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| |  |  | | --- | --- | | |  | | --- | | to me  https://mail.google.com/mail/u/0/images/cleardot.gif | |   Dear Dr. Hartman,  The Editorial Board of The American Naturalist has reached a decision regarding your article, "Building a better frog trap: the benefits of mal-adaptive habitat choice for metapopulations with different life history strategies." Your manuscript has been evaluated by two reviewers and by Christopher Klausmeier, one of our Associate Editors. After reading the manuscript, the reviews, and Dr. Klausmeier's comments (pasted below), I regret that I find myself in agreement with the Associate Editor's recommendation that your paper be declined.  There was a great deal that the Associate Editor, reviewers, and I liked about your interesting manuscript. In linking a fairly abstract model to empirical data, in this case on population dynamics of Cascades frogs in variable environments, it clearly fulfills our mission to promote research at the theoretical-empirical interface. However, a series of critical issues have been raised about the modeling approach that we feel would be difficult to address successfully. Even more importantly, I agree with Dr. Klausmeier that ultimately, it’s difficult to see the generality and impact of the results, beyond its clear value for understanding this one study system. We must emphasize the goals of The American Naturalist: to publish papers that are of broad interest to the readership, to pose a new and significant problem or introduce a novel subject to the readership, to develop conceptual unification, and to change the way people think about the topic of the manuscript. Your paper unfortunately falls into the category of “with extensive revision, a very nice paper, but not one well-matched to the goals of this journal.”  As a result, I cannot accept your manuscript for publication. Because of space limitations, we can accept only 20% of submissions. Unfortunately, this means that we must decline many good manuscripts that are worthy of scientific publication. Declined manuscripts are not eligible for resubmission in a revised form. I am sure that you will find this outcome disappointing. However, the helpful and detailed comments you’ve received here will no doubt be of assistance as you consider the next step for this manuscript. Thank you for thinking of The American Naturalist as an outlet for your work, however, and best wishes for your future research.  Sincerely,  Judith L. Bronstein Editor-in-Chief The American Naturalist   MS #55426 Author: Rosemary Hartman; Noam Ross  xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx ASSOCIATE EDITOR'S RECOMMENDATION  Dear Judie:  We've received two reviews of the manuscript by Hartman and Ross and I've given it a careful reading myself. Inspired by the biology of the Cascades frog, the authors develop a model of a stage-structured population in a temporally fluctuating two-patch landscape. One patch contains a predator that the focal species can not detect, making it an ecological trap. They show that dispersing to the "trap" patch can potentially increase the average population growth rate, due to bet-hedging in the stochastic environment.  Both reviewers provided many detailed comments for the authors. Reviewer 2 was largely positive, finding the manuscript interesting and well-written, but had a number of questions about whether the model adequately captured all of the relevant biology of the frogs. Reviewer 1 was more critical, raising many technical and stylistic issues. In their private comments to the editor, they also suggested that much is already known about the advantage of using sinks, so this manuscript may not be novel enough for publication in the Naturalist.  Unfortunately I tend to agree with reviewer 1, having had many of the same questions while reading the manuscript. The issues can be addressed, but still the paper will be in the uncomfortable middle ground between broadly applicable, general theory and a detailed, parameterized analysis of a particular system. I also wonder if a patch that is on average worse but often better, is best described as a "trap" or "sink" in those years when it is actually the better patch. This might be a simpler explanation for why it is often beneficial to disperse there.  I hope the authors find the reviewers' extensive comments useful. The manuscript could be revised, but I would find it more appropriate for a less selective journal such as Journal of Theoretical Biology.  Chris Klausmeier Associate Editor xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx   Reviewer #1:   please see attached .pdf for complete review which begins as follows:  "This is an interesting paper about the value in using a sink with a variable environment in a two-habitat system for a species that has two stages (juvenile and adult). The baseline case is juvenile movement and juvenile predation in the sink, but cases in which adult move or experience predation (or both) are also examined. They find advantages to using the sink (in terms of increasing growth rate) in some cases for all four scenarios except adult movement with adult predation, with the greatest benefit with juvenile dispersal and adult predation when adult survival is low and juvenile recruitment is high (there is a trade-off between these). The manuscript does not include density dependence, or temporal autocorrelation in juvenile recruitment (which is the quantity that varies), both of which could increase the value of the sink.  I found the text, and especially the explanation of the results, hard to follow in some places (as detailed below)."   xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx   Reviewer #2:   Reviewer: John Fieberg, University of Minnesota  \*\*Summary\*\*: the authors explore the fitness consequences of ecological traps in stochastic environments using a metapopulation model motivated by the life history of the Cascades frog. They model a life history strategy where adults are sedentary, but juveniles disperse. The environment consists of two patches (one with predators on juveniles and one without). They find that, for low to moderately high levels of predation, growth rates are maximized when some individuals disperse to the predator patch. The benefit to dispersal to the predator patch depends on the level of predation, the degree of stochasticity, and the degree to which the two patches are correlated in their dynamics (with the benefit disappearing if the environment is deterministic or if the two patches exhibit a high degree of correlation). The authors then consider a few other life history strategies to determine if the conclusions change.  I found the paper to be interesting and very well written, and I expect the topic to be of interest to readers of American Naturalist. The analyses are compelling and very well done. Below, I offer a couple of comments regarding the form of the model. In addition, I've made a number of minor editorial suggestions that I hope will improve clarity.  \*\*Model Structure\*\*: the authors' models assume that all individuals within a particular stage-class behave similarly. Dispersal rates do not depend on the type of patch the juvenile finds itself in (predator or predator-free) and there is no genetic component influencing dispersal in the model. Are these features realistic? Would it be appropriate to consider heterogeneous dispersal probabilities (with, say, a bi-model distribution to reflect a mixture of "likely" and "unlikely" dispersers)? Would it be reasonable to assume that there is some base level of dispersal, but that some individuals are more drawn to traps than others? In this latter case, it would seem that dispersal away from trap habitats may be less likely than dispersal away from the predator free patches. How would these changes influence results?  I sort of wondered about these assumptions early on, but the discussion (lines 295-309) also seems to highlight a potential benefit to considering a genetic component to dispersal. If the authors were to allow for different types of individuals (those that tend to disperse and others that do not, individuals drawn to the trap and others not), then they could explore whether these strategies form an ESS (evolutionary stable strategy).  The authors also assumed that adult survival was constant (not temporally varying). This assumption seems reasonable for their motivating example and also a good approximation for many other species. Nonetheless, I wonder about the influence of this assumption on the general conclusions drawn for the other life-histories considered (pages 13 & 14). If there is no variability in adult survival, then it is not surprising that there is no benefit to the trap when adults are preyed up and adults disperse. I suspect this result could change if stochasticity were included in the model for adult survival. Thus, I wonder about the robustness of conclusions like "bet-hedging can be less useful when the ecological trap drives adult, rather than juvenile, mortality" "…(last sentence of the abstract). It would help to know - and perhaps modify this conclusion to make it clear that it is dependent on certain model features (e.g., variability in juvenile survival >>>> variability in adult survival). The same comment applies to lines 213-214 and lines 319-320 (i.e., are these conclusions dependent on the fact that adult survival did not vary temporally?).  Minor Comments (mostly editorial in nature):  1. Line 40: would it be more appropriate to state "when organisms had perfect" (rather than "where organisms had")?  2. Line 128: I suspect low water years or late freezes (the failure state) will likely exhibit a high degree of spatial coupling (relative to dispersal distances). Thus, there may be little benefit to the trap in this case.  3. Line 171: maximization is also trivially easy in the case of sigma and rho (i.e., the expression is maximized at values of 0 in both cases, correct)? Well, actually, I guess that is not true if one allows for negatively correlated environments (maximization occurs when rho = -1)….  4. Line 175: I would drop the "toward the predator patch" and just state "the value of dispersal changes" - since the rate of dispersal from predator to non-predator patch is assumed to be the same as the rate of dispersal from non-predator to predator patch.  5. More detail in the Figure 3 caption would be helpful, i.e., clarify "dispersal-growth rate curve," "spatial autocorrelation in year type" (by referring to the two patches), and "this is the average of 100000 years."  6. Lines 183-184: the definition of delta-log-lambda[sMax] here is in terms of juvenile dispersal, but the authors consider adult dispersal in Figure 4 as well. It would help to define this parameter more generally. Also, it would help to clarify what the authors mean by "attractiveness-growth rate curve."  7. Lines 195-196: I recognize that increasing investment in adult survival means decreased investment in juveniles (based on how you evaluated this tradeoff [lines 190-192]). Yet, Figure 4 is constructed using "proportional investment in juveniles" along the x-axis, so I think it is best to state results using this metric, i.e., I'd suggest "Greater investment in juvenile recruitment (and, thus, less in adult survival) increased delta-log-lambda[sMax] (Fig 4, solid line).  8. Line 202, Model A2: in effect, this change just reduces the value of S1 and increases the value of J1, so it is not all that surprising that you see very similar qualitative dynamics as model A1.  9. Lines 208, 210: see comment #7. I'd suggest re-writing these results using something like, "if…, then increasing investment in juveniles increases the value of dispersal as reflected by delta-Log[sMax]." And, "allowing…, results in a uni-modal relationship between investment in juveniles and the value of dispersal."  10. Line 211-212: "the delta-log-lambda[sMax] is lower when adults disperse than when juveniles disperse" - this result does not seem to hold for low levels of juvenile investment (there are cases in Figure 4 where the adult dispersal, predation on juveniles curve is higher than the two juvenile dispersal curves [e.g., investment < ~0.4]).  11. Line 213-214: again, I guess this result is not very surprising since adult survival is constant (i.e., in this case there is no reason to bet hedge).  12. Lines 219-220: should probably add the caveat that these results are true IF the environment is stochastic and sites are not highly correlated. Or, state that dispersal "can be better."  13. Figure 4 caption: second to last sentence, delete the "and" (in "and but"). Also, it seems to me (based on the figure legend) that the dash-dot line corresponds to "adult dispersal, predation on adults" (note, "predation" is misspelled as "predtion" in the legend), that the dotted line corresponds to "adult dispersal, predation on juveniles," and the dashed line corresponds to "juvenile dispersal, predation on adults."  14. Lines 240-241: seems like "poor information" is something different than an ecological trap. In the former case, I think of an individual having "unbiased, but imprecise" information - whereas in the latter case, individuals have "biased, but precise" information.  15. Line 267: Saether and Bakke 200 should be Saether and Bakke 2000.  16. Lines 274, 275: "Therefore, an ecological trap…" This sentence seems to follow more from the first sentence in this paragraph than the one immediately preceding it.  17. Lines 288: again, I would state this in a way that is consistent with Figure 4, "the low values of log-lambd[smax] associated with low values of juvenile investment (and thus, high investment in adults), …"  18. Lines 295-297: again, I wonder if the reason for the lower benefit of the trap in this case is just an artifact of adult survival being constant. Also, the statement that "each individual adult will spend the same proportion of time in the trap habitat" seems false. Maybe on average and in the limit, but individuals that start in the trap habitat will likely spend more time there (particularly if predation rates are high…they may never escape this habitat!).  19. Line 306: delete "an" before "dispersal."  20. I did not do an extensive check on the literature, but did not find the Carpenter et al. manuscript referenced in the paper.  xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx  xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx | | | | |