12 September 2016

Dear Editor,

I am pleased to submit a revision for “An Algorithm for the von Bertalanffy Seasonal Cessation in Growth Function of Pauly et al. (1992)” to be further considered as a Technical Note to *Fisheries Research*. The revisions in this manuscript are based on comments from two anonymous reviewers sent to me on 2-Sep-16 and on my rereading of the manuscript. Direct responses to both reviewers and a detailed list of changes to the manuscript are in the attached pages. I hope that you will find that the modifications to the manuscript both address the reviewers’ concerns and have strengthened the document. **Please feel free to contact me if you have any questions or concerns related to this submission.**

**Respectfully,**

****

Dr. Derek H. Ogle

Professor of Mathematical Sciences and Natural Resources

**Reviewer #1: Technical comments:  
  
1. The methods section describes how starting values were selected manually or with the R function vbStarts. Were various starting values tried to ensure that nls achieved a globally optimal solution? This is recommended in the help file of vbStarts, but is more generally just good practice when using any optimizer.**

DHO: I had not tried various starting values. However, I have now tried three other sets of starting values for each model fit and shown these results at the end of the Supplementary R code. Some alternative starting values led to a lack of convergence. In a few instances, alternative starting values led to different parameter estimates. However, in all of these instances, the overall fit (as measured by the RSS) was worse than for models with the reported parameter estimates. I did not report these results in the manuscript as I do not want to distract the reader from my main point (how to fit the Pauly et al. model). However, as mentioned above, this code is included in the Supplementary R code.

Also, relative to changes related to the reviewer’s next comment, I no longer used the vbStarts() function to derive starting values. The methods were modified to describe this change. **2. I'd like to thank the author for providing the Supplementary R code. The code is easy to follow and well commented, and will be a useful reference for anyone attempting to apply the algorithm to other data sets. Unfortunately, when I ran the code, nls did not converge in two cases, showing the error message, "Convergence failure: false convergence (8)." (I'm using Rv3.3.0, which is the same version cited by the manuscript.) In the first case (Mosquitofish site 2, equation 3), I altered the starting values and was able to achieve convergence (same fixed ts and NGT starting values as for Bonito, and same site 2 results as shown in Table 1). Strangely, if I gave the solution (from Table 1) as starting values, it still failed to converge. In the second case (Mosquitofish site 4, equation 3), I was unable to find starting values that would result in successful convergence. I'm not sure where the problem lies, but this should be investigated, and perhaps the code could be made more robust before becoming publically available.**

DHO: I can not fully confirm this, but I believe this issue may be related to the reviewer using a different operating system as a colleague noted the same problems using a Macintosh machine running 64-bit R. After further investigation, I believe that this problem largely stemmed from poor starting values. As such, I have used new starting values that were derived by visually fitting the VBGF rather than using the vbStarts() function. With these new starting values, I have not been able to recreate the convergence problems noted by this reviewer on my Windows machine, several other Windows machines, or my colleagues Mac machine. **3. I'm not aware of any theory that justifies using AIC when fitting by nonlinear least squares. It's typically used when fitting with maximum likelihood. That said, from a practical viewpoint, the application of AIC is internally consistent here, so I wouldn't expect model ranking to differ if maximum likelihood were used. However, it is possible that relative differences among models might change. I think this topic should be addressed in the manuscript, either by fitting with maximum likelihood, or by justifying the validity of AIC when using nonlinear least squares.**

DHO: My methods follow the use of AIC for nonlinear models described in Ritz and Streibig (2008; p. 107), which I now cite in the revised manuscript. In addition, the likelihood values can be computed directly from the RSS values if, as done here, the residuals are assumed to be i.i.d. from a normal distribution. With this, the AIC can be computed from the RSS (or with the least-squares method).

**4. In the Bonito example, it appears from Table 1 that the estimate of C in Equation 2 was at the upper bound of 1.0, in the original fit and in numerous bootstrap iterations. In some cases, such behavior can indicate that the optimizer got "stuck" at a local, rather than global, minimum. If that's an issue here, a logit transformation might help, such that the estimation is done in unbounded space, but is then transformed back into (0, 1) space. Or, the value of C might be fixed at C=1, in which case there would be one fewer estimated parameters, and consequently a lower AIC value. In this example, the lower AIC would likely result in a shift of which model was best.**

DHO: In this situation, I followed the procedure used in Stewart et al. from where the Bonito data originated. I agree with the reviewer’s comment (and I would not have constrained C), but I was attempting to show how, if the scientist is going to constrain C to be less than 1 as Stewart et al. did and the model results in C=1 that the Pauly et al. model may be a useful alternative. Thus, I have chosen to leave the model fitting process as in the original manuscript. **5. When looking at the data in Figure 3, I wondered whether they would be explained equally well (or better) by the common, non-seasonal von Bertalanffy model. And I think the answer is no. I fitted the common model for the Bonito data and Mosquitofish site 4 data, and the AIC values (based on nls for comparison) were higher than those of the seasonal models in both cases (Bonito AIC = 1444, Mosquitofish site 4 AIC = 4170). I didn't examine the other two data sets, but including this model alongside Equations 2 and 3 could strengthen support for the seasonal models. It might also be good practice for other studies, and therefore worth demonstrating in this paper, at the author's discretion.**

DHO: It is not my intent with this manuscript to strengthen “support for seasonal models”, rather it is simply to provide a method to compute the Pauly et al. model. However, I believe that the reviewer’s comment that it is good practice to include the typical VBGF for comparison to seasonal models. Thus, I have added the typical VBGF to the results (which required modifying the format of Table 1), to Figure 1, and the R Supplementary Code. Furthermore, it should be noted that the bootstrapped confidence intervals in Table 1 were very slightly modified from the original manuscript because the random seed used to control the bootstrap results was modified by the addition of this new model. **Reviewer #1: Editorial comments:  
  
44. This line could mention that the common von Bertalanffy function occurs as a special case when C=0 (i.e., equation 2 collapses to equation 1).**

DHO: Done.  
 **81-86. This section describes how the manuscript could fill a critical gap in the current state of knowledge. However, it's not really clear to me what exactly the prior deficiency is. I understand that t' is a function of estimated parameters. But, is this fundamentally different from many other nonlinear optimization problems where values are functions of estimated parameters?**

DHO: I have attempted to modify this section to more clearly articulate the issue. The real problem is not so much that t’ is a function of the estimated parameters (which I had stated in the original manuscript), but that t’ must be derived from t, NGT, and ts and there is no mathematical equation that describes how to do this. Unfortunately, in the absence of this equation, there also is no algorithm (prior to this manuscript) that describes (in any detail) how to derived t’ from t, NGT, and ts. So, the issue is really the complexity of the relationship between t and t’ and not the dependence of t’ on estimated parameters. Hopefully the new text better articulates this issue. **103-4. I was confused initially about what becomes a whole number, mostly because the example describes converting t=2.9 to 2.5, which isn't whole. I understood after reading step 2, but perhaps some more careful wording in step 1 would help avoid that confusion.**

DHO: Generally removed reference to whole numbers as it is more clear to call this the “number of completed full growth periods.”  
 **141. "I used …"**

DHO: Fixed.  
 **145. "divided by the quantity 1 minus …**

DHO: This was removed because a different procedure was used to derive starting values.  
 **156. Equation 3 fit slightly better, but the two models are really indistinguishable, given the common criterion that delta AIC < 2 for Equation 2.**

DHO: Text was modified to address this correct concern. In addition, delta AIC values were added to Table 1 to better facilitate such comparisons. **Table 1. Mention in the table caption that the values in parentheses represent 95% confidence intervals, as stated in the Fig 3 caption.**

DHO: Done. **Figure 2. I think the tick labels above and below the X-axis would be easier to read if the font size were a little smaller. I find the numbers somewhat difficult to distinguish.**

DHO: Done.

**Reviewer #2: remarks and suggestions:  
  
## Introduction  
- Line 75. I agree that the way Beguer et al. (2011) fitted the function is not clear in the paper. Indeed, the equation was not really the one of Pauly as the authors dropped the implementation of t' because of the lack a comprehension of the original paper... . By the way, Linf was not fixed, but constrained so that it can not be upper than maximum L observed. The only difference with Pauly is that there is no loop-calculation of t'.   
Maybe a way to present it would be similar to the following one. This would also help to argue in favor of the present technical note to make this t' calculation clearer.  
"...whereas, probably because of the lack of clarity of Pauly et al. (1992) on t', Beguer et al. used a modified version of Equation 3 without (the loop-calculation of) t', simply replaced by t in their equation."**

DHO: I have re-read the Beguer et al. (2011) paper and it clearly states that they used the Pauly et al. (1992) model, even displaying the model in their Equations 2-4 (though, their interpretation of the parameters do not appear to be correct). In addition, on the second column of their page 607 it says “Contrary to fits within the classical VBG, the asymptotic length (Linf) of the seasonal VBG was fixed.” There is no indication of how their t’ was calculated (i.e., it did not suggest that my Equation 3 was modified replacing t’ with t). In other words, none of what the reviewer says in this comment was evident from their paper.

I had contacted the first two authors (Beguer and Rochette) of the Beguer et al. (2011) paper when working on my manuscript. In that process, I reviewed their R script and did note that they did not actually fit the Pauly et al. (1992) model as their paper suggested. I considered using a personal communications in the current manuscript to note that Beguer et al. (2011) did not actually use the Pauly et al. (1992) model. However, in further communications with Rochette, who appeared to do the analysis for that paper, he noted that he had moved to another position and would not have time to pursue my questions of their work any further. To honor this, I did not pursue the personal communications angle and wrote the text that appears in the manuscript, which I believe to be accurate based on the published works of Beguer et al. (2011).

So, I am willing to make the changes that this reviewer has suggested, but I don’t know how to cite it, as the information this reviewer suggests is not available in the Beguer et al. (2011) paper. **## 2.1. Calculating t'  
- Line 95. ...the calculation of t' in equation 3 depends on...**

DHO: Fixed. **- The six steps algorithm. The text of the six steps is well written but is difficult to keep in mind. As the author present this technical note in a way that equation 3 could be implemented in any nonlinear model fitting software, the text should help in presenting the equations of the loop calculation. Moreover, the examples chosen should be as much as possible refering to values chosen for Figure 2, which really helps the comprehension of the text. To not show to many equations, a balance could be found between clarifying the steps using representation (and values) of Figure 2, and using inline equations.**

DHO: The steps of this algorithm do not ultimately result in a useful or simple equation. However, I take this comment as the reviewer wanting more clarity for the calculation of t’. I have attempted to provide more clarity by (a) providing example calculations for two ages underneath each step and (b) choosing ages and example parameter values that match Figure 2 (as suggested by this reviewer). From this, I also simplified (and, hopefully, clarified) some of the language in all steps and moved some of the information in Step 1 into the preceding paragraph.

**## 2.2.  
- Line 131. ... more appropriate fit than ... (not then, I guess)**

DHO: Fixed. **- Line 138. This part of the sentence is grammatically strange to me : "Data from three locations were chosen to examine here to demonstrate..."**

DHO: Modified. **## 3. Results … Line 156 - 160. Australian bonito data. Even if the AIC is lower, I would not say it is better with such a small difference. Parameter estimation are also quite similar and as the confidence intervals are provided, we can not really say that t0 are different. To me, the conclusion of this case is that there is no best model.**

DHO: Similar comment to Reviewer #1 (for line 156). See response there. **## 4. Conclusion  
- Conclusion is probably too fast. I agree saying that "equation 3 is likely not the globally best seasonal growth model", in particular with regards to the example of mosquitofish. In terms of purely statistical index using the AIC, Equation 2 is globally better. But, as mentionned in the results, Eq. 2 also respond "too dramatically" to part of the sample at site 2. Ecologically speaking, this drop should be questionned (but this is not the aim of the paper).  
Equation 2 surely allows more flexibility in the fit of 'seasonnally varying' growth data. But, depending on species studied, the decline in size could be unreliable (in contrast to weight maybe). This requires a finer analysis of bounds of C, even fix C to 1.  
I would say that Equation 3 is doing well for what it is supposed to do (as title suggests) : fit a growth with periods of "seasonal cessation in growth", which means size is not changing for a fixed period of time each year. However, using Equation 2 allows for more flexibility, thanks to parameter C that allows seasonal decrease, cessation or modified increase in size (or in weigth). When C=1, example of this paper showed similar results with Pauly equation. However, as shown by fit in Site 2, using a more flexible equation requires more caution in interpretation.**

DHO: I am not sure that there are any modifications I need to make relative to this comment. I don’t know what “Conclusion is probably too fast” means. The remained either fees like “discussion” that I hope to spur with this technical note or comments that are beyond the narrow scope and data of this short technical note. Thus, I have made not changes to the manuscript based on this comment. **## Figure 2 … - As 'Winter Point (WP)' is not a parameter of the equation, it is called WP in a paragraph lost between the two equations. Could you remind the reader in the legend of this figure that WP is the Winter Point, to not search for it in the text. For the other parameters, we know where to look at them in the text as they are in the equation.**

DHO: Done.

**Documentation of Changes (line numbers from original manuscript)**

31 – Removed “seasonal” … see comment for lines 90-91.

32 – Added comma after “Schnute and Fournier”.

36 – Added comma after “Bacon et al.”.

44 – In response to reviewer #1, changed “(C=0)” to “(i.e., reduces to Equation 1; =0)”.

45 – Removed “for” from “(for 0<C<1)”.

52 – Added period after “al” in “Huusko et al” and comma after “Huusko et al. (2011)”.

58 – Added comma after “Pauly et al.”.

83-86 – In response to reviewer #1, attempted to better explain why computing t’ is complicated. This resulted in a major modification to these sentences.

90-91 – Deleted “seasonal” and changed “Equation 2” to “Equations 1 and 2”. Thus, this statement is not focused on only seasonal growth models, but includes more general growth models. This changes follows the suggestion from reviewer #1 to include the typical VBGF.

95 – Added “in Equation 3” before “depends” in response to reviewer #2.

97-118 – Major modifications to the text describing the six steps and the paragraph leading into those steps. This is in response to reviewer #2. Section 2.1 now covers lines 97 to 130, whereas it covered lines 94 to 123 in the original manuscript. The editors/typesetters may have a better way to present the calculations (i.e., rather than bullets underneath the numerical headers), but these changes for clarification did not result in a substantially larger section.

121 – Added comma after “Team”.

122 – Added version number for FSA.

131 – Per reviewer #2, correctly changed “then” to “than”.

137 – Per reviewer #2, changed the first phrase of this sentence to read more easily.

141 – Per reviewer #1, changed this sentence so that it does not use “We used”.

142-146 – The description of the starting values was greatly modified as vbStarts() is no longer used. See my responses to reviewer #1 with regard to convergence issues in the R Supplement. Required adding the Ritz and Streibig (2008) citation.

148 – Added a sentence that multiple starting values were used to confirm finding the global minimum. Required adding the McCullough (2002) citation.

149 – In response to both reviewers, added a sentence that states that models with delta AIC values <2 are indistinguishable. This required adding the Burnham and Anderson (2002) citation.

152 – Added comma after “Baty et al.”.

156-160 – In response to both reviewers, rewrote these sentences to acknowledge that the fit of Equations 2 and 3 to these data were indistinguishable.

173-174 – In response to both reviewers, modified this sentence to acknowledge that Equation 3 was not the “best” model in 3 of the 4 test cases.

175 – Removed “the” in front of “seasonal”.

Acknowledgments – Added reviewers and my colleague that helped me test the R Supplementary code on other operating systems.

References

* Added Burnham and Anderson (2002).
* Added year and DOI for Huusko et al.
* Added McCullough (2008).
* Added DOI for Nickelson and Larson.
* Updated version number in R reference to v3.3.1.
* Added Ritz and Streibig (2008).

Table 1

* Had to completely reformat to include the results for the typical VBGF. Also added the delta AIC column.
* Values for confidence intervals were slightly changed due to inherent randomization in bootstrapping.
* Per reviewer #1, added note about 95% confidence intervals in caption.
* Modified caption to include the typical VBGF and delta AIC values.

Figures

* Slight modification of Figure 1 caption to make it clear that this was the Somers (1988) VBGF.
* Slight modification of Figure 2 caption to make it clear that this was the Pauly et al. (1992) VBGF. Also defined WP per reviewer #2.
* Added results for typical VBGF to Figure 3. Modified caption accordingly and made it clear which VBGFs corresponded to which Equation. Modified the font size on the axes as suggested by reviewer #1.