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Beresford, N.A.; Adam-Guillermin, C.; Bonzom, J.-M.; Garnier-Laplace, J.; Hinton, T.; Lecomte, C.; Copplestone, D.; Della Vedova, C.; Ritz, C.. 2012 Response to authors' reply regarding “Abundance of birds in Fukushima as judged from Chernobyl” by Møller et al. (2012). *Environmental Pollution*, 169. 139-140. 10.1016/j.envpol.2012.05.013

Letter to the Editor

Regarding Mousseau et al.'s response to Beresford et al. (2012)

The response of Mousseau et al. to our letter on their paper "Abundance of birds in Fukushima as judged from Chernobyl" (Moller et al. 2012) is appreciated and we thank the editor for giving us the opportunity to comment on it. However, the response does not address the main recommendation of our letter: *'to make all of the underlying data for their Fukushima study available and provide further clarity on their statistical approach. This could readily be achieved by supplying additional supplementary material linked to the article on-line'*.

We suggested this because of the importance of the paper's topic, and because the original paper does not provide sufficient information for the reader to evaluate the conclusions presented (the response does not rectify this). As is, readers must speculate and this may lead to misinterpretation and potentially unwarranted criticism of the work. For instance, in trying to evaluate the study on the basis of the information as presented a number of questions are raised some of which we elaborate on below.

From Table 1, the terms *Area*Species* and *Radiation*Area*Species* are reported as highly significant (with an overall model R^2 of 0.14). The interpretation of these interactions is that abundance does change with radiation, but in complex ways described by a total of 28 different slopes and intercepts (with various positive and negative relationships), as influenced by different combinations of area (Chernobyl v's Fukushima) and species (i.e. species differed significantly in abundance in different ways in two areas). Any conclusions concerning the relationship between abundance and radiation that do not take both area and species into consideration are not justified by the statistical analysis as presented in Table 1. In particular, conclusions, such as:

"*In these 14 bird species there was a significant negative relationship between abundance and radiation (Table 1), as found for all bird species (…)*"

"*…and there was also a significant difference between areas, with density being higher in Fukushima than in Chernobyl*"

"*Species differed significantly in abundance (Table 1)*"

"*The radiation effect differed between areas, as shown by the interaction between radiation and area (Table 1)"*

appear to be unwarranted in view of the statistical analysis as reported. Additionally, the degrees of freedom in Table 1 (i.e. 16,716) suggest that the model has not taken into account the further confounding variables discussed by the authors.

Questions related to the estimated slopes presented in Table 1 of the Electronic Supplementary Material 2 of the original paper are also raised. Firstly, the majority (60%) of the abundance versus radiation data reported for Fukushima are described by a positive slope (i.e. abundance was observed to increase with increasing radiation)! Secondly, 10 of the 45 data points from Fukushima have slopes based on a sample size of one; how is it possible to estimate a slope from a single data point (no p-values are reported for the individual species slopes presented in the supplementary material)?

The above demonstrate the potential problems in the readers' interpretation of the study as presented. We therefore, reiterate our previous recommendation that the editor works with the authors to make the underlying data available so that it can be independently interpreted. As we suggested in our initial letter this could easily be achieved by making additional supplementary

material available linked to the original article or to the subsequent correspondence being published here. This is the only way to allow other research groups to perform meta-analyses and independently check the robustness of the conclusion regarding the causal link between radiation levels and bird abundances. The provision of further supplementary material presenting the statistical code used to conduct the analyses would further improve clarity.

Whilst the paper presents interesting results, concluding causation to a correlation of bird abundance and radiation levels for the Fukushima area is, we consider, inappropriate with only one sampling period and no baseline data. The abundances observed may well have existed prior to the accident and there may have been in-direct effects associated with the earthquake, tsunami, resultant relocations of human populations and changes in farmland management during 2011 as a consequence of radioactive fallout from the Fukushima accident.

On a more technical note in their letter Mosseau et al. quote radiation measurements in $\mu Sv\ h^{-1}$. The sievert (Sv) is the unit of effective dose and equivalent dose for humans. For other organisms, only the gray (Gy), the unit of absorbed dose, should be used as the estimation of effective and equivalent doses requires the application of weighting factors which have not been defined for organisms other than humans. We accept that the authors are likely using the results of their contamination monitors as a measure of relative exposure between their study sites. However, the use of Sv in such studies is inappropriate and will lead to misinterpretation. Information on the spatial scale of the areas monitored would also help the reader given the high spatial heterogeneity of radionuclides in the study environments. Furthermore, such simplistic measurements of 'radiation' are unlikely to give comparable measures of total exposure (internal and external) to birds at Chernobyl and Fukushima given the different radionuclides present and the likely variation in transfer from soil to birds.

Finally, we note that Mosseau et al. refer to a paper by some of the authors of this letter (Garnier-Laplace et al. 2012) in their response. We draw the attention of readers to the main recommendations of the Garnier-Laplace paper as they are pertinent to this discussion: '*We call for more robust strategies in field sampling, with adequate design to deal with confounding factors. ..... A strict rigorous comparison is needed of controlled tests and field studies. Field data sets outcoming from robust strategy are still needed .....*'.

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