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## ***Interactive comment on “Use of rare earth oxides as tracers to identify sediment source areas for agricultural hillslopes” by C. Deasy and J. N. Quinton***

**Anonymous Referee #3**

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### Introduction

The aim of this manuscript is to use rare earth oxides (REOs) as applied sediment tracers of erosion on a UK agricultural hillslope. Currently, there is a great interest in the development of such applied tracers in order to obtain spatial measurements of erosion and deposition at the event scale and for more extreme events. The more traditional sediment fingerprinting techniques (such as the radionuclides) which rely on bomb fallout or natural inputs to soil, only provide medium to long term estimates of erosion; therefore, new techniques need to be developed in order to provide short term, spatial data on sediment movement within landscapes. This paper applies REOs

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to examine erosion rates from different parts of the slope and differences in erosion between minimum till and ploughed fields.

Overall I support the publication of this paper, subject to substantial corrections which are needed to clarify some contradictory and counterintuitive statements about processes and also to address some of the shortcomings with the methodology. The paper is missing some key measurements which would have made a much stronger case for the processes the authors are advocating. The lack of these measurements leads to a highly speculative discussion and a more black-box approach to this spatial problem. Below I will elaborate these points further.

Main comments:

1) REOs in suspension: The authors advocate using rare earth oxide powders in suspension in water as a method for applying the tracer on to the soil (section 2, lines 18 – 19). However, in section 1 (line 21) the authors state that the best method for incorporating REOs into soil is by dry mixing. This is a contradiction and therefore needs a clarification of the accuracy of using REOs in suspension. Given the novelty of the method, more data are needed to evaluate the accuracy of this technique in relation to other methods in the literature that are mentioned by the authors. Also, the reference to Pryce (pers comm.) is not appropriate here as it does not provide any information to the readers that can be independently evaluated. Given that dry mixing is not even relevant to the paper and it contradicts the methodology applied, I would remove this statement and citation.

2) Method of REO spreading: One of the main novelties advocated in this paper is the method of non-intrusive REO spreading. Many statements are made about the increased accuracy (section 3.2.3, line 8 – 11) and reduced surface disruption of using this technique etc. but these statements are not backed up by any measurements of REO-sediment binding following application. The reported assumed tracer percolation depth of 1cm is based on previous laboratory experiments, which are likely to be

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very different from the field setting. Why is it assumed and not measured? It would have been more robust to have quantified the spatial variability in REO binding depth throughout the fields and for the different REOs post-application. As is, I think that statements of “increased accuracy” are overinflated and unsupported and should definitely be toned down given the lack of measurements. “Visual inspection” of spreading uniformity (section 3.2.3, line 4) is insufficient for a scientific paper that relies on precise measurements of spatial variability of tracer application and binding depth. Measurements were also needed to establish precise initial concentrations of tracer in the soil. Given the grass cover, measurements were needed to establish the extent to which the tracers were potential binding to the grass instead of the soil. This may explain the low depletion rates. Section 3.2.1 (lines 10 – 24) is a speculative statement with no quantitative evidence to support it.

3) Runoff and erosion: Section 3.2.2 makes a series of speculative and totally counter-intuitive statements about runoff and erosion which need to be clarified and explained. It does not make any sense why the upslope areas would contribute more runoff and sediment than the downslope areas (lines 4 – 9) especially since the upslope areas are shallower (lines 10 – 15). According to runoff theory, runoff increases downslope, so the tracer data may be the culprit in this contradiction. Post-event spatial data of deposited tracer (within tracks and ploughed areas) are needed to evaluate sources and pathways – the black box approach taken here (assumed applied tracer versus recovered tracer downslope) is not sufficient to evaluate pathways and leads to speculation in the discussion.

4) Section 2: explain why you are using GLM statistical analysis and what it tells you.

5) Section 3, lines 12 – 13: why are you averaging all erosion data from all slopes and across all events? Averaging erosion rates over 3 distinct events and over two different tillage types is meaningless. Line 23: what is the reason behind the high erosion rates from the upslope areas? It doesn't make any sense. See also, point 3) above.

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6) Justify the choice of Pr, Nd, Sm and Gd over the other REOs.

7) The Introduction discusses how erosion from agricultural fields in the UK is a significant problem for sediment and nutrient transport. However, the data and discussion presented at the end of the paper, state that the erosion rates are low, typical of UK arable land. This contradiction needs clarification – is erosion a problem or not? How do the erosion rates measured by the REOs compare to other measurement techniques previously employed in the same area (e.g. presented in table S1)?

8) Table 1: Did Event III really have runoff for 227 hours? Why are you presenting data from all 4 tanks averaged? Wouldn't you expect the different tillage types to produce different erosion rates? The high calculated erosion rates from event I may be due to flushing of non-incorporated REO.

9) Fig. 2: There is too much overlap of the error bars to distinguish between REOs. Also, Shouldn't the tillage types be distinguished in this plot? Hillslope areas corresponding to REO should be labelled.

In summary, I feel that this paper is missing some fundamental measurements to make it airtight and scientifically robust. Given the stated novelty of the application, there really needed to be more quantification of the initial spatial concentrations, a more extensive testing of the technique prior to application, as well as more post-event measurements of REO movement and deposition throughout the fields. These measurements would have eliminated the need for speculative discussion which occurs throughout section 3. I will recommend publication because the technique is relatively new, but I would urge the authors to eliminate all speculation, to rethink the strange hydrological statements (i.e. runoff and erosion is highest upslope), to address the shortcomings with the application (i.e. lack of measurements and characterisation of the initial concentrations and spatial patterns, as well as the grass problem) and to tone down their statements of superior accuracy given the lack of any data. The strange observations may well be related to problems with tracer incorporation and binding, so they really

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need to be upfront with this.

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Interactive comment on Solid Earth Discuss., 2, 195, 2010.

**SED**

2, C72–C76, 2010

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