Comments to the reviewer #1

Abstract describes the eventual problematic in the study area about the hidrogeological characteristics of the Yucatán peninsula, however, the manuscript does not evaluate this problematic and has as objective a geophysical method that can give information about, but it is not explained how this information can solve the described problematic. This means that the main objective of the manuscript and description included in the abstract is not the own objective of the manuscript itself. The obtained information in the manuscript can collaborate, with the joint analysis of other techniques and methodologies, at the evaluation of the contamination of aquifers in the area, however this subject is not developed in the manuscript. In this sense, abstract does not describe what the manuscript evaluate, and differs from the obtained conclusions obtained in the manuscript, that it is centered in the geophysical data.
Yes, the main objective is the application of a non-usual geophysical tools to solve the sinkhole geometry. If the sinkhole would be the main objective we would rather apply many geophysical techniques as you mentioned. This is an electromagnetic tool that has been used few times for sinkholes and it has used mainly in the aerial way. That is why, we mentioned in the text that aerial EM has even worst resolution (they use 1D inversion and they are far from surface). We found just one paper about EM-LIN application for sinkholes on land, but the authors just put the raw data without any further processing (it is referenced). McNeill popularized this kind of equipment and he developed the 1D inversion. But geology requires 3D inversion for complex problems. We developed the 3D inversion method and it was published in Geophysics in 2012 for a public of mainly geophysicists (it is referenced in the text). In this manuscript, we are trying to popularize this EM tool and the 3D inversion method for non-geophysicists with an application to sinkholes. Along this research, we found that some caves were close to the surface, becoming this, more important by the risk of collapse. We do not know about rock mechanics and how to evaluate such a risk. As geophysicists we see that those caves are close to surface.
If you do not mind, we prefer to keep as the main objective, the application of this geophysical tool for a sinkhole problem.
We did some changes in the abstract. If you do not like, please tell me. My email is mperez@cicese.mx (Marco A. Perez-Flores). I would like to have a more interactive discussion. In such a way I can attend your suggestions faster.

There are sentences that require rephrasing, eg. “In order to guarantee the sustainable use of this groundwater resource knowledge on the hydrogeological characteristics, such as geometry and position, of caverns and sinkholes and the depth of the freshwater/saltwater mixing zone (halocline) is needed.”; or “These authors performed EM-34 measurements but they did no further processing, like perform a geophysical inversion.”
We saw the mistakes and we corrected them on the text. Thanks!!. The complete manuscript was also corrected by an Amglo-speaker.

Authors indicate that they do not show the results from the 2D inversion, something that can be of interest when a new processing approach is presented. However, later when authors explain their 3D approach, they are exhibiting 2D sections (or its integration that can be referred as a 2.5D distribution, see figs. 4 and 5 for example). In this sense, I am not sure about if the 3d analysis described represents a 2D analysis with a perspective view. In data inversion it should be of interest to include all data, it could be that the carried out inversion makes reference to the 2D inversion, because if a real 3D exists, they should present the map view of different depths, or geophysical bodies in 3D fashion.
In this manuscript we are not developing a new geophysical technic or new 3D inversion method. The new 3D inversion method was proved and published in the Geophysics Journal in 2012 (referenced in the text). As a short history, We began by applying 2D inversion and we saw many unreal small features in the 2D conductivity models that we did not like. This happen because data has 3D information and we were using a 2D inversion tool. We decided to do 3D inversion with the equation published in the paper from 2012, but we found that our profiles were done with arbitrary azimuth. So we have to spend some time to solve the problem by mean of a coordinate rotation of the original equations. In such a way, we got a unique equation valid for any angle. It is not a new 3D inversion method, it is the same. We will attach the 2012 paper, where you will see that such equations are there. And that the method was proved with the response of a known underground conductivity model and then recovered it as you said. In this manuscript, it is easy to proof that for azimuths of O^0 or 90^0 degrees to this single equation, you will get the two equations already published in 2012. I explain this manuscript, but

we also did some modifications in text in order to be more clear. In the 2012 paper explained that the quality of the integral can be monitored. Such integral must be unity or one. With this coordinate rotated equation, the integral was already unity as it must be. We think it is not necessary to mention this in the manuscript, because we need to add at least two equations more and more text. But if you consider that it is import. We can do it.

Researchers from other universities are beginning to use these equations (Geophysics, 2012) and this generalized equation will be well accepted by them and others, we hope.

The 3D conductivity model obtained did not show those small features obtained in 2D, meaning that 2D inversion can not lead with the 3D geology complexity. The field data are truly 3D and if we use a 2D inversion tool, the software will show non-real 2D features in order to explain the fully 3D data.

Geophysicists use 1D and 2D inversion when the data is not enough for a 3D inversion, but also when the 3D inversion has not been developed. This is not the case. We did not want to show the 2D conductivity models because we would have to explain a lot, why the 2D and the 3D looks different even that they are very similar. Confusing the readers maybe.

In this manuscript we show cross-sections of the 3D conductivity model. We have found that it is very difficult to show static pictures of any 3D image to somebody. You need to use an interactive software to freely rotate the 3D image or a short video, if not, we could confuse more the reader.

If we put the 2D models in figure 4, we would see discrepancies where the models cross each other. Instead, with the cross-sections of the 3D model, you will not see any discrepancy in such crosses. The reader could be more confident, we think.

We propose to erase any word about 2D inversion in the manuscript. This is a 3D conductivity model only.

In the case of figure 5, I do not find a significative signal that can be related to the underground flow in the area or related to cavities in the underground, in this sense a higher detail and resolution of figures is required in order to evaluate the real meaning of these changes and the potential availability to identify them from the rest of the area (this can be done in a discussion chapter in the manuscript).

Yes, the resolution is not enough or what we would like. This equipment EM-34 uses only 3 separations (source and receiver) those are 10, 20 and 40 m. This is because for every separation the frequency must be different. But we also used VMD and HMD. VMD with 10 m should observe the shallowest targets. That is why we think that some water conduits are shallower than 10 m, but we do not know, the number in meters. HMD with 10 m looks deeper than HMD. We can not improve the resolution.

About other terminological issues, I am not sure if the term "underground rivers" is the most recommended due the characteristics from the area, I mean, that the use of underground rivers can mean many things. Here I suppose that this term make reference to a preferential water flow path through the carbonates by grouts, fractures and caves. This means that there could exist an underground conduit or group of conduits where water flow, more than an underground river itself.

We changed 'underground rivers' by 'underground water conduits. Is it Ok? Sorry, we are geophysicists.

In summary (mainly referring to the conclusions chapter) one of the main highlights from the article is to apply a previous mathematical method for the inversion of 2D profiles, in this case, with arbitrary directions. This is carried out on contrary than the usual parallel and perpendicular profiles net survey. The idea to perform the inversion by homogeneous directions is to simplify the inversion method and to evaluate, in a systematical pattern, the pixel size for the inversion (element or objet defined for the model). I am not sure if the only change in the orientation of the profiles can be performed by the orientation change of the data coordinates In this case what happens is that the data distribution for the inversion does not represent a homogeneous map view distribution, producing that this requires to be analyzed in detail in the discussion chapter.

It is only a 3D conductivity model not 2D. It is usual to do a mesh of many paralleled profiles in order to get a 3D model. Spaces between profile and profile it is so short, that the interpolation of the 3D model it is quite reliable. In this case, because of the jungle, we used the available pathways. In this case, the spaces between one profile and another could be large and the interpolation of the 3D model could be not very confident. We ran a 3D inversion because of the geology complexity, but we did not want to show a resistivity map at different depths, because there are large gaps without information. We are showing the 3D model exactly bellow the profiles. That is more reliable.

I believe that if we want to perform a new approach for data inversion modifying previous usual approaches, it requires comparing, for example, the results of the inversion by means usual and new data distribution. In this case, it has been applied a new mathematical approach without comparing if this results can be of application for the studied case. In this sense, the most recommended approach should be to perform the survey in two different survey grids and to compare results. In the case that the study area does not permit this approach, the mathematical simulation can be an alternative, at least, to evaluate if the approach can be of application in the area. The conclusions, where a integration of data and its application should be included, as stated in the abstract, is not evaluated or developed.

In Perez-Flores et al. (2012), we developed the 3D conductivity inversion and it was tested as you mentioned before. Here, we are using it. But we only did a simple modification to the equations for Horizontal Magnetic Dipoles (HMD). The integral is still unity or very close. That is the quality control parameter. For VMD the equations remain the same.

There it not a discussion chapter, moreover when geophysical results are ambiguous in terms of changes of the properties of the indirect obtained results, and moreover the direct data from the area does not present a clear correspondence with the models, the lack of discussion decrease the interest of the manuscript. In this sense, I am not sure if the geophysical model represent a good approach for the carried out analysis, at least looking at the presented results, when at the same time a change in the processing methodology is presented without to compare with previous methodologies, the results are not unambiguous, the comparison with the known data are not straightforward (or they are not interpreted and discussed in terms of resolution and accuracy).

We cannot improve the resolution as we explained before (VMD with 10 separation looks the shallowest targets). The method is not new in this manuscript, but we are presenting a generalized equation for VMD with profiles at any azimuth. The methodology is correct, we think.

Maybe the interpretation that we are obtaining from the resistivity images are not the better. We have been discussing between us, what more or what less to say. We tried several grids for the 3D model. This is the best. We spent several months running the programs varying the grid, the smoothing, etc. This is the best model that we got.

Attending to the document of answers to previous review, there are some subjects highlighted in the previous review, that has been answered but not included in the manuscript or the recommendations have not been considered.

The use of roof for the thickness of the carbonatic unit over the cavity induces to mistakes, as roof is a surface (later I will enter in this subject that was also highlighted in my previous review).

That is true, Roof is a surface. We are proposing RM (resistive mass). Because this RM is the sum of the dry limestone mass and the air-filling mass in the cave.

In that sense, when we said that RM is thinner than 10 m, means that maybe there is air-filling the cave and the dry limestone mass could be even thinner.

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Dear Natascha Topfer,

I want to move the authors order if possible. I want to switch the Perez-Flores position with my position Ochoa-Tinajero.

I am very busy attending other projects. Dr. Pérez-Flores is presently attending the correspondence with you and doing the changes requested by the reviewer. It is better that he continues as a first and correspondence author.

Thanks in advance.

M.Sc. Luis Ochoa-Tinajero
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