

I have carefully evaluated the manuscript entitled *Soil Stoichiometric Characteristics and Influencing Factors in Karst Forests at Microtopography and Microhabitat Scales*. The authors quantitatively assess soil stoichiometric characteristics and balance in karst forests and examine their response mechanisms to several environmental factors. The experimental design is scientifically sound, and the data and analyses are adequate to address the scientific questions posed. However, the logical flow of the writing in some parts of the manuscript needs improvement, and the authors should revise these sections carefully. I recommend publication after the authors have addressed the comments and revised the manuscript accordingly. The comments are as follows:

#### Major concerns

1. The Introduction devotes considerable space to highlighting the importance of studying soil stoichiometric characteristics and their responses to environmental factors. However, it does not address two key issues, which weakens the logical progression and justification for the subsequent work: (1) Which environmental factors influence soil stoichiometric characteristics, and by what mechanisms they do so; (2) What soil stoichiometric processes can be inferred from soil stoichiometric balance, and how environmental factors affect these processes. In addition, the second paragraph provides an extensive description of the uniqueness of the study area, but it does not clearly explain how these site-specific characteristics may shape soil stoichiometric characteristics and balance.

2. Plant nutrient content is considered as a factor influencing soil stoichiometric characteristics only in the VPA analysis. It is therefore unclear why this variable was not included in other correlation analyses. In addition, the Introduction does not describe the relationship between plant nutrient content and soil stoichiometric characteristics or balance. The Results section also lacks a presentation of the data on plant nutrient contents. Together, these omissions make the inclusion of this factor in the VPA appear abrupt and insufficiently justified.

3. The Conclusions section is not written in an appropriate manner. It should briefly restate the study objectives and then concisely synthesize the key findings and overarching patterns derived from the Discussion, in direct correspondence with the scientific questions raised in the Introduction. At present, the Conclusions are overly long and the main points are difficult to identify.

#### Other concerns

1. The Results section lacks key quantitative findings. Please provide the essential numerical results to strengthen this section.

2. Section 2.4. The classification criteria of microhabitat across the different categories appear to overlap, and some of the terminology is ambiguous. Especially, multiple terms are used repeatedly to describe “gully” (e.g., gully, solution gully, stone gully, and soil-dominated gully). However, the distinctions among these terms are not clearly defined, which may cause considerable confusion for readers. Besides, if the three types of microhabitat are defined using multiple indicators (e.g., bedrock exposure ratio, soil cover area, gully erosion depth), it would be clearer to present these criteria in a table. If possible, please also provide representative field photographs for each category

to facilitate understanding.