**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. **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: **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: **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 was following the modeling procedure shown in the Stewart et al. manuscript 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 constraint C to be less than 1 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: I have added these models to the results (which required modifying Table 1), to Figure 1, and the R Supplementary Code. **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: Fixed.  
 **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:  **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 above 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 thought of 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, (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.

Section 2.1 now covers lines 97 to 129, 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.

**## 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:  **## 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:  **## 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.