Comments on “Kinematic properties of regions that can involve persistent contrails” by Sina Maria Hofer and Klaus Martin Gierens (https://doi.org/10.5194/egusphere-2024-3520)

This paper uses two months of data from the German Weather Service (DWD) aviation weather forecasts (WAWFOR) created with ICON during the D-KULT project. Specifically, the manuscript aims to derive the kinematics of the center of probability (COP) of ice supersaturated regions (ISSRs) and the wind speed at these points.

The paper first describes the data set used and then explains how ISSRs are identified and their respective COPs are determined. To monitor the movement and speedy of ISSRs, the change in position at the location of the ISSR is determined by identifying pairs of ISSR between three consecutive times. In addition, the extension (in the horizontal direction) is determined by the number of consecutive model grid points that are flagged for ISSR. The horizontal extent is determined and a covariance matrix is used to calculate the rotation of the ISSR. This information is then used to determine the ISSR kinematics in terms of speed, direction, and rotation, but also in relation to the underlying wind field.

The main idea of the paper, is to infer the advection of ISSR by the wind field, which is much more accurately to predict than the ISSR themselves. This might provide insight into the life time and spreading of contrails. While this idea is of general interest for mitigating contrail formation and spreading, where information about the location of ISSR is needed, there are several major comments that have to be addressed before this paper can be considered for publication in ACP. The specific major and minor comments are listed below.

Major comments:

(1) The entire study is based on two months of data from the D-KULT project. While I understand that dedicated simulations are limited in time, I see major problems with this short time span. Investigating only two months does not even cover a single season of the year, and therefore does not account for changes in global and regional circulation patterns that can occur throughout the year. Even more problematic is the general possibility that these two months and the derived ISSR kinematics may be influenced by an atypical wind field. This would render the investigation and numbers unusable. The authors should at least provide an overview of the general wind pattern in the area (direction, pressure systems) during the two-month period and compare this period with, for example, the 10 year monthly averages for April and May.
While the problem of representation is briefly mentioned in lines 262 to 266, it should be made clear in the introduction and summary.

(2) Follow-up to major comment (1): Even if the two months are representative for an average April and May, these two months, as mentioned in (1), cannot be considered as representative for a longer period of time, nor for the entire Earth. In this respect, the title of the manuscript is too general and promises more than the study can deliver. Therefore, I suggest two options:
(i) The authors explicitly mention the investigated time frame and the specific region in the title, e.g., “Kinematic properties of regions that may involve persistent contrails over the

North Atlantic and Europe during April and May.”
or
(ii) present the two months of data as a test case for their proposed method of inferring ISSR kinematics. This would also require a rewording of the title, e.g., “A proposed method to infer kinematics of ISSR applied to two months of aviation weather forecast data.”
At the very least, it must be clear that the conclusions given in the study are limited to a very short time period and a specific region, and are not as general as the title suggests.

(3) Throughout the manuscript, several statistical parameters are determined, while the information gained from the statistical tests and parameters is not contextualized with existing literature or used later in the text. For example, in lines 177 and following, the Weibull distribution is introduced and used to fit the wind speed distribution. However, nowhere in the text is the Weibull distribution compared to existing literature, nor is it stated what the information gained from the fit can be used for. The authors mention that Dixon and Swift fitted Weibull distributions but what did they to with the information? The authors should continue their discussion of the Weibull distribution and what its purpose is, e.g., where this information can be applied. The wind speed distributions are likely sensitive to season and location.
It is also a general deficiency of the manuscript and not limited to the use of the Weibull distributions that mere numbers and statistics are given, but the discussion and conclusion of the analysis is short and potentially interesting ideas are not followed to the end. Perhaps I missed the implications of several statistics, but then the authors should better clarify their individual intentions behind the given statistics and parameters.

(4) Follow-up to major comment (3): Some of the results are not very surprising, maybe even trivial, as the authors themselves admit, for example in line 189. To make the study more informative and to use the potential of the proposed method, it would be good to actually look at the ambient conditions - wind direction, speed, and temperature - where significant differences between the kinematics of the ISSR (COP) and the wind field appear. Much more can be learned from where and when differences occur than just identifying similarities between COP and the wind field.

(5) The authors explain the identification of ISSR, the COP, and the derived motion of ISSRs. For example, an eastward motion is denoted with an angle of 90°. However, the definition of the wind direction is not clear. In meteorology, wind direction indicates where the wind is coming from. See https://confluence.ecmwf.int/pages/viewpage.action?pageId=133262398 . Considering the investigated area (23.5°W to 62.5°E and 29.5°N to 70.5°N), I assume a primarily westerly wind (wind is coming from the west and moves to the east), meaning a wind direction around 270°. Looking at Figure7(right panel), which has a peak at around 90-100°, one has to assume that the wind direction that is given in the manuscript indicates where the wind is going (wind is going east and defined as 90°). Am I interpreting this correctly? If I am wrong in the assumption, for example because April and May were dominated by easterly winds, then I must apologize. But please check and clarify the definition.

(6) L211-212: If the correlation between the speed of each COP and the wind at the COP is low, does this mean that ISSR simply do not move with the wind, and even move in the opposite direction? The authors write: “But it simply indicates that the speed differences are positive

and negative with similar probability". Later the authors also write in lines 220-221: " This shows that both ISSRs and the wind usually move in very similar directions, that is, the motions are aligned." This is somewhat contradictory.

(7) L211-212: I find it an interesting feature that ISSR and wind are in opposite directions, but it is not discussed. It would be very informative to read what happens in cases where COP and the wind move in opposite directions. What are the dynamics behind this? In such situations, I would expect the largest discrepancies between prediction and observation / reality when ISSR are assumed to be advected with the wind.

Minor comments:

L6: Please clarify what is meant by "**material** ice crystals". Ice crystals are always material objects.

L7: Abbreviate Ice supersaturated regions with "ISSR"?

L8: The use of "their" is not clear in this context. Does it refer to the ice crystals, the ISSR, or the wind field?

L27: "..thermodynamic condition, the so-called Schmidt-Appleman criterion,…" please consider rephrasing this sentence. It raises the impression that Schmidt-Appleman criterion is a thermodynamic condition but it is a combination of several conditions taking into account the temperature and humidity (supersaturation)

L73-74: This is already mentioned in lines 42-43. Please consider removing duplicate information.

L97-98: What do the authors mean by "three candidate partner" since the authors mentioned distances between pairs (two)? Please write more clearly. The authors probably mean the single ISSR appearing in three consecutive time steps?

Subsection 3.3: A separate subsection for calculating wind speed is not needed. It could be combined with Section 3.2. This would also be the part wind direction definition should be added. (see major comment 6).

Section 3.6: The example could already be used during the introduction of the different metrics and parameters. This would make definitions more illustrative and better to understand. The authors might consider this in a revised version of the manuscript.

Figure2: The major and minor axes determined in panels (a) to (c) look rotated by a certain -α. From the text, I would assume that one of the major axes should align closely with the longest extension of the ISSR (blue region).

Figure 3 and 7: The authors might consider adding (a) and (b) to the left and right panels of the plots, respectively.

Figure3 caption: The authors may want to write "standard deviations (dark blue lines)" as it might be confused with the blue bars. The authors may also compare the fit of the Weibull distributions with the measurements by saying "quiet well" and "excellent". Please specify "quiet well" and "excellent".

Figure 3 and Figure 5 could be combined into a single plot by using marginal distribution plots, i.e., plotting Fig3 (left) along the x-axis of Fig5 and Fig3 (right) along the y-axis of Fig5. The same holds for Figures 7 and 8.

Figure4: While I do not think there is much additional information in this plot beyond what is already written in the text (lines 186-192), I suggest limiting the x-range to, for example, an interval of [-60,60]. Then the marginal deviation from 0 ms^{-1} velocity difference might become visible. In addition, the choice of colors should be reconsidered. The red line on light blue is very similar to the dark blue lines. This might be problematic for color blind people people or when printed in b/w.

L172: "…,respectively." is missing at the end of the sentence.

L175: "…,respectively." is missing at the end of the sentence.

L186: What do the authors mean by "real wind speeds"? Is there a difference between wind speed and real wind speed?

L186-190: For a distribution that closely resembles a normal distribution (no skewness) shouldn't the peak of the distribution be close to the mean and median? Is the shift in the peak simply due to the selected bin size of the distribution?

L201: $p< 2.2*10^{-16}$ is this a reasonable number to give? I would assume that this number is already close to numerical accuracy.

L204: "…, but they are real." What is the intention of this phrase? The authors applied to Kolmogorov-Smirnov test to determine whether the distributions are similar. According the authors calculation, the test was negative and the hypothesis of equal distributions was rejected. So the distributions are different, and there is no need to further convince the reader.

L227: Please remove "as stated above" or specify what the authors are referring to.

L227: What is meant by "a real rotation"?

L242-243: The authors only consider for horizontal motion. One reason given is ".. another one can appear in that vicinity, so that this one is interpreted as the actual ISSR". But this incorrect identification could also happen on a vertical level.?

L295: "Pseudo-velocities" in lowercase.

L305: This brings up a new point not really discussed before. Could the authors further elaborate on how they would use their proposed method to validated forecasts? What would the authors compare in case of such a validation?