Reply to Topic editor's comments and suggestions

Dear authors,

Thank you for your careful and thorough revision of the manuscript, which has substantially improved.

As pointed out by both reviewers, however, a small number of minor issues and clarifications remain to be addressed before the manuscript can proceed to publication. Those remaining comments are intended to help further strengthen the manuscript and will not require additional external review.

I encourage you to carefully consider and address the points raised by the reviewers in your revised submission and accompanying response letter. Once these final matters have been satisfactorily resolved, I anticipate that the manuscript will be ready to move forward in the publication process.

Thank you for your continued efforts in improving the paper.

Best regards,

Thomas Poulet

Re: We appreciate the time and effort that you and the reviewers have devoted to evaluating our work. We have carefully considered all of the remaining comments and suggestions raised by the reviewers and have made the corresponding revisions in the manuscript. We have also uploaded the new evidence layer data, revised several references, and provided a detailed point-by-point response in the response letter. Thank you again for your consideration of our manuscript. The updated dataset and accompanying descriptions are provided in the supplementary materials (https://doi.org/10.5281/zenodo.22347352).

Reply to Referee #1 Dr. Nathan Bowman's comments and suggestions

The discussion surrounding the categorisation of uncertainty, while improved, can still be streamlined. The shortcomings of applying MPS-DS to geophysical and geochemical data need to be acknowledged, especially the claim that simulation mitigates the issues of traditional interpolation techniques, yet the authors workflow applies MPS-DS to an already interpolated raster of aeromagnetic data.

Re: We sincerely appreciate the reviewer's careful assessment and helpful suggestions, which have further improved the reproducibility and conceptual clarity of the manuscript.

We have streamlined the discussion of uncertainty categorization and revised the corresponding description of the DS methodology. We clarified the limitations of applying DS to geophysical and geochemical data. These variables are continuous and exhibit anisotropy and heterogeneous spatial characteristics. Such features may not always be fully represented by a single TI, and the quality of the TI can strongly affect the simulated realizations. Moreover, when the input data are derived from an interpolated raster, the simulations may inherit some smoothing effects and assumptions introduced during interpolation.

Accordingly, we have revised the manuscript to avoid overstating the ability of DS to overcome the limitations of traditional interpolation methods. Instead, we emphasize that DS provides multiple spatially plausible realizations, allowing spatial variability to be characterized and propagated into subsequent mineral prospectivity mapping.

General comments

The authors are congratulated on a much-improved draft. The revisions have resulted in a manuscript with a clearer structure that is easier to follow, as well as a clearer discussion of the methodology and interpretation of the results. By focusing the discussion on uncertainty decomposition and the relationship between prediction probability and uncertainty, it is now clearer how the framework presented in this paper can be used to quantify and communicate uncertainty in mineral prospectivity mapping (MPM). The manuscript could be improved further by clarifying terminology related to uncertainty and explicitly acknowledging the shortcomings of the simulation techniques employed. Overall, I believe this manuscript has the potential to make a meaningful contribution to the fields of mineral prospectivity mapping and uncertainty quantification.

Re: Thank you very much for the positive and encouraging assessment. We have further clarified the terminology related to uncertainty and streamlined the discussion of uncertainty decomposition. We have also acknowledged the limitations of the application of DS to geophysical and geochemical data and the potential influence of the preceding interpolation process. We believe these revisions have improved the clarity, rigor, and transparency of the manuscript.

Specific comments

The inclusion of additional data and code in the appendices is appreciated and supports reproducibility. However, the file evidence.tif is still not georeferenced, and it remains unclear what it represents. As I do not have access to MATLAB I cannot recreate the workflow in its entirety and cannot comment on the reproducibility of the work. The use of terminology has been tightened considerably. It is now much clearer what types of uncertainty the authors are referring to throughout the manuscript. However, in the Introduction the terms aleatoric and epistemic uncertainty are defined as "data-related uncertainty" (line 61) and "model-related uncertainty" (line 63), respectively. The authors then present Zuo's categorisation of uncertainty (lines 64-66) before describing data-related and model-related uncertainty again in lines 69-75, stating that these uncertainty types will be the focus of the study. As Zuo's categories are not referenced elsewhere in the manuscript, and the terms "data-related uncertainty" and "model-related uncertainty" are used consistently throughout the remainder of the paper, this section could be streamlined by removing lines 60-69.

Re: Thank you for this detailed and constructive comment.

First, we have replaced the original "evidence.tif" and uploaded a new file providing the necessary georeferencing information to improve the interpretability and reproducibility of the dataset. We have also added a clearer description of the evidence layers represented by this file in the supplementary materials (https://doi.org/10.5281/zenodo.22347352). Specifically, "evidence layers.tif" is a raster dataset comprising 44 layers for the study area, including geological, geophysical, and geochemical evidence layers. These consist of one fault layer (geological layer_fault.tif), one intrusion layer (geological layer_intrusion.tif), two formation layers (geological layer_taihua formation.tif and geological layer_xionger formation.tif), one aeromagnetic geophysical layer (geophysical aeromagnetic layer.tif), and 39 geochemical layers (geochemical layer.tif).

Second, following the reviewer's suggestion, we have streamlined the discussion of uncertainty terminology in the Introduction. Specifically, we have removed the redundant discussion of Zuo's categorization and simplified the definitions of aleatoric and epistemic uncertainty. The manuscript now consistently uses "data-related uncertainty" and "model-related uncertainty" throughout, which improves clarity and avoids unnecessary repetition.

The additional detail provided on the interpolation of geochemical and geophysical data is appreciated. In their response to my initial comments, the authors state that MPS-DS was used to generate stochastic realisations on a 500 m grid from an original 2,000 m grid. On line 321, the authors state that the aeromagnetic data are represented by a gridded raster that underwent a first-order derivative transformation, and that the resulting raster was used as the geophysical input. Elsewhere, the manuscript argues that simulation techniques such as MPS-DS mitigate the oversmoothing associated with traditional interpolation methods and enable uncertainty to be quantified at unsampled locations (lines 87-93, 212-215). However, the workflow described here appears to involve resampling an already interpolated and smoothed raster, which is inconsistent with these claims and may limit the ability of the simulation approach to overcome the shortcomings of interpolation. While I do not object to the use of MPS-DS as a resampling technique, I believe this limitation should be explicitly acknowledged and discussed in the manuscript.

Re: Thank you for this important and constructive suggestions. The limitations associated with applying DS to an already interpolated raster have now been acknowledged in the revised manuscript:

"It is worth noting that the application of DS to geochemical and geophysical data also has limitations. These variables are generally continuous and may exhibit anisotropy and heterogeneous spatial characteristics, which may not be fully represented by a single TI. Consequently, the quality and representativeness of the TI can strongly influence the simulated realizations. Moreover, when DS is applied to pre-interpolated geophysical data, the resulting simulations may inherit part of the smoothing effects and spatial assumptions introduced during the initial interpolation, thereby potentially propagate interpolation-related bias into the simulated realizations."

Recommendations

This manuscript should be accepted with technical corrections. While the methodology and results support the authors' conclusions the discussion surrounding the categorisation of uncertainty can be tightened, and the shortcomings of applying MPS-DS to a geophysical raster need to be acknowledged in the manuscript by the authors. However, neither of these issues impact the manuscript's ultimate findings: data uncertainty contributes more to the overall uncertainty of a MPM than model uncertainty, and uncertainty can be reduced by improving the quality and representativeness of evidential layers. This is a finding that many practitioners in MPM may intuitively recognise, but one that has been explicitly quantified in this study.

Re: We sincerely thank you for the constructive comments and for recommending acceptance with technical corrections. We have further streamlined the discussion of uncertainty categorization and acknowledged the limitations associated with applying DS to an already interpolated geophysical raster. We believe that the revisions have further improved the clarity and rigor of the manuscript.

Reply to Referee #2's comments and suggestions

Thanks for answering to the various comments of the reviewers. Most of them have been integrated in the revised manuscript, however, a few still need to be integrated, in particular with respect to initial comments made on sections 2.1.2 and 2.2.

Re: We thank the reviewer again for this helpful comment, which has allowed us to further improve the manuscript. We have carefully revisited the reviewer's suggestions in Sections 2.1.2 and 2.2. The corresponding text has been revised to improve the clarity and completeness of the methodological description. All relevant changes have been highlighted in the revised manuscript.

Section 2.1.2, the answer to the comment has not been integrated to the revised manuscript. It still needs more justification of why DS is the most appropriate tool in this case. E.g. reproducing complex pattern that cannot be simulated by two-point statistics and introducing/showing what is used as TI would be a way to do it. Do the TI exhibits stationary characteristics that cannot be reproduced by two-point statistics? It is also not quite clear in that section what is expected from DS given that Fig 2 implies that initial samples form a full regular grid mesh. Is it about down-sampling? What are the spatial characteristics of the data gap or data samples?

Re: Thank you for this important and constructive comment. In the revised manuscript, we have expanded Section 2.1.2 to clarify these aspects.

First, we provide a more explicit justification for using DS. Unlike conventional two-point geostatistical methods, which primarily characterize spatial dependence through pairwise correlation or variograms, DS is a multiple-point geostatistical simulation method that directly reproduces complex spatial patterns learned from the TI. This is particularly suitable for the present study because the spatial distribution of geochemical anomalies may exhibit nonlinear, non-Gaussian, and geometrically complex patterns that cannot be adequately characterized by two-point statistics alone. Therefore, DS allows the spatial structures and higher-order patterns present in the TI to be preserved during the reconstruction of the geochemical evidence layer.

Second, we have clarified the construction and role of the TI. The TI used in this study was derived from the available geochemical observations and represents the spatial patterns and local configurations of geochemical element concentrations in the study area. Rather than assuming that the TI follows a stationary Gaussian spatial structure that can be fully described by a variogram, DS searches for similar local patterns in the TI and reproduces these patterns in the simulated realizations. Thus, the purpose of DS is not simply to reproduce two-point spatial correlation, but to retain the higher-order spatial characteristics of the observed geochemical patterns.

Third, we have clarified the purpose of DS in relation to the apparent regular grid shown in Fig. 2. The geochemical exploration dataset was collected at discrete sampling locations with a 1:200,000 scale. The original stream sediment geochemical data was conducted at an average density of one sample per 1 km^2, after which four adjacent samples were composited to represent a 4 km^2 area grid cell, resulting in a regular 2 km × 2 km grid. Although the resulting dataset is displayed as a regular grid, the geochemical information remains spatially sparse relative to the target 500 m × 500 m prediction grid. Therefore, the objective of DS is not conventional down-sampling. Instead, DS is used to reconstruct plausible fine-scale spatial realizations from the relatively coarse-resolution geochemical information while preserving the spatial patterns and variability observed in the TI. Multiple realizations are generated to represent the spatial uncertainty associated with this reconstruction.

We sincerely thank the reviewer for this comment, which helped us clarify both the methodological rationale and the practical role of DS in our workflow.

Section 2.2, how is the negative sample generation parameterized?

Re: We have added a detailed description of the negative sample generation procedure in Section 2.2. Carranza et al. (2008) summarized four criteria for generating negative training samples: (1) negative samples should be selected sufficiently far from any known mineral deposit; (2) the number of negative samples should be equal to the number of positive samples; (3) the locations of negative training samples should be selected randomly; (4) the selected negative training sample locations should adequately represent the range of all variables.

Specially, negative samples are commonly constructed by randomly selecting locations subject to geological constraints, using known occurrences of other mineral deposit types, or selecting drill holes with economically insignificant mineralization, while excluding known mineral deposits and their surrounding areas of potential mineralization influence. In this study, negative samples were randomly selected from areas located at least 10 km from known mineral deposits. The 10 km exclusion distance was determined empirically based on the geological characteristics of the study area and is consistent with the maximum mineralization control distance adopted in this study. This exclusion criterion was applied to minimize the potential influence of known mineralization on the negative samples and thereby reduce the possibility of incorrectly labeling weakly mineralized areas as negative samples.