

"Giant worms *chez moi!* Hammerhead flatworms (Platyhelminthes, Geoplanidae, *Bipalium* spp., *Diversibipalium* spp.) in metropolitan France and overseas French territories" by Dr. Jean-Lou Justine and collaborators is a good example of citizen science reporting new localities of introduced Bipaliinae representatives and two new putative species of this group, one of them being particularly beautiful. The authors have also obtained many new COI sequences for different Bipaliinae representatives. The purpose for engaging citizens in land flatworms science is laudable and has been proven to be effective.

The information provided in this work is interesting by itself and will be certainly useful for future works on this rather understudied land planarians group (which sometimes are confused with "small snakes", leeches, and tapeworms!).

After reading the manuscript, I would like to make some comments that could help to improve it;

> An 'Aims' subsection within the 'Abstract' section would be clarifying.

> Throughout the text the Bipaliinae representatives are 'commonly' referred as 'bipaliines'. I am curious about this being a correct form to name this group. Could the authors please provide any reference?

> I suggest to include the original references for the *B. kewense* reports on São Miquel Island in the Azores and on São Tomé Island in Gulf of Guinea. (Line 74-75)

> Although it seems that the COI sequences are not very informative at all to infer the relationships between the different hammerhead worms species presented in this work, I would recommend the authors to carry out the phylogenetic analyses using more suitable phylogenetic inference programs other than MEGA. I strongly suggest to just carry out a Maximum Likelihood analysis using the **RaxML** program and a Bayesian Inference analyses using **MrBayes**. Afterwards, either the topology of one or the other method should be shown with the supports of both approaches on the corresponding nodes.

> Which is the total percentage of missing data in the final COI alignment? Could the authors please include this information?

> Lines 147 to 151 should be placed in the 'Results' Section. I suggest to move them to line 189.

> Barcoding is specially useful to identify species in those cases where cryptic species and/or species with very similar external appearance are involved. However, the most common invasive land planarians analyzed in here (i.e. *B. kewense*, *B. vagum*, *D. multilineatum*) present very characteristic external patterns and colorations that would be enough to identify them at naked eye. Actually, in the present paper it has been proven that many different specimens with an overall external similarity present identical COI sequences. Thus, barcoding would be specially useful in cases such as the black species (which presents a colouration shared with a number of other species) but not strictly necessary in the other species analyzed.

On the other hand, *B. vagum* specimens in the paper has been referred to this species only on the basis of their external appearance as the inner morphology of none of them has been inspected. The same applies to *D. multilineatum*. *B. kewense* would be a different example because the copulatory apparatus of the specimen from Cuba, which is molecularly identical to the rest, has been checked in Morffe et al., 2016.

Therefore, I would emphasize the usefulness of just the external morphology in many species of Bipaliinae to identify them easily. Somehow, this was already pointed by Kawakatsu and collaborators in 2005: "In the experience of the senior author...stripes and pigmentation patterns on the dorsal surface represent valuable taxonomic characters for species identification". In these cases, molecules will be useful to inspect if there is molecular diversity or they are clonal (as the authors of the present paper have found) rather than to identify the species (excepting when dealing with specimen fragments or very immature specimens, as the authors mention in the text).

I propose to the authors to take the comments above into account.

> Both lines 224 and 232 describe the head of *B. kewense*. Is it because one describes the dorsal side and the other the ventral side?

> Could the authors please provide bibliographic references for the sentences ending in lines 241, 293 and 322?

> It is not clear whether the description of *B. vagum* refers to specimens collected for the present paper or is just based on the bibliography; Line 317: Did the animals collected presented gonopore?

> Line 242: 'Predation of earthworms' section only refers to a nice set of photographs. I would integrate this information elsewhere and/or give more information (e.g. where and when the predation behaviour was observed, etc.) or remove it. It could be included in the following section 'Morphological evidences of reproduction by scissiparity' under an 'Ecology and reproduction' or 'Observations' section title. It could also be included in the 'Remarks' section, where predation is already mentioned in line 264.

> I am specially interested in the two putative new *Diversibipalium* species presented in this work. Could the authors please include a 'Distribution' or 'Distribution and ecology' section if possible before the 'Morphology' section in the two new species?

> As mentioned elsewhere in this review, according to Kawakatsu et al., 2005 "...stripes and pigmentation patterns on the dorsal surface represent valuable taxonomic characters for species identification". I think that the new species from Mayotte could be named as a new *Diversibipalium* species as nothing similar is reported in the bibliography. On the other hand, to my knowledge no Bipaliinae report from Mayotte has been published. Therefore, this species could be formally named (instead of just *Diversibipalium* sp. "blue" from Mayotte (Indian Ocean)) on the basis of its exclusive external appearance and distribution with little hesitation. I wouldn't encourage the authors to do so with the black species and I would keep it named as it is.

> I suggest to briefly discuss somewhere in the manuscript (e.g. in the species Remarks section) why the new species from Mayotte is likely an introduced species (it probably is due to the volcanic origin of the island). The authors could also mention the high hammerhead worms diversity in the 'nearby' island of Madagascar and consider that it might have been the original source of the Mayotte specimens. Whether they arrive in Mayotte by human or natural means is not possible to be known but speculations are welcome.

> Would it be possible to include a photography of a preserved specimen of the *Diversibipalium* sp. "blue" from Mayotte in order to compare it with a living specimen? This would point to cautionness when describing specimens directly from preserved specimens in alcohol without any previous notes or photos on their colour in future studies.

> Do the authors have information on the relative and/or absolute width of the creeping sole and the eyes distribution of the two new *Diversibipalium* species? If so, I suggest to include it.

> Line 365: "Whether the species is already described or not is not an easy question to answer; see the discussion". In the discussion it is said in lines 406-407 that "the species is thus probably new". Which sounds a bit contradictory. I agree *Diversibipalium* sp. "blue" from Mayotte is indeed new. I suggest to include lines 400 to 407 in the new species sections and clarify.

> In the line 348 it is said that 'In the discussion we list a few species which have the same black colour pattern.'. I am not able to find the list. However, I suggest to discuss this in the 'Remarks' section of '*Diversibipalium* sp. "black" from Metropolitan France' considering both the dorsal and ventral colouration of the species compared.

> Line 408: The section "Predation in bipaliinaes" does not seem to fit within the scope of the paper. Although the information provided is indeed very interesting, this is not strongly related to any analysis or discovery by the authors. I would suggest to delete this section.

> Although I am not a 'denialist' (it is obvious that many introduced species are causing trouble worldwide), I think it would be interesting to include a more extensive explanation of why the expansion of Bipaliinae in France should be concerning. The authors could add a new section just to discuss this. I consider that this would be within the scope of the paper;

Do they outcompete other species? Is there any evidence of decreased earthworm populations in Europe due to Bipaliinae presence? Which is the probability of a geographic expansion after decades being confined to gardens and hothouses? Which is the reported damage elsewhere in Europe? Why would it be important to put more efforts in study the putative impact of these animals?...Paragraph starting in line 492 gives the impression that they are not a serious concern.

> Minor:

> Line 25: 'semicircular', 'lunate', or 'falcate' instead of ...'hammer' shape...

> Line 191: Açores (Portuguese) or Azores (English)

> Line 221: *B. kewense* in italics