Reviewer's report

Title: Effects of the prudent diet versus low fat diet in the management of patients with chronic hepatitis C

Version: 1 Date: 22 February 2013

Reviewer: Amedeo Lonardo

Reviewer's report:

MAJOR

Title – I feel the definition “Prudent diet” should be replaced in the title and throughout the manuscript by something much more specific. I note the Authors use “normoglucidic diet” in the Discussion. Could prudent diet become a low-calorie normoglucidic diet? Moreover, I note the title fails to name physical activity and it should. Both these changes would make the title mirror more accurately the content of the study making its reading more attractive to those potentially interested in going beyond the title alone.

Summary – It quite unclear whether the aim of the study was weight control alone as opposed to liver histology outcome (fibrosis) given that both aims are named in different sections of the summary though histological analysis was seemingly not included in the study design. I suggest to be more consistent with the statement contained in the manuscript “reducing insulin resistance and hepatic steatosis and fibrosis by nutritional intervention”.

The conclusion “Nutritional therapy should be an essential component of care for people infected with hepatitis C because this medical condition through its associated entities should be considered as a metabolic disease” sounds more like the background than the conclusion and should be deleted. “Present study establishes the positive impact of an educational program in management of patients with hepatitis C.” Do the Authors really believe that HCV infection should be managed by lifestyle changes alone without administering antiviral schedules?

Highlight that compliance was significantly different between the 2 diets.

Introduction – The concept of HCV infection as a metabolic disorder may be supported better by quoting specific references, including; (Lonardo A, et al. Hepatitis C and cardiovascular risk: facts and controversies Hot Topics In Viral Hepatitis, Issue 24, 2012, ISSN 1973-9648.)

Study design –

It is unclear to this Reviewer whether the Authors are proposing lifestyle changes as the only treatment option in those with HCV infection. Please address the issue that lifestyle changes are deemed to additive to proper antiviral treatment schedules which remain the standard of care (Adinolfi LE, et al. Expert Opin
Pharmacother. 2011;12:2215-34.)

It is critical to describe accurately criteria for inclusion and exclusion. Moreover, the technique followed to randomize patients (e.g. sealed envelopes; randomization list; telephone call to a randomization center) should be described.

It would be of interest if the Authors might be willing to discuss better why the wanted to compare these 2 diets and, shortly, what they differ in.

“The serum concentrations of adiponectin, leptin, resistin, TNF alpha, IL6 were measured by enzyme-linked immunosorbent assay (ELISA). Samples were prepared at the appropriate dilutions and paired samples were assayed together according to the instructions of the manufacturers. The intra- and inter-assay coefficients of variation for resistin, leptin, and adiponectin were: <4% and <7%, <8% and <10%, <5% and <7%, respectively.

Insulin, peptide C and citokines as adiponectin, leptin, resistin, interleukin-6 (IL-6), tumor necrosis factor (TNF-alpha) were measured at baseline and 12 months.”

Delete the techniques of all those biological principles not reported in the study.

Hepatic steatosis (HS) was evaluated using ultrasonographic criteria – Please quote recent semiquantitative indices used to assess steatosis which have been validated against histology (Ballestri S, Liver Int. 2012;32:1242-52.) and proven useful in the specific settings of lifestyle interventions (Scaglioni F, Clin Res Hepatol Gastroenterol. 2012 Dec 26. ).

Discussion - The Authors might be willing to discuss that the prevalence of the Metabolic syndrome in their study appears to be higher than reported in previous european studies (reviewed in Adinolfi LE, et al. Expert Opin Pharmacother. 2011;12:2215-34.). Moreover, at variance with data from the feneral population, the prevalence of MetS appears to be more elevated in men. Does this imply a particular selection of cases ?

English – The study has several spelling and grammar mistakes and definitely needs editing by a native English.

MINOR

were associated with improvement of ALT levels[,] and steatosis [(Ueno, Andersen)].--> Add the references were missing; use consecutive numbers rather than the Authors’ names

Tables 5. and 6. “Hepatic function”--- Liver tests

Tables 6. And 7. These tables are quite difficult to follow. Would it be possible to improve/delete them ? Consider putting them as an appendix.

Quality of written English: Not suitable for publication unless extensively edited
**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

'I declare that I have no competing interests'