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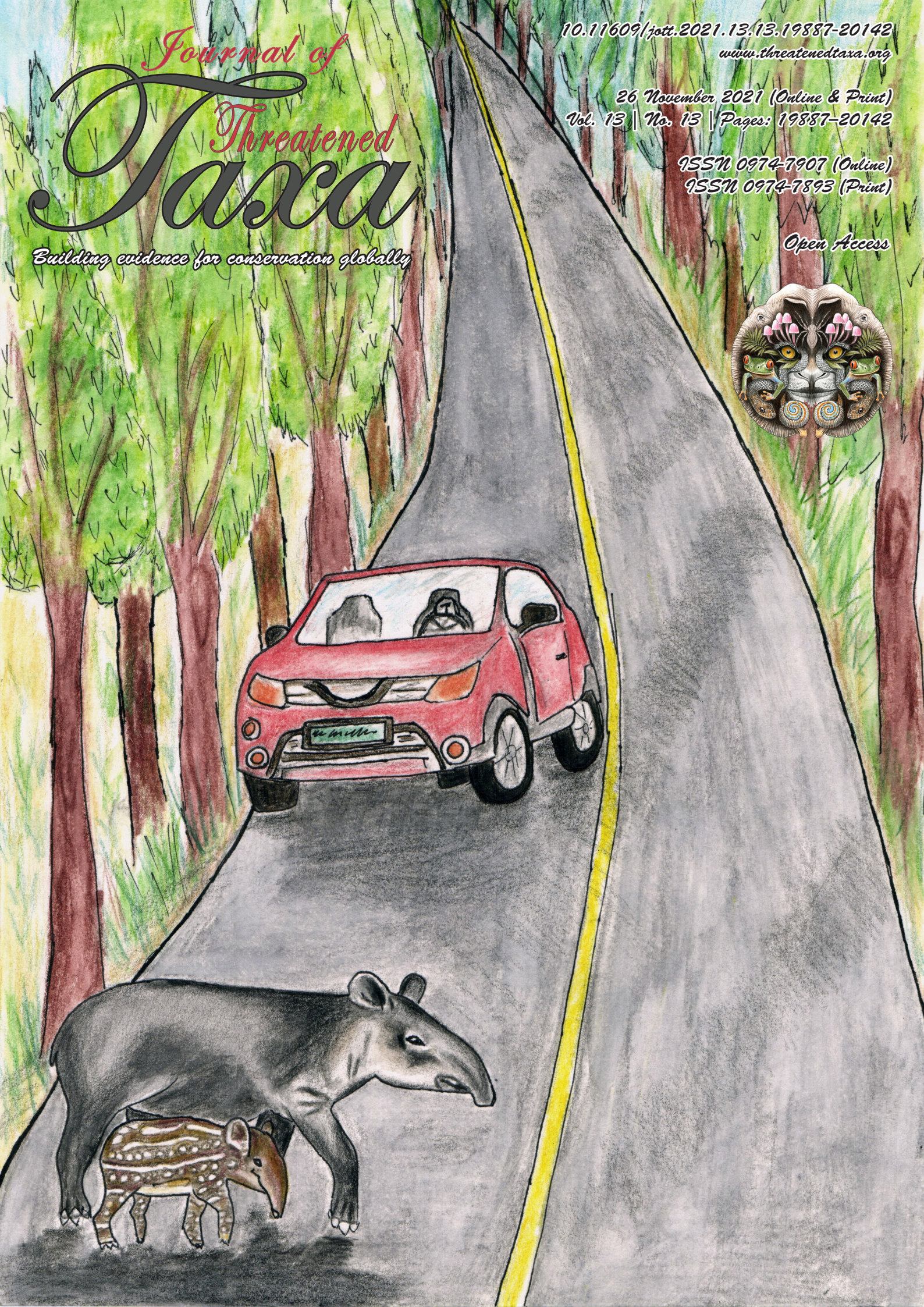
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Caption: Lowland Tapir *Tapirus terrestris* (Medium—watercolours on watercolour paper) © Aakanksha Komanduri.



If habitat heterogeneity is effective for conservation of butterflies in urban landscapes of Delhi, India? Unethical publication based on data manipulation

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Paul & Sultana (2021) published a paper on effectiveness of habitat heterogeneity for conservation of butterflies in urban landscapes of Delhi based on field sampling carried out in 2015–2017. Though the work presented in the paper is part of the Ph.D. Thesis work of the first author supervised by Das, S.K. and Singh, R. (Paul 2019), we found serious data manipulation of the data embodied in the Thesis (after award of the degree) to bring out the publication. Since our University name is tagged with the publication and the original work is from our lab only, we highlight them in the following paragraphs pointwise.

1. Paul & Sultana (2021) have distorted perception of the subject to build and test false theory with data forgery. Because, they have mentioned they have carried out sampling/assessment of nine different habitats in different urban landscapes and determined diversity indices across these habitats (Table 4) to test the hypothesis, is habitat heterogeneity effective for conservation of butterflies in urban landscapes of Delhi? In the original work these habitats were not sampled, rather transects laid across six different urban landscapes comprising sets of microhabitats were sampled (Section 3.2.4., Paul 2019) and the authors have referred those microhabitats as habitats in the paper (Section 4.2.2.1., Paul 2019). The original work is already published with another title (Paul & Sultana 2020). Hence, how they calculated diversity indices across different microhabitats (referred as habitats in the paper) for which sample size and sampling time for each microhabitat compared in the paper was not fixed?.

2. Paul & Sultana (2021) have very wisely changed the actual data collection methodology. In the original work data collection was made both from ‘Pollard Walk’ method between 1000 h and 1600 h (during which

butterfly species, their microhabitat, and activities were recorded in selected transects) and opportunistic search (at few instances to include microhabitat information of Common Evening Brown *Melanitis leda* which were mostly sighted during dusk) (Section 3.2.4., Paul 2019), but in the paper authors have mentioned data was collected in selected transects between 1000 h and 1600 h using ‘Pollard walk’ method only. Moreover, in the original work 1 km transect was covered in one hour during ‘Pollard walk’ (Section 3.2.4., Paul 2019), but in the paper authors have mentioned 0.5–1 km transect was covered in one hour for the said sampling. Again, how they calculated diversity indices across different microhabitats (referred as habitats) based on data collected from both ‘Pollard Walk’ and ‘opportunistic search’ methods in which the later method was also not standardised across different landscapes sampled in terms of sample size and sampling time? Further, if they have sampled the nine microhabitats (referred as habitats) by Pollard walk method as mentioned in the paper, which area of Delhi they selected transect of 0.5–1 km of bird droppings? and which area of Delhi they selected transect of 0.5–1 km of artificial light in the day time between 1000 h and 1600 h? They have also misused different statistics (Shapiro-Wilk test,

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Kolmogorov-Smirnov test, ANOVA) to test these data and hypothesis (Table 5).

3. Keeping in mind 'artificial light' is not a microhabitat, in the thesis it was replaced by actual site of sighting (only a single such sighting was recorded) and hence, 'house wall' was inserted to replace artificial light. Unfortunately, even after the final editing of the thesis, by mistake 'artificial light' still continues along with 'house wall' for description of nine microhabitats (referred as habitats in the paper) of butterflies sampled during the study (Section 4.2.2.1., Paul 2019). However, in the Venn diagram for microhabitats in the thesis 'house wall' rather than 'artificial light' was mentioned (Figure 12, Paul 2019) as given below. This figure is also tampered by the authors in the paper (Figure 2).

4. Paul & Sultanas' (2021) data in Figure 2 is duplicated in Table 2 with wrong headings as second heading of Table 2 should be 'Number of butterfly species exclusively found or shared' instead of 'Number of butterfly species' because the actual number of butterfly species recorded in 'flowerbeds' and 'grass' were 31 and 19 (Table 1) instead of 6 and 4, respectively (Table 2). Further, from the data presented in Table 2, the authors have inferred that the generalists can exploit a greater number of microhabitats (referred as habitats) compared to specialists found only at selected sites without clear mentioning about which species were generalists or specialists. If they have considered that exclusively found species in particular microhabitats (referred as habitats in the paper) like 'flowerbeds' and 'grass' as specialists, then it is their misinterpretation of data from ecological point of view as 'grass' was present in all and 'flowerbeds' in 50% of urban landscapes (Table 3) in which they have laid transects for sampling. Also, the authors in third paragraph, Results section of the paper have mentioned that flowerbeds alone carry 15% of the total microhabitat (referred as habitats) share (Table 2), followed by grasses with 10%, while 2.5% was observed overlapping among various microhabitats. What is the actual % overlapping among various microhabitats (referred as habitats)? In the following lines, the authors have further mentioned *Melanitis leda* (rice crop pest) is the single candidate for the artificial light source, having 2.5% of the independent share, which accidentally got noticed during another type of field study at dusk. How they calculated % independent share of this species and what was another type of field study?

5. The authors have not included the microhabitat (referred as habitats) preference of butterflies discussed in the original study (Section 4.2.2.1., 5.2.2.1., Paul 2019) in the paper (Paul & Sultana 2021). In the thesis it is

clearly mentioned that 31% sightings was for flowerbeds followed by hedges/crops/bushes (29%) and grasses (13%) that reflects microhabitat preference in terms of number of sightings rather than number of species. Further, the authors in second paragraph, Discussion section of the paper have mentioned that Northern ridge being a city forest also share the similar kind of environment as of Aravalli Biodiversity Park but due to human encroachment and trespassing, flowerbeds were missing (Paul & Sultana 2021). Such statements are confusing as a particular microhabitat (referred as habitat) may be missed out in a randomly selected transect in a particular urban landscape, but it does not mean it is absent in that entire landscape. In the same paragraph they have also mentioned two important statements related to COVID-19. First, with the outbreak of COVID-19, as the sky and air are getting unadulterated by the automobile pollutants, there are chances for the more specialist species to cope with the changing environment. Second, with further division of COVID-19 hotspot zones into red, orange, and green zones the chances of reviving city butterfly increases manifold. If the statements are a mere speculation by the authors or there is any scientific base on which authors have stated this?

The above mentioned comments on the paper (Paul & Sultana 2021) clearly indicates academic misconduct by the authors in the form of data manipulation and fabrication. Also, unethical publication destroying academic integrity. The first author of the paper has carried out Ph.D. work under our supervision using our lab facilities and funding by our University, for which we had spent valuable time of our life for the entire period of her Ph.D. research work starting from conceptualization of work, research design, arranging necessary outside facilities including official permission from IARI to help in the research work, data analysis, interpretation of result, evaluating every six monthly progress report, assistance in drafting, writing, proof-reading, editing, and finalization of the thesis. Though the student has acknowledged the contribution of the guides in the Acknowledgement section of the thesis (Paul 2019), but after award of the degree she has published papers (Paul & Sultana 2020, 2021) from her Ph.D. work not only without the names of her supervisors, but also without acknowledging them in those papers for their contribution. Further, though the hard copy of the thesis submitted by the student bears the copy right of Guru Gobind Singh Indraprastha University, the student has given second authorship to a project scientist of another reputed University in the said publications

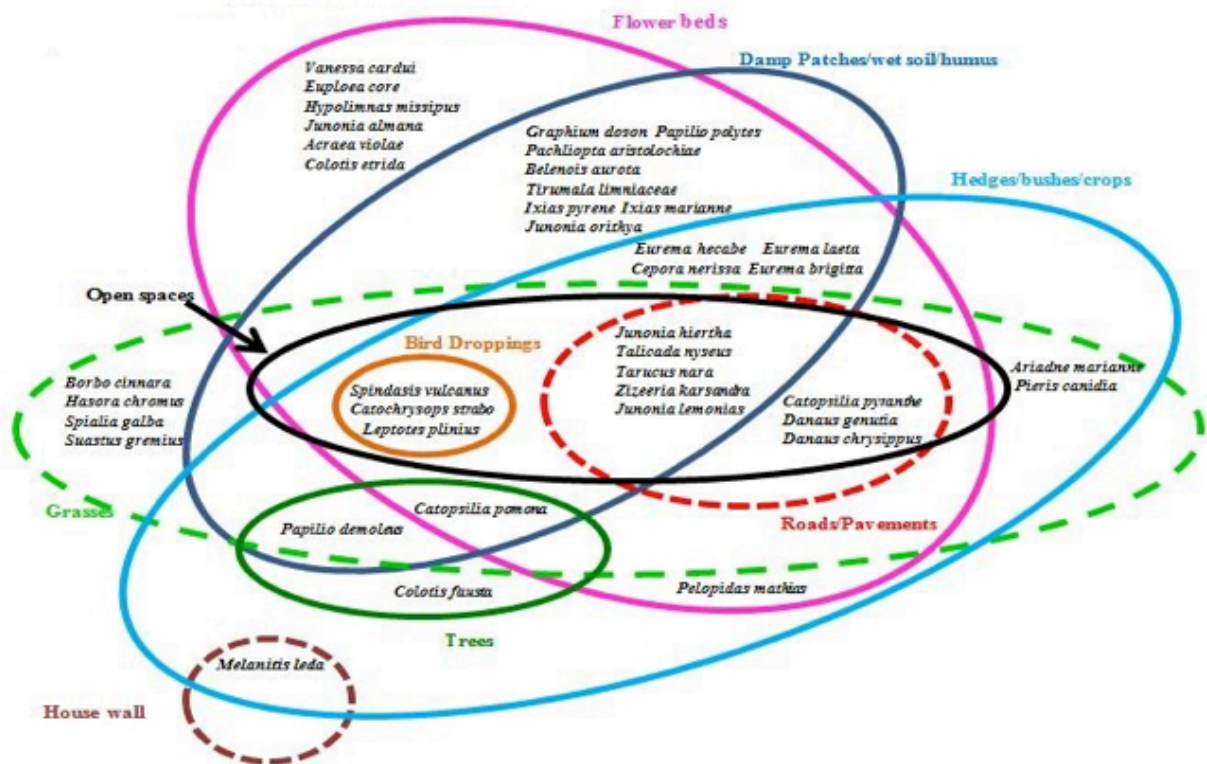


Figure 1. Microhabitats recorded for butterflies (Source: Figure 12, Paul 2019)

without permission from the University. As per our knowledge no help was taken from the second author for the Ph.D. work, except for official permission from the scientist-in-charge, Aravalli Biodiversity Park to carry out sampling in that Park, where the second author works. We want to know, in what capacity the second author was associated with the Ph.D. research work of the first author? And how has the second author really conceptualized the work, assisted in data analysis, and interpretation of result in the publication as mentioned in author contribution of Paul & Sultana (2021)?

According to guidance document prescribed by UGC for good academic research practices (Patwardhan et al. 2020), while it is vital to maintain high research quality, it is also important that research is conducted in a culture that supports honesty and integrity to ensure the highest standards of ethical practice and behaviour. Since, in the

publication by Paul & Sultana (2021) our University name is tagged, we are herewith just expecting honest reply from the authors on the above mentioned comments.

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