

Here's an example of letter I wrote in response to reviewers comments. Important elements of this letter include: 1) an overall summary of the major changes that have been made, and 2) specific responses to more minor editorial suggestions. The actual comments of the editor and one reviewer are in normal font and my responses are italicized.

The overall summary should discuss what you perceive to be the biggest problems with your paper. For example, any points that more than one of the reviewers brought up are probably important. Any points that required a major change in the paper are probably important. Minor points can just be listed out in a bulleted form.

Dear Editor:

I am pleased to resubmit for publication the revised version of MS#03-375 "Evolutionary potential of *Chamaecrista fasciculata* in relation to climate change: II. Genetic architecture of three populations reciprocally planted along an environmental gradient in the Great Plains." I appreciated the constructive criticisms of the Associate Editor and the reviewers. I have addressed each of their concerns as outlined below.

The most substantial revision concerns the length of the manuscript. Following the reviewer's advice, I have pared down the length 25% from 65 pages to 49 pages. This was accomplished primarily by: (1) eliminating two redundant traits (leaf area and total leaf area) which shortens Tables 2 and 4 and eliminates Figures 8 and 9, (2) presenting the information in Figures 5-7 in table format, (3) cutting Table 1 and referring the reader to the companion manuscript, (4) cutting Table 4 and including the heritabilities in Table 2, and (5) moving the information of Table 5 into the text. In addition, I have rewritten parts of the paper to provide more clarity (see specifics outlined below).

ASSOCIATE EDITOR COMMENTS:

Most of the reviewers' concerns focused on instances where the writing lacked clarity or brevity, or both. Reviewer 2 also made specific suggestions regarding revising the tables and figures. On the whole I agree with the reviewers comments, and I would encourage the author to follow their specific recommendations as closely as possible.

For both papers, you need to (early on) spell out how that particular paper relates to other the companion paper and the Etterson and Shaw Science paper. Since the same data set was used in all three, you need to clearly state the purpose/scope of each paper in relation to the others.

Differences between the papers are now explicitly stated in the last paragraph of the introduction.

You need to lay out why you're now doing classic Lande-Arnold, whereas you previously did COVa.

This is briefly alluded to in the last paragraph of the introduction and discussed in more depth in the discussion of the companion paper.

REVIEWER COMMENTS:

Specific Concerns

The Introduction contains numerous sentences that while true, give the sense of reading a lot of truisms of evolutionary genetics about additive genetic variance, heritability, genetic correlations, etc. While I agree that a fair amount of this material needs to be reviewed and defined, especially for readers that are not evolutionary geneticists, I think it would be possible to slightly re-word many of the topic sentences of these paragraphs to make them more interesting and novel for people that are already familiar with these concepts.

I have tried to make the topic sentences more engaging.

Towards this end, after the Introduction or first time these concepts are introduced, I would suggest eliminating or dramatically shortening any sentences that remind the reader of what a heritability, cross-environment genetic correlation, within-environment genetic correlation, etc. are used for, what they indicate, etc.

Done.

2. Some key references are missing from parts of the Introduction. Some of Mayr's work from the 1950's belongs in the sections about gene flow preventing local adaptation. Likewise, Kathleen Donohue's work on genetic architecture is relevant for the paragraph about estimating genetic variances under multiple field environments. Lande 1979 is also something that should be cited for the paragraph on within environment genetic correlations constraining the evolutionary response. Finally, there are numerous additional references that can be given about the constancy of the G matrix .

Mayr 1963 and Donohue et al. 2000 have been added. Lande 1979 is cited again in the introduction. Several references regarding the constancy of G-matrix *across environments* have been added.

3. On page 6, the discussion of across-environment genetic correlation is written as if the trait under consideration is fitness, which should be pointed out.

The topic sentence of this paragraph now specifically states that I am referring to the across-environment genetic correlation for fitness.

4. On page 14, it is noted that the maternal and dominance variance could not be estimated individually because of the crossing design. Yet the rest of the manuscript refers to maternal variance components, which gives a confusing impression. This should be clarified.

The manuscript now consistently discusses dominance variance and maternal effects as a single confounded variance component referred to as V_{DM} .

5. On page 15, the likelihoods need to be better described. Likelihood of what? i.e., the likelihood that the parameter is equal to some value versus the likelihood that the parameter is equal to zero? Clarify this for non quantitative geneticists. In addition, the

note that the additive variance components directly determine the rate of selection response only applies to outcrossing organisms which should be pointed out.

The structure of the log likelihood ratio tests has been clarified.

The wording has been changed to reflect the fact that V_A determines the rate of evolution in outcrossing organisms.

6. On page 16, some description should be given about how the across environment additive genetic covariance is calculated. This is often the most difficult part of calculating a cross-environment genetic correlation.

I now explicitly state that all of the components for calculating the cross-environment genetic correlation, including the additive genetic covariance, are standard output from the Quercus program.

7. The Discussion section contains several phrases that are repetitive with the Introduction that should either be eliminated or altered to include conclusions based on the current work (e.g., statements about climate change, migration, gene flow/local adaptation). In addition, the introductory paragraph of the Discussion does not give any of the conclusions of the work or set up a preview of the remaining sections of the discussion.

I have eliminated phrases that are repeated from the introduction and reorganized paragraphs to highlight results presented in this paper. The first paragraph of the discussion has been rewritten to address the criticisms outlined above.

8a. I also found that the lengthy description of the differences between the author's findings and the findings of Kelly 1993 to be too long. The basic conclusions of this paragraph are general and non-specific enough (e.g., it could be breeding design, statistical power, real biological differences, etc) that this entire section could be shortened or possibly eliminated.

This detailed paragraph was requested to be added by the previous reviewers. I has now been shortened by more than 1/2.

8b. Likewise, the description of the artificialities of the experimental design could be dramatically shortened or eliminated—many of these are inherent to the approach of doing quantitative genetic field experiments or will immediately occur to readers anyway. If absolutely necessary, perhaps these could be folded into the Methods sections as caveats or asides, so as to not break up the conceptual focus of the Discussion section.

The paragraph of caveats in the discussion has been eliminated.