

We thank S. Mylevaganam for his comments on our manuscript. To facilitate comparison of the comments and our replies by the readers of this interactive discussion, we have compiled the comments in one file and replied at the end of each comment in blue

Community comments by Sivarajah Mylevaganam, 14 Sep 2026

Soil erosion has become a critical global phenomenon with profound implications for essential ecological cycles, food security, and the sustainable development of nations. Globally, numerous attempts have been made to precisely estimate soil erosion; however, achieving flawless accuracy remains challenging due to the sheer complexity and multi-dimensional nature of the driving environmental factors.

Recognizing the urgent need to mitigate this issue, Germany enacted the German Soil Protection Law and its accompanying decree to safeguard the nation's environmental livelihood. In the current version of the manuscript, the authors argue that this legislation was established with limited or insufficient knowledge of key driving forces, specifically the recurrence intervals or return periods (RP) of erosive rainfall events. Consequently, the authors aim to investigate this issue at a fundamental level to ensure that future policy and environmental legislation are guided by rigorous scientific evaluation rather than ad-hoc assumptions. Based on the current draft of the manuscript, the following points are brought forward for detailed discussion:

1. LINE 50-52: The central problem statement of the manuscript focuses on the German Soil Protection Law and its respective ordinance, arguing that the framework fails to adequately include the return period (RP) of erosive events. While the manuscript correctly identifies the timeline of the legislation, the current version lacks substantial evidence or background to fully justify this claim. To strengthen the introduction, it would be highly beneficial if the authors provided deeper context in lines 50–52 regarding how the legislators originally formulated these guidelines. Clarifying what foundational assumptions or baseline historical data the legislators relied upon when the framework was established would significantly anchor the study's rationale. Currently, the text adopts an overly critical stance toward the 2021 legislative updates, which risks coming across to readers as an unfair dismissal of the regulatory body's efforts rather than an objective scientific critique. Reframing this section to focus on the technical opportunities for optimizing the law—backed by historical context—will make the research objectives much more compelling and well-received by the audience.

Our starting point was that return periods for erosive rains were unknown until today, despite an enormous and rapidly growing body of research on soil erosion. We mentioned the German Soil Protection Law to illustrate the relevance of this scientific knowledge gap, not to criticise the law in any respect. In our revision, we will rephrase the sections that mention the law to make it clear that our intention is exclusively scientific, not legal. We are grateful to S. Mylevaganam for identifying this potential misunderstanding of our text by readers.

2. LINE 80-213: The methodology employed to resolve the problem statement (line 50-52) is highly debatable. In hydrology, it is well-established—though under-documented—that soil erosion or the erosion damages [line 50–52] is not a phenomenon dictated solely by rainfall magnitude and duration. The authors' current approach might be preferred by administrative engineers for bureaucratic simplicity, from a scientific standpoint, this is a major oversimplification. The underlying soil type plays a critical role in governing actual erosion damages. To make this research robust, the authors must incorporate specific soil types, map their spatial distribution across Germany, and demonstrate how these spatial variations alter the outcomes and conclusions of their research. In addition to soil type, localized soil management practices (e.g., tillage, cover cropping, crop rotation) represent a critical variable that significantly alters soil vulnerability to erosive forces. This influence is particularly pronounced when dealing precisely with the low-magnitude, high-frequency rainfall events that the German decree legally regulates. Ignoring management practices overlooks a primary operational variable of how the law functions in practice. The manuscript needs to account for or explicitly discuss this human-induced variable.

The comment is based on a misunderstanding of the concept of rain erosivity. We do not analyze the return periods of erosion events but of erosive events. This distinction is fundamental. Erosivity is entirely a rain property that applies before the rain hits any vegetation or soil. Return periods of erosion events would require different data and a different approach.

To our knowledge, the largest existing dataset that would allow us to calculate return periods for erosion events was published by Fiener et al. (2019), but even this dataset is much too limited in space and time for this task. Hence, we rely on modelling tools like the Revised Universal Soil Loss Equation, which separates erosivity in a broad sense (including flow accumulation along the slope expressed in the *LS* factor) and erodibility in a broad sense (including the resistivity by soil, vegetation and protection measures quantified in the factors *K*, *C* and *P*). We contribute to this modelling approach, but there is still a long way to go, as not only return periods for erosive rains are required, but also the seasonalities of all factors influencing erosivity and erodibility.

Fiener, P., Wilken, F., and Auerswald, K.: Filling the gap between plot and landscape scale – eight years of soil erosion monitoring in 14 adjacent watersheds under soil conservation at Scheyern, Southern Germany, *Advances in Geosciences*, 48, 31–48, <https://doi.org/10.5194/adgeo-48-31-2019>, 2019.

3. **LINES 109–114:** The authors claim that there are **357,779 pixels** covering the country at a 1 km spatial resolution, which then changes to **3,884 pixels** when the resolution is resampled to 10 km. Considering the initial value of 357,779 pixels as the benchmark, a 10-fold spatial aggregation should theoretically yield approximately **3,577 pixels** ($357,779 / 10$). This leaves an unaccounted discrepancy of **307 pixels**, which depends heavily on the specific masking method used during coarsening. The manuscript does not explicitly explain the cause of this pixel count inflation. Are pixels outside the defined domain of interest inadvertently being incorporated into the coarser resolution dataset?

We did not apply 10-fold aggregation but instead used 100-fold thinning, selecting every tenth pixel in a row and every tenth line. The discrepancy between the number of pixels used and the mathematical simplification in this comment is simply due to Germany's shape. There was no selection by us that could bias the data.

Intuitively, one might expect that this approach could yield a number smaller than 3578 but not one larger. The simple reason is that any data point in the border region represents a smaller area than 10×10 km. This is especially true for islands and thin bulges of the country's shape. For countries smaller than Germany, the discrepancy between the theoretical and actual numbers would be even greater, while it would decrease for larger countries. It would increase for countries with more irregular borders and shapes that deviate even more from a square shape, with side lengths that deviate from multiples of 10 km.

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4. **LINES 115–118:** The manuscript states that the R-factor ranges between 46 and 454 units, with approximately 90% of these values falling between 62 and 163, and values exceeding 250 localized within the Alps. The authors also note that the average R-factor for the "mean year" 2007 was 96. It remains unclear what is meant by the phrase "mean year 2007." Furthermore, the authors should provide a standard mean value to allow for a direct comparison with previously published 2007 literature. As currently written, the broader motivation behind listing all of these specific, uncontextualized numerical values is outside the necessary scope of this section. Moreover, the concentration of the highest R-factor values within the Alps—a region predominantly characterized by **non-arable land**—suggests a fundamental mismatch in the study's framework. Since the core policy issue raised by the authors focuses on managing erosion risks under the

Federal Soil Protection Act (which primarily impacts agricultural landscapes), highlighting extreme values in non-agricultural, alpine zones does not help resolve the specific problem statement posed in this manuscript. The authors need to better align their spatial analysis with the actual domain of application, rather than using these uncontextualized outliers to justify generic, open-ended future research directions.

We clearly explained in our text that the R factor shows a strong trend over time, which is a major complication for estimating return periods that can be applied not only to the past but also to the future. When observations exhibit a systematic temporal or spatial trend, the overall arithmetic mean is an estimate of the marginal mean over the sampled temporal or spatial domain. It should not be interpreted as a representative value for an arbitrary time or location, because the conditional mean varies systematically with time or space. Regarding S. Mylevaganam's comment, the mean R factor is an appropriate summary of the entire sampled domain but not a representative value at a particular time except for the mean of the domain (the mean year), and it cannot be applied outside the sampled domain.

Another major complication arose from the fact that a small part of Germany includes a share of the Alps with pronounced orographic rainfall. For us, it would have been much easier excluding this clearly identifiable part as suggested by S. Mylevaganam. This would, however, turn our work useless in the Alps, where still arable fields exist, although rarer than outside the Alps. Even if no arable field existed, the Alps could not be excluded, as erosion also occurs on grassland, forest land (often at rates close to what is called “geological” erosion), or bare areas (e.g. where snow creep has removed vegetation).

5. LINE 50-52: Considering the core objectives outlined in **LINES 50–52**, it is questionable whether calculating the Return Period (RP) in isolation provides a hydro-geomorphologically sound solution. Soil erosion damage is not driven exclusively by isolated factors like R , EI , and RP . Following the authors' grid-based evaluation, a rainfall event occurring in a specific cell would realistically be influenced by antecedent or simultaneous conditions in adjacent cells. In grid-based surface modeling, this is standardly addressed using an **8-point routing scheme**, where the eight surrounding cells dictate directional flow and runoff accumulation. Consequently, even a minimal local rainfall event can cause severe erosion if the accumulated runoff from surrounding cells is accounted for. Therefore, evaluating the RP of erosive rains in spatial isolation may not accurately address the problem statement posed by the authors.

See our reply to comment 2. We may mention that already almost four decades ago, we were likely the first to use runoff accumulation in a GIS environment for erosion modelling as suggested by S. Mylevaganam (Flacke et al., 1990).

Flacke, W., Auerswald, K. and Neufang, L. (1990): Combining a modified Universal Soil Loss Equation with a digital terrain model for computing high resolution maps of soil loss resulting from rain wash, Catena, 17, 383-397, [http://dx.doi.org/10.1016/0341-8162\(90\)90040-K](http://dx.doi.org/10.1016/0341-8162(90)90040-K), 1990.

6. The authors must clarify Equation 1 in light of the statement made in **LINE 86**. According to the text, the R-factor represents the **long-term mean annual sum of event erosivities**. However, Equation 1 mathematically defines a summation of all erosive rainfall events occurring within a single year at a specific cell. The authors must explicitly clarify whether this single-year summation is indeed what they are defining as a "long-term mean annual sum," or correct the formula/text accordingly.

Please note that Eqn 1 defines the annual R factor (i.e. R_t of an individual year t) but not the mean annual R factor (i.e. the mean of many years). This equation for the annual R factor is not only the basis for calculating the mean R but it is particularly required here to derive the mathematical and statistical approach of our work.

7. **Line 106:** The authors state that radar rainfall (RR) technology is evaluated for the exact radar pixel in which the rain gauge (RG) data was obtained. It is unclear what is mechanically meant by this statement. To the best of our knowledge, rain gauges provide localized point measurements, whereas radar data represents spatial grid cells (pixels). Attempting to pair a pixel directly to a point measurement implies an implicit assumption regarding the spatial representativeness or areal coverage of a single gauge. Is this the intended meaning of the authors? Furthermore, how does this specific approach align with or depart from established prior work in radar-gauge merging/validation? While providing citations is necessary, the text must explicitly articulate the underlying spatial concept to the reader rather than relying on references to do the explanation.

S. Mylevaganam understood our text correctly. We compared spatial averages of RR cells with point measurements of rain gauges. Nowhere do we claim that they are equal; in fact, we analysed the influence that this difference in spatial representation has on return periods. Given that S. Mylevaganam correctly understood the experimental setup, no further explanation is necessary.

8. **LINE 85-90.** The description of the rainfall erosivity parameter (EI) requires critical clarification. The manuscript states that EI is the product of the total kinetic energy of a storm multiplied by the "maximum intensity within 30 minutes". However, the technical definition of rainfall intensity intrinsically includes a time duration in its denominator (i.e., rainfall depth divided by duration). As currently written, the phrase "maximum intensity within 30 minutes" is mathematically and

conceptually ambiguous. It is unclear whether the authors mean the maximum localized intensity recorded during any 30-minute window within a specific storm event, or if this value is evaluated across an entire annual dataset. Because this parameter serves as a foundational component of your erosivity calculations, the exact mathematical formulation and temporal window used to isolate this maximum value must be made explicitly clear to the reader rather than assuming its definition.

The R factor is an almost 70-year-old concept that is used in thousands of publications. A more detailed explanation seems unnecessary. In particular, the suggestion that our sentence may be misunderstood in that $I_{\max,30}$ has to be determined across an entire annual dataset is unlikely for everybody who has ever dealt with the R factor. Others won't be interested in our work. However, when reading the phrase in isolation, we noticed that "maximum intensity within 30 min" can be misinterpreted. We will rephrase the text to read "maximum average intensity of moving 30-min-long periods".

9. **Lines 101–106:** The manuscript indicates that the analysis is based on rain gauge data spanning only from 2001 to 2016. Given that the current year is **2026**, this dataset is significantly outdated relative to contemporary climatological standards. Over the past decade, many regions have experienced pronounced natural and anthropogenic shifts in climate and weather patterns. Restricting the analysis to data prior to 2017 is difficult to justify—particularly for Germany, a highly developed nation with substantial public and institutional investments in atmospheric and scientific research. The authors must explicitly state the rationale for omitting the last decade of records. Did the national meteorological service fail to document these years, or was there a deliberate transition to satellite- or radar-based monitoring post-2016 that led to the exclusion of newer ground station data?

The main reason is that we used the only consistent and published dataset on rain erosivities. This dataset ends in 2016. We regard rain-gauge erosivities as outdated for practical applications, anyhow, because of the extreme spatial gradients in the rain erosivity of individual events over a few hundred meters (see, for example, Fig. 6 in Fischer et al., 2018).

From a practical point of view, our return periods can also be extended into the future because we explicitly accounted for climate-change effects. However, in a few years, it may be useful to calculate event erosivities from rain-gauge measurements again to compare return periods with those predicted by our work. This will allow the identification of potential climate-change effects that are currently not visible, if they exist.

Fischer, F.K., Winterrath, T. and Auerswald K.: Temporal- and spatial-scale and positional effects on rain erosivity derived from point-scale and contiguous rain data,

10. LINE 90-95: In scientific writing, it is standard practice to define variables alongside their respective units upon first introduction. Because this standard applies universally across disciplines, the detailed discussion regarding unit conversions in lines 90–95 feels somewhat redundant and dilutes the core focus of the manuscript. As long as each dimensional variable is clearly annotated with its chosen unit, the presented data will be easily understood by the reader without requiring an extensive list of alternative regional unit systems. I suggest streamlining this section to keep the text concise and impactful. Additionally, there appears to be a confusion in lines 92–93. The unit $\text{MJ}\cdot\text{mm}\cdot\text{ha}^{-1}\cdot\text{h}^{-1}$ is presented as equivalent to $\text{MJ}\cdot\text{mm}\cdot\text{ha}^{-1}\cdot\text{h}^{-1}\cdot\text{a}^{-1}$. The inclusion of the "a" term changes the dimensional meaning and implies a completely different variable altogether.

We agree that unit conversion should be a standard exercise in science. However, experience and the confusion of units show that this is not the case. This is particularly true for the *R* factor, given the complexity and diversity of its units. Hence, we wish to keep this paragraph. It will be particularly helpful to young scientists. To the best of our knowledge, nowhere else is the history of the different units of the *R* factor and the reasoning behind the changes explained. Hence, this is not only a mere unit conversion but original work.

11. LINE 80-213: In the current version of the manuscript, Section 2 (Materials and Methods) is divided into only two subsections: **Section 2.1 (Rainfall Erosivity Data)** and **Section 2.2 (Data Evaluation)**. There appears to be no dedicated subsection outlining the core methodology, or computational workflow. Without a distinct **Section 2.3 (Methodology)**, the current structure gives the impression that the section covers only the dataset description and its basic evaluation, leaving the actual experimental design unclear. The authors should introduce a dedicated methodology subsection to provide a crystal-clear, step-by-step explanation of the analytical framework used in this study.

We have called section 2.2 Data evaluation, but it could also have been titled 2.2 Methodology (note that the subheading “Other statistical methods” is also included in this section). We see no need to rename the section or add another section. The assumption of S. Mylevaganam that “Data evaluation” means “Basic data evaluation” has no foundation, given the detailed description.

12. **Lines 450–455:** Since multiple authors are involved, the current version of the manuscript utilizes abbreviated initials (e.g., "TW") to outline individual contributions. The authors should verify whether this specific formatting aligns with the journal's standard guidelines, as high-impact journals typically require a systematic, standardized framework. Moreover, since one of the authors has **claimed authorship** for calculating the rain radar(**RR**) data, the methodology used to assign credentials among the co-authors must be clearly explained. This is necessary to delineate the scope of each author's intellectual contribution, ensuring transparency and scientific integrity.

We will verify again that we have followed all the journal's guidelines, and we are also confident that the editor and the editorial office will uncover any deficiencies if they exist.
