

Interactive comment on “Stable isotopes as ecological tracers: an efficient method for assessing the contribution of multiple sources to mixtures” by M. N. Bugalho et al.

Anonymous Referee #1

Received and published: 22 July 2008

The authors describe a modeling approach to estimate the minimum and maximum contribution (i.e. range) of different food sources in an organism's diet based on stable isotope data. The modeling approach is based on linear programming and is interesting because it is more efficient than existing methods. Such existing methods, e.g. IsoSource (Phillips & Gregg 2003) and SOURCE / STEP (Lubetkin & Simenstad 2006), give similar results but these produce a large set of (sensible and insensible) solutions that later have to be filtered for the sensible ones.

Overall, the manuscript is readably written, but I do find that the introduction needs rewriting to better introduce the main problem addressed in this paper. Finally, I sug-

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gest some additional topics for the discussion.

The main problem that I have with this paper is the scientific contribution is a bit meager. The modeling approach in this paper has been introduced in an earlier study in which diets were reconstructed from data on n-alkanes (see Barcia et al. 2007). In the current paper, data on n-alkanes have been replaced with isotope data. There are no novel empirical data or really novel conclusions drawn in the paper. The main scientific contribution is the software (embedded in an excel spreadsheet) that is developed to make the method available to novel users. I have tried and used the spreadsheet: the interface is basic, but proved easy to use.

In all, I suggest a significant revision of the manuscript based on the recommendations below. The focus should be turned to the novel scientific contributions (no repetition of findings from other authors). Perhaps after this rewrite, I can recommend this paper for publication in Biogeosciences.

General comments

1 - The introduction does not read very well. Is it really necessary to state that stable isotopes are stable and to give a reference to strengthen this statement?

Next is a short description of fractionation and the authors conclude that "This property makes stable isotopes useful as natural integrators and tracers of ecological processes (Dawson et al., 2002)", but in the next paragraph they use the same reference to state that "An important aspect of stable isotopes is their use as tracers as they allow following the fate and transformation of a resource. This requires that the different potential sources have distinct isotopic values and that stable isotopes do not undergo significant fractionation (Dawson et al., 2002)". These statements are rather counterintuitive.

The main problem addressed in the paper is that mixing models can be used to decipher source contributions when the number of possible resources equals the number of isotope tracers +1. Current approaches that aim to tackle this problem are 1) inef-

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ficient because they generate a large number of solutions from which the insensible solutions are subsequently removed and 2) require grouping of sources to make calculations feasible. Mentioning a series of papers in which stable isotopes have been used is redundant and distracting (Page 2427 line 4-8 and line 17-22).

I would also like to see a better explanation of the difference with the method from Lubetkin & Simenstad (2004). As far as I understand, they also delineate the ranges of diet contributions, do they not?

2 - Why were the original d15N data omitted from the analysis? The versatility of the linear programming model allows including them. Here the method may show its value as compared to the computationally expensive IsoSource.

3 - The overall results are very similar as in the original paper by Benstead et al. 2006. This may not be surprising, because the analysis is based on the same data. However, in many instances the authors note that "other researches have also shown" and "this result is not surprising". Apparently, other authors have solved the same non-unique mixing model with an inferior method, but nevertheless arrive at very similar conclusions. If the linear programming method does give novel insights, these should be put at the forefront. I suggest shortening the Results and Discussion sections to highlight the additional insights that are gained (perhaps by including d15N data?) with the modeling approach.

4 - The first paragraphs of the discussion are well written, here the novel science is described. I would like to suggest two additional discussion topics.

First, the ranges that are determined are in fact the corner points of a high dimensional solution space. Within this solution space there are correlations between the contributions of the different food sources; the ranges of each food source are not independent of each other. In other words, the maximum contribution of one food source is only obtained when the contributions of other food sources are set to values that allow this maximum to occur. For example, the results indicate that most mangroves contribute

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roughly $<10\%$ to the diets of the studied consumers. It is important to realize that if one mangrove species contributes 10% to the diet, then the contributions from other mangroves should be zero or close to zero. There is no discussion on this in the manuscript. It is touched upon though in the Results section (Page 2432, line 26-28 and Page 2433, line 12-14) where the authors claim that at least one of the seagrass species enters the diet of three consumers in a relatively high proportion. Strictly, this cannot be concluded from the ranges in Table 3. A better explanation of the correlations between the food sources would clarify this.

Second, a recent paper has been published in which Bayesian methods are introduced in mixing models (Moore JW, Semmens BX, 2008. Incorporating uncertainty and prior information into stable isotope mixing models. Ecology Letters 11(5): 470-480). This is a significant advancement in my opinion, since the Bayesian solution method does not focus on extreme corner points, but on the probability distribution in the solution space. Therefore I think this method should be discussed in the current paper.

5 - In the Results section, reference is made to *L. equulus* and *U. vittatus* in Table 2, but that consumer is not present in Table 2.

Specific comments

Pg 2427 line 23: change in into of and delete "reflect those isotope values in food webs"
Pg 2428 line 13: change intense into expensive Pg 2428 line 27-28: "This approach ... " is a weird sentence. I do not understand what is meant here. Pg 2430 line 24: insert is before subject Pg 2431 line 3: give a reference to linear programming solver. This is the core method of the paper and reference should be given to theory. Pg 2437 line 10: Fey should be Fry

Change d14S into d34S in the captions of Table 1-4.

Interactive comment on Biogeosciences Discuss., 5, 2425, 2008.

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