Dear Ralf,

I would like to thank you for the time you devoted to our manuscript and for the very constructive comments and corrections you provided. Please find hereby a point-by-point response to your review, in blue characters.

As you will see, we took most of your remarks in consideration and amended the manuscript accordingly.

We hope this latest version will satisfy you.

Best wishes,

Julie, on behalf of all co-authors.

N.B: the line numbers provided in our response corresponds to the line numbers of the edited manuscript (track change).

Referee #2:
The manuscript of Julie Meilland and coauthors on "Population dynamics and reproduction strategies of planktonic Foraminifera in the open ocean" adds valuable perspectives to the discussion on the reproduction strategy of planktic foraminifers. Assemblage data from plankton tows sample in-situ populations are suited for analyses of the respective population dynamics. Using the same statistical approaches, which have been applied in earlier studies facilitates comparability, and confirm earlier results and interpretations.
Despite the 14-day long data set that "only" covers half of a synodic lunar cycle, i.e., half of the full reproduction cycle assumed to be executed by most modern planktic foraminifers, the paper presents valuable assumptions on the reproduction systematics of four species.

We know the referee has himself worked extensively on the topic during his career and therefore thank him for this positive and encouraging statement.

Overall, the paper is well written concerning the reproduction strategies of modern planktic foraminifers. In contrast, the section 4.4. on "Consequences for proxies and biogeochemical cycles", which discusses the paleoceanographic implications of the finding presented here, is rather poorly executed, and may be rewritten, or removed from the manuscript. I would suggest publication of the paper following some larger and smaller improvements given in the following.

We understand the referee's remark about section 4.4. This section is important to us, we therefore decided to keep it but to rephrase it partly (L.707 to L.751). We also toned down the title by writing "implications" instead of "consequences".

1. More general comments
Plankton are present in the ocean at all times. As ecological conditions change, small (juvenile, dormant, or resting) stages of some species may profit and start growing to be eventually

sampled by plankton nets of a certain mesh size. The required ecological conditions do not possibly occur at predefined water depths intervals, but may vary across the regional and global ocean. This is possibly also the case in the study presented here. Whereas it is implied in the paper (although not explicitly stated) that the region sampled and analyzed here may be characterized by rather homogenous hydrologic conditions, this may not be true for the ecological conditions affecting the population dynamics and (assumed) systematic reproduction of planktic foraminifers. As can be seen for the Figure 2 (panel A) of the manuscript, the region is heavily affected by eddies (Fig. 1). Both cold and warm core eddies are characterized by upwelling and downwelling at their centers and margins, which affects trophic state and vertical transport of waters bodies and the plankton included within. Vertical transport of planktic foraminifers both up and down the surface water column may consequently act here in the same way as it does in other regional of the ocean, and would affect the population dynamics and interpreted reproduction scenarios discussed here.

We agree with the referee, the region is highly dynamic and we emphasize potential vertical and lateral transport more in section 2.1. (L.149 to 152 and L.174) and in section 4.1. (L.497 to 501), stressing that our observations reflect the dynamics of multiple populations rather than of a single one.

Ontogenetic terms such as juvenile and adult are not used in the correct way in many places of the manuscript, and may need to be changed to “small” or “large” (e.g., lines 109, 226, 281). Just assuming that individuals of a certain size of a certain species would be “adult” in the sense of being capable of reproduction, as in the Figure 6, is speculation.

We agree and respectively replaced “juvenile” and “adult” by “small” and “large” when it was necessary, as suggested by the referee.

Proof of reproduction may only come from the presence (or absence) of gametogenetic (GAM) calcite on top of the shell (e.g., G. bulloides, Schiebel et al., 1997). Unfortunately, GAM calcification does not occur in some species such as G. ruber, and size of earliest possible reproduction may be identified from population dynamics. There is no proof for the assumption that reproductive maturity is reached as late as in the very large size classes as shown in Figure 6. If this would be the case, reproduction would be possible only in the few specimens that grow very large. Alternatively, the change from "mortality" to "reproduction" may occur at the size class from 140-180 microns and 200-300 microns in G. ruber and G. menardii, at which size the adult stage is reached.

We thank the referee for drawing our attention to this fair point. It is indeed a difficult “cut” to make without having a clear sign of reproduction as the GAM calcite mentioned by the reviewer. We therefore rephrased a bit saying that the value given in Figure 6b are “conservative” or “safe” but precising that one cannot exclude that reproduction might already have start in the 140 – 180 µm for *G. ruber ruber* and in the 300 – 400 µm for *G. menardii*.

In O. universa, "trochospiral and spherical" may be the correct term (line 209). Please see also lines 411-412, and 415, where speculation about the unproven connotation of test size fraction and ontogenetic stage is repeated.

We replaced the terms “juvenile” and “terminal” by “trochospiral” and “spherical” throughout the manuscript, as advised by the referee and speculations about test size fraction and ontogenetic stage have been toned down throughout the manuscript based on the referee’s comments.

2. More specific comments
Line 168: “concentrations” may be changed to “standing stocks” in case of live assemblages

Changed

Line 311: the statistical significance should be proven by numbers, and not assumed

We agree with the referee and statistical significance is based on numbers throughout the entire manuscript (Table A4 and A5).

Line 387: "reproductive mortality" this is a strange term for the process you want to describe here. Please google "reproductive mortality", which has a completely different connotation. I would suggest to simply use "reproduction" instead.

The term has been changed.

Lines 395-396: This may be written in passive, since it may not be the foraminifer’s decision: “This is because the life of a foraminifer ends at gamete release."

We used the referee’s suggestion.

Lines 425-426: Better start sentence with: "Small individuals..." The size class >100 microns does possibly not include juvenile stage of most of your species, but rather neanic and adult stage only. Please have a look at Schiebel and Hemleben (2017, and references therein); most importantly the papers of Geert-Jan Brummer.

We used the referee’s suggestion and now start the sentence by “Small individuals…”

Lines 428-429: „The constant presence of juveniles and dead specimens of foraminifera from all species suggest that reproduction may have occurred continuously during our survey.” This is a misconception. This only shows that plankton grows and dies at all times.

We understand the referee’s suggestion however the sentence is very speculative.

Line 440, and other places: The concept of synchronized reproduction was possibly introduced by Ahuve Almogi-Labin (1984); see also Erez et al. 1991; the first ideas on this may have emerged as early as in 1967 from Berger and Soutar...

We replaced “introduced” by “support”.

Line 445: We have learned from Spero et al. (2015) that the spherical chamber of O. universa

may include up to seven day and night layers of calcite, which means that gametogenesis may not really be imminent upon first formation of the spherical chamber.

We added the reference and replaced “imminent” by “relatively close”.

Lines 477-478: „This contrasts with the OVM pattern suggested for G. menardii by Schiebel and Hemleben (2017),…” These patterns vary with ecological conditions. I have found more individuals of G. menardii at greater depth, which would also largely exclude photosymbiont activity.

We added a sentence saying that the discrepancy between your observations and ours for *G. menardii* could suggest that the OVM pattern varies regionally and based on the ecological conditions.

Lines 482-482: According to Takagi et al. (2019), photosymbiosis in G. menardii is merely facultative.

We specified that photosymbiosis in *G. menardii* is facultative.

Line 496: This is possibly the latest (and only secondary) reference of many earlier (and original) references.

Indeed, this and earlier studies are presented in the introduction as they are the ones who “set the scene” but we deliberately choose in the discussion to focus more on the studies of Bijma, Hemleben and yours as it allows for more direct comparisons. Your study also has the advantage to not be limited to the Red Sea where most of the trajectory of OVM coupled to lunar synchronicity has been studied and discussed. We also indeed tend to cite Schiebel and Hemleben 2017 extensively instead of all the “original references” as the book does a great job at providing the most complete “update” on this ecological question and as one could refer it.

Lines 529-535: Why should gametes develop this strange behaviour and escape from the place where they are released? Gametes are possibly released at certain depths to provide them with optimum conditions for survival, and straight ascent would decrease the survival rates. This makes no sense.

We understand the referee’s point and we shortened and rephrased the section from L.638 to L.641.

Line 549: Survival of a population would be ensured if only one offspring of one parent would make it to reproduction, which would be much less than 5 % in case of 100,000 offspring.

This is correct in the case of asexual reproduction (only weakly documented thus far) but not for sexual reproduction that would need gametes release from two specimens in order to produce at least one successful zygote (extensively discussed in Weinkauf et al., 2020, in discussion).

Lines 571-572: „… but often interpreted literally, assuming that all specimens follow the depicted ontogenetic trajectory." This is possibly your very personal interpretation of the literature.

We rephrased the sentence.

Line 619: Why ALL 3 clades? How many clades are there according to your information? I would count on 4, which includes the Hastigerinidae.

We agree with the referee, this is why we systematically specified "main" clades as the fourth one is only constituted of the Hastigerinidae. To avoid some misunderstanding we specified it.

Figure 7: I read the depth trajectories in the opposite direction. Reproduction may occur around day 1 or 2 near the thermocline / DCM. From day 6 or 7 (3 at the earliest in G. glutinata), more small individuals occur in the overlying water column; these small individuals, however, did already grow to the size of >100 microns. In the following, increasingly more larger individuals of G. ruber and G. glutinata occur in the surface water column. This is quite similar to the development of G. bulloides in the NE Atlantic (Schiebel et al. 1997). Larger G. menardii did not occur in the surface water column; this is quite similar to what I have seen in G. menardii (Schiebel and Hemleben 2017).

The complete reproductive cycle may indeed as well be a complete physical cycle in the water column. We hereby present one half of it and the referee's suggestion illustrates the second half. As we however do not have reliable data below 100 µm we cannot trace the trajectory of these small individuals and would prefer to only show the "descending" part of the cycle here.