the period they are observed and the actual period of observation may not be the same for all of them. These complicating factors rule out the possibility of simply calculating the average time between the 2 events or the mean age of the studied participants as the authors did in Table 3, where they compared the mean age of diabetic nephropathy patients and patients with ESRD from other causes. A special statistical technique, survival analysis [4], is needed to look at the interval between diagnosis and death when the first timing is not the same for everyone, the second does not necessarily happen to everyone, and hence the study participants are observed for different periods of time.

References

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Authors' response

We are glad that our work stimulated Dr Afifi from Oman to send his views. We believe that if he had read the article more thoroughly, he would have found answers to most of his criticisms. However, we would like to clarify the points he raised.

There was no need to repeat the extent to which diabetes is responsible for chronic kidney disease in the United States and Western countries. We, as well as all nephrologists are well aware of this issue and have quoted it in our work. We would like to draw his attention to the fact that what applies to Western countries does not necessarily apply to developing countries for a variety of reasons he should know. Indeed, differences exist between developed and developing countries and we should always investigate and analyse our problems rather than quoting data from the United States and Europe. In this respect he should read Table 1 that showed variations in the prevalence of this disease ranging from 4.7% in some countries to 50% in others.

Dr Afifi postulated that our study was a cohort study although we never said that it was. We clearly stated that it was a six-

year multiple cross-sectional study. The writer should know that there are many limitations to a cohort study in Egypt and in most developing countries as well. The most important of these limitations is the unavailability of complete medical records and the reluctance of colleagues to share their records. We would be happy to share in data management and analysis with the writer if he has such data.

He suggested that we have contradictory results because “the mean age of patients with diabetic nephropathy was higher than that in patients having other causes of ESRD” and that “mortality among diabetic patients with ESRD was higher than in patients with ESRD from other causes”. There is nothing contradictory in these findings. Indeed, the mean age of diabetic patients with ESRD was about 10 years higher than that due to other causes (Table 3) and this is simply explained by the fact that type 2 diabetes needs many years to develop into ESRD, whereas other causes of ESRD, particularly infections causing glomerulopathies, occur at a much younger age. The mortality among diabetics was

also much higher than in other causes of ESRD (Table 2). This does not need much explanation as we are sure that the writer

agrees that diabetics suffer from increased mortality due to micro- and macrovascular complications.

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Risk factors for hypertension in UAE

Sir

I have read with great interest the article by Sabri and colleagues [1] in the Eastern Mediterranean health journal and I appreciate the authors' work. However, I would like to make few comments because of the importance of hypertension and its risk or associated factors.

The authors mention in their abstract as well as in the results and discussion sections that hypertension among cases was high in low-income participants. Looking at the bivariate analysis (Table 1) and the multivariate analysis (Table 4), we would see the reverse. Table 4 shows that those with higher income (income equal 5000 dirhams or above) were 2.69 times more likely to be hypertensive than others.

The authors contradict themselves again where they show that the crude odds ratios for obese participants ($BMI \geq 30 \text{ kg/m}^2$), those with no physical activity, and those with positive family history of diabetes were 0.46, 0.72 and 0.28 respectively (Tables 1 and 2). This suggests these factors are protective against hypertension, whereas in

Table 4 the adjusted odds ratios for the previously mentioned variables are 4.29, 1.8 and 2.58 respectively, suggesting that obesity, sedentary lifestyle and family history of diabetes are risk factors for hypertension.

The cause behind such contradiction is the order of rows in the Tables 2 and 3 from which the odds ratios were calculated in the bivariate analysis. The correct order is to put the exposed or the risk factor in the first row and the non-exposed in the next [2,3]. If the rows are reordered in the bivariate analysis tables, the correct odds ratios for variables studied are obtained and these would be 1/existing figure.

The authors mention in the sampling section that cases and controls were age, sex and nationality matched. They say that they recruited 500 hypertensive patients, 64 did not participate and thus 436 cases and 436 matched controls were included in the final analysis. From Table 1 the number of cases and controls varied markedly in sex, age group and slightly in nationality. The prob-