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Interactive comment on "The nitrogen, carbon and greenhouse gas budget of a grazed, cut and fertilised temperate grassland" *by* Stephanie K. Jones et al.

Anonymous Referee #2

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The study of carbon (C) and nitrogen (N) as well as greenhouse gas (GHG) fluxes in managed grasslands and their response to management measures is of great scientific and practical relevance. Long-term studies of these processes are of particular interest, but very rare.

The strength of this manuscript lies in the long time period covered (nine years overall) and in the number of processes relevant to C/N budgets and GH Gbalance that were measured at the same site.

Against these stand a number of weaknesses.

Firstly, the majority of the measured processes were measured only for a (sometimes

small) part of the whole period and were largely published as part of previous publications. Other quantitatively important processes were either modelled or estimated from literature data.

Specifically, measured data include: - management data: stocking densities (weekly), C and N concentration of harvested biomass (total of three cuts) and of organic fertilizer (total of two applications) - wet and dry N deposition - C and N leaching: 2 years, published as part of Kindler et al. (2011) - N2O emissions: 4.5 years, partly published in Di Marco et al. (2004), Jones et al. (2011) and Skiba et al. (2013) - soil NOx emissions: 1 year - soil methane emissions: 4.5 years, partly published in Skiba et al. (2013) and apparently (?) as part of Levy et al. (2012) Global Change Biol 18: 1657-1669 - CO2 exchange: partly published in Skiba et al. 2013 and apparently included in a submitted paper by Levy et al.

The following data were modelled using the LandscapeDNDC model: - Soil N2 emissions through denitrification for eight years, soil NOx emissions for eight years, N2O emissions for 3.5 years (apparently partly published in Molina-Herrera et al. (2016)?)

The following data were estimated using literature data and expert estimates: - livestock weight gain/wool yield and associated C and N export, methane emission from organic fertilizer application and from ruminant digestion, NH4 and NO3 emissions from fertilizer application and livestock excrements

As indicated, the majority of measured data have already been published as part of separate publications, including the GHG fluxes of the years 2007-2010 (apparently published in Skiba et al. (2013); site description and measurement methods appear to be the same, although values partly differ). The extent to which data sets were included that were, at least in part, previously published or currently submitted, is not generally made clear (which means that some of my above attributions may also be mistaken or incomplete). Consequently, the main novelty of this manuscript seems to be the data aggregation to derive a total C and N budget and to present data on soil C and N stock

changes over seven years.

Secondly, the data represent only a single site with a non-uniform management over time. Year-to-year differences in management together with different ways of data generation over years (measurement versus modelling versus averaging over years) mean that inferences about weather or management effects on the studied processes are not really possible.

At various places, the authors do compare years with cutting and grazing (2002 and 2003) to grazing-only years (2004-2010). I find this comparison very problematic for various reasons: e.g. (i) the two years with silage cuts include the year with the highest annual rainfall and the highest N leaching (which, however, was modelled, not measured), (ii) the two years with silage cut do not include any application of organic fertilizer, while the years with grazing-only do (from a farming-system approach, cutting and in-barn feeding goes along with spreading organic fertilizers, while all-year-grazing does not).

Similarly, there are several instances where climate versus management effects on parameters that were mostly modelled are discussed. For example in L716-734, between-year variation of N leaching is discussed in relation to annual rainfall and annual stocking rates. However, 2007 is the only calendar year for which leaching data are available throughout. Any other annual variations are the result of the LandscapeDNDC model and would therefore appear to depend on the way details of management (such as stocking rate) are parameterized in the model.

Thirdly, I am not sure to what extent the experimental design is able to cover the spatial heterogeneity of the study site. For example, it is mentioned in L253 and L602f that the site included a frequently waterlogged hollow as well as a slope. For C and N leaching, only data from the slope are used. Presumably, these terrain differences and associated differences in water household would influence not only leaching, but also many of the other investigated processes. There is, however, no indication if and

how the sampling scheme of other parameters (e.g. eddy covariance footprint, soil N2O, CH4 and NOx emissions, soil sampling) differentiated between slope and hollow positions.

Leaving aside these considerations, I believe the paper could be strengthened by shortening certain parts, especially the Introduction and paragraph 3.1 of the results section. I also include a PDF file with some more specific comments.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-221/bg-2016-221-RC2-supplement.pdf

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