Response to Reviewers:

We thank the reviewers for their continued efforts to improve the manuscript. As Reviewer 2 found our revised manuscript acceptable for publication, here we primarily address on-going concerns from Reviewer 1.

The most important concern raised is that the areas with erosion due to seepage erosion vs. overland flow are delineated morphologically. Without additional data proving that Type 1 morphology was caused only by overland flow and Type 2 morphology was formed only due to seepage erosion, we are not able to make that case. This is an important concern, and we address it below as best we can.

Below are the Reviewer's comments related to this followed by our response:

R1: *I have a major concern about this work. The attribution of the contributing area to overland or seepage flows relies only on the morphological distinction between channels of type 1 of low slope and channels of type 2 of high slope. The firsts are attributed to overland or runoff flows and the seconds to seepage erosion through sapping events. Here with the presented data we cannot determine if this assumption is correct. Moreover, I note that for seepage erosion experiments with an inclined granular bed, the upper channels display also a branching shape of low slope with the merging of several sub-channels into a larger stream. See "Lobkovsky, A. E., Jensen, B., Kudrolli, A., & Rothman, D. H. (2004). Threshold phenomena in erosion driven by subsurface flow. Journal of Geophysical Research: Earth Surface, 109(F4)", "Smith, B., Kudrolli, A., Lobkovsky, A. E., & Rothman, D. H. (2008). Channel erosion due to subsurface flow. Chaos: An Interdisciplinary Journal of Nonlinear Science, 18(4), 041105", and "Schorghofer, N., Jensen, B., Kudrolli, A., & Rothman, D. H. (2004). Spontaneous channelization in permeable ground: Theory, experiment, and observation. Journal of Fluid Mechanics, 503, 357-374".*
Please comment.

The authors must prove that the channels of type 1 are really created by the erosion of overland flows only. Otherwise the conclusions of the article are not supported by the data.
If not; the bed inclination would be caused here by the imposed uplift. High slope channel heads created by sapping would occur when the height of the granular bed above the emergence of the water stream exceeds a certain value and would correspond to a particular case of seepage erosion at the end of experimental runs. As the infiltration rate is always smaller than the precipitation rate, I suppose there is always infiltration and a non-negligible contribution of seepage erosion, even when the bed is initially saturated in water. Then, some of the non-contributing area can participate to the filling of the water table and thus to the seepage erosion.
Please comment.

> We recognize the reviewer's concerns that our delineations of Type 1 and Type 2 channel heads are based solely on morphology, with the interpretation that Type 1 are derived from overland flow and Type 2 are derived from seepage erosion. Reviewer 2 was concerned that our experiments were biased towards overland flow because the experiment began fully saturated. Reviewer 1 has the opposite concern, that erosion may

be dominated by seepage erosion throughout, and that we need to demonstrate conclusively that overland flow alone was responsible for Type 1 erosion. They reference a series of experiments investigating seepage erosion in granular material (Lobkovsky et al., 2004; Smith et al., 2008; Schorghofer et al., 2004).

Reviewer 1 states that we "*must prove that the channels of type 1 are really created by the erosion of overland flows only*". We cannot do this, because we do not think the channels were eroded by overland flow alone. Erosion is unlikely to be unimodal, driven only by overland flow or by seepage erosion. The conditions in our experiments allowed for both processes to occur.

The Reviewer expresses concerns that our "*attribution of the contributing area to overland or seepage flows relies only on the morphological distinction between channels of type 1 of low slope and channels of type 2 of high slope.*" We want to note that Type 1 and Type 2 channel heads have quite distinct morphologies. Measurements were made of prototypical examples of each type of channel head, and those data were then used to set the threshold criteria for classifying all channel heads as either Type 1 or Type 2 based on both local slope and relief at the channel head. Those criteria are listed in Table 3. It is misleading to state that our attribution relies only on low slope vs. high slope.

To help illustrate the differences between the two channel head types, we included an additional figure (Fig. 4) showing a photo of one of the experiments with both Type 1 and Type 2 channel heads. In this revision, we have also added in a figure in the Supplemental file showing channel profiles for both Type 1 and Type 2 channel heads from the last time step in all six runs. Channel profiles are generally linear except near the channel head where they are concave and steep on Type 2 profiles and linear all the way to the channel head on Type 1 profiles. They are quite distinct.

In order to map them in a systematic way, we employed thresholds of both local slope and relief, using data collected from prototypical examples of the two channel types. In places where overlaps occurred, they are mapped as Type 1, thus there is a bias towards Type 1 channel heads because of this. We openly acknowledge that in the paper.

We have tried to be very clear in our paper that we are interpreting the different morphologies as erosion due primarily due to overland flow and seepage erosion. It is stated as an interpretation. Since we did not directly measure flow pathways during the experiments, it will have to remain as an interpretation. The interpretation is based on our observations, experimental conditions, and observations from other studies in the literature.

First our observations. The experiments officially began once the system was fully saturated and overland flow was occurring. This is when base level fall was initiated, providing some relief in the system. Although the infiltration capacity exceeded the precipitation rate, infiltration capacity is different than infiltration rate. When the subsurface saturated, no more water could infiltrate. Thus, we had observable saturation overland flow in all runs. Runs with a higher infiltration capacity could move water through the subsurface faster and thus infiltrate greater amounts of rainfall. Runs with higher precipitation rates or lower infiltration capacities would have a greater fraction of the precipitation flowing across the surface. The balance between overland flow and subsurface flow thus depended on the ratio between infiltration capacity and rainfall rate. Both surface and subsurface flows occurred in all runs, and erosion via both overland flow and sapping occurred in all runs. We are interpreting the dominance of one vs. the other at the channel heads based on their morphology.

Second, comparisons with the literature. The papers by Lobkovsky et al. (2004), Schorghofer et al. (2004), and Smith et al. (2008) all have similar experimental set-ups, with a sloping surface and a measurable groundwater table producing sapping either in the form of channelized flow or mass wasting. The channels can have a channelized form, often with a rounded channel head, which is quite different in appearance from the channel heads we labeled Type 1. The amphitheater-shaped channel heads we labeled as Type 2 closely resemble the mass wasting channel heads found in the experiments with higher water tables. This is consistent with the development of more Type 2 channel heads as relief developed in the experiments. We feel confident that the Type 2 channel heads were predominantly eroded from seepage erosion. We do not see our Type 1 channels in the seepage erosion channel examples given in the cited studies by Lobkovsky et al. (2004), Schorghofer et al. (2004), and Smith et al. (2008). This may be because those experiments had no surface flow.

An additional study relevant here is Berhanu et al. (2012), which is similar to the other ones referenced above. Berhanu et al. starts with a flat surface rather than a sloped one and includes both rainfall and introduction of water from the subsurface. Their parameters were established to allow for erosion via seepage erosion only, not overland flow, and the channel morphologies they developed are quite different from the channel heads we labeled as Type 1. They found noticeable and measurable differences in channel head morphology based on how water was introduced to the system (from below only vs. combined with rainfall). The channel heads developed via seepage erosion in the presence of rain are even less like our Type 1 channels than some of the examples in the experiments of Lobkovsky et al. (2004), Schorghofer et al. (2004) and Smith et al. (2008).

We feel confident that our channels began through erosion via overland flow based on initial experimental conditions and observations of overland flow. We feel confident that our Type 2 channel heads were eroding via seepage erosion based on morphology of the channel heads, comparisons with literature, and experimental conditions that allowed for substantial subsurface flow to occur. We have added in additional citations in our manuscript to the papers referenced in this discussion as they help support the interpretation of Type 2 channel heads as developing from seepage erosion. Although branching channels were found in seepage erosion channels of Lobkovsky et al. (2004), they have quite different morphologies compared to the channel heads we labeled Type 1. Given the initial conditions of full saturation and our observations of overland flow, we

> stand by our interpretation that these channel heads were eroded primarily via overland flow.
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> Berhanu, M., Petroff, A., Devauchelle, O., Kudrolli, A. and Rothman, D.H., 2012. Shape and dynamics of seepage erosion in a horizontal granular bed. *Physical Review E*, *86*(4), p.041304.

Other comments:

In the previous experiment and field studies about seepage erosion, the average slope (here due to the uplift) is a key parameter, which is not here sufficiently discussed to my opinion. For example the experimental channels have very different shapes between Lobkovsky 2004 and Berhanu 2012 (case without rain).

> We have gone back to the experimental data and extracted longitudinal profiles from the last time step for each run. Those are now included in Supplemental Figure S7. Channel slopes are dependent in part on the depth of incision and distance from outlet of the channel head. Channel profiles are generally linear except near the channel head where they are concave and steep on Type 2 profiles and linear all the way to the channel head on Type 1 profiles. Slopes on the channels downstream from the channel heads range from 0.11 to 0.22, steep enough for both processes of overland flow and seepage erosion to occur.

1) Abstract: The sentence: "Seepage-driven erosion was favored in substrates with higher infiltration rates, while overland flow was more dominant in experiments with high precipitation rates, although both processes occurred in all runs. » is not clear and even contradicts itself.

> We disagree that this statement is contradictory. Substrates with high infiltration rates had more Type 2 erosion than substrates with low infiltration rates. Experiments with high precipitation rates had more Type 1 erosion than experiments with low precipitation rates. All runs had channels with both Type 1 and Type 2 channel heads at some point during the run. Thus, both processes occurred in all runs. We do not think that overland flow and seepage erosion are mutually exclusive. Surface water can be contributing to erosion via overland flow, while subsurface flows may be contributing to erosion via seepage erosion. Both processes could thus be occurring simultaneously. Our interpretation is that when one type dominates over the other, it can lead to different morphologies in the channel heads.

2) The data processing of the DEM is not sufficiently specified. Several tools, like "sinks", "watersheds", "Basin" are evoked but not defined. Are they specific to the commercial software FARO® SCENE? It would be better to define the mathematical operations and precise then the "tool" of the software or the algorithm. I suppose that the software finds the local minima in the elevation data after removing the average slope. Am I right ?

> These terms are specific to ArcGIS software. We have gone back through the manuscript to use more general descriptions of the algorithms, followed by the name of the tools used in the ArcGIS software and made it clear that these are ArcGIS algorithms.

3) The interpretations at the grain scale are missing. How the cohesion due to the clay modifies the erosion threshold (Shields criterion for example)? Here the addition of clay is only seen as a way to decrease the infiltration rate and favor overland flow. The increase of the erosion volume with the clay at the end of experiments is not really explained.

> We did state that the clay plays two roles: it decreases infiltration and increases cohesion which impacts the erodibility of the material. Although we have measurements of infiltration capacity, we did not make direct measurements of how clay modifies cohesion and critical shear stress. However, other experiments that use sand-kaolinite mixtures do have information on how much cohesion or yield strength was increased with additions of kaolinite clay. We summarize this briefly in the paper and provide more information below.
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> Reddi and Bonala (1997) investigated the impact of kaolinite clay additions to fine sand of between 10% and 40% kaolinite. They subjected the mixtures of clay, sand, and water to a variety of tests. We took their cohesion measurement data for mixtures, fit a curve to the 10%, 20% and 40% kaolinite data and extrapolated it back to 0%, 2%, and 6% kaolinite. For all mixtures, we fixed 0% clay to be 0% cohesion. For "wet of optimum" conditions, the cohesion for 2% clay was between 1.4 and 3.5 kPa; for 6% clay, the cohesion was between 6.0 and 10.4 kPa. We thus predict that the cohesion in the 6% clay run would be 3 to 4 times as high as in the 2% run. The range is based on the type of regression used. Reddi and Bonala (1997) found a linear relationship between cohesion and critical shear stress, thus changes in cohesion are likely reflected in changes in critical shear stress.
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> Both Marr et al. (2001) and Ilstad et al. (2004) used sand-kaolinite-water mixtures to model subaqueous debris flows. Ilstad et al. (2004) measured the yield strength of mixtures between 5% and 32.5% kaolinite, with an exponential relationship between the clay content and the stress required for deformation to occur. Extrapolating that relationship to our clay contents (0%, 2%, and 6%) would give a yield stress of 3.4 Pa for the 0% clay content, 4.3 Pa for 2% and 7.2 Pa for 6%. Yield stress should be sensitive to both the sand grain size and the water content. Ilstad et al. (2004) used 500 micron sand, while Marr et al. (2001) used 110 micron sand, similar to our experiments. The water contents in Ilstad et al. (2004) of 35% were similar to our mixtures at the start of the experiments when fully saturated. Because we do not have a perfect match between sand grain size, water content, and kaolinite content, we cannot directly use estimated yield strengths from these publications, but the difference in yield strength may be similar. Thus, based on Marr et al. (2001) and Ilstad et al. (2004) data, we expect that the 2% clay content runs would have a yield strength ~30% higher than 0% clay runs. The 6% clay runs would have a yield strength ~120% higher than 0% clay runs. The yield strength is directly related to the critical shear stress required to erode sediment.
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> Ilstad, T., Elverhøi, A., Issler, D., & Marr, J. G. (2004). Subaqueous debris flow behaviour and its dependence on the sand/clay ratio: a laboratory study using particle tracking. *Marine Geology*, *213*(1-4), 415-438.

Marr, J. G., Harff, P. A., Shanmugam, G., & Parker, G. (2001). Experiments on subaqueous sandy gravity flows: the role of clay and water content in flow dynamics and depositional structures. *Geological Society of America Bulletin*, *113*(11), 1377-1386.

Reddi, L. N., & Bonala, M. V. (1997). Critical shear stress and its relationship with cohesion for sand. kaolinite mixtures. *Canadian geotechnical journal*, *34*(1), 26-33.

4) Figure 14. Please increase the size of the symbols.

We made the symbols smaller on that figure on purpose, so that the standard deviations could be visible. The larger symbols hid the error bars. At your request we went back to the larger symbols and noted in the caption that the error bars are smaller than the symbols.

5) The implication in the field is interesting in part 5, but remains largely speculative due to the lack of quantitative comparisons to my opinion. However, in the conclusion theses implications are satisfyingly presented as "possible".

The comparisons with the field are intended to spark reflection on how multiple drivers may be contributing to erosion at the channel heads depending on substrate and relief. As you note, we treat this as “possible” rather than as proven.