

Interactive comment on “Reconstructing long-term gully dynamics in Mediterranean agricultural areas” by Antonio Hayas et al.

Anonymous Referee #4

Received and published: 1 August 2016

General comments

This paper is a very interesting study of gully erosion in a wide agricultural area, cultivated with important crops. Information from a wide time period is analyzed. The scientific questions addressed are within the scope of HESS. The complexity of gully erosion phenomenon in agricultural areas is highlighted and discussed in detail. The Monte Carlo-based approach proposed for reconstructing gully erosion rates from orthophotos is original and helps to get the most of the available information. The paper is well written and both methodology and analysis of the results are correct. Thus, the paper could be published after moderate revision.

Specific comments

Introduction is particularly good: concise but including all the key aspects, with well

[Printer-friendly version](#)

[Discussion paper](#)



selected and suitable references.

It would be desirable to explain in more detail the degree of representativeness of the selected study area.

P5. “In order to measure gully width in a representative way, 35 stretches were selected...”. “Gully top width and depth were measured at 27 representative sections...” It seems to me that the number of measurements of these key variables is too low. Please, justify.

The Monte- Carlo approach selected is very interesting. However, the authors should explain why they don't consider other methods (such as photogrammetry from stereoscopic pairs, etc.) to estimate gully depths.

P6. “Field observations suggested that a triangular section is a reasonable approximation of most gully sections, so a shape factor $k = 0.5$ was adopted in order to compute the simulated sections”. It is not enough just to say that “field observations suggest”. The shape factor k is very important for the results obtained and the considered value of 0.5 has to be justified.

P7L9. I wonder if “extreme annual” makes sense...

P7. I think that the meaning of the expression “anomalous” used in this paper deserves deeper explanations.

P8. “From 1984 to 1999 and 2009 to 2011 there was an increment of 14.6 m ha⁻¹ and 23.6 m ha⁻¹ respectively, which account for 84% of the total drainage density growth”. The duration of the two periods is very different, what has to be considered in the discussion.

P10, paragraph 3.7. When calculating the erosion rates in ton ha⁻¹ yr⁻¹, is the considered surface area constant or varies from year to year? Please explain.

P10, L27. Is not the Mediterranean climate temperate?

Technical corrections

P2L16: “New” P3L11: remove comma P7L1: “difference” P8. “In most of the analyzed period variations on drainage density occurs are small. However, there are two significant periods where the increase”. Please rewrite.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-239, 2016.

HESD

Interactive
comment

[Printer-friendly version](#)

[Discussion paper](#)

