**Reviewer 1**

1. Because “integrative taxonomy” is in the title I was expecting more in the materials and methodology section about how the morphological characters/measurements were analyzed. However, other than stating that several measurements were made, nothing more was said. Were these measurements to be compared with museum specimens of other species or descriptions of other species? Were other morphological analyses performed (i.e., examining and comparing these specimens with specimens of other species to determine presence or absence of key characters)? So, something more inclusive needs to be stated in this section describing how these specimens of A. lumut were compared morphologically with other species. I assume that since only comparative material from A. jeetsukumarani were listed in the material examined section that comparisons with specimens of other species were not made and that specimens of A. lumut were compared with original descriptions of the other species. I have found that by making side by side comparisons between species of these toads other subtle differences can be picked up.

Deleted the integrative taxonomy part from the title and introduction together with the associated references.

2.Also, a short section on how the specimens were procured should be included at the beginning of the materials and methods section.

I don’t think this is necessary. It would only be stating the obvious. We already mentioned fieldwork in the introduction.

3. “Outer palmar tubercle” is stated in materials and methods section for the manus length but “outer metacarpal tubercle” is stated in the description of holotype section. Are these the same character?

Yes they are the same. Changed everything to outer metacarpal tubercle (ln 138).

4. At the end of the phylogeny and genetic divergences section a statement should be inserted that explains the significance of these genetic distances, e.g., they are within distances that occur between different species of this genus.

Added a sentence at the end of the section to justify the genetic distances (ln 225–226).

5. In the results section, the results of the morphological comparisons or a statement that morphological characters were observed to differ between A. lumut and other species should be added before the systematics section.

Added a sentence to that effect (ln 141–142)

6. The holotype number is not stated. I assume that this will be entered before final proof but I have seen mistakes in published papers so I just want to call this out.

Inserted (ln 243)

7. In the diagnosis section, several characters that I saw in the comparison section were omitted (i.e., relative length of first and second finger, presence or absence of tarsal ridge, and others that I have pointed out in the marked up text).

Added additional characters to diagnosis (ln 256–272).

8. Females of A. lumut are stated as being bigger than females of A. endauensis. But the SVL of 28.5 for A. endauensis is within the range of A. lumut. So this may not be a character.

Deleted this character from Comparisons section (ln 356).

9. The term “small rock cracks” in the distribution and natural history section is confusing. Does this mean actual cracks within a rock or spaces between rocks and rocks and the substrate?

I meant small cracks within a rock. Changed it to “rock fissures” (ln 419).

10. I cannot see the details stated in the last paragraph of the description of the holotype in the figures 3C and 3D, referenced.

I can’t think of a way to improve this.

I have several suggestions and edits within a marked up text of the manuscript. I think this manuscript should be published in Zootaxa after making these minor revisions.

Addressed all suggested edits.

**Reviewer 2**

This paper describes a charismatic new species of frog in the genus Ansonia. The authors do a good job with the systematics and taxonomy, and the methods employed are appropriate.

The main criticism I have of this paper regards the framework established in the title and introductory paragraph. There is a disconnect between the first paragraph and title of the paper and the remainder of the manuscript. There is a suggestion that this study will implement recent analytical tools (statistical, sequencing, imagery analyses) to delineate species within an “integrative taxonomic” framework Based on that intro paragraph I was expecting some coalescent-based molecular and statistical analyses as well as some interesting analyses of the morphology-something beyond the standard taxonomic appraoches of the past 20 years. The manuscript however does not employ these types of analyses, and instead relies on very traditional taxonomic approaches to determine taxonomy.

While the species is clearly distinguishable based on mtDNA sequence data, and there are morphological features that can be used to provide diagnosability, I don’t think that this study should invoke itself as an “integrative taxonomy”. The authors cite papers that have disparate definitions of “integrative taxonomy” but all suggest that for taxonomy to be integrative multiple quantitative analyses from independent datasets are needed at least one of which sets up the taxonomic hypotheses to be tested, or as suggested in the Schlick-Steiner et al citation if the discovery approach is used then analyses of three disciplines should be used. This paper really uses a single analytical tool to designate a taxonomy e.g. a mtDNA gene tree. There is no quantitative analysis of the morphology in this study it is purely qualitative. While the authors have collected morphometric data, they don’t provide any analyses of these data. Based on the introduction, I was expecting some type of rigorous statistical analyses such as a PCA of DFA or some type of cluster analysis of the morphometric data. The authors present a table of measurements for the type series this is good and useful, but simply descriptive and really is just a basic standard of traditional taxonomic approaches. Based on the very limited material examined section, it appears the authors lack the necessary comparative material to provide a thorough morphometric analyses. Inclusion of additional morphometric data and appropriate statistical analyses are needed to count the morphology as an independent analyses as required by “integrative taxonomy”, or an additional datatype is needed, or the authors really need to better justify how this study falls under the umbrella of "integrative taxonomy".

I think that a single gene tree is often fine for species descriptions and this has been the standard practice for the last 20 years (since sequencing technologies became widely available), this in combination with some diagnostic morphological features provides good support for this taxon, it just doesn’t fit the “integrative taxonomy” paradigm espoused by the cited sources. Every taxonomic paper does not have to use the latest analytical methods, but the integrative taxonomic framework suggested in the title and introduction is lacking. The paper is a solid species description, but the title and particularly the entire first paragraph should be heavily revised to reflect the actual content of the paper, which utilizes very standard taxonomic practices. This isn’t a criticism of the methods or the approach or the findings, but just of the way the manuscript is currently framed

Additionally, I suggest the following:

Additional background on Ansonia including the broader geographical scope would be useful. The introduction should provide more detail of the specific taxonomy within the group with a greater focus on the species in the Thai-Malay Peninsula. I think more information from the Dring paper might also be interesting. Not having Dring in front of me is there a line drawing or photo that could be reproduced as a figure?

Added a new paragraph (ln 58–66). The only relevant part of Drings paper is mentioned in the discussion section, where he reported on the occurrence of *A. malayana* from Gunung Lawit (a separate but adjacent mountain) and proceeds to give a list of characters that clearly corresponds to the new species rather than *malayana*.

In the species description, the locality data for the paratypes should be included in full not just- same as holotype -especially since several of the paratypes were collected on a different date by different people. The full altitude and GPS coordinates should be included as well.

Done (ln 247–252).

Diagnosis Inger 1992 provides additional generic characters for Ansonia. Probably worth citing.

Added citation. Additional characters in Inger (1992) were mostly for tadpoles, which we do not have data for.

Is there anything you can add regarding the reproductive biology of the species?  Based on photograph of the holotype appears to be gravid (?). A description of the eggs and clutch size ranges would be a good addition. Were the males calling? Have the authors found tadpoles and if so can they include them?

Males were not calling and no tadpoles were found. The females were gravid and I’ve examined one of them. Unfortunately, I was unable to determine clutch size due to the less than optimal condition of the preserved eggs. Added some information at the end of the nat hist section (ln 432–433)

Why does the discussion start with the additional population of jeetsukumarani? It wasn’t even mentioned anywhere prior in the manuscript. If it is so important that the discussion leads with the discovery of the new population then introduce the new population somewhere earlier in the manuscript.

One of the authors wanted it to be mentioned. I personally think it is irrelevant to this study and have deleted it.

The material examined should include everything that you examined. Only four specimens are mentioned.

I only examined specimens of the new species and 15 specimens of *a. jeetsukumarani* from the new locality, which are listed in the appendix. I compared these data to Wood et al 2008 and Wilkinson et al 2012 that had comparative data for all other Ansonia. This is mentioned in the new sentence I added in M&M (ln 141–142)

You mention in the results that nine new sequences were added to genbank. On the figure 1 tree I can see 6 A. lamut what are the other 3 new sequences? Or are they just some combination of 12 & 16s for those 6 specimens (which means there are missing data since then there should be 12 sequences)? And on that note you have five specimens in your type series why not include all 6 of the specimens that are included on your tree in the type series? To clarify the above the authors need to add one additional table with all of the samples included in your tree and the genbank accession numbers and the citation for the study that generated them.

There were 6 new sequences of the new species and 3 new sequences of *A. jeetsukumarani* from a new locality. I’ve clarified this in the M&M section (ln 168–176).

Table 2. Two taxa are just listed as numbers without a tax-id-what species are they? Is this another new species? It isn’t mentioned anywhere in the text. If you aren’t planning to talk about it here and this is some follow up species description then why not remove it from the tree and the distance matrix? What is the point of including it to simply ignore it? It has KUH numbers so perhaps it isn’t something you have evaluated? Since there is no table to explain if these are your sequences or pre-existing sequences obtained from genbank, it isn’t clear what these are or why they are here.

Those two species were from Matsui et al’s 2010 *ansonia* phylogeny. There is now a Table 2 that has all the data for samples used in the phylogenetic analysis which should clear up the confusion.

Figure 4. The colored shadows are a nice idea, but they do not translate well when printed in B&W put the colored symbols underneath the frogs.  Please use a shape set that differs more than hexagons and circles, maybe triangles and circles -in the places where the localities overlap the shapes are difficult to tell apart in some instances particularly when printed in black&white.

Can you reference clearly the two mountain ranges as well as the major places referenced in the text on the map so that people not working in Peninsular Malaysia can quickly see find these places. For example where is Dring’s Gunung Lawit? Is that one of the localities indicated for A. lamut on the map?

I’ve also added a short section on the distribution of *A. malayana* in the discussion (ln 456–460).