The role of soil community in plant population dynamics: is allelopathy a key component?

In his recent TREE news & comment, Watkinson drew attention to the role of soil microorganisms in plant population dynamics. In particular, he reported on the dynamical framework for the inter-relations between the composition of plant and soil communities, proposed last year by Bever et al. It is between the composition of plant and soil on the dynamical framework for the inter-relations plant population dynamics. In particular, he reported that the soil community is something that plant population biologists cannot ignore.

Allelopathy has been defined by Rice1 as ‘any direct or indirect harmful or beneficial effect by one plant (including microorganisms) on another through production of chemical compounds that escape into the environment’. If we apply this definition to allelopathy as a potential component in the feedback model proposed by Bever et al.2 It is worth adding allelopathic interactions to this picture. Allelopathy has been defined by Rice1 as ‘any direct or indirect harmful or beneficial effect by one plant (including microorganisms) on another through production of chemical compounds that escape into the environment’. If we apply this definition to allelopathy as a potential component in the feedback model proposed by Bever et al.2 It is worth adding allelopathic interactions to this picture.

The second concerns the feedback model proposed by Bever et al.2 The authors do not depict any direct or indirect interaction between the two plant species in their model. Nevertheless, one could add connections between them because of the potential occurrence of direct allelopathic interactions among plant species. I agree with Watkinson’s conclusion that ‘the soil community is something that plant population biologists cannot ignore’ (if indeed they do), but they should not ignore allelopathy either.

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References

Reply from A.R. Watkinson

Pellissier is quite right to draw our attention to allelopathy as a potential component in the interaction between plants and microorganisms in the soil. But readers familiar with John Harper’s strong views on the subject3 will not be surprised to know that I, having been a student of his, am also rather sceptical about it. Unfortunately, Rice’s definition quoted above is not at all helpful in defining allelopathy so broadly as any harmful or beneficial effect, direct or indirect, produced by a chemical that just happens to have escaped into the environment. That means that carbohydrate exudate from the root or the chemical compounds from a damaged piece of root are potential allelopathic agents. Most people would not accept that as allelopathy and indeed it is not what is studied. Others restrict the definition of allelopathy to a form of interference competition by means of chemical compounds produced by one species that reduce the performance of other species4. Whether this interaction is direct or indirect is – I believe – critical, especially when one considers how allelochemicals may have evolved. I suspect that the direct interaction is relatively rare, unfortunately it is impossible to say how rare or common a phenomenon it is, as many of the criticisms made by Harper3 and others of the methodologies involved in demonstrating allelopathy, and in particular the use of leachates, still apply. I would not dispute that chemical compounds (carbohydrates, proteins, phenols) from the roots of plants may have an impact on the microbial community and thus potentially on other plants as outlined in my original article. But are simple carbohydrates allelochemicals?

A computer literature survey (BIDS) of references to allelopathy in the past 10 years revealed that the subject barely merits a mention in the mainstream ecological literature: e.g. American Naturalist (5), Journal of Ecology (5), Oikos (1), Ecology (5), Oecologia (10). While agronomists, weed scientists and foresters clearly have more time for the concept, most of the 455 references to allelopathy are in the specialist Journal of Chemical Ecology (100) and Phytochemistry (23). There remain few attempts to relate the results of laboratory experiments to field situations.

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References

Reply from J. Bever, K.M. Westover and J. Antonovics

Watkinson2 and Pellissier provide valuable perspectives on our model of the impact of the soil community on plant population dynamics3. The routes for such feedback can indeed be quite complex. In our work within a grassland in North Carolina, USA, we found that the accumulation of host-specific pathogens from the genus Pythium plays an important role in generating the negative feedbacks on plant growth that are common within the system4,5. However, we have also found...
In recent TREE review, Connor et al.² provide an excellent and thought-provoking comparison of behavioral ecological patterns between toothed whales (odontocetes) and terrestrial mammals, particularly primates. The authors end their review with a provocative call for efforts to quantify the relationship between behavioral ecology and brain size among odontocetes in a similar manner to Dunbar’s analyses for primates.³ Nevertheless, in doing so they leave the reader with the impression that some qualitative studies have not yet been attempted. There are two studies that do exemplify the very approach Connor et al. advocate: Westover and colleagues⁴ provide quantitative support for the ‘combination of convergence and novelty’ suggested by observational and qualitative comparisons of brain and behavior between odontocetes and primates.

First, there is a significant positive correlation between pod size and encephalization quotient (a measure of relative brain size taking into account brain-body allometry) among 21 odontocete species from all six odontocete families⁵. Therefore, the relationship between one measure of sociality (i.e. social group size and brain size) appears to be similar in primates and odontocetes.

Second, although there is a positive correlation between encephalization level and gestation length among primates⁶, in an analysis using the same encephalization values for the 21 odontocete species already mentioned, there is no significant relationship between encephalization and gestation length among odontocetes⁷. Rather, body size accounts for more of the variation in gestation length than encephalization among odontocetes. Therefore, there is quantitative evidence for differences in brain and life history relationships across primates and odontocetes.

My point here is not to criticize Connor et al. but to strengthen and extend their emphasis on quantitative analyses of odontocete behavioral ecology by showing that these kinds of studies are already underway. It is, of course, important to continue to further these studies while moving towards formulating and testing hypotheses about the evolution of cetacean brain–behavioral relationships and its implications for general mammalian evolution.

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Quantifying brain–behavior relations in cetaceans and primates

In their recent TREE review, Connor et al. provide an excellent and thought-provoking comparison of behavioral ecological patterns between toothed whales (odontocetes) and terrestrial mammals, particularly primates. The authors end their review with a provocative call for efforts to quantify the relationship between behavioral ecology and brain size among odontocetes in a similar manner to Dunbar’s analyses for primates. Nevertheless, in doing so they leave the reader with the impression that some qualitative studies have not yet been attempted. There are two studies that do exemplify the very approach Connor et al. advocate: Westover and colleagues provide quantitative support for the ‘combination of convergence and novelty’ suggested by observational and qualitative comparisons of brain and behavior between odontocetes and primates.

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