We would like to thank the reviewer for his thorough review, which will help us improving the quality and the scientific soundness of our work.

In the following we reply to the reviewer's points and requests, which are here cited in italics.

I would suggest, however, a better clarification of the newness of the article, since it is not so clear. Is there any step of the analyses that is new in the current study or it is just the first time is applied in this degassing area? All these procedures integrating different tools (DISGAS, TWODEE-2, DIAGNO) were already applied before? This should be better clarified.

We agree with the reviewer that the novelty of this work should be better emphasized. In the reviewed version we will do this by further emphasizing in the abstract, introduction, discussion and conclusion that:

- Thanks to the latest version of VIGIL, we could carry out a more robust hazard assessment without imposing an a priori scenario, considering the natural variability of meteorological conditions and applying the most appropriate transport model (i.e., dense gas flow or dilute gas transport).
- Unlike the original work of Chiodini et al. (2010), who considered a single dense gas flow scenario with very little wind, we explored the effects of the full meteorological variability using the appropriate gas transport model, without imposing a pre-determined scenario. Such an approach allowed us also to capture the seasonal control on the hazard assessment.

Authors estimate probability maps not only for the CO2, but decided also to include the H2S. Was this approach already applied in other sites? Considering that CH4 may also be hazardous in certain concentrations and that the CH4 concentration in the emission is also high (Table 1 from the article), did authors consider to apply the methodology also to the CH4?

We are successfully applying this approach of converting among gas species in other sites of interest. To our knowledge, this modelling approach has never been applied by other authors in other sites.

From Table 1 one can estimate the molar ratio between CH4 and CO2; this is the parameter used by VIGIL to obtain an estimate of the concentration of a secondary gas specie (CH4) starting from the concentration of the main gas specie (in our case, CO2). This molar ratio is lower than the molar ratio between H2S and CO2 that we used in our work to produce the outputs shown in Figure 8. This implies that the predicted CH4 concentrations would be lower than the H2S concentrations of Figure 8a, which represent our worst case scenario since it is a map of H2S concentration at 5% exceedance probability. In the worst case scenario, then, we could expect maximum concentrations of CH4 in the region of few hundreds ppm. It is difficult to find Threshold Exposure Limits (TLV) for CH4, however some sources report a minimum TLV of 1000 ppm over 8 hours by NIOSH (well above what we would expect if we applied CH4 calculations with VIGIL); the second limit generally reported is 500,000 ppm, which would cause asphyxia; in the between there are the concentrations that would cause explosions (50,000 – 150,000) (https://www1.agric.gov.ab.ca/$department/deptdocs.nsf/all/agdex9038/$file/729-2.pdf?OpenElement&fbclid=IwAR1kqzolBzj46xk1fyTuQULkl3dsQiYdDI8LlIcnVLbVhEPQ3

3HIycI7v_c, https://pubs.geoscienceworld.org/eg/article-abstract/22/3/85/138156/Does-methane-pose-significant-health-and-public, https://journals.lww.com/epidem/fulltext/2011/01001/methane_and_natural_gas_exposure_limits.771.aspx). For these reasons, we do not think that it is worth including CH4 in our analysis for the area.

Chiodini et al. (2010) applied the TWODEE-2 code to the same dataset at Mefite. Authors should better discuss what are the major differences obtained between the two different studies especially when DISGAS is applied to similar wind conditions as the ones applied in the previous study.

As already mentioned above, Chiodini et al. (2010) considered a specific dense gas flow scenario with very little wind, whilst we conducted a scenario-independent hazard modelling with considerations on the seasonal control. In the revised version we will clarify this point.

Some more general comments:

Section 1. I suggest authors to review the references used for the CO2 thresholds and exposure time. Authors refer in one case to an Italian reference (Settimo et al., 2022) together with some studies that do not focus on these thresholds but use them based on other literature. I suggest one of the options: or authors include the references and add "and references therein", or, in a better way, use more fundamental and specific literature on these thresholds, impacts and exposure time. Some recommendations are: Blong, 1984; Wong, 1996; IVHHN, NIOSH - https://stacks.cdc.gov/view/cdc/19367 (line 21 – page 1). I also suggest checking the references sequence in the text. Some of them do not appear with the chronological sequence.

In Section 4.1 we indeed used "Settimo et al. (2022) and references therein" but we are happy to use the references you suggested in this section and the Introduction.

Section 2. On the characterization of the area I suggest authors to better characterize the area including mention that the emissions are cold and the fluxes previously estimated by Rogie et al. (2000) and Chiodini et al. (2010). For instance, Rogie et al. (2000) report that CO2 concentrations > 30 vol.% were measured at an height higher than 2 m. This is an interesting aspect to recall in the discussion to compare with the results obtained in the current study.

The cold sources are mentioned in line 36, 374, 411 and in Section 4.1 we specified the temperature used in the modelling of the source. However, we agree with the reviewer that this should be mentioned in this section too, since it deals with the emissions' description. Furthermore, since this is a hazard modelling work, we chose to focus on the outputs at 2 m above the ground, which is the height where human beings breathe. Given the very high concentrations at 2 m, we can safely expect high concentrations also at higher levels as observed by Rogie et al.

Section 4. Tables 2 and 3 in my opinion need to be improved. Another column should be added in both tables to mention the maximum recommended exposure time for each of the thresholds. I would split the effects and the exposure times. Then, the last column would report the "tested exposure time in the current study". Otherwise it seems that humans can be exposed to 10 vol.% during 1 hour (Table 1) and this is not true and could even be *lethal. In fact, in Table 2 authors need to add that in this last threshold (100 000 = 10 vol.%) death can also be one of the effects. Same comment for the 500 ppm associated to the H2S. Still in what concerns Table 2, I suggest authors to use specific literature (NIOSH, Blong, Wong, as mentioned above, and complete the symptoms per threshold considered, since there is lack of symptoms in the table). The IDLH mentioned in the text (line 259, page 10) for the H2S also exists for the CO2 and should be mentioned (and added in the table). I suggest to review this section based on additional literature.*

We agree with the reviewer and we will implement the suggested modifications.

Section 5. The discussion and evaluation of the hazard considering different seasons is a very interesting contribution that should be applied in other degassing areas. Nevertheless, it is important that future studies attempt to couple seasonal degassing maps with the meteorological data associated with the different seasons. Several studies on degassing areas showed that CO2 is usually higher during winter comparing to summer, and for this reason the evaluation of this coupled (and eventually contradictory) effect will be very interesting. Is this the first study that reports these seasonal maps? If yes, this is not mentioned in the abstract and it should.

We will further emphasize these findings in the abstract. Additionally we will further explain that our outputs, specifically the concentration maps, are 24 time-averaged. In our view this is one of the reasons why the effect of winds, which is evident in the seasonal control, dominates over other effects that may dominate in specific time windows of a day. Specifically, the effect of temperature inversion, which can occur under stable conditions especially in winter and enhances the gas concentration at the lowest levels, plays a role during the night and early morning.

*The are **some general technical comments** that I would like to call the attention, namely the need to control all the figures and tables that do not appear correctly in the text. This needs to be carefully checked:*

We will check all the technical points listed below.

- *For instance, in line 29 – page 1, authors refer Figure 1, but this sentence refers to Figure 2b.*
- *Line 78, page 3 – the links to the webpages of NIOSH could be added as references (e.g. NIOSH, 2019), and then the links appear in the references list.*
- *Line 85, page 3 – authors should add the units also for the isotopes in the table.*
- *Figure 1 was redrawn from Chiodini et al. (2010). Looking at the literature, there are several geophysical studies (e.g. https://www.mdpi.com/1424-8220/23/3/1630; https://pubs.geoscienceworld.org/ssa/bssa/article-abstract/113/3/1102/620681/Hydrothermal-Seismic-Tremor-in-a-Wide-Frequency?redirectedFrom=fulltext) that were recently developed and I wonder if they could be used to improve the scheme showed. Authors should check. Anyway, remember that Figures 1 and 2 need to be checked as they are wrongly referenced in the text.*
- *Line 97, page 4 – check the chronological sequence of the references.*
- *Line 204, page 8 – when appears "figure 2" should appear "Figure 2".*
- *Figure 2 – I think Figure 2 could be improved by inserting in Figure 2b a square with the location of Figure 2c.*

- *Line 221, page 9 – check what figure should be mentioned in the text. It does not refer to Figure 1.*
- *Line 245, page 9 – the number of the tables also need to be checked along the text. I believe that authors refer to “Table 2” and not “Table 1” as it is written.*
- *Table 3 – there is a reference missing in the first line.*
- *Line 285, page 13 – I think authors meant “winds blowing” instead of “finds blowing”. Check.*
- *Lines 294 and 295, page 14 – I could not see in the figure the statement “is also not negligible… along the main”. Maybe I misunderstood the sentence, but please check.*
- *Line 307, page 14 – Table 4 instead of Table 3.*
- *Line 318, page 14 –I wonder why authors decided to check the exceedance probability of 16%, and not any other percentage? What was the criteria?*
- *Line 382, page 21 – authors mention that certain concentrations of CO2 may be very dangerous for the human health, but I would even add for the “human life”, as in certain concentrations CO2 is lethal and it was even reported casualties in the area.*
- *Line 404, page 22 – “was evaluated” is repeated in the sentence.*