

# Comment on “Rosiglitazone treatment in nondiabetic subjects with nonalcoholic fatty liver disease”

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**Letter to the Editor** We read with great interest the article by Saryusz-Wolska et al.<sup>1</sup> dealing with the effect of rosiglitazone treatment on the liver function and insulin sensitivity in nondiabetic patients with nonalcoholic fatty liver disease.<sup>1</sup> The authors revealed that rosiglitazone treatment improved the liver function and insulin sensitivity after a 6-month follow-up. In our opinion, some points of this work are not clear enough.

First, although the homeostasis model assessment-estimated insulin resistance (HOMA-IR) index is an inexpensive and simple tool used extensively for the determination of insulin resistance (IR) in large-scale studies, the threshold value for IR has not yet been standardized. Thus, different HOMA-IR levels used to define IR may result in the variability of waist circumference cut-off values to predict IR. For instance, in a retrospective study on 2746 healthy subjects, Wahrenberg et al.<sup>2</sup> defined a HOMA-IR value above 3.99 as IR. Other studies provided different estimates: Bonora et al.<sup>3</sup> reported a HOMA-IR value of 2.77 for IR among nonobese individuals without metabolic disorder, and Ascaso et al.<sup>4</sup> used a cut-off value of 2.6 for subjects with normal glucose tolerance. These different HOMA-IR cut-off values for IR in different studies may reflect the varying characteristics of study populations, suggesting the need for defining population-specific HOMA-IR cut-off values for the prediction of IR.

The authors selected patients without diabetes mellitus and metabolic syndrome whose HOMA-IR levels were  $3.73 \pm 1.89$ . When looking at the standard deviation ( $\pm 1.89$ ), HOMA-IR levels in some patients were presumably lower than for example 2.0 (the HOMA-IR levels of specified Polish populations of patients have not been stated in the article). Accordingly, if there were any reasons for the administration of rosiglitazone treatment to healthy patients (except for nonalcoholic fatty liver disease) and patients without

diabetes or IR – these reasons should be clearly stated in the manuscript.

Secondly, the authors selected healthy subjects matched for age and body weight. Healthy subjects had the HOMA-IR value of  $2.34 \pm 1.87$ , hemoglobin A<sub>1c</sub> of  $5.6 \pm 0.27$ , and the body mass index of  $30.4 \pm 3.3$  kg/m<sup>2</sup>. Presumably some of them had IR, and the reasons should be indicated why IR treatment had not been initiated in these subjects. Furthermore, it was not stated whether ultrasonographic examination was used or not in these subjects.

Finally, the authors selected male subjects for the study. However, the section on patients and methods does not mention this selection criterion. Moreover, the fact that only male subjects were recruited was not mentioned either in the study title, hypothesis, or discussion.

We hope that the above comments might add to the value of the article by Saryusz-Wolska et al.<sup>1</sup>

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**Author response** The authors did not submit their response.