Invited reply

On the obvious positive interspecific relationship between abundance and distribution: a reply to Blackburn and Gaston

Thomas Kuhn described normal science as ‘... research firmly based upon one or more past scientific achievements ...’, that ‘... does not aim at novelties of fact or theory and, when successful, finds none’ (Kuhn 1996). Kuhn divides scientific enterprise into three faces: normal science, crisis and revolution. In his view, normal science is paradigm driven and the enthusiasm to find supportive results inevitably leads into publication bias; supportive studies get accepted more easily, resulting in an overestimate of the true effect size. Only after a while does the weight of odd observations and scattered evidence against the paradigm begin to be enough for the paradigm to enter a crisis and finally the paradigm may be altered or even rejected. If one subscribes to Kuhn’s view of scientific progress, it seems that one important task of scientists is to challenge the prevailing paradigms.

Recently, we (Komonen et al. 2009) challenged the paradigm according to which one of the most general patterns in ecology is a positive interspecific relationship between abundance and distribution. Blackburn & Gaston (2009) (B&G) took up our challenge and criticized our paper on two grounds. First, there is a methodological critique and second, B&G criticize us for dismissing the positive relationships by suggesting that ‘90% of relationships that are positive are artefacts’. First B&G criticized our method to control for sampling effort. Although we agree that applying corrections into a dataset may generate unwanted biases, it appears that the imaginary example B&G provide to pinpoint a potential problem in our method is mistaken. As B&G state, in their example, the uncorrected density of both species $\alpha$ and $\beta$ is 1, while there is a 10-fold difference after the sampling effort is accounted for ($\alpha = 1$ while $\beta = 0.1$). Where B&G’s argument fails is the statement that both species were sampled at each site to the same intensity. While this is indeed correct, sampling effort at each site is not relevant in the context of our paper. We are asking questions where the sampling effort for each species (rather than at each site) is of interest. What B&G failed to notice is that in their example, species $\alpha$ and $\beta$ faced a 10-fold difference in the sampling effort: species $\alpha$ faced a sampling effort that was 1 (one visit to the one-occurrence site of species $\alpha$) while species $\beta$ faced a sampling effort that was 10 (one visit to each of the 10-occurrence sites of species $\beta$).

B&G further argue that the negative pattern we report is owing to a bias generated by our method, and more specifically that the rarest species would be most heavily biased. To this end, we can analyse our data to see whether this is the case. By subtraction, we calculated the difference between the uncorrected density and the density corrected for sampling effort. This difference reflects the effect of our correction on the estimate of the density. For B&G’s expectation to be correct, there should be a significant correlation between this difference and the distribution of the species. However, there is no such correlation (Spearman’s $r = 0.04, n = 95, p = 0.708$), indicating that our method does not generate bias in relation to the rarity of the species.

The second criticism seems to stem from an unfortunate misinterpretation of our paper, leading B&G to make our position to look more extreme than what it in fact is. Rather than claiming that positive relationships are artefacts, our main message, clearly spelled out in the abstract and discussion, was that perhaps we should consider the possibility the abundance–distribution relationship is not linear over the entire range of distributions. Indeed, based on the literature and the data presented in the paper, we suggested that there seems to be ‘a positive non-trivial relationship for widespread mobile species but no, or indeed a negative one for more rare species’ (Komonen et al. 2009; see also Päivinen et al. 2005). B&G have earlier noted themselves that the relationship at hand appears to vary considerably across assemblages, from significant positive to significant negative and to all shades in between (Blackburn et al. 2006). Indeed, they show that 10 per cent of the relationships are negative and 18 per cent non-significant, making nearly one-third of published observations inconsistent with a positive relationship. These findings are difficult to reconcile with B&G’s statement that they ‘find nothing in Komonen et al. (2009) to alter the widely held view that the relationship between abundance and distribution is linear across the entire range of distributions: the pattern may be negative but far more rare species are included and positive when more widespread species are included.

Janne S. Kotiaho1,2,* Atte Komonen3 and Jussi Päivinen4

1Centre of Excellence in Evolutionary Research, Department of Biological and Environmental Science, and 2Natural History Museum, FIN-40014 University of Jyväskylä, PO Box 35, 40014 Jyväskylä, Finland
3Department of Ecology, Swedish University of Agricultural Sciences, Box 7044, 75007 Uppsala, Sweden
4Natural Heritage Services of Metsähallitus, PO Box 36, 40101 Jyväskylä, Finland

*Author for correspondence (janne.kotiaho@jyu.fi).


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