Author’s response to reviews

Title: Socio-demographic and Clinical Features of Irish Iatrogenic Hepatitis C Patients: A Cross-Sectional Survey.

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Author’s response to reviews:

Dear Dr. Bucceri,

Attached please find a copy of the revised manuscript entitled “Socio-demographic and Clinical Features of Irish Iatrogenic Hepatitis C Patients: A Cross-Sectional Survey” which I am resubmitting for review by your journal as requested. I have endeavoured to address the concerns outlined by each of the reviewers in the revised manuscript. Please see the following points of response in relation to the comments made by the relevant reviewers.

· Reviewer 1 (Alessio Aghemo):
  
  o Comment 1: “While the authors seem to acknowledge the study may be biased by the low response rate (28%) to the survey and by the fact that the studied population does not seem to reflect the general HCV population in Ireland (high rates of unemployment and of permanent disability), this is not stressed enough in the discussion and in the conclusions. Some comments explaining the potential biases in the study might have and therefore mitigating the findings by the authors need to added in the conclusions section.”

In order to address these concerns regarding study biases we have expanded the discussion section on the methodological, sample and bias issues that arose – so that a clearer picture of the findings is given facilitating informed interpretation of the data presented. A paragraph has also been added to the conclusion section. With regard to the study being biased by not reflecting the general HCV population in Ireland: the study cohort was a unique group quite distinct from the general HCV population and it was not the purpose of this evaluation to examine the general HCV population or reflect it, but to evaluate the impact of iatrogenic HCV infection. The evaluation of this unique cohort was seen as an advantage of the current study rather than a bias. Any comments in relation to the general HCV cohort in Ireland were made in order to give background data and to highlight the discrepancies between the two groups. Regarding the employment rates quoted and the issue arising: these were based on the general Irish population (not general Irish HCV population) and we have clarified this by citing “general/ national Irish population statistics” where
appropriate.

Comment 2: “The decision to categorise all symptoms reported by the population as being Hep-C related in the absence of another definitive cause might further complicate the interpretation of the data. In fact one cannot wonder whether the study is just reporting symptoms of an ageing population (disturbed sleep, lower limb pain....) more than symptoms associated to Hep-C. As a matter of fact Symptoms were not associated with HCV virus positivity, confirming what I just mentioned. In my opinion the authors need to add data regarding symptoms of a similar non-HCV infected population. A small control group would in fact make the data much easier to digest, if this is not possible adding some data from an historical control group (past published cohort) would strengthen the findings by the authors”.

Symptoms reported were based on a checklist of symptoms compiled from those included in previously published research as associated with hepatitis C. In the original version of the manuscript we had acknowledged the absence of an age-matched control group as a limitation, however in keeping with other international research published on symptoms of hepatitis C as discussed in the revised manuscript, (which were evaluated without control groups) this was not incorporated in the study design. Control groups were predominantly used in studies which used validated outcome measures relating to symptoms of hepatitis C (e.g. fatigue impact scale for fatigue). The length of the customised study survey in terms of respondent burden precluded the use of validated measures for all symptoms of hepatitis C here. In response to the request for inclusion of a control group, the data was not available for the current study however reference has been made to historical control groups in 2 previously published studies on similar symptoms. Further emphasis has also been placed on the absence of a control group as a limitation of the study in terms of cause and effect in the revised manuscript. The relationship of symptoms (or lack of as is the case here) to virus status is discussed in more detail – and similar findings in other research have been presented, in addition to highlighting issues with missing data.

Comment 3: “The introduction section needs to be shortened as it runs way too long in my opinion, I would cut all parts regarding liver disease progression which are redundant and concentrate more on the study background”.

The introduction section was revised to exclude all data relating to liver disease progression as requested. Further detail was included regarding symptomatology for background purposes.

Comment 4: “Table 3 needs to be redone as it is very hard to read in its current form. I suggest to try and put Age group, Gender, Virus status, Mood disorders, haemophilia, CCI score and Duration of infection as columns on the first line, and Hepatic, Univariate, Multivariate, extrahepatic.... as lines. That could make the table easier to read.”

Table revised as requested.
Reviewer 2 (Slawomir Chlabicz):

Point 8: “Background section of the Abstract should include not only the context but also the aim of the study Discretionary Revisions”.

Abstract has been revised to incorporate the study aim as requested.

I hope that this revised version of the manuscript meets the requirements of the journal and the reviewers. Please do not hesitate to contact me with any queries regarding the above or for any further clarification.

Yours Sincerely,

Olivia McKenna.