Subject: Fw: response to your response
From: Heather Kharouba <Heather.Kharouba@uottawa.ca>
Date: 9/9/20, 11:30 AM
To: "Wolkovich, Elizabeth" <e.wolkovich@ubc.ca>

Haven't read it yet... Will read it tonight.

-Heather

From: Michael Singer <michael.singer@plymouth.ac.uk>
Sent: September 9, 2020 2:27 PM
To: Heather Kharouba <Heather.Kharouba@uottawa.ca>
Cc: Camille Parmesan <camille.parmesan@plymouth.ac.uk>
Subject: response to your response

Heather, Lizzie, Greetings.

We have digested your response to our MS and we hesitate to submit it, for several reasons. First, we have made some minor modifications to our MS in response to your response, so your response and our MS now have some mismatches. For example, we accept that we should not have designated eco-evo process-based models as "ecological." We have modified that part of the MS and added a reference. We have also, in response to your comments, modified our description of the relationship between your glossary definition of baseline asynchrony and our checkerspot study. Therefore, to submit the response and the MS as they stand risks puzzling readers. We attach the current version of our revised MS, which can still be modified prior to resubmission.

Your response acknowledges no conceptual contribution from our critique and ends by stating that we have provided "details of two interesting systems" but that our points "do not conflict with our paper or require corrections to our paper." If this were the case, and we had neither identified errors in your paper nor made any conceptual advance, there would be no reason for NCC to publish our critique, and they would be quite right to reject it. However, we do think that we have revealed errors and made new contributions, both conceptual and factual.
Minimally, we request that you modify your response to match the current version of our critique sufficiently that readers would not be confused if both were published together. If your revised response takes the same tone as the current version, we will submit it along with our revised critique and argue our case for publication with the editors.
There are two further sources of confusion that, for clarity, we ask you to address. First, in your description of our Figure 2, that disinters the Varley & Gradwell baseline study of Winter Moth, you state that the two lines in our Figure show "shifts in population density for winter moths (eggs) and oaks (budburst)." In fact, as the Figure caption states, neither graph shows population density and both graphs show only the moths; one shows total generational mortality of moths in each year across the entire life cycle, while the other shows neonate mortality due to asychrony with oak.

The second source of confusion is that, in your response, you state that our Figure 1, taken from Tikkanen & J-T, is less appropriate than the Figure in your paper because it is "derived from an equation not data." However, this is not right; look at the METHODS in Tikkanen & J-T and you will see that the Figure that we reproduce is derived from the same experiment as the Figure that you reproduce, with the addition of the fecundity data. Tikkanen & J-T write: "Tolerance to starvation was considered to be the predominant component of fitness if larvae emerged before budburst. After

budburst, the declining rate of fitness was estimated by multiplying the rate of pupal survival by the declining rate of fecundity." Plus, they give a graph, their Fig 4, copied here below, showing the strong effect of late hatch on fecundity. This effect is a major feature of the system - to omit it is to mislead readers as to the nature of natural selection on synchrony in this case study. You may argue that it is in principle indirect to derive fecundity from pupal weights as the Tikkanen & J-T paper does, but in practice it is not. Winter Moths do not feed, so there is a very direct conversion from weight to fecundity, shown clearly in the attached graph from Holliday 1977 that we reproduced in our Phil Trans paper.

You note that the V&G study is deficient in that it doesn't measure phenological synchrony. True, but how big a deficiency is this? V&G measure year-to-year variation in mortality that they ascribe to variable asynchrony. They show mortality at this stage is the principal component of overall generational mortality and the "key factor" driving population fluctuations. Surely, from some perspectives, this is the bottom line? As far as baseline studies go, it must be rare to get more than this, since pre-baseline ecologists were not interested in climate change or phenological synchrony, they were interested in population dynamics, so it's a happy coincidence when a study of pop dynamics accidentally reveals phenological asynchrony as a cause.

Similarly, in our checkerspot study we measured mortality that was caused by observed asynchrony: we saw hosts die and larvae starve. We did better than V&G by measuring the pattern of host senescence over time, but the study was still done from the perspective of understanding qualitative causes of insect population dynamics (attached 1972 paper), not from the perspective of measuring phenological synchrony, and this must surely be normal in baseline work.

We would hope to achieve a relationship between our critique and your response that would be constructive and informative, and that would also help to rescue our 2010 paper from its orphan status. In that paper, we introduced the concept of adaptive baseline asynchrony and discussed the roles of coevolutionary arms races and life history tradeoffs in generating this phenomenon. Thus, Singer & Parmesan 2010, and not Visser et al 2012, should be the original source for the concepts of "adaptive mismatch" and adaptive baseline asynchrony. Marcel deliberately buried our paper; as it stands your paper and the Renner & Zohner review are inadvertently keeping it buried.

sincerely,

Mike & Camille

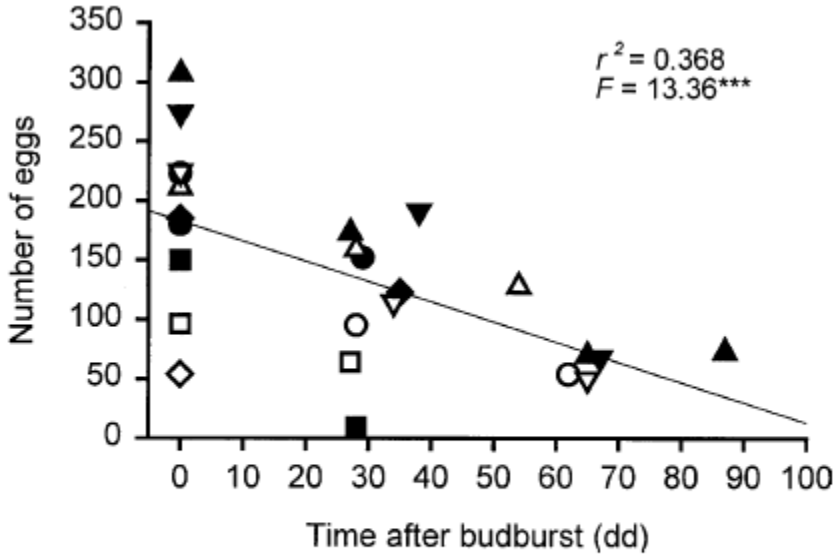

Fig. 4 Decline in the fecundity of *O. brumata* females in relation to the delay between budburst of *Q. robur* and initiation of feeding. Equation for regression line is: Y=182.9–1.7X. Symbols as in Fig. 1

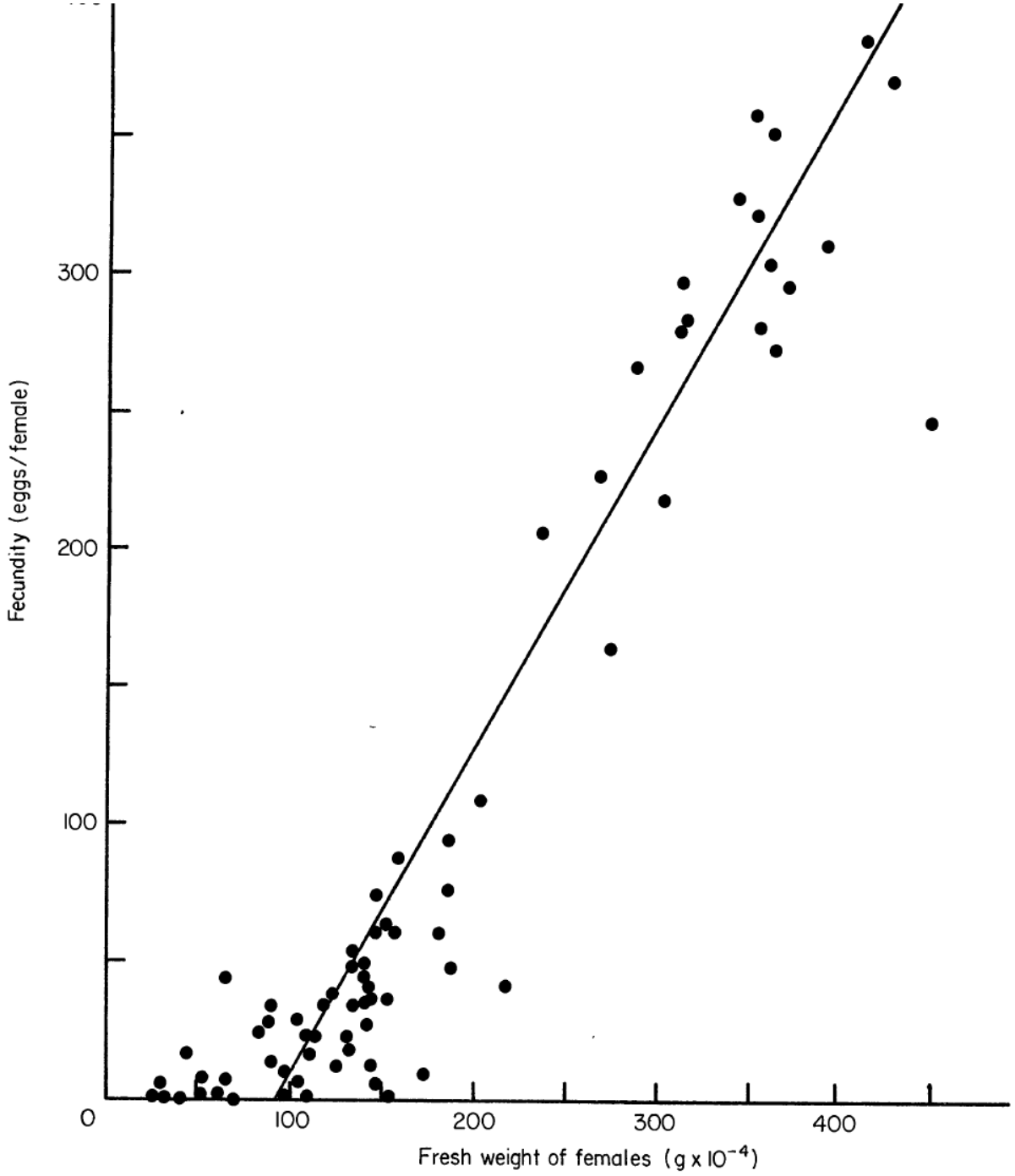

FIG. 1. Regression of fecundity (y) against fresh weight (x) of female winter moths for the 1972–3 season. Equation of the line is $y = 0{\cdot}954\,x - 67{\cdot}68$ ($F_{(1,94)} = 440$; $P < 0{\cdot}001$).

Attachments:

singer-1972.pdf	648 KB
kharouba-Wolkovich-9sept2020.docx	86.0 KB