Pfister et al. describe an isotope tracer study to unravel N mineralisation and subsequent N cycling in tidal pools of rocky shores, in which they distinguish amongst the activity of mussels (and associated microbes), microbes and phototrophs. To interpret the results, the authors present a new model to account for isotope dilution and continuous N cycling which proves very useful to recover N transformation rates from the available observations. Although the study is well designed, the manuscript would benefit from a clearer description of the components in the system and their interactions that the authors consider. Below I make suggestions to improve the clarity of the manuscript. Overall however this is a very nice study that deserves to be published in Biogeosciences.

Major points:

1. There are many missing references in the literature list, which was quite annoying. Just to name a few: Worm et al. 2006, Layman et al. 2010, Stief 2013 and Heisterkamp et al. 2013. Please check whole paper thoroughly.
2. The Introduction and the first section of the Materials and Methods should be merged. The introduction is quite general, with terms such as nitrogen regeneration, remineralisation, transformation, 'interacting contributions of microbes and macrobiota', competition between phototrophs and fauna-hosted nitrogen-metabolizing microbes and so on. Unfortunately, it doesn't get clear in the introduction what the authors are actually studying and which components of the rocky shore they are targeting. In fact, the clearest part of the 'intro' is given the first section of the Material and Methods section (Lines 111-126), where amongst others Fig. 1 is introduced. Therefore, I suggest to shorten the Introduction and merge the first part of the Method section with the Introduction.
3. Potential overlap with Pather et al. 2014. On lines 218-219, it is indicated that detailed Methods are given in Pather et al. 2014, but it is also important to indicate whether there is data overlap between the present manuscript and Pather et a. 2014. If so, it should be clarified what the novelty (apart from the new model) is of the present study.
4. Units of N transformation rates. The rates are presently expressed in L-1, it seems that transformation rates are actually mediated by the benthos (either mussels or benthic algae uptake [Line 394-395]. Hence, I suggest to express all rates per m2 instead of per L. Firstly to standardise for the depth differences among tidepools and secondly to ease comparison of N cycling rates with other hard- and soft-bottom ecosystems (which I would encourage).

Minor points

1. Line 24: What is meant with "interdependency" here? It would be useful to make a clearer distinction throughout the manuscript among the microbial components considered: fauna-associated microbes and biofilm/sediment-associated microbes (and phototrophs?). Lines 33-35 would already benefit from a clearer distinction.
2. Line 30: The term 'animal presence' is unclear here, because do the authors refer to the activity of fauna (e.g. NH4 release) that influences N cycling or the fact that fauna-associated microbes are present or both?
3. Line 122: The authors first describe that traditional source-sink models are inadequate in the present situation (lines 115-120) and then the first model they describe for their study is such a source-sink model. I suggest to present the new model as (1) and the source-sink model as (2).
4. Line 166: change "multiple, simultaneous processes" to "multiple processes simultaneously".
5. Line 171: The notation of concentration in brackets is awkward. Why not use the common square brackets for concentrations?
6. Line 235: I guess the delta values have been converted to atom% (see Line 149) in stead of the 15N/14N ratio. Note that the [15N] is calculated from atom% x nutrient concentration.
7. Line 265: briefly describe the methods for nutrient and isotope composition here.
8. Line 299: I do not understand the replication in this study. There were 5 tidepools with and 5 tidepools without mussels (Line 220), but I can't find back how the NH4 and NO3 treatments were divided over these tidepools. E.g. were the same pools used for day- and nighttime experimentation, if so, was it checked whether label from earlier experiment was gone?

9. Line 274-277: The authors used the fitting routine modFit to estimate the parameters. Were the field observations weighed in the model cost to account for the differences in units (and absolute values)? I.e. any fitting routine penalises deviations between model and data stronger for variable with high values (e.g. [NH4] is >> [n15Ni], so a model-data deviation for [NH4] weighs heavier on the model cost as compared to a deviation of [n15Ni]).
10. Line 284-286. The convergence to the same sum of squared deviations (SSD) is indeed a good indication that the fitting routine found *a* best parameter set, but it can still be that the same SSD is reached with different sets of best model parameters. Have the authors also checked whether also model parameters converged after the fitting?
11. Line 299-301. Were stats done on the rate parameters (which are concentration independent, e.g. u) or on transformation rates (i.e. 2*u*[NH4]).
12. Line 339-343: The percentage calculations discussed here are unclear, please rephrase and refer to Fig. 1 or the rate parameters.
13. Line 359: So were the 'concentration-independent' model parameters compared or the total nitrogen transformation rates?
14. Line 420: …observation… should be …observations…
15. Line 420-422: unclear sentence (operation of other N processing pathways?), please clarify.
16. Line 453-455: Remineralisation rates dropped strongly during the night. Do the authors have an explanation for this strong reduction? Oxygen and temperature are lower during the night, but is this sufficient to explain the drop?
17. Line 496: Why do the authors suggest that measurement error is the prime factor for the observed variability among tidepools?
18. Fig 1. should have 2*u for ammonium uptake u for nitrate uptake as described in the text.