Article details: 2014-0041	
Title	
Authors	Maxim Trubnikov, Ping Yan, Jane Njihia, Chris Archibald
Reviewer 1	Ming Fu Liu
Institution	Calgary Health Region, Health System Analysis Unit; Quality, Safety and Health Information
General comments	General Comments: This study made a contribution to the cohort study literature. However, major modifications are needed to make improvements in both the description of statistical methodology and the exposition of the paper. Exposition: The procedure of Selvin (2004) was used to assess birth cohort effect as an age group by reporting period interaction. This is an important point for the analysis. This statement should be given immediately when the method was described to give the reader a clear picture of the method rather than being given in discussion section. The use of Terminology: **The term "cohort" was used to refer to both "birth cohort" and "age group" in the text** and in the tables and it was used inconsistently, and therefore caused many confusions. **It is preferred that "cohort" should be used for "birth cohort" only. Even though it is acceptable but not preferable for the authors to use "age cohort" for "age group" in some cases, the term "cohort" should never be used for "age group" in the text or the tables to avoid confusion with "birth cohort". The authors need to edit the text and** tables all through carefully and correct the misused terms accordingly. Statistical modeling: Log-logit transformation, i.e. $f(p) = \log(-\log(p/1-p))$ was used to transform HCV. But no explanation or justification was provided for such a transformation which did not appear in the literature. In the major reference, Selvin (2004) used $f(p) = \log(p)$. When a more complicated transformation is used, some justifications should be given and the parameters estimated should be properly interpreted. For example, if the $f(p) = \log(p)$ is used for transformation, the regression coefficient is interpreted as rate ratio. The researchers may have encountered some computational difficulties when simple transformations were tried. An extended discussion will be helpful. An interaction term in a statistical model is not a single factor. The model should be called two-factor with interaction model. Specific Comment: Page 6 line 20: **"1995 through 2000 accounting for ..." should be "1996 through 2000 accounting for ...".** Reference 26: Page 6 line 51 to Page 7 line 3: **"..., the coefficients suggested a decrease in ..."** The statement is not clear. More explanations are needed. This is related the model parameter interpretation. Page 7 lines 6 to 12: This statement is not clear. The last column of Table 4 shows almost all sources of variation are significant. Given the fact that HCV rate was log-logit transformed, the meaning of **"stable additive effect" may be quite different for different readers. Explanation is needed.** Page 7 lines 14 to 26. This is not useful at all. The residuals were obtained by subtracting the effects of age group and reporting period from the observations (log-logit transformed). That the same model was used again on the residuals did not provide any extra information. The model seemed to explain more variation simply because that the variation in residuals was much less than the variation on the observations as variations due to age group and reporting period were removed from the observations. Page 7 line 31: **"... the magnitude of the associations" should be "... the magnitude of the interactions"** Page 7 lines 49 to 52: **"The period component ... the interaction of the two."** On what evidence is this statement based? Page 14 lines 22 to 28: The link is broken. Page 21 lines 58:

	"cohort on LN-transformed …" **Log has been used throughout the paper, this is the only occasion that LN is used. "log-transformed …"** should be used to keep consistence.
Reviewer 2	Jason Grebely
Institution	The University of New South Wales, The Kirby Institute for Infection and Immunity in Society
General comments	Studies providing information to inform optimal strategies to enhance hepatitis C virus (HCV) testing in Canada are essential. This is particularly important given recent **recommendations from the United States for birth cohort screening in "baby boomers (those born between 1945 and 1965)" based on data that this is a cost-effective and feasible approach for enhancing the proportion diagnosed in the United States.** This study by Trubnikov et al investigates the association of birth year and reporting period with rates of reported HCV cases from the Canadian Notifiable Diseases Surveillance System (CNDSS). The authors demonstrate that compared to those born in 1941-1945, males born between 1946 and 1970 had 21% to 40% higher rates of HCV notification and females born between 1946 and 1975 had 12% to 43% higher rates of HCV notification. The pattern does look somewhat different to the United States, given that the early post-baby boomer period (up to 1970 in males and up to 1975 in females) is at high risk as well. This suggests that incidence continued to be high through the late 80s and early 90s. The major strengths of this study are that it is novel (for a Canadian setting), the methodology is sound and that these data are needed to inform strategies for HCV testing in Canada (in the absence of a large population-based study to evaluate the prevalence of HCV among different birth cohorts in the Canadian population). However, as it is written, it is very technical for the broader readership of CMAJ. Also, there is some important information that should be included to better inform the reader about the surveillance system that has been used. Also, the discussion could be greatly improved to better place these results in the context of the current setting, with a stronger discussion of the limitations of this analysis. Major points: 1) A description of the CNDSS is crucial for this paper. What is the system for the inclusion of notifications into CNDSS? How many notifications per year are reported to the CNDSS (could be at least added as Supplementary Material)? This information should be supplied in the methods. Potential biases of the CNDSS notification system should be discussed in the discussion. 2) Do the authors know the estimated proportion of notifications which make up the total estimated HCV infected population (e.g. what is the proportion with diagnosed infection that are identified through this surveillance system)? In Canada, the total undiagnosed population is much lower than the United States and this is a key issue which needs to be discussed when considering comparisons of birth cohort screening between Canada and the United States. If a greater proportion of the population is undiagnosed (as in the United States), it has a great impact on whether targeted **screening to "baby boomers" will be cost-**effective. As such, the recommendations for baby boomer screening in Canada needs to consider this and this should be added to the discussion. 3) The observed trends in HCV incidence very much depend on HCV testing patterns. So, if younger at-risk people are not getting tested in more recent years, it will have a large impact. Do the authors know of any data in Canada on HCV testing patterns among younger at-risk individuals over this period? 4) The Discussion could be improved to better place these results in the context of the issue at hand, namely testing of HCV among Canadian baby boomers. Specifically, it would be good to compare these results to what is known in the US and from other settings about rates of HCV using a similar methodology (or looking at similar trends). Also, it would be good to touch on the public health and policy implications of these findings for Canada. Lastly, the discussion is not in the format which is generally accepted for CMAJ (main findings; explanation and comparison with other studies; limitations; and conclusions and implications for practice and future research).