**Reviewer’s report**

**Title:** Incident Gout and Chronic Kidney Disease: Healthcare Utilization and Survival

**Version:** 0  **Date:** 30 Nov 2018

**Reviewer:** Sara Muller

**Reviewer's report:**

**Abstract**

I would like to see more number in the abstract. Specifically, how many people were in the CKD and non-CKD groups?

The Results section is quite muddled up and jumps around a lot. It would help to be clearer.

**Background**

There is some terminology that might be best changed. For example, it would be better not to refer to gout "sufferers", and describing those with and without CKD as routine and complex patients is probably unhelpful - I would just say with and without CKD.

**Study population**

I don't understand the sentence on lines 149-150. How can you exclude data from subsequent years, when you don't have this data?

Is the SES at the end of follow-up all that is available, as this is technically inappropriate, as it is measured after exposure and so could be consider to be on any causal pathway? I doubt this makes much material difference, but should probably be discussed.

**Statistical analysis**

The use of GEE models seems reasonable for the question, but I wonder about the use of the Normal distribution for the purchase of allopurinol, is this not count data?

I agree that updating the models for the onset of CKD would change the question, but I wonder if the failure to do so makes the question less clinically relevant? The authors should discuss this more. If the non-CKD people develop CKD then this is likely to bias any association towards the null.
There is no mention of testing the proportional hazards assumption. Whilst the Cox model is fairly robust to departures from proportional hazards, this should still be checked.

Results

I do not understand how sUA can be considered controlled at diagnosis if the person has a new diagnosis of gout (line 268-270).

I thought people with gout-related prescriptions before the index date had been excluded (line 271-272).

Rather than fitting separate models for the different age groups, did the authors consider fitting an interaction term? This may have been more appropriate.

I am not convinced that the survival findings are necessary if this paper is about health care utilisation.

Tables

It is not necessary to include mean and median in Table 1.

I cannot make sense of Table 2. What are the units.

Figure 1 would be much more informative as a table with confidence intervals.

I am concerned by the number of people to whom analysis is attributed (line 416).

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

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