

# Rebuttal letter concerning the submission (#2014:04:1960:0:0)

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We are grateful to David Johnston, academic editor of PeerJ, for the opportunity to improve the manuscript. We also thank Alistair Dove and one anonymous reviewer for their valuable input.

## REVIEWER 1

Generally I found the paper well written. The authors seem to have been careful justifying their choices of models and data, and the story flowed well. I have only a few major questions/concerns described below (by order of importance):

### 1. Sampling effort bias.

- (a) In Fig. 3, could the curve observed be an artefact of sampling? Please include a similar curve (i.e., overall spatial coverage) for the sampling effort in the same chart.

RESPONSE: **Accepted & changed.** We replaced the figure in the manuscript by Fig. RL-1. This time we included a similar density curve corresponding to the search effort along the transect as well as an estimate of the boat density after being corrected for effort. The results and the conclusions did not change as a consequence; however because we are aware that our sampling scheme was not ideally balanced, as a word of caution, we included the following sentence in the Methods section: *We were only able to record spatial effort between October 2013 and December 2013, therefore the obtained boat distribution might only be an approximation.* We also slightly modified the Results section, it now reads: *Most of the boats visiting the MPA for whale shark tourism are encountered on a 5 km stretch between Nalaguraidhoo Island (Sun Island Resort & Spa) and Maamigili Island.*

- (b) Also, it would be good to see the sampling effort in the temporal charts (weekly and per season).

RESPONSE: **Accepted & partially changed.** We included replaced the Manuscript Table 1 by Table RL-1, which displays the number of surveys per weekday, year and season (our model explanatory categorical variables). However we decided not to include this information in the chart as suggested mainly because our modeling approach is relatively insensitive to sampling effort. First, Generalised Linear Models and Generalised Least Squares do not require balanced experimental designs; the parameters are estimated to minimise the likelihood function regardless of the number of samples in each category. Second, we used resampling methods (bootstrap and jackknife) to estimate the yearly statistics, which provides a way to account for any possible distortions caused by eventual

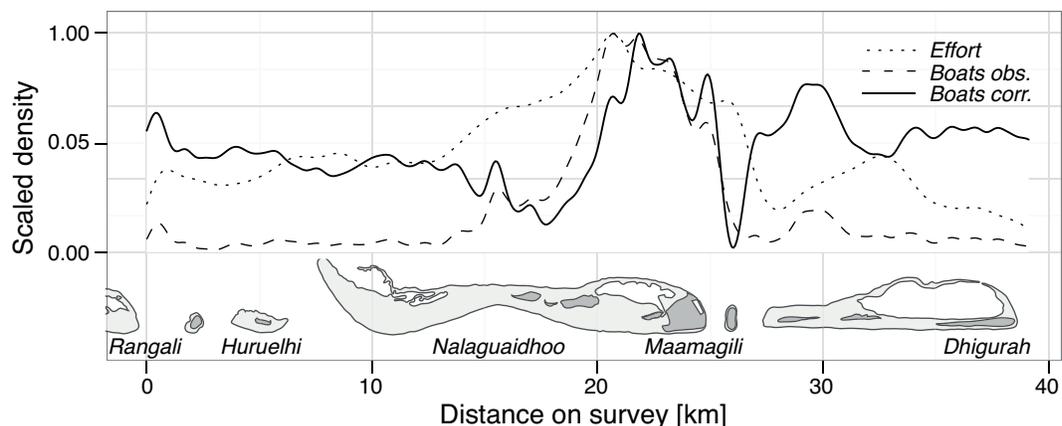


Figure RL-1. Scaled density of survey effort and observed touristic boats in the South Ari MPA.

**Table RL-1.** Number of surveys.

**(a)** During high season.

Year	Friday	Monday	Saturday	Sunday	Thursday	Tuesday	Wednesday	(all)
2011	3	2	2	6	2	3	5	23
2012	4	14	5	11	13	11	14	72
2013	1	18	4	12	14	20	18	87
(all)	8	34	11	29	29	34	37	182

**(b)** During low season

Year	Friday	Monday	Saturday	Sunday	Thursday	Tuesday	Wednesday	(all)
2011	0	0	0	0	0	0	0	0
2012	1	3	4	0	1	0	2	11
2013	1	8	3	4	2	6	7	31
(all)	2	11	7	4	3	6	9	42

low sample sizes in a specific period. The consequence is that differences in effort across different years, seasons or weeks won't produce biased estimates but will only affect the degree of confidence of the estimate (i.e. less effort means larger confidence intervals).

- (c) Authors did not seem to have accounted for this bias in the models – but it can highly influence results.

**RESPONSE: Not changed.** We accounted for sampling effort in the models and only confidence intervals would be influenced by relative differences in sampling effort. Please see previous response for more details.

2. Prediction results and procedure used.

- (a) It was not clear how the authors used the models to predict as there is not validation or prediction results shown (except for the one main result for each response variable). Is Table 5 a modelling result? Please clarify in the legend and present details on how you achieved these results.

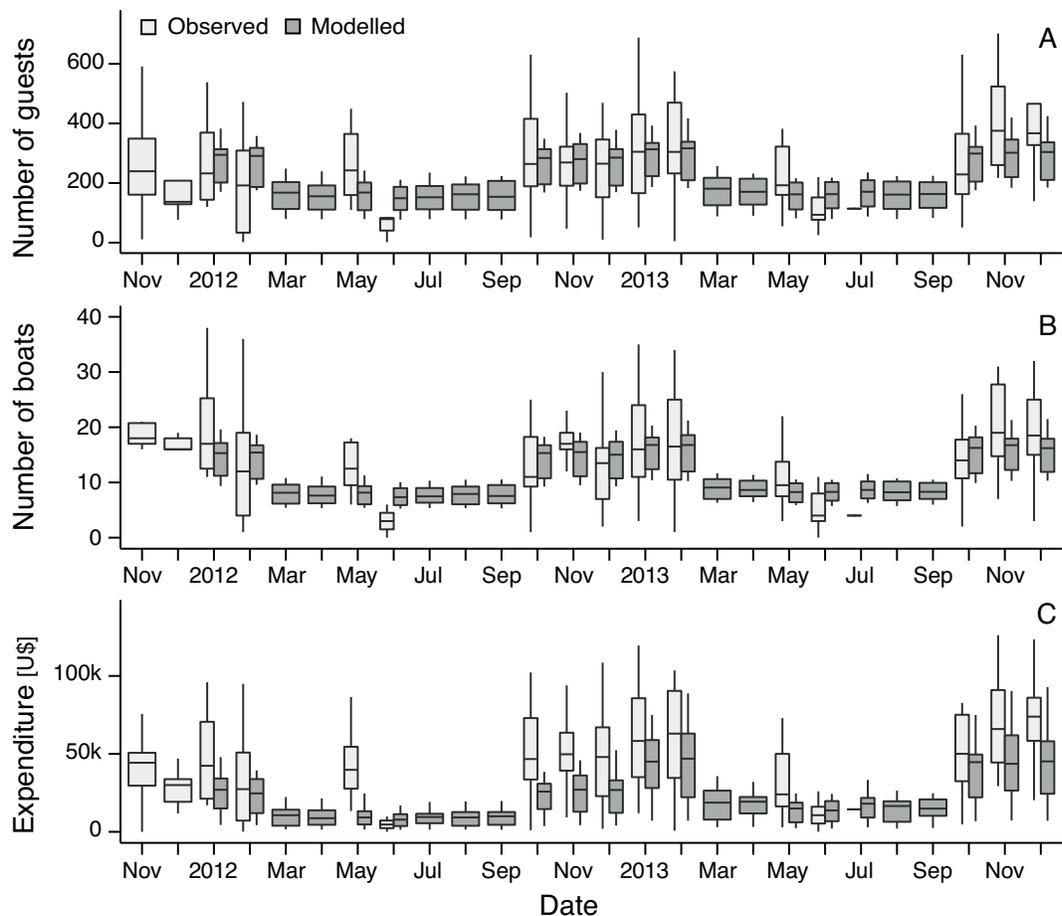
**RESPONSE: Accepted & changed.** Yes Table 5 is the main modeling result. We improved the caption and it now reads: *Yearly total expenditure and guests in the MPA calculated by adding daily model predictions within a year. Confidence intervals (CI) and standard errors (SE) were calculated by jackknifing the expenditure model and by bootstrapping the guest models.* We also clarified how we achieved these results in the text, please see the response to the next comment.

The model numerical results are also shown in the Supplemental Information (SI) section (Manuscript former Table S2 and S4). We decided to put them in this section because we already show them graphically in the manuscript (manuscript Figure 2) and because our aim is to obtain yearly estimates and not to analyse the relative importance of our model predictors. However, we have no problem moving those tables out from the SI section if the reviewer or the editor consider it best.

- (b) It seems that the model used the daily estimates obtained by the survey as response variable and then the authors predicted a response for each day of the year (?) and then added these all up to get to a year value? The procedure used needs to be clearly explained, and if the calculations were made per day, then these could be shown in a chart where you would also plot the sampled data for comparison.

**RESPONSE: Accepted & changed.** Response variables were properly indicated in the beginning of the data analysis section; there we wrote “We modeled six variables: daily number of vessels associated to tour operators (resorts and liveboards), daily number of visitors (from resorts, liveboards and total number of guests), and daily direct economic expenditure on whale shark excursions”. We recognise that the procedure could have been more clearly written.

We modified the final paragraph of the same section; it now reads *We used the models to daily predict the six response variables from January 1, 2012, to December 31, 2013, including those days*



**Figure RL-2.** Observed and modelled daily (a) number of guests, (b) number of boats and (c) expenditure aggregated by month.

when surveys were not conducted (due to limited sampling we did not predict any value for 2011). We then computed the annual number of visitors and annual expenditure by adding the daily results within each year. Because of the importance of quantifying the accuracy of our yearly estimates, we computed means and confidence intervals of the annual number of visitors by bootstrapping the models with 1000 replications. Due to the more complex parameterization of the expenditure model, we calculated the corresponding standard errors using the Jackknife method leaving one sample out at a time.

We also tried to produce the graphs the reviewer kindly suggests, however the display of daily results and observations produced a graph cluttered and hard to understand. To improve clarity, we instead aggregated the results in a monthly basis. We added Fig. RL-2 in the Supplemental Information section of the manuscript.

- (c) Because there were no surveys done in some months, I would suggest the authors to be careful presenting predictions for those months. Perhaps, show values with and without those months included.

**RESPONSE: Not changed.** We gratefully appreciate the prudent approach suggested by the reviewer. However it is not possible to present results excluding some months because they are not an unit of analysis in our models. In fact, we don't present predictions for any specific month in the manuscript, instead our predictions are limited to the daily bins used in the models, and the yearly bins in which we aggregated the daily results using bootstrapping or jackknifing. We carefully chose our units of analysis to be season, weekday and year because we performed surveys in all those periods and because we believe they capture best the variability in the response variables.

3. All tables, equations and charts: need better descriptions and need to include the meaning of all symbols used. E.g., eq. 1 is not explained in the text. Table S1, does not mention the meaning of symbols.

RESPONSE: **Accepted & changed.** We changed and improved the captions in Table 4, 5 and S1 as well as Figure 2 and 3. We also modified the the paragraph that includes the equation to better explain the nomenclature: *Daily direct expenditure on whale shark excursions (E) was calculated based on the most parsimonious model (Supplemental Information Table S1):*

$$\log(E + 1) \sim 1 + w + s + y + u \quad \text{var}(\varepsilon_i) = \sigma_s^2 \times \sigma_w^2 \times \sigma_y^2 \quad \varepsilon_t = \phi_1 \varepsilon_{t-1} + \eta_t \quad (1)$$

where *Day of the Week (w)*, *Season (s)*, *Year (y)* and *Wind Speed (u)* are the fixed effects, and the variance of the residuals ( $\text{var}(\varepsilon_i)$ ) is allowed to be different for each category of *w*, *s* and *y* ( $\sigma_s^2$ ,  $\sigma_w^2$ ,  $\sigma_y^2$ ). The model also takes into account the temporal autocorrelation; the residuals at time *t* ( $\varepsilon_t$ ) are a function of the autoregressive parameter of first order ( $\phi_1 = 0.123$ ), the residuals of the previous observation ( $\varepsilon_{t-1}$ ) and noise. Detailed results of the model estimates can be found in Table S2).

4. Economic analysis: A better introduction to the different types of economic analysis could be included in the introduction to make it clear that this approach is new. It starts directly with ‘direct spend’ without previous introduction to the subject. Also, I suggest using italics when referring to ‘direct spend’ to make it clear that the reference is to a method.

RESPONSE: **Accepted & changed.** We modified the paragraph in the introduction so that it follows a logical order and the direct spend method is not presented surprisedly. It now reads *Tourism revenue can be considered a type of non-consumptive direct use value (for a description of value types see Turner, 2003). The value of a natural location or a non-consumptive activity can be evaluated from a non-market perspective by using contingent (e.g. willingness to pay) and travel cost methods, or complimentary by using market-based valuations like those obtained by measuring expenditure. The direct spend method, which has been previously used to evaluate the impact of elasmobranch watching, provides a “minimal very conservative estimate of the economic value of natural areas”...* As suggested, we also used italics throughout the paper when we want to indicate that we are referring to the method.

5. Title: could be shortened and / or improved. Suggestion: ‘Economic impact of whale shark tourism in South Ari Atoll, Maldives’

RESPONSE: **Accepted & changed.** We are very thankful for this observation. We modified the title to “Whale shark economics: a valuation of wildlife tourism in South Ari Atoll, Maldives”. This new title is slightly shorter and we believe is more descriptive, memorable and appealing.

#### **Minor comments**

**Abstract** change ‘2012-1013’ to ‘2012 and 2013’.

RESPONSE: **Accepted & changed.**

**Abstract** Replace ‘72 to 798 thousand’ with ‘72 000 -78 000’ (throughout the paper).

RESPONSE: **Accepted & changed.** We modified as suggested in four locations in the manuscript.

**Lines 13/14 and 36** Include references.

RESPONSE: **Accepted & changed.**

**Line 73-75** I like this and think it could be developed in the first paragraph of Introduction (and then in the Discussion as well) to broaden the scope of the paper and its relevance.

RESPONSE: **Not changed.** The reviewer refers to the sentence “The results and recommendations we provide can be used to enhance the management of whale shark tourism at this location and encourage similar valuation studies in other wildlife attractions around the world”. We are thankful for this suggestion, however we consider we already discuss that topic by providing suggestions on how management can be improved based on our results. Specifically we discuss (a) the weekly visitation pattern, the crowding it generates and how licensing and days distribution between stakeholders might alleviate this problem; (b) how economic results and previous research encourages the financing of the MPA through visitor fees; (c) the necessity of more efficient search

methods to reduce the spatial conglomeration of boats; and (d) the importance of biological and economic valuations in effective management. We believe that a further recommendations will distract from the main aim of our study.

**Line 208-209** was this info used as strategy for sampling? How does it bias the results?

RESPONSE: **Accepted & changed.** The reviewer refers to the sentence “Approximately 75% of the boats visiting the MPA for whale shark tourism are encountered on a 5 km stretch between Nalaguraidhoo Island (Sun Island Resort & Spa) and Maamigili Island”. We did not know this information before the sampling, so there was not any bias in the experimental design. We hope that by including the spatial effort and using it to correct boat density (as shown in Fig. RL-1) we properly address the concerns regarding spatial sampling bias. Please see comment 1a for more details.

**Line 218** which techniques?

RESPONSE: **Accepted & changed.** The reviewers refer to the sentence “By taking into account temporal autocorrelation and using techniques that allowed us to estimate uncertainty, we believe that our estimates can be statistically superior to valuations that select a sample of guests and average individual expenses ...”. Here we refer to bootstrap and jackknife, so we modified the sentence to make it clearer: *By taking into account temporal autocorrelation and using resampling techniques (bootstrapping and jackknifing) that allowed us to estimate uncertainty ...*

**Lines 249 – 253** not clear.

RESPONSE: **Accepted & changed.** The objective of that paragraph was to illustrate that even though whale shark tourism in Maldives started relatively recently, their guest share characteristics with the more mature market in Ningaloo. To make it clearer we changed the order in this paragraph. It now reads *In Maldives, with the emphasis on high-end resorts, the relative importance of diving has declined in recent years. Although less developed, its whale shark tourism industry shows some similarities with the mature whale shark industry at Ningaloo Reef, Australia. Catlin (2010) explain that in Ningaloo the visitor profile has shifted from a specialist tourist interested in wildlife experiences to a generalist visitor with greater interest in the non-wildlife aspects. As whale shark tourism becomes more popular in South Ari, tour operators must put emphasis on a high-quality experience rather than in the encounter itself, especially in an industry where word of mouth is the most important mode of promotion.*

## REVIEWER 2

This is a very well written article that addresses an important issue in the conservation management of marine megafauna ecotourism: how valuable is a living animal population to a nation's economy. Specifically, what direct value does whale shark ecotourism in South Ari Atoll, Maldives have? While I am not an economics researcher, the survey design seems appropriate and the statistical methods are similar to those used in scientific analyses. I can find no major flaws with the design, the analysis or the interpretation and have no substantive criticisms of the writing in this manuscript and so I recommend that it be published as is.

RESPONSE: We are thankful to Reviewer 2 for his encouraging words.