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Interactive comment on "Stable isotopes dissect food webs from top to the bottom" *by* J. J. Middelburg

Anonymous Referee #1

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On page 14926-14927, the author describes the scope of this review, pointing first to the fact that some excellent reviews exist on e.g. the use of stable isotopes in animal ecology, plant ecology, trophic transfers, estuarine biogeochemistry, as well as several dedicated books on stable isotope ecology. The primary aim of this manuscript is then described as identifying avenues for research rather then providing an exhaustive account of studies to date.

I ended up having mixed feelings on this review. Sections 2-5 are short summaries on the principles of using stable isotopes in foodweb studies, some notes on emerging proxies (dD, d34S, D14C), compound-specific SI analyses and tracer/labeling studies, and these take up the bulk of the manuscript. However, as pointed out by the author they are far from exhaustive and thus do not really add to the exisiting literature. They

are well written and point out some key references or examples for each of these topics, but each of these sections do not offer much more than a typical introduction of a paper using one of these approaches.

Section 6, then, should hold the most promising part of the manuscript, i.e. identifying avenues for research / development of techniques or approaches. I have to admit, however, being somewhat dissapointed here too. Section 6.1. does little more then re-stating what we know already, i.e. IRMS is becoming an increasingly more avaible technique, and the advent of GC- and LC-IRMS or NanoSIMS offers great potential for studying the microbial components of foodwebs. Section 6.2. summarizes Krumins et al. (2013) and summarizes the potential of tracer experiments whereby mass balance considerations can be incorporated. While there are no scientific flaws in this review, I did not find the overall scope very convincing, and would suggest to consider restructuring the review and reconsidering the overall scope. As pointed out by the author, there are some excellent reviews out there which we don't need to see duplicated. As such, the first sections can likely be considerable shortened, and more emphasis can be put on Section 6. There is considerable potential in expanding on section 4/5 and making this the focus of the review, it is also one of the key areas in which the author has been at the forefront of developments and applications. It is an approach which for many young researchers is very appealing, but where I think there is no thorough review/synthesis available. This could include a much more exhaustive overview of studies performed so far, across different ecosystems/environments, and should include a thorough discussion of the data handling or modeling tools available to describe/exploit the data resulting from tracer experiments - often a weak point for ecologists but important to consider from the very design of an experiment. How should we design such tracer experiments, how does a general model look like (with different complexities), how do we minimize errors and uncertainties; ... these are questions that researchers struggle with and where a solid review by and experienced authority can make a difference.

In summary, the current manuscript does not hold as much novel information as I would like to see for a review, nor the amount of detailed recommendations for future studies.I realize this is a matter of opinion, in terms of scientific content there are no major flaws in the mansucript. I do see substantial merit in a substantially re-orientated review, focusing on a more limited topic but in far more detail as outlined above.

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