

Magn. Reson. Discuss., referee comment RC1
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Comment on mr-2021-36

Malcolm Levitt (Referee)

Referee comment on "Mechanisms of coherent re-arrangement for long-lived spin order"
by Florin Teleanu and Paul R. Vasos, Magn. Reson. Discuss.,
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The paper by Teleanu and Vasos is a historical overview of some of the early methodology developments and associated theory, in the field of long-lived states. Personally I find it interesting to learn of the chain of reasoning which led to some of these sequences, in particular the sequence introduced in the JACS 2007 paper, as well as others leading to singlet-triplet coherences. The operation of this very clever sequence is far from obvious but it has proved very useful in the study of long-lived states in the far-from-equivalence regime ("weak coupling"). However I doubt that my personally finding an article interesting is sufficient to justify publication in Magnetic Resonance, whose stated aim is to publish "significant innovation regarding new insights into magnetic resonance methodology". In my opinion the new insights described here are modest. At the same time I have to acknowledge that the very wordy exposition might have obscured some original aspects that I failed to notice. I therefore suggest that the authors revise the article to accentuate with far more clarity and directness the original aspects of their work. The journal editors can determine whether I have misjudged the case, but I expect that a historical review of some old pulse sequences, however interesting, is not a suitable topic for an original research paper. Perhaps it could be part of a more extensive review article, which should, however, in that case, cover more than the methods described here.

Some additional comments are as follows:

* An abstract should be a brief and clear summary of the article contents. The current abstract does not fit that description and contains many digressions and pieces of exposition which do not enlighten. It should be cut down by a large factor.

* The article concerns itself exclusively with systems in the weakly coupled limit, which was the main focus of singlet-state research in the 2000's but has since been somewhat displaced by interest in strongly coupled and near-equivalent systems. I was rather disappointed that the article included so little discussion or review of methodology in that regime, which perhaps holds more current interest.

* page 4 includes the phrase "providing the two spins are rendered identical". I know what the authors mean, but this phrase is misleading. Two nuclei of the same isotopic species are, of course, always identical.

* page 6 includes the phrase "changing the chirality of the magnetic system". The issue of

chirality and in general, the symmetries of molecules and associated fields, is a very complex and deep issue, and I am not sure the authors are able to underpin this rather casual statement by rigorous theory. If so, they should do so. If not, they should steer clear.

* the term "pulsation" is used in several places where "precession" is probably intended.