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Erosion on arable land : a reflection on what we know, what we do not know and what we should know

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One of the key issues of (water) erosion research has been the development of mechanistic, process-based models: the implicit aim of this research effort was to improve not only our understanding but also our predictive capabilities. It is now clear that such models only partially fulfill their promise: the overall predictive capability of process-based models is often of the same order of magnitude than the predictive capability of empirical models based on regression. Yet we now do understand far better the fundamental limits to prediction than we did 20 years ago and we also know that these limits cannot be changed, barring irrealistically high efforts on data collection in the field and the laboratory. At the same time we notice that efficient erosion control techniques can often be implemented based on existing knowledge. While knowledge gaps still exist, it may be questioned whether further advances in soil erosion research will lead to improved soil conservation.

So, what then is the future of soil erosion research? While it is clear that some knowledge gaps with respect to erosion prediction still do exist and need to be filled) it is also evident that striving towards improving soil erosion prediction cannot be the (only) goal of the next decade. In this presentation we reflect on what directions future research may take. It appears that we still have surprisingly little insight in the consequences of soil erosion: for example, far less research has been devoted to the effect of erosion on crop productivity than to the prediction of interrill erosion. Not only the effect of soil erosion on soil functioning is underresearched. There is also very little information on the effects of soil erosion on the functioning of ecosystems at various scales one of the reasons being that our understanding of the linkage between hillslope and fluvial systems in still weak: often, we only have a limited idea of the factors controlling sediment delivery and our understanding of temporal variations

in sediment deliverly is little to none. We also have little information the interaction between sediment erosion, deposition and transport processes and key biogeochemical cycles. Solving these issues may need a change in modeling strategy whereby a spatially explicit, correct representation of processes and their interactions may be more important than a high temporal modeling resolution. If we attempt to fill in these knowledge gaps we will discover for which processes our knowledge is insufficient to understand the response of the whole system: this need to understand may then replace our ability to model or monitor as the most important driver of erosion process research.