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Comment

Flawed Meta-Analysis of Biodiversity Effects of Forest Management

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Introduction

It appears that the negative effect of forest management on biodiversity has become an axiom. Whether the negative effect, however, is a fact based on solid empirical evidence is not self-evident. Most of the studies that address the issue suffer from a lack of geographic extent and taxonomic narrowness. Therefore, a synthesis drawing together results from the individual studies is direly needed. In their recent paper, Paillet et al. (2010) rise to this challenge and present a formal pan-European meta-analysis of data from 49 papers representing 120 individual comparisons across 10 taxonomic groups. Their synthesis has the potential to be a landmark paper in ecological research, but also to affect pan-European forest policies and conservation prioritizations. In any meta-analysis, selection of studies to be included is critical for the conclusions to be reliable, but in such a potentially high-profile contribution as the synthesis by Paillet et al., a particularly high level of scrutiny of the data is called for. Here we draw attention to four major shortcomings in Paillet et al. that undermine the conclusions of their meta-analysis.

Independence of Observations

Because the methodological details of the individual studies in a meta-analysis are lost, it is the duty of authors to ensure that the studies included were conducted with good scientific methods, including proper replication with respect to the question at hand. The question Paillet et al. address is whether there are consistent differences in species richness between unmanaged and managed forests at a pan-European scale. This calls for selecting studies in which a group of independent, unmanaged forests has been compared with a group of independent, managed forests. Paillet et al. included a number of studies in their meta-analysis that are classically pseudoreplicated (Hurlbert 1984) relative to their question: samples drawn from one managed forest patch were compared with samples from one unmanaged forest patch. Because the replication is not at the proper level, the results of these studies are not indicative of differences in species richness in relation to management. This problem might have been accounted for in the meta-analysis with weighting the effect sizes with the number of true replicates instead of the number of within-patch pseudoreplicates. Nevertheless, there is a further, more profound complication in the use of pseudoreplicated studies as a part of meta-analysis: such studies typically have inflated effect sizes because of the artificially low variation among replicates that are actually interdependent samples of the same species pool.

A good example of within-patch pseudoreplication is the carabid beetle studies Paillet al. used. Five (Magura et al. 2000, 2003) out of the eight comparisons made with carabid beetles were pseudoreplicated, and, additionally, represented exactly the same type of forest management. Beetle samples from one patch of unmanaged deciduous forest were compared with samples from one patch of former deciduous forest converted into spruce plantation. Based on these data, one can hardly draw a conclusion that forest management in general has a particularly strong negative effect on that taxonomic group. Furthermore, Paillet et al. extracted data from papers in a manner that created pseudoreplication. This is exemplified

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by their treatment of Vellak and Paal's (1999) data. The original design of Vellak and Paal's (1999) study (three replicates of managed forests compared with three replicates of natural forests) allows for analysis of the effect of management, and it would have been appropriate for Paillet et al. to include the study in their meta-analysis as one data point. They split the study into three different managed versus natural forest comparisons, however, which means true replication was lost.

Splitting one study design into several comparisons causes yet another problem. All such comparisons are affected by the same local forest characteristics and management history, and it is questionable whether they represent independent observations. The interdependence of the samples is further emphasized when splitting of studies resulted in data from the very same unmanaged forests being included in different, apparently independent, comparisons (e.g., Martikainen et al. 1999, 2000; Magura et al. 2000, 2003; Hjälten et al. 2007).

Biased Taxonomic Distribution

The distribution of taxonomic groups is strongly biased among the variables "time since abandonment" (TSA) and "management intensity." Paillet et al. conducted a regression analysis between the TSA and the effect size across all the taxa and conclude that "...the older the management abandonment, the higher the species richness in unmanaged than in managed forests" (p. 106). Nevertheless, what they fail to notice is that the studies with shortest TSAs ($\leq$20 years) are strongly biased toward vascular plants (which show a positive general response to forest management), whereas the studies with longest TSAs (160 years) mainly concern saproxylic beetles and fungi (which show negative general responses). Therefore, the negative slope of the regression appears to be an artifact arising from a biased taxonomic distribution. Paillet et al. then used regression equations to show that "species richness became higher in unmanaged forests around 18 and 43 years after management for carabids and fungi, respectively" (p. 106). Because the data on the TSA range from 42 to 70 and from 50 to 160 years for carabids and fungi, respectively, these extrapolations are unfounded and any conclusions based on such an approach are likely to be flawed.

The biased taxonomic distribution also affects their result that the overall response of forest management is significant only when management intensity is very high (clearcut with species change). Here, five out of the 12 studies of this management intensity concern carabid beetles. Furthermore, these particular data were derived from above-mentioned pseudoreplicated comparisons and thus have inflated effect sizes. When these studies are excluded from the meta-analysis, the result changes from a significant negative effect to a nonsignificant positive effect (random-effects model, Hedge's $d_+ =$ 0.40, 95% CI bootstrap = -0.12 to 0.82). Clearly, there are no grounds for the generalization that there is an overall negative effect of forest management in this management group.

Exaggerated Taxonomic Generalizations

Several times Paillet et al. used a functionally constricted group of species as a surrogate of a higher taxonomic group. This is particularly evident among fungi. All of the 12 studies of fungi included in the analysis concern saproxylic fungi, which depend on dead wood. None of these studies include information of any other functional groups, such as the mycorrhizal species or litter decomposers. Despite this, Paillet et al. draw a conclusion that forest management reduces the species richness of fungi (i.e., the whole kingdom).

Paillet et al.'s use of data from studies of saproxylic beetles exemplifies another of their exaggerated generalizations. Six out of 17 studies of saproxylic beetles selected by Paillet et al. were of bark beetles. Bark beetles represent <10% of all saproxylic beetle species, are restricted to the very first stages of wood decay, and several species of that particular group thrive well in managed forests because they are able to use, for example, logging slash or edges created through clearcutting or they are able to kill trees within monocultures. Therefore, the bias toward studies of bark beetles likely hinders elucidation of the actual magnitude of the effect of forest management on the entire group of saproxylic beetles. The weaker effect of management on bark beetles can be seen by comparing the papers of Martikainen et al. (1999, 2000): there are two comparisons between natural and managed forests, and the effect sizes are -0.64 and -1.65 for bark beetles and -1.96 and -3.10 for all saproxylic beetles. Moreover, these two papers use data collected in the same sampling event: data on bark beetles in Martikainen et al. (1999) are part of the data on all saproxylic beetles in Martikainen et al. (2000). Therefore, Paillet et al. should not have included the two as independent studies of saproxylic beetles in the meta-analysis.

Incoherent Inclusion Criteria

Finally, the inclusion criteria of the studies and the actual inclusion of the studies seem to be less than perfect. In the supplementary material, Paillet et al. state that "To be included in the analysis, the paper had to give summary data (i.e., mean, standard deviation, and sample size)...." For an effect size to be obtained from a study, all that is needed is sample size and either the probability value or any test statistic (Rosenthal 1991). Thus, Paillet et al. may have overlooked a great deal of relevant literature. On the other hand, Paillet et al. did not always obey their own rules. They state that "individual studies that compared mature forests with a young regeneration phase or clearfellings were excluded..." (p. 103). Nevertheless, they included, for example, four comparisons from Sippola et al. (2002), who compared old-growth forests with 15-year-old clearcuts.

Conclusions

We share Paillet et al.'s concern that there are critical geographic and taxonomic knowledge gaps in forest research and agree that using overall species richness as a measure of biodiversity value is likely to be misleading. By ignoring the identity and function of the species, one runs a high risk of losing those species one would most like to conserve. Nevertheless, we believe Paillet et al.'s meta-analysis may not improve understanding of the effects of forest management on biodiversity because the authors did not scrutinize the data they selected to include in their study or use the proper analytical techniques; thus, their scientific conclusions and recommendations for conservation priorities are suspect.

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