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Interactive comment on "Rainfall simulation experiments in the Southwestern USA using the Walnut Gulch Rainfall Simulator" *by* Viktor Polyakov et al.

Anonymous Referee #2

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This paper consists of a brief account of rainfall simulation experiments undertaken in dry environments of the SW USA. The associated data sets contain experimental locations, summary data for 272 experiments, and plot surface photographs of the experimental plots. The paper is generally clear, and sketches the general context of the experiments. Completeness of the data does not seem to be mentioned, and an examination shows that flow velocity data are often not available, for instance. Some of the column headings in the data tables are unclear; as an example, the column headed 'precipitation' in the rainfall simulation data actually refers to the intensity of the simulated rainfall. The fact that this is quoted to 2 decimal places (100th of a mm/h) is concerning, since the field measurements certainly do not have that kind of precision.

The title of the paper seems somewhat vague. Rainfall simulation is a method or tool, but the title might be better to include something about the goals and purpose of the rainfall simulation. This might be 'Runoff and erosion assessment from plots in the southwestern USA: data from rainfall simulations". The list of keywords could usefully include 'Arizona' and 'Nevada'.

The writing has minor blemishes, including lack of definite and indefinite articles in many places. As examples, line 148 should refer to 'a' high torque stepper motor; line 165 should refer to 'an' electronic depth gauge; lines 174-175 should refer to 'a' stopwatch. I am not sure what 'Table X' refers to in line 211.

In terms of content, the paper is under-referenced. Many claims need supporting references. An example is the sentence beginning in line 34. Does this refer to both wind and water erosion? How does the effect of soil erosion compare in magnitude to the effect of water scarcity? The land resource area classifications (e.g. line 50) also require clarification for an international audience.

Also in terms of substantive content, there should be some discussion of the appropriateness of 12 m2 plots to capture the wide-area nature of runoff and erosion in the field location. Was the size and shape of the plots chosen for reason of process representativeness, or was it dictated by the available apparatus? If the latter, do the authors have some comment on the dimensions used? What kinds of ground slopes were represented? When grazing is mentioned, what was the grazing intensity? When fire is mentioned, what was the spatial extent and intensity of the fire?

Erosion from the plots during the experiments will have been some function of the imposed rainfall and runon, together with plot surface conditions. In this context, how were rainfall intensities in the range 65 – 180 mm/h (line 222) selected? For the Limy Uplands sites at Walnut Gulch, where the annual precipitation is 290 mm (line 75), the depth applied in just two rainfall simulation experiments would significantly exceed the total annual precipitation (45 minutes of wetting-up at 65 mm/h followed by about 1 h

at say 120 mm/h to represent the mid-range of intensities used, amounts to 170 mm of applied rainfall in a single simulation). Does the annual precipitation in this area actually come in just one or two events of this size? For how long does rainfall at say 150 mm/h usually persist at this field site (and likewise at other sites and intensities)?

The paper refers to characterising a 'steady-state sediment yield' (line 254 and elsewhere). I doubt that such a thing exists, except (as a remote possibility) in rainfall simulation experiments. There are commonly sediment supply limitations and supply exhaustion, or the evolution of rills and new sediment sources; in any case natural rainfall does not remain at a fixed intensity for hours at a time. Indeed, this raises an additional issue that the authors do not mention, namely, the use of fixed intensities, apparently applied in a rising sequence over quite short periods of time (100 mm/h, followed by 125 mm/h, then 150 mm/h, then 180 mm/h. etc). What might this series of increasing intensities, applied in rapid succession, have done to the plot surface and to the sediment supply? How might the results have been affected had the experiments been run with successively lower intensities instead of increasing intensities?

Overall, I felt that the paper needed some more serious discussion of issues such as those just raised, and how they might affect the application of the data made available in the online depository. The paper does mention that 3 runoff samples per experiment might have been too few (lines 253-254) and that the possibility that some experiments might have been affected by wind interference should not be forgotten (line 257). The paper also mentions, but does not explore, the challenge of scaling-up the plot data to hillslope or watershed scales (lines 255-256). It would be useful to learn from the authors about how this might be approached, as they are most familiar with the data and their strengths and limitations. In this context, it would be useful to see more references to literature arising from the experiments described, especially in relation to the effects of grazing, or fire, or brush treatments, or ecological transitions, as outlined in lines 60-61.

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