Reply from W.J. Sutherland, G.A. Parker and P.A. Stephens

We admire the fine primatological research to which Hanscort rightly draws our attention, and which shows the relationships between rank, group size and feeding behaviour. There does, however, seem to be a difference in terminology between our work and theirs. We considered interference in terms of a quantified relationship between population density and mean or individual intake rate, as used in innumerable entomological and some ornithological studies. Many primate studies, for sensible reasons, consider interference as a change in foraging efficiency, activity budgets, or feeding/foraging durations with group size; however, these measures may have a complex relationship with intake rate. Some studies do relate intake to rank (e.g.Refs 2–5) and group size (e.g. Ref. 5).

Although in such examples the data are not presented in a manner that can be used to calculate the strength of interference, we suggest it would be extremely useful, and probably reasonably straightforward, to use such data to do so — both to incorporate within theoretical models and provide comparisons with other taxa.

William Sutherland
School of Biological Sciences, University of East Anglia, Norwich, UK. NR4 7TJ (w.sutherland@uea.ac.uk)

Geoff Parker
Population Biology Research Group, School of Biological Sciences, Nicholson Building, University of Liverpool, Liverpool, UK. L69 3BX (gap@liverpool.ac.uk)

Philip Stephens
School of Biological Sciences, University of East Anglia, Norwich, UK. NR4 7TJ (philip.stephens@uea.ac.uk)

References

CORRESPONDENCE

Assessing ecosystem health

I recognize the importance of much of the material in Rapport et al.’s recent TREE article, but criticize the basic theme around which it is organized. There are problems with the concept of ecosystem health, and in particular with the assumption that it is both analogous with and contributory to human health.

There are well known difficulties in producing a rigorous definition of human health, but the idea that all was well before civilization came along and that this amounts to much of ‘noble savage’ mythology is wide of the mark. Wild populations of animals are not normally free of parasites, and we must imagine ‘primitive’ humans as also living in a fluctuating equilibrium with disease organisms of all kinds. Also, a population living with endemic malaria would not be regarded as a healthy one in modern terms. We presumably wish to define ‘healthy’ in terms of what can be expected under modern conditions, rather than in some primitive state that could be described as more ‘natural’. The ecosystems visited by the early European settlers in Africa were probably reasonably healthy in their own terms. That did not stop them being called the white man’s grave.

This immediately raises problems when we talk about humans as part of a wider ecosystem. The health of that system includes the welfare of the malaria parasite, and there is a logical negative correlation between that and human health. This is the fallacy of supposedly holistic views that assume no fundamental conflicts of interest, and it is one of the basic problems of the Gaia hypothesis. Even if we leave humans out of it, the health of an ecosystem is still not a variable that can be defined in value-free terms. Rapport et al.’s example of the Ponderosa pine ecosystem includes some very clear value judgements: for instance, parasites of trees are an indicator of poor ecosystem health whereas saprophytes signify good health. This is obviously being seen purely from the standpoint of trees and their human consumers, rather than from any more objective view of overall ecosystem health. The second problem with the ‘healthy ecosystem’ approach to the ‘services’ provided for humans is that mention is rarely made of the biggest of them all — the production of food. Highly productive agriculture always seems to involve ecosystems that by any definition are degraded. The biggest conservation dilemma is whether our population can continue to be fed and clothed. Nourished and warm without destroying most of what is left of even-paucity ‘natural’ ecosystems in the process. The present catastrophic destruction of the Amazon forest ecosystem described in another TREE article cannot be condoned, but would our objections be as easy to defend if there were a convincing scheme to replace it with productive and sustainable agriculture?

The discussion of the global ecosystem as a holistic unity ignores these dilemmas completely. If the prime determinant of human health is adequate nutrition, it is unlikely to correlate with the measures of ecosystem health being proposed.

None of this is to deny the importance of an ecological view of both agriculture and human health, and much that is said by Rapport et al. is very valuable, but the ecology involved needs to be focused and relevant to specific problems, and not encumbered by mystical ‘holistic’ terminology.

D.A. Wilkins
School of Biological Sciences, University of Birmingham, Birmingham, UK. B15 2TT (wfp@pemail.net)

References

Reply from D.J. Rapport, A.J. McMichael and R. Costanza

The letter of D.A. Wilkins raises several issues concerning the validity of the ecosystem-health concept. His critique centers on the question of whether ecosystem health is both analogous with and contributory to human health. We have argued for this view, citing a number of references to case studies where the health metaphor has been a primary motivation, as well as number of studies where the human health consequences of ecosystem health status are transparent.

Wilkins goes on to raise other issues, some of which appear to bear little reference to our article. For example, he suggests that we are advocating or implying that ‘all was well before civilization came along’. We find no reference to this point of view, and would like to issue with it. We have argued, however, that human-dominated ecosystems have recently become highly degraded with considerable risks and costs to humans1,2. He goes on to argue that our examples of healthy and unhealthy Ponderosa pine forests (Box 2 of our article) is considered purely from the standpoint of trees and their human consumers. We disagree. We have suggested and documented elsewhere3,4,5 that, apart from any heavy grazing and fire suppression, these forests have become highly dysfunctional — evidenced, for example, by reduced species diversity, increased disease prevalence, reduced rates of decomposition and nutrient cycling. These indicators, to be sure, have implications for humans, as well as the species that comprise this ecosystem.

The fact is there are no convincing schemes, such as argued by Wilkins, to replace the Amazon forest ecosystem, which has now been destroyed to a point of no return by human acts. The global impacts (global warming, etc.) should be an eye opener for modern civilizations when they consider their dependence upon these systems for their own survival.

Wilkin’s point that ‘discussion of the global ecosystem as a holistic unity ignores certain issues, may be a valid point. However, our article did not discuss the global ecosystems as a holistic unity, rather we showed how analyses of particular systems, contextually based, can be carried out within an ecosystem-health framework. Finally, we think it unlikely to brand the efforts of many investigators referred to in more than 50 selected references as being flawed by ‘mythical, ‘holistic’ terminology. This would be news not
Correspondence

David Rapport
College Faculty of Environmental Design and Rural Development, University of Guelph, Canada. N1G 2W1 (drrapport@ecnres.uoguelph.ca); Faculty of Medicine and Dentistry, The University of Western Ontario, Canada. N6A 5B7

Robert Costanza
Center for Environmental Science, Biology Dept, and the Institute for Ecological Economics, University of Maryland, PO Box 38, Solomons, Maryland, PO Box 38, Solomons, MD 20688, USA (costanza@ucr.uences.edu)

Robert Costanza
Center for Environmental Science, Biology Dept, and the Institute for Ecological Economics, University of Maryland, PO Box 38, Solomons, Maryland, PO Box 38, Solomons, MD 20688, USA (costanza@ucr.uences.edu)

References

Resource sharing in plant–fungus communities: did the carbon move for you?

The evolutionary speculations of Perry1, in his news & comment report on Wilkinson’s paper2 on resource sharing in plant–fungus communities are, perhaps, premature. The evidence that plants do share resources is at best equivocal. Where carbon does move between plants, the evolution of mycorrhizal fungi is difficult to explain by genetic drift alone. Fitter et al.2 have suggested interplant transfer is restricted, however, the evidence for all of these is uncertain or restricted.

As yet, the evidence for all of these is uncertain or restricted.

References

Contrary to what Fitter et al. state, transfer of labeled carbon into the shoots of abietic mycorrhizal (AM) plants has been shown in two species, because any effects on fitness must be large variability in the amounts transferred between any two plants, even when they potentially share mycorrhizal fungi. For this reason, I am unable to conclude in a short experiment what evidence for interplant transfer; in terms of functional importance remain unproven by the experimental evidence currently available.

Alastair H. Fitter
Angela Hodge
Tim J. Daniell
Dept of Biology, University of York, PO Box 737, York, UK. YO10 5YW (ahf1@york.ac.uk)

David Robinson
Scotch Crop Research Institute, Invergowrie, Dundee, UK. DD2 5DA

Reply from D.A. Perry

Conversely, what Fitter et al. state, transfer of labeled carbon into the shoots of abietic mycorrhizal (AM) plants has been shown in two species, because any effects on fitness must be large variability in the amounts transferred between any two plants, even when they potentially share mycorrhizal fungi. For this reason, I am unable to conclude in a short experiment what evidence for interplant transfer; in terms of functional importance remain unproven by the experimental evidence currently available.

Alastair H. Fitter
Angela Hodge
Tim J. Daniell
Dept of Biology, University of York, PO Box 737, York, UK. YO10 5YW (ahf1@york.ac.uk)

David Robinson
Scotch Crop Research Institute, Invergowrie, Dundee, UK. DD2 5DA

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