Reviewer's report

Title: Defatting of acetone leaf extract of Acacia karroo (Hayne) enhances its hypoglycaemic potential

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Reviewer: Ephrem Engidawork

Reviewer's report:

* This is an interesting study that attempted to examine the hypoglycemic effect of an extract from enzyme inhibition to downstream signaling pathways. However, there are some inconsistencies that should be explained.

* Qualitatively defatting of the plant material did not influence the presence of secondary metabolites. However, quantitative studies showed that defatting led to loss of phenolic compounds, although the authors did not say whether this loss was statistically significant.

* Following defatting, solvent mixtures appear to yield more secondary metabolites with antioxidant potential than single solvent. It would have been better had the authors used a combination of solvents.

* The authors used two methods for evaluating antioxidant capacity and defatting produced opposite effect in the two methods, a decrease in DPPH and an increased activity in the iron reducing assay (here also nothing was said about the statistical significance of the difference). The two assays usually tend to provide a similar trend, as antioxidant capacity in both methods involves electron-donating mechanisms. It would have been good had flavonoid content determination was done, as they also significantly contribute to antioxidant capacity.

* Defatting produced better α-amylase inhibition than non-defatted extract and polyphenols were implicated for the difference, although defatting tended to decrease polyphenols. Flavonoid content might have provided better explanation.

* Glucose uptake did not vary with defatting in the two concentrations used in muscle cell lines. However, variation was observed with the lower concentration in adipocytes, but not with the higher concentration. What could be the source of this variation?

* Extract and insulin combination treatment produced an inconsistent and at times antagonistic effect, casting doubt on the potential use of the extract for treatment of diabetes. This seemingly
antagonistic activity was not observed only in translocation but on kinases determined in the study. I think this issue should be looked at seriously and the authors need to come up with an acceptable explanation. Moreover, it would be good if the authors show the link between the kinases determined and insulin signaling pathways or blood glucose regulation.

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

Yes

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.

Yes

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I am able to assess the statistics

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