Review of Delord et al., "The challenges of independence: ontogeny of at-sea behaviour in a long-lived seabird", Revision 1

I appreciated the chance to review the revised version of this paper and apologize for the delay in providing my review.

I was impressed by the extensive work that went into this revision. Overall, the analytical changes made by the authors largely address my concerns related to the analysis itself. The PCA approach is a helpful way of condensing the different wet/dry variables while revealing some of the tradeoffs between them (e.g., duration vs. number of wet bouts). Meanwhile, the GAMMs account for among-individual variation and allow for continuous temporal processes, which makes the results clearer to interpret. The analysis in its current form generally seems sound.

However, I still experienced some confusion in contextualizing and interpreting the results, and had to read through the paper a few times to fully grasp them. I think this stems from the fact that the results and discussion aren't always linked back clearly to the hypotheses presented at the start of the paper. The PCA adds an extra layer of complication since one has to remember in some cases what each of the synthetic variables represent. I think this issue could be solved by some condensing and re-framing, particularly in the Introduction/Methods, as well as some thought to data depiction, particularly adding partial effects plots to more clearly show the contrasting effects of time, seasonality, and sex across different life stages. My overall suggestion would be to refer back to the hypotheses in structuring the results, tables/figures, and discussion. This would help the reader clearly link the findings of the paper with the ecology of the species and the authors' initial predictions.

Comments on the tables/figures

Tables 1-2: These two tables could potentially be combined to show definitions, sample sizes, and tracking durations for each group. Some of the information in Table 1 (particularly definitions and behavior) repeats information in the text and could be condensed.

Tables 3 & 5: I might suggest rearranging these so that the models are shown from lowest to highest AIC scores, and calculating Δ AIC values compared to the top model rather than the null. This makes it easier to see which model(s) received the most support and compare against other tested models.

Tables 7-12: I would suggest combining these into a single table and making it a supplementary table (since these comparisons are also covered in Table 6). Although measuring dimporphism is important and relevant, I'm not sure it's the main contribution of this study, and it seems overrepresented in the tables.

Figures 1-2: I think these figures present a useful contrast and appreciate that the authors listed the primary contributing variables on the Y-axis for each. It might also be helpful to briefly re-state in the caption what the primary variables were that structured each axis.

Additional figure/table suggestions:

Table S1: This table is very helpful, and I found myself referring to it a lot while reading. I might suggest making this part of the main document and minimizing as much as possible the description of the hypotheses in-text, since I found the table very easy and straightforward to interpret. References supporting each hypothesis could be incorporated into the table itself if needed. It also seems notable that the three columns in this table correspond approximately to the three principal components. I wonder if there is a way to make this clearer in the table and/or in the text of the article, since it will make interpretation of the results more straightforward if the reader can refer directly to this table to see how a given predictor was expected to behave and compare that with how it actually behaved. In general, creating clearer links from these hypotheses to the results and subsequent discussion would help to structure the flow of information. One minor point/question: I am not sure why a two-period activity schedule is assumed for adults (reduced activity during molt?). There seems to be some overlap between B and C. It might be simpler to confine Hypothesis C to seasonal effects (i.e., external environmental change) and Hypothesis B to molt/energetic effects (i.e., internal requirements), assuming that's appropriate.

Figure S6: This is also very useful and could be a main figure, since it directly shows the contrast between adults, immatures, and juveniles. Indeed, Figure S6 seems to directly correspond to Hypothesis A in Table 1. It would be great to add the main loading variables to the Y axis (as in Figures 1-2) and potentially add subfigures for PC2 and PC3, since the principal components correspond approximately to the three columns in Table S1. If possible, it would also be helpful to consider including additional figures corresponding to the other hypotheses (potentially partial effects plots for the corresponding covariates in the GAMMs) to make it easy for the reader to see which hypotheses were supported and where the study might have diverged from expectations.

Comments on the text:

Lines 691-727: This section could be considerably reduced and some of the information and references moved to Table S1.

Study Species and Data Loggers section (Lines 730-): This contains a lot of background information interspersed with methods, making it a bit hard to follow. I'd suggest focusing this section more explicitly on the methodology, with information on the species presented as necessary at the start of the section to define life stages.

Lines 733-744 and 751-776: More like background information; duplicated in Supplement – delete?

Lines 832-845: Information on the principal components and variable loadings could be summarized in a table to reduce text and allow for easy reference.

Lines 869-873: Explain why both T-tests and GLMs are needed?

Line 839: "components"

Line 877: Missing "R" before citation.

Results: I would suggest reorganizing this section to match the hypotheses. Lines 882-887 (and 906-911?) could be presented as an introductory paragraph, followed by separate paragraphs discussing

differences among stages, specific effects of time since departure, month/season (and differences among stages), and sex-specific effects, with a figure illustrating each paragraph.

Line 906: “juvenile Amsterdam albatrosses”

Line 918: This pattern is visible in Figure S6 (another reason it might be useful as a main figure!)

Lines 936-939: This could be rephrased to focus on what the study did do (rather than what it didn’t do). Something like “Our study allows us to compare foraging behaviour among life stages in a long-lived endangered seabird species, while also providing new insights into the development of foraging patterns in naïve individuals over a multi-year period.”

Lines 949-951: It might be helpful to show the equivalent of Figure S6 for PC2 and PC3 to illustrate this increasing similarity.

Lines 951-953: I wonder if it would be useful to present a comparison of juvenile behavior lagged by 1 year with non-lagged juvenile, adult, and immature behavior, to help illustrate the increasing similarity to other life stages while controlling for seasonality? Not sure if this is helpful; just a thought.

Line 989: Should be “to rapidly fly”

Line 997: Make section heading consistent with Table S1?

Lines 998-1002: Would suggest removing this sentence (repeats previous section) and focusing exclusively on seasonal/environmental changes across all groups.

Line 999: Should be “juvenile Amsterdam albatrosses”

Line 1000: “more adult-like behavior”

Lines 1007-1010: Were there any specific behavioral changes during the austral winter that might be linked to this seasonal change in productivity? Partial effects plots for the GAMMs showing the effect of month might be a helpful reference here for complex/non-linear changes.

Lines 1011-1017: I’m not sure if this is relevant, since the reproductive period was not included in this study. If the results of this study show evidence of carry-over effects in sabbatical adults, then it would be helpful to make that case here. Otherwise, I’d suggest deleting this paragraph.

Lines 1018-1032: Largely repeated in the Supplement – delete?

Line 1034: I would suggest making subheading titles and hypotheses consistent (e.g., use “Sex differences in activity” as the title for Hypothesis D)

Line 1038: The “body-size hypothesis” is mentioned here, but not explained earlier—I assume this is a relic of the previous version? Perhaps rephrase. I’m also not sure it makes sense to refer to this as a body size effect, since the sample was not sufficient to separate effects of body size from those of sex and sex is essentially used here as a proxy for body size. Even in non-dimorphic species, sexes sometimes behave differently, which suggests that behavioral differences could be related to sex-specific energetic requirements or physiology. It would be more accurate to say that the sexes differed, with a likely driver of that difference being dimorphism and concomitant effects on wing-loading.

Line 1046: Missing parenthesis.

Lines 1044-1054: I might suggest moving some or all of this section to the supplement, to focus more clearly on the results of this study and how they did or didn't support the hypothesis already described.

Lines 1063-1073: This paragraph provides context on the influences of body size and wing-loading that would have been useful before the previous paragraph—perhaps swap these two paragraphs?

Lines 1082-1085: This is really interesting!