The author analyzes the computation of provisions using two techniques and comparing the results. One of the two techniques, namely the micro-level stochastic model, is deeply introduced and explained from a theoretical point of view. I think that the thesis should be further polished in many points. Nevertheless, the theoretical part is well done and relatively easy to follow. I have some concerns for the empirical section where many assumptions (eventually unrealistic) are made and some interesting parts introduced theoretically are ignored preferring more naïve approaches. I think that the author does not discuss enough the implication of these assumptions. Moreover, some results are interpreted in a non-intuitive way in my eyes.

I do think that the thesis fulfill the standards for a Master Thesis at Charles University.

Here, I list in more detail my comments and doubts:

Abstract and Introduction:

1. The abstract should be more self-reading. i.e. even a non-expert reader should be able to get an idea of the topic of the thesis. In the abstract, I would avoid non-explained acronym as MTPL. Same for the titles of Chapter and Sections (CKP, RBNS).
2. The keywords "micro-level" and "reserving" are not clear.
3. Some more references in the introduction are needed, for instance, the sentence "Traditional estimation methods based on cumulative triangles" should refer to some book, at least. And, also, the concept of "micro-level stochastic model" should have a proper reference. Moreover, I think that the Introduction does not clarify the content and the contribution of the thesis enough. Indeed, the Introduction would need some sentences from the beginning of each Chapter to make more clear the idea of the thesis and of the tools used in the investigation. Again, the acronym must be defined at the first citation, e.g. IBNR.

Chapter 1:

4. Same issue as before for acronyms RBNS, IBNR and UPR. Moreover, a reference with a detailed description would help. The sentence "There have been many methods proposed for calculation of these provisions" also needs a reference. Sometimes, the author uses the term "micro-level stochastic model", other times, the author writes "micro-level stochastic reserving model": are the same? In case, the use of homogenous terminology would help to not create confusion. There are some references about micro-level stochastic model at the end of the paragraph, but they should be closer to their first mention.
5. The words "mass character" and "which are homogeneous in underwritten risk" are not clear.
6. The Remark about the dimension of the dataset is vague. It would be nice to have an order of magnitude of the set such that the asymptotic results are guaranteed with some reference to support it.
7. The definition of $w(t)$ is not clear. Does the variable t represent the time? If so, does the exposure to risk of the portfolio depend only on the time? This is quite strange. The author should elaborate more and, in case, highlight more assumptions. It is enough the assumption that the portfolio is homogeneous to say that the function $w(t)$ is proportional to the number of policies? Or the risks carried by the policies need also to be iid?
8. Why property (i) of Definition 1 must hold a.s.?
9. In Theorem 3, maybe it is $E[N_t]$? Or $E(N_t)$? The notation without brackets and with the space looks strange to me. Same in other points of the thesis.
10. I would like to see a more extended interpretation of the real impact of the assumption that Z_t and Z_s are independent from each other.
11. I cannot find anywhere in the whole thesis a precise and clear, even if concise, definition of what is a *micro-level stochastic model*… Did I miss it?

Chapter 2:

12. I understand the reason to merge the Health damage and the Annuity but it would be nice to have the number of each of them before the merging. Moreover, why this merge does not compromise the results due to heterogeneity while the merge of the Material damage would? I would like to see in Table 2.1 also the split between Health damage and Annuity to justify the merge due to homogeneity.
13. It is not clear what are the LoBs. Please, explain the acronym.
14. Looking at Table 2.1, it is not clear the unit measure of the quantities reported in the table. Is it money? CZK? Euro? Same issue for other Tables and related Figures.
15. In formula (2.1) I would not use \lambda unless it really refers to the \lambda of the Poisson process described in previous chapter.
16. I think that Czech Republic must have also the capitol R.
17. After description of all the statistics related to the dataset from 2000 to 2017, it was very surprising to read the sentence "Due to lack of data on risk exposure for period between 2000 and 2003, we will leave it out of further considerations". So, it means that the data of the empirical analysis are not the same that gives the previous statistics? In case, I think that the sentence should be stated at the very beginning of the section and all the empirical analysis (statistics included) should refer to a unique period (2003-2017). Similarly, for the sentence "Since it is unlikely that all of the claims that have occurred in the most recent years have already been reported, we will use different upper integration bound that corresponds to 31st December 2014".
18. Figures 2.4 and 2.5: it is not clear the impact of the assumption of piece-linear function. Moreover, it is really not clear why the functions goes to zero. Finally, and more relevant, is there a reason why the two figures look exactly the same, expect for a re-scaling of the y-axis?

Chapter 3:

19. The assumptions of independence made at the beginning seems to be very strong. I understand that considering the joint structure of the distributions would increase significantly the complexity of the model, but still the author should give some hints of the loss of information, and how much this could impact on the results, or, at least, to describe a real setting in which the independency could be reasonable.
20. It is well-known that the choice of the distribution function that better fits the data is a hard job, and, at a certain point, one must choose one distribution. Still, the Q-Q plots indicate that the chosen distributions are never able to catch the shape of the tail of the real data. I'm wondering how much this further approximation could affect the results. I would like to see more comments on this. Maybe the author could propose possible extensions considering Alpha-Stable distributions to fit better the tails.
21. The captions of all Tables and Figures are not clear and do not allow to identify uniquely each Table and each Figure.
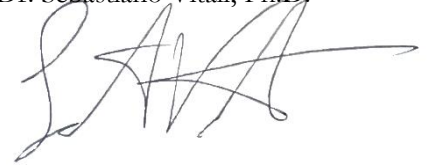
Chapter 4:

22. It is not clear which parts of the approach described in the first Chapters are going to be substituted with the Monte Carlo approach. This does not introduce a further approximation in the method? Moreover, the algorithm to generate Monte Carlo simulations is hard to follow and the adoption of pseudo-code would facilitate the reading.
23. The Mack Chain-ladder model needs, at least, a reference. Moreover, the differences between the Mack Chain-ladder model and the micro-level approach are not clear.
24. Finally, I got lost with the comparison between the two approaches and the interpretation of the differences. The evidence of the heavy tail was not a genuine feature of the data that we were not able to catch by selecting the distribution? Therefore, I would say that the Mack Chain-ladder model is able to recover a realistic situation while the micro-level stochastic model is not. Instead, the author seems to appreciate more the thin tails. Could the author elaborate more on this?

Conclusion:

25. There is a double "caused", maybe even triple. I think that a second reading made by the author could avoid these mistakes.
26. I understand the suggestion of having an "observing period" to check whether the model works well or not. That is why, typically, the dataset is often split in a training set and in a testing set. But, as I understood, the author preferred to use the whole dataset as training set. Could the author elaborate on this?

29.7.2019   Dr. Sebastiano Vitali, Ph.D.