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LETTER TO THE EDITOR

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We are pleased that our note has attracted some attention and welcome the opportunity to respond to the points made by Goodman *et al.* That said, we do not feel that these authors adduce any new evidence; we focus therefore on what we perceive to be the three main questions raised. The first is whether the spectrum of a particular sample corresponds to a single free radical species. For virtually all the systems we have studied (e.g.[1]), there is no evidence that it does not: the shape of the spectrum is independent of the history of the sample. The only exception that we have seen we have published, that of oak seeds[2]. We have stated[2] unambiguously that in the oak system there were two spectral features 'consistent with the presence of two different radicals whose relative concentration depended on moisture content', at least in X-band measurements. As to the question of whether the spectra observed for different species arise from chemically identical free radicals, we would simply say that over several years, in various biological systems, we have, as a standardizing procedure, recorded spectra of plant samples first separately then mixed with the same desiccated moss, the amounts of the two biological components being adjusted so that the spectra of each separately were of comparable intensity. As we have reported[1,3,4], the spectra of the mixtures are indistinguishable from those of the separate components. Goodman draws attention to the limitations of X-band measurements. We are fully aware of these; indeed, were we not we might never have made measurements at W-band[3]!

The second question is over our interpretation of the spectrum as that of a quinone. In the absence of any further experimental evidence giving support to a different identification, this is not a constructive question. The fact is that while several laboratories, including Goodman's, may have recorded similar X-band spectra over many years, we took the initiative to explore the spectrum at higher frequencies (Q- and W-band, the former unpublished, the latter published in conjunction with colleagues at Berlin - see[3]); we described spectral data of authentic quinones as a comparison; and, finally, we provided a reasoned argument based on experimental data, not opinions, on the identity of the stable radical as a quinone. Goodman's contention that we did not consider alternatives is wholly inaccurate. We are on record [1] as saying that 'the apparent spectral coincidence with moss seen throughout our observations does not, of course, preclude the existence of other radical species, some of which may reflect the highly varied forms of phenolic compounds present in angiosperms though not so in bryophytes'.

The third point, on the association of the epr spectra with senescence is in our view a semantic commentary on the age-old problem of distinguishing living tissue from dead. Goodman's comments are not original and echo, closely, those we made in an earlier review of just this problem[5]. Nearly thirty years ago, one of us claimed that stable radicals observed in humic acid fractions from peat and soil were semiquinone ions [6]. To our knowledge, no-one has provided experimental evidence to dispute that. And there is no other more widespread end- product of senescence, death and decay in plants than humic acid!

Goodman ends his letter with a hint of 'current evidence of different radicals with similar epr parameters in plant tissue'. We look forward to the publication of this evidence and its interpretation. In the meantime we will continue to address this important area with all the modern epr techniques to which we can gain access.

References

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