Adjusted prevalence of metabolic-associated steatotic liver disease (MASLD) in a Mexican

Ajuste de prevalencia de esteatosis hepática metabólica (MASLD) en una población mexicana

Dear Editors,

Fatty liver disease took on an identity of its own more than 50 years ago.[1] Since then, it has been referred to by many different names and has been a disease in search of its own denomination. This is not by chance; the names have changed as the secrets of the disease have been revealed.

The name currently in use is metabolic dysfunction-associated steatotic liver disease (MASLD),[2] which emphasizes the importance of metabolic dysfunction in its pathophysiology.

This new denomination reconsiders the diagnostic criteria, making them less restrictive. Now, in addition to hepatic steatosis, cardio-metabolic risk factors, such as manifestations concurrent with metabolic dysfunction, are included, and patients with significant alcohol use or other chronic liver disease are not excluded. Thus, based on these conceptual changes, some large case series have adjusted their original prevalence figures that had been described with the diagnostic criteria of the previous denominations.[3,4]

In 2020, our group conducted an epidemiologic study on an open population of 585 volunteers in Veracruz.[5] We utilized the diagnostic criteria of the nomenclature in use at that time, which was metabolic dysfunction-associated fatty liver disease (MAFLD), resulting in a prevalence of 41.3%, which by the way, was one of the highest reported worldwide. When we applied the new MASLD diagnostic criteria to the sample, there was a slight increase to 42.5%. Table 1 shows the general results.

As can be seen, the reported prevalence showed a minimal increase of 1.2%, in both men and women, which is very similar to that reported in other case series.[6,7]

It is important to point out that, according to the MAFLD criteria, dual injury was not considered in the sample, given that we had excluded the volunteers with significant alcohol use (>30 g/day in men and >20 g/day in women), as well as volunteers with other known chronic liver diseases. Therefore, by incorporating those excluded groups, prevalence would likely be even higher.

Our findings are consistent with those of comparable reported analyses, showing that, regardless of the diagnostic criteria utilized, the prevalence of nonalcoholic fatty liver disease (NAFLD), MAFLD, and MASLD is very similar.[6,7]

Table 1 Prevalence comparison of MAFLD and MASLD in the sample.

	Total	Prevalence		Difference
	n (%)	MAFLD n (%)	MASLD n (%)	
	585 (100%)	242 (41.36%)	249 (42.56%)	+ 1.2%
Women	418 (71.5%)	158 (37.79%)	163 (38.99%)	+ 1.2%
Men	167 (28.5%)	84 (50.29%)	86 (51.49%)	+ 1.2%

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Declaration of competing interest

The authors declare that there is no conflict of interest.

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R. Bernal-Reyes R[a,*], B.A. Priego-Parra[b,c], M.E. Icaza-Chávez[d], J.M. Remes-Troche[b]

[a] *Sociedad Española de Beneficencia, Pachuca, Hidalgo, Mexico*
[b] *Centro de Investigaciones Biomédicas, Universidad Veracruzana, Xalapa, Veracruz, Mexico*
[c] *Instituto de Investigaciones Médico-Biológicas, Universidad Veracruzana, Veracruz, Mexico*
[d] *Departamento de Gastroenterología, Hospital Star Médica de Mérida, Mérida, Yucatán, Mexico*

* Corresponding author at: Justo Sierra 116, Col. Periodistas Pachuca, Hgo. CP 42060, Mexico.
E-mail address: raulber@yahoo.com (R. Bernal-Reyes R).

The importance of adequate dietary reporting in nutritional epidemiologic studies

La importancia de un adecuado reporte de la dieta en estudios de epidemiología nutricional

Dear Editors,

We have read with interest the article, ''Dietary characteristics of Mexican patients with irritable bowel syndrome: Is there a distinction from the general population?'' by Amieva-Balmori et al.,[1] which addresses a relevant theme, but we consider it pertinent to make some observations to enrich the methodological and conceptual discussion of the study.

The analysis does not specify the clinical moment at which the patients were evaluated nor under which criteria the health status of the controls was confirmed. For the controls, it is necessary to demonstrate that the absence of disease is not synonymous with health (being healthy). The authors should have clarified how they ensured that the controls did not present with other diseases. For the patients, it would have been ideal to describe the ''serious diseases that could affect nutrient intake''. We also believe it is important to emphasize that ''dietary characteristics'' is an incorrect designation used by the authors regarding their nutrient analysis. Dietary characteristics are those that indicate what and how we eat, and in that sense, the authors fail to describe the intake of specific ingredients, dietary patterns, diet quality, types of sugars, and trigger foods. Only evaluating the nutrient profile limits the possibilities of identifying the dietary factors that trigger symptoms in irritable bowel syndrome (IBS).

We also wish to comment on other methodological questions. The first is the description of the selection process of patients as ''voluntary participants'' (the literal translation of the Spanish text referred to here would be ''voluntary invitation''), which is not clear and could lead to doubts as to the randomness of the sample and even be misinterpreted as recruitment coercion. Second, the authors carried out a FODMAP analysis limited to a qualitative categorization, restricting the capacity to infer relative differences between groups that would be a greater contribution and benefit to the readers.[2] Third, certain aspects that authors of articles on nutritional epidemiology should consider are clinical differences between groups that should be controlled in the study design, because disparities in age and body mass index (BMI) can be important biases. Likewise, reporting data utilizing the term ''vegetables'' as separate from fruits, cereals, and legumes needs to be clarified, given that taxonomically, they all belong to the vegetable kingdom. It would have been relevant to discuss the possible role of eating disorders in the population studied, as has been done previously.[3] And last, the lack of an