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Solid Earth
Discussions

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

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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 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.

We thank the reviewer for their support of the paper, and welcome their suggestions for its improvement.

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.

We think that the reviewer has misread the relevant paragraph. We do not state that the best method for incorporating REOs into soil is by dry mixing, we say that 'Although direct mixing of REOs with soil has been shown not to substantially change the physicochemical properties of soil aggregates (Zhang et al., 2001), the mixing works best in dry soil (Pryce, Pers. Comm.)'. The reference to Pryce is entirely relevant, as this work compares the different mixing methods and provides contextual support for our study. Pryce now has a paper in final draft, and this work is contained in his PhD thesis (see response to reviewer 2 comment above). We feel this statement is necessary as the justification for the paper is to develop a new application method which has advantages over the existing direct mixing methods and which will allow REOs to be applied to large areas without creating disturbance.

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 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.

We appreciate the reviewer's concern about the use of the word 'accurate' in section 3.2.3 in the absence of supporting data. However, as stated, we do consider the application method to be an improvement on the methods used in previous studies where

soil and tracer mixes were broadcast, either by hand (Polyakov et al., 2004; Polyakov et al., 2009) or using a fertiliser spreader (Stevens and Quinton, 2008), as the spray application rate in this case is calibrated, and feel it is appropriate to leave this statement in the paper.

As the reviewer will appreciate, it is very difficult to compare the REO sediment binding efficiency of the methods used by Polyakov, Stevens and Quinton, and in this paper under field conditions, and as we have referenced and extracted information available from the study undertaken by Pryce and Quinton comparing application methods under controlled conditions, we have gone as far as we can on addressing this issue. Laboratory information is commonly used to support decisions taken for field experimentations, and while we recognize that this may not truly reflect field conditions, the binding depth would have been similar throughout the field experimentation area. We would also highlight the impracticability of quantifying the spatial variability in REO binding depth throughout the fields and for the different REOs post-application, as this would have necessitated us walking all over the hillslope we had just sprayed, and removing and disturbing the tracers whose movement we were measuring in this study. The tracers in solution are like paint, and very sticky. To avoid contamination and disturbance, we had a spraying approach designed so that we did not have to walk on the surface we had just sprayed.

The winter wheat cover on the hillslope could not have accounted completely for the low depletion rates of REOs eroded from the hillslope. As discussed in answer to reviewer 1 comments, yes, some physical interception of REO by the crop canopy will have occurred. We did not measure this but expect it to have been minimal. As seen from figure S1 in Supplementary material, the crop cover was not close to 100%, and the crop was only a few cm high at the time of REO application. As there were no differences in crop cover between the different hillslope areas or segments, any interception would have been constant throughout the experiment.

We consider it is appropriate to include this 'speculative comment' in the discussion in

Section 3.2.1, as although we were not able to measure this, the antecedent rainfall events may well have affected the REO binding, as discussed by Polyakov et al. 2009.

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.

We disagree with the reviewer on this point. Our data suggest that the upslope area makes a greater contribution to erosion at the hillslope length scale. We need to provide a realistic explanation for this, and from our experiences working at the site, we suggest that this could be because this hillslope area is more convex, leading to higher erosion rates as observed by Montgomery et al., 1997, or because the slope is shallower here, leading to more efficient movement of sediment into less incised wheel tracks, or because ponding of water at the top of the hillslope feeds directly into the tracks, which export eroded material efficiently to the base of the hillslope. The tractor wheel tracks have a crucial role to play in sediment transport from arable hillslopes, as demonstrated by a number of recent studies (e.g. Withers et al. 2006, already referenced in the paper, and Silgram et al. 2010, Earth Surface Processes and Landforms, Volume 35, Issue 6, pages 699–706).

Erosion measured at the plot scale could be greater in the other hillslope areas, but due to connectivity and transport processes, measured erosion at hillslope length scale is actually greater from the top slope. We appreciate that these ideas are new and may appear contradictory, but studies of erosion on hillslopes at this scale are novel, and

because we are working in real fields with real management issues – for example the necessity for tractor wheel tracks – traditional erosion theories may not be appropriate here.

As discussed in response to a comment by reviewer 2, we did collect some additional data necessary on transport of REOs within the hillslope, which we would have liked to include but could not justify presenting it in this paper.

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

We used General Linear Model analysis rather than a straight ANOVA as this allowed us to determine the treatment effects through the other factors brought in by the experimental design used in this study, which was partly dictated by the broader study of mitigation treatments. GLM analysis allowed us to analyse the treatment effects while taking into account the treatment types (minimum tillage and plough, tramline and no tramline), plus other influential factors such as event and plot number (location).

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.

We argue that these data are not meaningless. As the statistical analysis shows no significant differences between the two tillage types, minimum tillage and plough, we are able to use all four hillslope lengths as replicates to consider the erosion rates for each hillslope area.

We have discussed the significance of the upslope area in response to an earlier comment.

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

The rare earth elements Pr, Nd, Sm and Gd were chosen over the other elements as these have been used in previous studies at the same site, and contextual data on these REOs are available from our laboratory.

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)?

As stated, excepting a limited number of problem areas, erosion rates in the UK are generally low. However, as stated also, even low rates of erosion can be a problem for sediment and nutrient transport, contributing significant levels of sediment, nitrates and phosphorus into receiving waters. On-site erosion, although it leads in the long-term to soil degradation, is not often a problem at this study site, while off-site erosion problems often are. It is difficult to compare the erosion rates measured here at event scale to rates measured using different approaches. The measurement techniques used for the data in table S1 are part of the same experiment, so it is not possible to compare these data. If the reviewer or readers require further information on how the erosion rates in the broader mitigation experiment relate to other erosion rates, then we would refer the reader to our final report and JEQ paper which are publically available and already referenced in the manuscript.

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.

Please see the response to the comment by reviewer 1 for information on the length of the events.

Table 1 shows information on the runoff events. We present averaged data for all tanks simply to show the general hillslope response and variability in the data. We

did expect the different tillage types to produce different erosion rates, but over the three events used in this study, there were no significant differences (probably due to hillslope variability). We can show the data individually if required but are unable to put errors on the data.

Yes, the higher erosion rates in event 1 may be due to flushing of non-incorporated REO, but we discuss this in the 'speculative comment' in section 3.2.1. The higher concentrations may also be due to the higher measured rainfall intensity as discussed.

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.

This point was also picked up by reviewer 2, and we can provide an alternative figure if necessary. As discussed earlier, we did not feel it was necessary to distinguish the different tillage types here. We have labeled the hillslope areas corresponding to the different REOs as requested.

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