Submission date. 11/05/2026

Associate editor's decision after peer review (28/07/2026).
Dear Dr. Tencatt:
Manuscript ID NI-2026-0077 entitled "Two co-mimic new species of Corydoradinae catfishes (Siluriformes: Callichthyidae) from the rio Tapajós basin, Brazil, with a discussion on their color pattern" which you submitted to the Neotropical Ichthyology, has been reviewed. The comments of the reviewer(s) are included at the bottom of this letter.
The reviewer(s) have recommended publication, but also suggest some minor revisions to your manuscript. Therefore, I invite you to respond to the reviewer(s)' comments and revise your manuscript.
To revise your manuscript, log into https://mc04.manuscriptcentral.com/ni-scielo and enter your Author Center, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision.
You may also click the below link to start the revision process (or continue the process if you have already started your revision) for your manuscript. If you use the below link you will not be required to login to ScholarOne Manuscripts.
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You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript using a word processing program and save it on your computer. Please also highlight the changes to your manuscript within the document by using the track changes mode in MS Word or by using bold or colored text.
Once the revised manuscript is prepared, you can upload it and submit it through your Author Center.
When submitting your revised manuscript, you will be able to respond to the comments made by the reviewer(s) in the space provided. You can use this space to document any changes you make to the original manuscript. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer(s).
IMPORTANT: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing the submission.
Because we are trying to facilitate timely publication of manuscripts submitted to the Neotropical Ichthyology, your revised manuscript should be submitted before 28-Aug-2026. If it is not possible for you to submit your revision by this date, we may have to consider your paper as a new submission.
Once again, thank you for submitting your manuscript to the Neotropical Ichthyology and I look forward to receiving your revision.
Sincerely,
Dr. Carlos DoNascimiento
Associate Editor, Neotropical Ichthyology
tepuglanis@hotmail.com

Anonymous reviewer #1
Recommendation. Minor Revision
Comments.
The manuscript is good, highly informative, and I recommend it for publication. I would like to congratulate the authors on the description of these two new species. However, the manuscript requires some adjustments, and I have several questions and suggestions.
My first question is: are the two new species syntopic, and were they collected together at the same locality and in the same microhabitat? If so, I think this should be clearly stated in the Abstract, as it would strengthen the mimicry hypothesis. In addition, if there is evidence of Müllerian mimicry associated with syntopy and convergence in color pattern, I think it is important to include photographs of live specimens showing this shared trait. Much of the discussion focuses on the unusual bluish coloration, which is uncommon among fishes. Therefore, if this coloration is to be explicitly emphasized as a shared feature, I recommend including photographs documenting this pattern in both species.
In the Abstract, line 16, you state: "The new Brochis can be distinguished from its congeners by the following features: a mask-like blotch; absence of...". This wording suggests that each character is individually diagnostic, whereas the species is actually distinguished by a combination of characters. Therefore, I suggest wording it as: "The new Brochis can be distinguished from its congeners by the following combination of features...". The same applies to Hoplisoma.
The last sentence of the Abstract, line 19, contains a redundancy. You state: "Considering their unusual color pattern, a discussion on its possible positive adaptive value of their color patterns is provided." In the context of Müllerian mimicry, stating that a trait has adaptive value already implies an adaptive advantage, so "positive" is unnecessary.
The diagnosis of B. bodrocki is very well structured. However, the first character raised some questions for me. First, could the cephalic/orbital pigmentation forming the mask vary in intensity depending on preservation? This seems particularly relevant for B. crimmeni, B. deweyeri, B. amandajanea, B. crypticus, and B. sychri, which have historically been problematic in terms of species delimitation and variation in color pattern. Is the "typical mask-like blotch" of B. bodrocki homologous to the mask observed in these compared species? Are these the only species mentioned in the diagnosis that share this feature? How did you distinguish a true mask from a simple concentration of chromatophores extending across the orbital region?
On page 10, line 10, you state that the mask "can be variably diffuse", but how diffuse can it become? Are there specimens in which the mask is nearly inconspicuous? If so, I think it would be important to illustrate this variation photographically.
The same question applies to the character "large dark brown or black patch around dorsal-fin base". What is the boundary between a true patch at the dorsal-fin base and a diffuse concentration of chromatophores in this region? Is the absence of a dorsal-fin-base patch in B. bodrocki constant, or can the concentration of chromatophores sometimes be interpreted as an incipient patch? It may be useful to clarify this in the description so that the diagnostic character can be applied consistently and replicated by other researchers. The same concern applies to H. jujuba. This character appears potentially subjective and dependent on specimen preservation, and therefore needs to be very clearly defined. The same applies to the lateral stripes in H. jujuba.
Much of the discussion focuses on the iridescent color pattern. I think it would be important to add a paragraph specifically discussing the coloration of these species. In my view, the differences among them are rather subtle. Could an old specimen faded after long-term preservation in alcohol still be reliably identified as B. bodrocki? In addition, I suggest providing a plate illustrating the syntopic species occurring with B. bodrocki, C. caramater, H. jujuba, and Hoplisoma sp. CW193, as well as a comparative

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plate including Brochis bodrocki, B. deweyeri, B. sychri, and B. crimmeni. Such comparisons would help address these issues and make the diagnostic differences clearer.
Regarding the diagnosis of H. jujuba, the first character, concerning the abdominal plates, is the one that concerns me most, because it may represent ontogenetic variation within Corydoradinae. Because this character is used to distinguish H. jujuba from most species of the genus, it needs to be particularly robust. Did you take body size into account? Did you evaluate whether this character is consistently present in both juvenile and adult specimens of H. jujuba, and compare juveniles and adults of the other Hoplisoma species?
Regarding the character "surpassing the suture between sphenotic and pterotic-extrascapular in a vertical", this condition seems somewhat ambiguous to me. Infraorbital 2 may actually contact the pterotic-extrascapular, or it may not contact it but extend very close to it while surpassing the vertically defined boundary. Is there intraspecific variation in this character? If some specimens show contact whereas others do not, it would be important to clearly state the frequency of each condition within the type series.
Regarding the character distinguishing H. jujuba from H. kanei, does this distinction remain reliable regardless of specimen preservation, specimen size, and length of time in alcohol? It may also be useful to discuss these two species more explicitly and identify additional morphological characters that distinguish them on conclusion section.
Please indicate in Figure 13 which panel is A and which is B.
Is the "large cartilage block" mentioned for Fig. 13B actually visible in the figure? I cannot unequivocally identify which area corresponds to the "large cartilage block". I suggest indicating it with an arrow, an abbreviation for the structure, or another appropriate marker to facilitate its identification in the figure.
I would write "blue-colored Müllerian mimicry ring" with a hyphen.
Regarding the Conclusion, do you consider the bluish iridescence itself to be part of the shared warning signal in this proposed Müllerian mimicry ring, or only the overall body color pattern? Given that the iridescent blue coloration observed in wild specimens is highlighted here and appears to be an interesting feature shared by both new species and Corydoras caramater, I suggest including, if possible, photographs of live specimens showing this coloration. This would be very interesting and would help illustrate and support the observations discussed in this section. Photographs of live specimens would provide particularly valuable documentation, especially because the authors also propose that this coloration may have adaptive significance.
I also suggest avoiding categorical statements such as "obvious aposematic", "cryptic effect", and "high levels of UV", as these are presented with greater certainty than the data in the manuscript appear to support. The occurrence of similar color patterns in three phylogenetically distinct Corydoradinae genera suggests that strong selective pressures may have repeatedly favored the evolution of a similar external phenotype. However, it remains unclear whether this convergence primarily reflects predator-mediated selection through Müllerian mimicry, environmental constraints imposed by the optical properties of clearwater habitats, developmental biases in the evolution of pigmentation patterns, or some combination of these factors.
Although the adaptive significance of the unusual bluish iridescence remains uncertain, the convergence of three syntopic Corydoradinae species toward a remarkably similar color pattern, combined with their apparent participation in a Müllerian mimicry ring, suggests that this coloration may have evolved under multiple selective pressures, potentially including crypsis, aposematism, and visual signaling. Testing these alternative hypotheses will require behavioral, ecological, and visual ecology studies.
Finally, why do you think these species converged on this color pattern whereas other syntopic species did not?

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Anonymous reviewer #2
Recommendation. Minor Revision
Comments.
Uma boa e detalhada descrição de duas espécies que parecem ser bem sustentadas como novas. A discussão às vezes tá meio truncada e difícil de ler, revisar. Acho que o azul não é muito diferente de um verde-esmeralda que muitas outras espécies possuem. A figura 9A é difícil de vender como azul, mas, enfim, os autores têm mais experiência no grupo e na coloração. Igualmente, acho a discussão válida, independentemente do comprimento ser um pouco mais pra cá ou mais pra lá.
Tiago
Page 2, line 12. Acho que se Pará State o State é maiúsculo, Jacareacanga o town deveria ser tb. ou City... como New York City.
Line 52. O Colorido de Hoplisoma é tão variavel e aparece o mesmo em tanto outros géneros que me parece inútil incluir isso na combinação de caracteres da diagnose do género.
Page 7, line 44. Vocês citam acima "barbel", cita aqui só uma vez aqui "mouth e barbel" estruturas já que devem estar relacionadas.
Line 9. Revisem os XX nos números do INPA e do MUBIO. é interessante já ter esse números para a versão a ser revisada. MUBIO e CITL estão na revisão de Sabaj? não os encontrei... talvez na versão de 2026?
Page 10, line . "dark markings on caudal fin". Essas são as "bands" que comentam no material e métodos? ou é qualquer tipo de marca na caudal?
Page 26. Por que não incluir as coordenadas exatas? Acho importante para possíveis futuras revisões e para ter uma localidad tipo exata! A não ser que os pescadores evitaram a dar algo mais detalhado. Com essa informação, não existe uma localidade tipo, já que o rio Pacu, deve ser bem largo. Outra coisa, a espécie é LC, ou seja é de menor preocupação em casa de pensar na sua proteção.
Page 34, line 24. Acho que esse último "i" traz mais confusão e poderia ser incluído na conta dos ramificados. As vezes parece que os individuos pequenos são i e os grandes são ramificados, e esse se deve mais a tua amostragem que alguma informação realmente significativa da natureza da espécies.
Page 34, line 54. Talvez isso seja material e métodos (Kiehl et al.,) 2006), como outros detalhes da morfologia.
Identification Key. Não tem outra alternativa para tirar Aspidoras de primera? é dificil as vezes visualizar as fontanelas e o ideal da chave é ser algo pratico.
Quando não está em contato com a placa nucal (para Osteogaster), é um espaço grande? Poderia descrevê-lo?
Discussion
Page 43, Line 32. Revisa a construção dessa frase.
Olhando as fotos, esse "azul" para aquele verde esmeralda que muitas outras "Corydoras" apresentam, tá bem parecido. Falar que é o primeiro anel mimetico com azul, me parece um pouco "forte". A foto 9A tá bem esverdeada.
Page 44, line 32 e line 37. Revisem a escrita de algumas dessa frase, acho que tá faltando uns conectores. Vocês falam que o segundo grupo é estruturas, más a frase que segue não "sobre o azul" não conecta muito bem com a frase anterior.
Page 45 line 17-33. Aqui tb, parecem várias frases com informações soltas sobre coloração, mas sem saber muito aonde os autores querem chegar. Realmente, o primeiro parágrafo que parece uma discussão é o que associa a coloração das novas espécies à citação de Umbers. O que quer dizer "best production"? É uma coloração mais próxima ao comprimento de onda do "azul", mais intensa? Aclarar isso.
Page 48. A água parece bem branca na foto. Vocês comentam sobre a mineração. Talvez isso possa afectar um pouco esse "aposematismo". Não seria repensar a categoria de ameaça, se o bicho tem essa distribuição tão restrita.

Author's Rebuttal Letter (31/07/2026).

We hereby submit a reviewed version of the manuscript now entitled "Two co-mimic new species of Corydoradinae catfishes (Siluriformes: Callichthyidae) from the rio Tapajós basin, Brazil, with a discussion on their color pattern", NI-2026-0077, by Luiz Tencatt, Willian Ohara, Vandergleison de Carvalho, Steven Grant and Marcelo Britto. Except for minor comments/suggestions by the reviewer related to 'style issues', we accepted all of his commentaries. His questions are all addressed below:

Reviewer: 1

Recommendation: Minor Revision

Comments:

The manuscript is good, highly informative, and I recommend it for publication. I would like to congratulate the authors on the description of these two new species. However, the manuscript requires some adjustments, and I have several questions and suggestions.

My first question is: are the two new species syntopic, and were they collected together at the same locality and in the same microhabitat? If so, I think this should be clearly stated in the Abstract, as it would strengthen the mimicry hypothesis. In addition, if there is evidence of Müllerian mimicry associated with syntopy and convergence in color pattern, I think it is important to include photographs of live specimens showing this shared trait. Much of the discussion focuses on the unusual bluish coloration, which is uncommon among fishes. Therefore, if this coloration is to be explicitly emphasized as a shared feature, I recommend including photographs documenting this pattern in both species.

R: Yes, they are captured together, in the same sites, close to each other. We stated this more than once along the manuscript. We added such photos; they share the same color pattern but are from different genera. Both are defended, by spines, and present color pattern features strongly suggesting aposematism. Both present axillary glands. So, they are syntopic, defended and likely aposematic. All of this was presented in the manuscript, along the text, illustrated by figures showing their color pattern in life. We added a new photo to figure 21 that better shows the blue sheen. It was not previously used due to its lower quality, but it was now included for clarity purposes. We included that they were found in syntopy in the abstract in any case.

In the Abstract, line 16, you state: "The new Brochis can be distinguished from its congeners by the following features: a mask-like blotch; absence of...". This wording suggests that each character is individually diagnostic, whereas the species is actually distinguished by a combination of characters. Therefore, I suggest wording it as: "The new Brochis can be distinguished from its congeners by the following combination of features...". The same applies to Hoplisoma.

R: OK, done.

The last sentence of the Abstract, line 19, contains a redundancy. You state: "Considering their unusual color pattern, a discussion on its possible positive adaptive value of their color patterns is provided." In the context of Müllerian mimicry, stating that a trait has adaptive value already implies an adaptive advantage, so "positive" is unnecessary.

R: OK, done.

The diagnosis of B. bodrocki is very well structured. However, the first character raised some questions for me. First, could the cephalic/orbital pigmentation forming the mask vary in intensity depending on preservation? This seems particularly relevant for B. crimmeni, B. deweyeri, B. amandajanea, B. crypticus, and B. sychri, which have historically been problematic in terms of species delimitation and variation in color pattern.

R: Regarding the first question, yes, like all color pattern traits, the preservation will eventually affect pigmentation. Nonetheless, we need to consider the actual phenotype displayed the species rather than considering such influence. For example, with time,

all pigmentation tends to fade, but this cannot be considered when describing a species for obvious reasons. The problem with the species mentioned by the reviewer are their poorly informative descriptions, allied with a small number of type specimens. However, we examined all of them and they are surely not conspecific with our new species, regardless of their possible taxonomic problems.

Is the "typical mask-like blotch" of B. bodrocki homologous to the mask observed in these compared species? Are these the only species mentioned in the diagnosis that share this feature? How did you distinguish a true mask from a simple concentration of chromatophores extending across the orbital region?

R: Well, that's a good question, but the aim of our study is not to perform a phylogenetic hypothesis, which is the only way to answer this question. With that said, what we call "mask" is surely not homologous considering minor differences in their shape. Despite of that, we are merely using the presence of a dark blotch transversally crossing the orbit as a diagnostic feature. A simple concentration of chromatophores does form a distinct blotch standing out from the surrounding coloration, as a "mask" does. In summary, only in a phylogenetic context this question can be answered, which is not the aim of our study.

On page 10, line 10, you state that the mask "can be variably diffuse", but how diffuse can it become? Are there specimens in which the mask is nearly inconspicuous? If so, I think it would be important to illustrate this variation photographically.

R: No, diffuse is just not so intensely dark, but it is still conspicuous (standing out from the surrounding coloration). This is why used "diffuse" instead of "inconspicuous".

The same question applies to the character "large dark brown or black patch around dorsal-fin base". What is the boundary between a true patch at the dorsal-fin base and a diffuse concentration of chromatophores in this region? Is the absence of a dorsal-fin-base patch in B. bodrocki constant, or can the concentration of chromatophores sometimes be interpreted as an incipient patch? It may be useful to clarify this in the description so that the diagnostic character can be applied consistently and replicated by other researchers. The same concern applies to H. jujuba. This character appears potentially subjective and dependent on specimen preservation, and therefore needs to be very clearly defined. The same applies to the lateral stripes in H. jujuba.

R: The boundary is how such pattern stands out from the surrounding coloration. There is no such thing as a "true blotch/patch", blotch/patch is something defined, differing from the surrounding coloration. In all living and preserved specimens, there is no blotch below the anterior portion of the dorsal-fin base. In the mentioned congeners, it is possible to see a well-defined blotch below the anterior portion of the dorsal-fin base, which clearly differ them. The fact that all pigmentation will eventually fade with time cannot interfere with the description/diagnosis performed in taxonomic studies, as we work with the actual phenotype displayed by the species and not with the degradation resulted by time. If you follow the reasoning of the reviewer, species that only differ by color pattern should not be described as one day the color will be gone and we will not be able to differ the degraded specimens. Regarding "lateral stripes in H. jujuba" we do not understand this statement as this species does not display such feature.

Much of the discussion focuses on the iridescent color pattern. I think it would be important to add a paragraph specifically discussing the coloration of these species. In my view, the differences among them are rather subtle. Could an old specimen faded after long-term preservation in alcohol still be reliably identified as B. bodrocki?

R: Again, the fact that all pigmentation will eventually fade with time cannot interfere with the description/diagnosis performed in taxonomic studies, as we work with the actual phenotype displayed by the species and not with the results of the degradation related to time and quality of preservation. If you follow the reasoning of the reviewer, species that only differ by color pattern should not be described as one day the color will be gone and we will not be able to differ the degraded specimens.

In addition, I suggest providing a plate illustrating the syntopic species occurring with B. bodrocki, C. caramater, H. jujuba, and Hoplisoma sp. CW193, as well as a comparative plate including Brochis bodrocki, B. deweyeri, B. sychri, and B. crimmeni. Such comparisons would help address these issues and make the diagnostic differences clearer.

R: We consider that a plate with the syntopic species is unnecessary as they are not directly related with both species described. In the case of C. caramater, the description is recent and open access, anyone can easily see it. Regarding the mentioned Brochis congeners, B. deweyeri, B. sychri, and B. crimmeni, although they are somewhat similar to each other, they clearly differ from the new species by having well-defined dark blotches conspicuously standing out from the clean, pale beige/yellowish background coloration, while the new species presents a conspicuously dusky coloration on flanks, in which diffuse dark markings don't stand out (to mention a single feature). We consider that such description allied with several photos of the new species are enough to understand the coloration of the new species, which (in theory) would be hardest to understand, as "well-defined dark blotches conspicuously standing out from the clean, pale beige/yellowish background coloration" seems pretty straightforward to understand.

Regarding the diagnosis of H. jujuba, the first character, concerning the abdominal plates, is the one that concerns me most, because it may represent ontogenetic variation within Corydoradinae. Because this character is used to distinguish H. jujuba from most species of the genus, it needs to be particularly robust. Did you take body size into account? Did you evaluate whether this character is consistently present in both juvenile and adult specimens of H. jujuba, and compare juveniles and adults of the other Hoplisoma species?

R: Yes, we did examine juveniles and adults, comparing it with all of the mentioned congeners. Even though smaller specimens eventually display smaller areas of "mosaics" of plates, the mosaic of plates on the belly is always present, which itself differ it from the species lacking coalescent plates (most Corydoradinae).

Regarding the character "surpassing the suture between sphenotic and pterotic-extrascapular in a vertical", this condition seems somewhat ambiguous to me. Infraorbital 2 may actually contact the pterotic-extrascapular, or it may not contact it but extend very close to it while surpassing the vertically defined boundary. Is there intraspecific variation in this character? If some specimens show contact whereas others do not, it would be important to clearly state the frequency of each condition within the type series.

R: The idea of stating that the edge of the bone surpasses the line of suture is just to give an idea of the size of the posterior laminar of the infraorbital 2. In the new species, it is larger than in most congeners, which typically present small posterior laminar expansions of infraorbital, not reaching the mentioned suture. As we stated in the paper, in some specimens there is a direct contact, whereas in others the direct contact nearly occurs. To provide the frequency is quite complicated as most of our specimens are not c&s, and, to visualize such character in formalin-preserved specimens, you need to "brush" off the head skin (with a tooth brush), removing the superficial layer of skin of the specimens. This would cause an unnecessary damage to the specimens as the most important thing here, in this specific case, is stating that, at most, the posterior laminar expansion is large enough to surpass the suture line, while in most corydoradines this expansion does not even reach such suture.

Regarding the character distinguishing H. jujuba from H. kanei, does this distinction remain reliable regardless of specimen preservation, specimen size, and length of time in alcohol? It may also be useful to discuss these two species more explicitly and identify additional morphological characters that distinguish them on conclusion section.

R: As extensively discussed, the fact that all pigmentation will eventually fade with time cannot interfere with the description/diagnosis performed in taxonomic studies, as we

work with the actual phenotype displayed by the species and not with the results of the degradation related to time and quality of preservation. If you follow the reasoning of the reviewer, species that only differ by color pattern should not be described as one day the color will be gone and we will not be able to differ the degraded specimens. With that said, H. kanei is indeed similar with some congeners, especially H. atropersonatum, and future studies are surely necessary to better understand their identity. Differ them from H. jujuba is pretty simple, however, as both of them present well-defined dark blotches conspicuously standing out from the clean, pale beige/yellowish background coloration, while the new species presents a conspicuously dusky coloration on flanks, in which diffuse dark markings don't stand out (to mention a single feature). This is a pretty easy, straightforward way to differ the new species from them. Ironically, the most similar congener to H. jujuba was not mentioned by the reviewer, H. colossus, which shares the dusky, darkish coloration with H. jujuba. The distinction between them is discussed in the paper, the new species lacks a large dark brown or black patch around dorsal-fin base (vs. present in H. colossus), as well as differences in the infraorbital 2 morphology are the easiest way to differ them.

Please indicate in Figure 13 which panel is A and which is B.

R: Ok, done.

Is the "large cartilage block" mentioned for Fig. 13B actually visible in the figure? I cannot unequivocally identify which area corresponds to the "large cartilage block". I suggest indicating it with an arrow, an abbreviation for the structure, or another appropriate marker to facilitate its identification in the figure.

R: Yes, it is. It is colored in blue and surrounded by reddish bones, i.e., mesethmoid and lateral ethmoid. I think the clear differences in coloration between what is cartilage and what is bone seems conspicuous enough to recognize the mentioned cartilage.

I would write "blue-colored Müllerian mimicry ring" with a hyphen.

R: Ok, done.

Regarding the Conclusion, do you consider the bluish iridescence itself to be part of the shared warning signal in this proposed Müllerian mimicry ring, or only the overall body color pattern? Given that the iridescent blue coloration observed in wild specimens is highlighted here and appears to be an interesting feature shared by both new species and Corydoras caramater, I suggest including, if possible, photographs of live specimens showing this coloration. This would be very interesting and would help illustrate and support the observations discussed in this section. Photographs of live specimens would provide particularly valuable documentation, especially because the authors also propose that this coloration may have adaptive significance.

R: Well, it is difficult to undoubtedly confirm if the bluish sheen plays a significant part in the aposematic signaling, but it seems plausible. As we stated in the manuscript, the fish is not exactly blue, but rather displays a blue metallic sheen in nature (aquarium specimens tendo to lose this coloration). So, depending on light, the specimen may shine more blue or green. In any case, we have a photo of a bluer H. jujuba, that we avoided to use because it is not in high resolution, but we included now to help the readers to see what we are saying.

I also suggest avoiding categorical statements such as "obvious aposematic", "cryptic effect", and "high levels of UV", as these are presented with greater certainty than the data in the manuscript appear to support. The occurrence of similar color patterns in three phylogenetically distinct Corydoradinae genera suggests that strong selective pressures may have repeatedly favored the evolution of a similar external phenotype. However, it remains unclear whether this convergence primarily reflects predator-mediated selection through Müllerian mimicry, environmental constraints imposed by the optical properties of clearwater habitats, developmental biases in the evolution of pigmentation patterns, or some combination of these factors.

R: Ok, we inserted “potential”, “putative” or “likely” to those sentences, just in case. Yes, it remains unclear as our purpose here is to provide a name for the two new species. A deeper investigation is still needed, as highlighted in the paper.
Although the adaptive significance of the unusual bluish iridescence remains uncertain, the convergence of three syntopic Corydoradinae species toward a remarkably similar color pattern, combined with their apparent participation in a Müllerian mimicry ring, suggests that this coloration may have evolved under multiple selective pressures, potentially including crypsis, aposematism, and visual signaling. Testing these alternative hypotheses will require behavioral, ecological, and visual ecology studies.
Finally, why do you think these species converged on this color pattern whereas other syntopic species did not?
R: Yes, you are correct. It is difficult to answer “why”s in science. First of all, although very interesting, this is just a brief, superficial discussion in our paper, based on what we observed on field expeditions. What I can say is that the other Corydoradinae in the same area do not live in the exact same sites as the trio (caramater, jujuba and bodrocki), CW193 (under description now), is a sand dwelling species, living with Characidium rather than other “corys”. Considering that the species forming this trio “swim together”, in the same sites, the benefits of the mimicry seem obvious when compared with its absence. The “why”s are surely complicated in science, we seem to be more focused on the “how”s.
Reviewer: 2
Recommendation: Minor Revision
Comments:
Uma boa e detalhada descrição de duas espécies que parecem ser bem sustentadas como novas. A discussão às vezes tá meio truncada e difícil de ler, revisar. Acho que o azul não é muito diferente de um verde-esmeralda que muitas outras espécies possuem. A figura 9A é difícil de vender como azul, mas, enfim, os autores têm mais experiência no grupo e na coloração. Igualmente, acho a discussão válida, independentemente do comprimento ser um pouco mais pra cá ou mais pra lá.
R: Yes, it depends on the light. We added a new photo that better shows what we see for the three mimics. The transition between a green and blue coloration was also better explained in the discussion.
Page 2, line 12. Acho que se Pará State o State é maiúsculo, Jacareacanga o town deveria ser tb. ou City... como New York City.
R: Ok, done.
Line 52. O Colorido de Hoplisoma é tão variavel e aparece o mesmo em tanto outros géneros que me parece inútil incluir isso na combinação de caracteres da diagnose do género.
R: Yes, it is variable, but including a general description is really helpful to distinguish it from Osteogaster, the reason why this general color pattern description was included, as all Osteogaster basically share the same color pattern.
Page 7, line 44. Vocês citam acima "barbel", cita aqui só uma vez aqui "mouth e barbel" estruturas já que devem estar relacionadas.
R: Ok, we changed this.
Line 9. Revisem os XX nos números do INPA e do MUBIO. é interessante já ter esse números para a versão a ser revisada. MUBIO e CITL estão na revisão de Sabaj? não os encontrei... talvez na versão de 2026?
R: Ok, done. We updated to the 2026 version, which presents both acronyms.
Page 10, line . "dark markings on caudal fin". Essas são as "bands" que comentam no material e métodos? ou é qualquer tipo de marca na caudal?
R: Not exactly, because a species may present blotches on caudal fin that are not vertically aligned, forming bands, as explained in the Material and Methods section. To make it a general statement to encompass both states, we used the presence/absence of dark markings, regardless of their distribution on the caudal fin.

Page 26. Por que não incluir as coordenadas exatas? Acho importante para possíveis futuras revisões e para ter uma localidad tipo exata! A não ser que os pescadores evitaram a dar algo mais detalhado. Com essa informação, não existe uma localidade tipo, já que o rio Pacu, deve ser bem largo. Outra coisa, a espécie é LC, ou seja é de menor preocupação em casa de pensar na sua proteção.
R: We understand the concerning but that's the data we have. Besides, the Code does not demand exact coordinates. A lot of fishermen do not share the exact location to avoid other people to explore the species. Some families depend on a couple of species to survive, so, it seems reasonable to respect such decision to omit this data. Moreover, the data we provided is surely enough to delimit the type locality. The argument that there is no type locality is completely unfounded, as the rio Pacu is a well known river, very close to Jacareacanga, you can easily find it in google maps just checking the surroundings of Jacareacanga.
Page 34, line 24. Acho que esse último "i" traz mais confusão e poderia ser incluído na conta dos ramificados. As vezes parece que os individuos pequenos são i e os grandes são ramificados, e esse se deve mais a tua amostragem que alguma informação realmente significativa da natureza da espécies.
R: We prefer to state exactly what we saw in the material, and no relation with size was founded. Therefore, we think that the formulae need to be provided with precision.
Page 34, line 54. Talvez isso seja material e métodos (Kiehl et al.,) 2006), como outros detalhes da morfologia.
R: Ok, done.
Identification Key. Não tem outra alternativa para tirar Aspidoras de primera? é dificil as vezes visualizar as fontanelas e o ideal da chave é ser algo pratico.
R: Maybe with naked eyes, but the second fontanel (absent in nearly all Corydoradinae, except for some Scleromystax and a few dwarf Gastrodermus) is in the middle of the parieto-supraoccipital. It can be easily observed with a stereomicroscope, and is being used to recognize the genus for decades now.
Quando não está em contato com a placa nucal (para Osteogaster), é um espaço grande? Poderia descrevê-lo?
R: Large and small in this case does not really matters, as the character in question refers to the presence/absence of contact between two structures. If there is contact, you will follow one path, if there is no contact (regardless of the size of the space) you follow the other. It would make sense if we were comparing two species lacking such contact if one them presented such space large/small. Our character here is not quantitative, but qualitative.
Discussion
Page 43, Line 32. Revisa a construção dessa frase.
R: Ok, done.
Olhando as fotos, esse "azul" para aquele verde esmeralda que muitas outras "Corydoras" apresentam, tá bem parecido. Falar que é o primeiro anel mimetico com azul, me parece um pouco "forte". A foto 9A tá bem esverdeada.
R: As we said before, the angle of the light may reflect it green or blue. We also said that specimens kept in captivity (as the specimen in photo 9) tend to lose such blue sheen. If you check the color pattern description, you will see that we mentioned the green metallic sheen too. Also, we didn't say that other species sharing this coloration do not exist, but rather stated that, considering the described ones, this coloration was never reported. In any case, we reformulated the sentence.
Page 44, line 32 e line 37. Revisem a escrita de algumas dessa frase, acho que tá faltando uns conectores. Vocês falam que o segundo grupo é estruturas, más a frase que segue não "sobre o azul" não conecta muito bem com a frase anterior.
R: Ok, done.

PEER REVIEW FILE

Page 45 line 17-33. Aqui tb, parecem várias frases com informações soltas sobre coloração, mas sem saber muito aonde os autores querem chegar. Realmente, o primeiro parágrafo que parece uma discussão é o que associa a coloração das novas espécies à citação de Umbers. O que quer dizer "best production"? É uma coloração mais próxima ao comprimento de onda do "azul", mais intensa? Aclarar isso.
R: We reformulated the paragraph in question and it is now more clear what we tried to say. “Best production” is exactly what the reviewer said, is a more accurate reproduction of this specific wavelength.
Page 48. A água parece bem branca na foto. Vocês comentam sobre a mineração. Talvez isso possa afectar um pouco esse "aposematismo". Não seria repensar a categoria de ameaça, se o bicho tem essa distribuição tão restrita.
R: Yes, the river is highly affected by illegal mining, however, considering that the species also occur in Conservation Units, it is quite difficult to have them in a treat category following the recommendations of the IUCN. We considered this but, by our own personal experience, LC is surely what they will be considered by the IUCN considering the surrounding protected areas.

Associate editor’s decision after peer review (15/08/2026).
Dear Dr. Tencatt:
It is a pleasure to accept your manuscript entitled "Two co-mimic new species of Corydoradinae catfishes (Siluriformes: Callichthyidae) from the rio Tapajós basin, Brazil, with a discussion on their color pattern" in its current form for publication in the Neotropical Ichthyology.
Congratulations for the acceptance of your article, and be aware on the following topics:
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Please send an e-mail to neoichth@nupelia.uem.br within five working days to let us know you are aware of all the important points mentioned above.
Thank you for your fine contribution. On behalf of the Editors of the Neotropical Ichthyology, we look forward to your continued contributions to the Journal.
Sincerely,
Dr. Carlos DoNascimiento
Associate Editor, Neotropical Ichthyology
tepuglanis@hotmail.com

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