Comment on esurf-2021-105
George Hilley (Referee)

Referee comment on "Drainage reorganization induces deviations in width-area-slope scaling of valleys and channels" by Elhanan Harel et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-105-RC2, 2022

Review of "Drainage reorganization induces deviations in width-area-slope scaling of valleys and channels" by Harel et al.

Summary:

This contribution uses field observations, TanDEM-X-derived DEMs, and high-resolution SfM (and GNSS) field surveys to study how valley and channel width vary with drainage area for watersheds experiencing drainage reorganization. To do this, they use a series of 12 watersheds, some of which have been previously documented, in the Negev desert. Here, a rift-like structure has produced a vertical offset on the order of 500 m, which cuts across the heads of a series of (formerly) west-flowing drainages. This offset creates a knickpoint, which redirects flow within a portion of the formerly west-flowing channels to the east. As the knickpoints expand, the divides migrate. However, incision has not been sufficient to erode a valley-depth of the landscape, and so the valley geometries of the former flow direction are preserved within this landscape. The authors exploit this fortuitous circumstance to measure how the headward migration of the drainage divide produces channel width - area relations at odds with the valley width - area relations encoded in the former regime. This, with some scaling arguments used to calculate stream power in the channels, suggests that divides may be more mobile than might appear due to a simple stream-power rule because of the narrowing of channel width that accompanies incision of the east-flowing channels.

General Comments:

1) This is a very nicely executed study in a fascinating natural system. The fact that the old valley network can be clearly delineated and measured is fortuitous for addressing this
2) The study is predicated on the idea that valley width scales as a function of watershed area in a way that is similar to channel width. While this may empirically be the case, the valley width (in fluvial environments) may be set by a different set of factors (e.g., erodibility of the bank materials that scales migration rate, human modification of channel systems such as mill ponds, landslide damming of rivers) than the channel width. I'm fine with an empirical measurement of valley width to demonstrate that scaling (for instance, the Beeson work), and these different factors are acknowledged. But, it could be valuable to dedicate a bit more description to this distinction – that one can be related, somewhat directly to the flows that traverse the channel, while the other will be related to the evolution and migration of the channels over time.

3) One contribution of this work is to present a novel method of extracting valley width, which is useful for a number of types of studies as the citations of the authors indicate. It seems to work particularly well for a class of valley morphologies in which the valley floor can clearly be distinguished from the valley side-slopes by assigning a (calibrated) slope threshold. It's worth noting that this is not generally the case, and so this method will fail when there is a gradation between valley floor and side-slopes. Also, it's probably worth noting that the valley width in this case is the width of the valley floor. Finally, I would suspect that the method works best when the valley and channel sinuosity are similar. Otherwise, might a sinuous channel lead to aberrant projections normal to the trend of a sinuous inner channel to the VBET? These issues might be discussed a bit more in the methods section.

4) The three classes of watersheds, "Undisturbed", "Beheaded", and "Reversed", seem to imply a conception of how water is routed through this landscape. In particular, if one were to simply consider the DEM, probably all of the watersheds would be "Beheaded" or "Reversed", since the escarpment cuts across the apparent heads of the former drainages. But, it seems from Figure 1b that the network is incised into a low-relief surface. Figure 1b thus implies that the distinction between a beheaded and undisturbed channel rests on whether or not the incised portion of the drainage has been cross-cut by the escarpment / other channels. Maybe I am confused about this, but if this is indeed the case, then a channel is undisturbed only if water is sourced exclusively from the incised portion of the landscape. If this is the case, I think the watershed areas must be calculated based on those incised areas of the landscape, rather than the entire DEM.

5) The idea that channel widths adjust more quickly than valley widths seems intuitive. Thus, in a landscape with such clear divide migration, one would expect channels widths to increase with area, while the valley widths might behave in an opposite manner, which is I think the main point of the paper. A couple of thoughts on this:

A) I think the reversed valley width scaling can persist for no longer than the valley width divided by the incision rate * average hillslope angle. The signature could be much shorter, but this would be the maximum amount of time that would be required to remove the former valley morphology.
B) I was not completely on board with the association of the valley and channel widths in the eastward-flowing channels for the stream-power calculation (and the inferences that flow from this). The authors assert that in the westward-flowing channels, there are a series of anastomosing low relief-channels that occupy virtually the entire valley width. I think this is being shown in Figure 7, where the width measurements for the stream power calculations are the yellow “width measurements”? Yet, the photograph of the valley seems to indicate that only a fraction of those valleys are occupied by a channel. If this is the case, the width effect on the stream power calculation might be somewhat overstated.

C) Another way to have a larger contrast in stream power between the reversed and beheaded channels might be to have increased infiltration and transient storage of precipitation in the valley alluvium on the low-sloped, eastward flowing channels relative to their west-flowing counterparts. I don’t know much about the flashiness of the Negev desert, but some alluviated valleys in California produce surface runoff only in very large storms because of the effect of infiltration into the low-sloped valleys. This might stand in contrast to the steeper-sloped segments that form as the drainage divide migrates. Perhaps this is worth some discussion as well?

Thanks very much for the opportunity to be part of this work. The field site is a real gem, which has been exploited well by the proponents. I am support of its acceptance to ESurf, pending consideration of some of the issues I mentioned above.

George Hilley.