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Interactive comment on "Aerobiology and passive restoration of biological soil crusts" *by* Steven D. Warren et al.

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Dear Drs. Warren, St. Clair, and Leavitt –

I have read your manuscript "Aerobiology and passive restoration of biological soil crusts", submitted to Biogeosciences. I find it very interesting, but I have mixed feelings about it. The basic premise is that biocrust propagules are blowing in the wind all the time and depositing in degraded areas. The propagules can promote the natural recovery of biocrusts, without the use of inoculants. While it is important to document the aerobiology of biocrusts, and that restoration can occur passively, I feel there is not much new here, and that your conclusion that production of inoculants may be the wrong path is overstated. I provide several comments that I feel you ought to address.

1. Novelty – The knowledge that biocrusts can passively recover from disturbance is not new, and that biocrusts can disperse long distances is also not new. In an old review of mine which you cite (Bowker 2007) I refer to passive restoration as the removal of a disturbance or stressor, you seem to be referring to the same basic idea, but more explicitly discussing how and why that recovery works. That to me is the novel piece – How/why does passive restoration work.

Substantial blocks of text are very similar to existing reviews. For example the sections on artificial restoration is not so different from Zhao et al. 2016. The natural recovery information in the following section has already been summarize din Weber et al. 2016 and elsewhere. Both of these are chapters in Weber et al. (eds) Biological soil crusts: an organizing principle in drylands.

2. Not all types of biocrust are effectively aerially dispersed. For example, consider the moss biocrusts that are so common in cool semi-arid steppes. They may be dominated by species that rarely produce spores due to rarity of male plants. Fragments of moss plants are not able to travel long distances on the wind. Likewise, many common lichen species do not produce many fruiting bodies and their spores are further limited by a need to encounter a suitable photobiont. I think much of what you discuss in the paper is more relevant to the cyanobacterial and other microbial components. These components may be absent or less important than mosses and lichens in quite a few habitats.

3. Large-scale field trials exist. Right now this manuscript is downplaying the multi-hectare scale work done by Yongding Liu, Chunxiang Hu, and Shubin Lan which involves the use of inoculants in conjunction with sand stabilization (straw checkerboard). China became the vanguard of this research area in the early 2000s.

4. I think that the pessimism toward inoculation-based techniques is overstated. First on L 123- 127, you argue that these techniques require too much water, are too costly, and often don't work. This sounds like every new technology. Just because universal

success hasn't been achieved, does not mean it never will be. I like to compare to farming. People have been practicing and fine tuning farming for $\sim$ 10,000 years; even so, farming still fails often, and we can be reasonably certain that there are many failures in the first few decades of farming. But, it has taken us far. In inoculant-based biocrust restoration, there are many new ideas to try that might increase field success rates. I think we could use more resources and more research groups working toward novel solutions and breakthroughs. L313-319 is even more negative. Can't we wait for the research to be done before giving up!?

5. The aerobiology section is definitely my favorite. You use it to set up the idea that the atmosphere is passively raining biocrust propagules. The unstated assumption at this point is that the delivery rate is generally sufficient to induce biocrust growth. That is not known. If the answer is no. . .then is artificial inoculation warranted?

6. The nexus section appears to show a success story of aerobiology as a passive restoration technique. Another view might be that this is a failure of aerobiology because no biocrusts were induced in areas without straw checkerboard despite that those areas must also have been experiencing propagule delivery. The recovery was induced by a labor intensive active restoration measure – stabilization of the dunes with straw barriers and shrub planting– after which the natural rain of biocrust propagules induces a crust. Even so, it required several years for this to happen. Dr. Liu's lab claims to achieve the same thing, adding inoculants, in weeks.

In many locations, recovery is not constrained by propagule availability, it is constrained by other barriers that may be self-reinforcing like erosion-deposition. Passive rain of biocrust propagules may play a role in restoring such areas but they are unlikely to be sufficiently effective on their own.

On a related note, if you seek a less-nuanced large-scale demonstration of passive recovery, you'll find it in Gao et al. Soil Biology & Biochemistry 105:49-58

7. Both passive and active recovery has the same primary weakness at large scales.

They are both subject to the socio-political will to remove disturbance agents (often elements of the local economy) over large areas. This can happen in China, but in few other places. Without this, both active and passive methods will be constrained to small and meso-scales. So, if the goal is specifically large-scale restoration. . .I don't think passive restoration is the "right tree" any more than production-introduction of inoculants. Thus, if we are confined to working at small and meso-scales in some places (e.g. the USA), does inoculation-based technology still seem too expensive and resource-intensive?

Minor comments – L53 – I don't find 18 species striking, relative to say, vascular plants, or especially relative to other bacteria. L69 – Proportionally little attention relative to what? If you're just saying that a small proportion of dryland restoration literature addresses biocrusts. . .say that. This statement was emphatically true in 2007, but the research landscape is changing fast, as is apparent from an entirely new review of biocrust restoration by Zhao et al. 2016. Biocrust restoration research may be far from mainstream, but it is no longer rare. L75- Relative to what? L91- I too have made this argument, that taking biocrust from one place to another is a zero-sum game. But I now think this is wrong because there are many planned disturbances in land management. Consider the construction of a photovoltaic array in an area supporting biocrusts. These areas may be up to several square kilometers. An area like that could be used to supply inoculant to a much larger area (say 10x larger, if inoculant supply rate was 10% or the ultimate restoration target), and would not in and of itself lead to additional disturbance. L118 – These materials were field tested here https://link.springer.com/article/10.1007/s11104-017-3300-3 with mixed success. In this case, the mixed benefit from adding propagule was short-lived and passive recovery "caught up". Our study site appears to be one that recovers well passively. L182- I'd add stable erosion/deposition rates. I agree with you that success depends on lining up all of these things at the same moment in time, but all of these can be manipulated to some degree to create restoration methods that can be used in conjunction with inoculation (augment soil conditions and resources, intelligent spatio-temporal ap-

plication of materials, artificial soil stabilization, etc.). L295 – decades L308-309 – I agree fully with this caveat

I'm sorry I couldn't be more positive, but I hope that you find my comments constructive – Matthew Bowker

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