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Interactive comment on "Semantic Description and Complete Computer Characterization of Structural Geological Models" *by* Xianglin Zhan et al.

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This is an interesting paper that attempts to solve an ongoing issue with 3D geological modelling, namely the underutilisation of non-geometric data in model assembly. I agree with the authors about the importance of taking this approach, but have serious reservations about the semantic schema that is presented in the paper as it stands. I encourage the authors to follow the suggestions for improvements in this comment and look forward to further discussion.

A) My first comment is very general which is that the authors fail to acknowledge the considerable efforts that have been made to address this problem in the past, starting

with commercial packages, such as: i. Earthvision: G. Stirewalt and B. Henderson, 1995, A Three-dimensional Geological Framework Model for Yucca Mountain, Nevada, with Hydrologic Application: Report to Accompany 1995 Model Transfer to the Nuclear Regulatory Commission; ii. Geomodeller: Calcagno, P., Chilès, J.P., Courrioux, G., Guillen, A., Geological modelling from field data and geological knowledge, Part I – Modelling method coupling 3D potential-field interpolation and geological rules, Physics of the Earth and Planetary Interiors (2007), doi:10.1016/j.pepi.2008.06.013 iii. Leapfrog: Cowan et al. 2003, Practical implicit modelling, 5th International Mining Conference, AUSIMM. and more recently iv. GemPy: de la Varga et al., 2018, Geosci. Model Dev. Discuss., https://www.geosci-model-dev.net/12/1/2019/gmd-12-1-2019.html In each of the above systems and indeed most modern implicit modelling codes, some semantic knowledge (fault/stratigraphy relationships, fault-fault relationships, stratigraphic column etc.) is a required input for model construction. Even before that, the Noddy system that I developed (sorry for the self-citation! Jessell, M.W. 1981. An interactive Map Creation Package, Unpublished MSc thesis, Unversity of London; Jessell, 1981, Jessell, M.W., Valenta, R.K., 1996. Structural geophysics: integrated structural and geophysical modelling. In: Declan, G.D.P. (Ed.), Computer Methods in the Geosciences. Pergamon, pp. 303e324) transforms a geological history to a 3D model and requires a semantic description of the geology. The authors' reproduction of Figure 1 from Thiele et al. 2016 is described as showing "only a skeleton of structural models, with little additional structural information". Of course this is true, but the authors ignore figures 6,7,8 & 9 from the same article that direclty refer to the semantic nature of the interfaces.

B) My second major concern for a paper that wants to develop a general framework for geology is that it provides a rather poor description of geology in semantic terms, with some important geological characteristics completely ignored, some semantic ideas that are distinct being grouped together, and others that are hierarchical subsets of each other being placed at the same level.

i) As an example of the incompleteness of the semantic description, I include any attempt to have variable internal properties (grain size, chemistry, intensity of foliation, petrophysics, mineralogy, porosity etc.) within volumes, of variable properties (fault thickness, gold grade…) within surfaces. This also means that considerations of hydrothermal overprinting of lithology and metamorphism are ignored. Since most 3D models are actually built to predict these properties, rather than the bulk lithology or structures, this needs to be acknowledged, but is clearly a challenge for the future.

A second aspect that deserves more consideration is the element of time, which here is treated as a simple rock formation-> tectonic event sequence, when in complexly deformed zones there may be multiple episodes of deformation, so that, as with metamorphism, the concept of volumetric overprinting relationships, which are carefully measured in the field, needs to be a part of the analysis if as the authors state, this is to be a complete semantic schema.

The current schema in Fig 8 also ignores all of the structures that may be found in rocks that require a tensor to describe the (crystallographic preferred orientations, anisotropy of magnetic susceptibility or of seismic anisotropy etc.). Although these may be small-scale properties for these models, they may provide important inputs to the modelling process.

ii) There is considerable confusion in the use of the different geological terms (Fig 7). First of all there is the use of non-existent (I believe) terms such as magma squirting and magma condensation, the inclusion of processes that are certainly outside the scope of a schema that has tectonic process as it top level member, such as bioturbation and arguably the input of extra-terrestrial material. Then there is the use of terms such as cementation and crystallisation, where cementation is a form of crystallisation (perhaps the authors meant to distinguish between crystallisation from a magma and from water?).

The terminology related to deformation is also confused. Some terms refer to stress

(extension/compression) some to strain (stretching), and some to volume strain (compaction). It would be wise to stick to strain-based terminology here I think. (Shortening & extension). I suggest the authors look at Means, W. 1976 Stress & Strain Springer. Compression/Stretching combines stress and strain terms in one box.

Secondly, faulting and folding are both processes that can occur when rock masses are extended or compressed, and are hence logically subsets of those behaviours. As previously mentioned, bioturbation is irrelevant at the scale of modelled described in this paper. It is unclear why the inclusion of magma intrusion as a distinct class, which "affects the existing rock masses" is contrasted with crystallisation which can take place in veins, which does not affect them? There could be a scale issue here, but there is nothing inherently different in terms of this discussion in terms of what the process does to the wall rock in veins vs dykes.

In the text describing Fig 8 (page 11, line 13) the authors state that "It should be noted that in structural geology, there is no corresponding association relation concepts between two disjoint structural elements." This worries me as almost every paper on structural geology describing a field study discusses overprinting relationships and relative or absolute timing, and disjoint structural elements are systematically compared. The age difference between two disjoint plutons is the simplest example, and again reflects how much the concept of time needs to be a major part of the analysis.

On page 12 starting at line 24 a series of different sematic definitions related to faults are presented. Fault contact, stagger relation, limit relation, cut, mutually stagger, trace. A fault contact is a clearly defined geometrical and semantic feature, whereas the others do not refer to process: "A limit relation means when a planar structure grows to another planar structure, the younger surface is terminated by the older surface and the younger one does not pass through the older one." These cannot be at the same semantic level, as one includes a notion of process and the other doesn't.

In Figure 8 I am not sure why Stratified structures are classed with Massive structures, to me these seem quite distinct topological concepts. Conversely I am not sure what the distinction is between stratified structures and planar structures is. I believe stratified structures are a sub-class of planar structures, along with penetrative foliations, linear features distributed on a plane etc. I refer the authors to Hobbs, Means & Williams, 1976, An Outline of Structural geology for a more useful description of these concepts.

In order to resolve these problems I urge the authors to collaborate with a field structural geologist so that they can clarify their schema. In addition, I suggest they base their semantic schema on existing ontologies that do not have these problems. Some suggestions:

I) Boyan Brodaric, 2004, The design of GSC FieldLog: ontology-based software for computer aided geological field mapping, Computers & Geosciences 30, 5–20. II) Babaie et al., 2006. Designing a modular architecture for the structural geology ontology. Geological Society of America Special Paper 397. III) Zhong et al., 2009. J. Zhong, A. Aydina, D.L. McGuinness Ontology of fractures J. Struct. Geol., 31 (3) (2009), pp. 251-259 IV) And the intriguing Zhong et al 2006, The ontology of Structural geology, EOS, AGU Abstract. I do not find the full paper for this, but it may be a mine of useful ideas for the current paper.

C) Section 4.2 and section 5 are poorly presented, with insufficient explanation of where the paper is heading. The list of steps in section 4.1.2, 4.2 and on page 32 are extremely hard to follow in their current form, a flow chart with examples of the steps would be much more useful.

D) The discussion, once the paper acknowledges properly previous work in the field, should show which parts of the work are new, and how they have advanced the field. It is much too short at present, and simply asserting that the new method is more reliable without direct testing against other methods is of no value.

Other minor comments

a) Figure 5 needs to be properly cited, even if though the author allows open reproduction b) Line 10: “Semantic description is the interpretation of an object at the semantic level”. Circular definitions are not very useful! c) The caption for Figure 15 refers to features that are impossible to see in the figures, either through the choice of yellow text, or through the lack of labels (I am not sure). For example: “(c) it can be seen that there are 25 points on the line l40”. I do not see a label l40, and I cannot see which 25 points are referred to? Most of the observations in the caption are not visible in the associated figure. d) Figure 18 and Figure 13 overlap so much that only Figure 13 is needed.

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