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.

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.

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.

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.