Reply to Reviewer #1

Thank you for providing critical and constructive comments regarding our manuscript. Following your and the second reviewer's comments, we have revised the manuscript significantly. The introduction has been rewritten completely to provide a more focused and clearer storyline. The results and discussion sections have been separated to improve the clarity, providing the detailed characterisation in the results section, while presenting respective discussion with more elaborate explanations in the discussion section.

Below, we provide answers to each of the reviewer's comments, with the original comments in red, our clarifications and answers in black, and newly added text in blue. Any reference to lines in our answers is given with respect to the original manuscript.

Major Comments:

1. The paper claims that a lack of correlation between CWC and downdraft intensity contradicts the role of condensate loading. This conclusion is not justified. Vertical velocity is influenced by multiple, competing factors, including pressure perturbations, phase changes, mixing, etc, and may not exhibit straightforward relationships with single microphysical variables. Grant et al. (2022) is cited in the manuscript to justify this hypothesis; however, the cited paper refers to the relationship between vertical velocity and the rate of condensate production, not directly to CWC, and this relationship was only shown for updrafts. Adiabatic compression during descent decreases a parcel's supersaturation, eventually leading to partial or total evaporation or sublimation of the condensate. This feedback can cause CWC to decrease as vertical velocity becomes more negative, making the expectation of a simple positive correlation between CWC and |w| in downdrafts, as stated in the manuscript, at least questionable.

   We thank the reviewer for pointing this out.

   The idea to examine the CWC was to see its effect on modulating the dynamical and thermodynamic responses in updrafts and downdrafts. This was motivated by the discussions from past literature (e.g., Knupp and Cotton, 1985) on downdraft initiation and maintenance in the clouds. According to them, hydrometeor loading and latent cooling are fundamental to the formation and maintenance of the downdrafts. Since CWC is the amount of cloud water present in the air, it represents the magnitude of hydrometeor loading.

   To give a clear context for the comparison with Grant et al. (2022), we have added the following information to line 240

   They show that the net effects of all microphysical processes, such as condensation, evaporation, deposition, sublimation, and cloud droplet activation, vary linearly with updraft velocity. However, information on the rate of condensate converted from water vapour is not available in the aircraft data used in this study, as information on the timely evolution (history) of the measured variables is not available.

We have added an explanation of the interplay between CWC and vertical velocity in the downdrafts to line 243

The adiabatic compression and warming in downdrafts can determine the amount of CWC. This reduces the relative humidity of the downdraft parcel and, when it eventually reaches sub-saturation, reduces CWC by evaporation/sublimation. Furthermore, stronger downdrafts may reduce CWC faster due to increased adiabatic compression and consequent warming.

Our findings challenge the 'classical' view of downdraft initiation and maintenance, by being subsaturated and driven by condensate loading, indicating that this view may be incomplete. We elaborate more on the classical versus new hypotheses in the reply to reviewer #2, comment 3. Therefore, we believe that our results can be useful for evaluating model performance and interpreting model results. Furthermore, it can help inform the flight strategies of future airborne field campaigns.

To provide more clarity to our study, we have split up the results and discussion. This gives us a self-contained results section that provides a thorough analysis of the unique observations, including their statistics. The discussion section here would be more specific to incorporating plausible physical explanations.

2. The interpretation that negative vertical velocities in ice-supersaturated air masses contradict the effects of sublimation/evaporation is an oversimplification. Supersaturation at the time of observation does not preclude earlier evaporation/sublimation that may have initiated the downdraft. Vertical velocity at a point reflects accumulated forcing along a parcel's trajectory, not only the local, instantaneous forcing. Without trajectory or time-resolved data, causal conclusions about downdraft drivers are not warranted. The authors acknowledge the possibility that the downdraft was driven by evaporation or sublimation prior to the measurement (lines 263-264), but still arrive at the opposite conclusion.

We agree that the supersaturation at the time of measurement does not preclude the earlier sub-saturation of the parcel. However, it points out a scenario where the evaporation no longer maintains the downdrafts by latent cooling. Moreover, if a downdraft parcel is supersaturated at the time of measurement, we think it is highly unlikely to be subsaturated at a previous time. This is because, during its descent, an initially subsaturated parcel would increase its temperature, thus maintaining its subsaturated state. Evaporation or sublimation of cloud particles in such a scenario would, at most, lead to a saturated state of the air parcel, but not to a supersaturated state. Additionally, the adiabatic compression decreases the supersaturation of the downdraft parcel, as mentioned in the reviewer's first comment. An exception to this is if there is an additional influx of moisture, e.g. through mixing between a neighbouring supersaturated updraft and the downdraft.

We have replaced lines 258 – 263 in the manuscript with the following lines:

Supersaturated regions of downdrafts are unlikely to be subsaturated at a previous time. If they were subsaturated before the time of measurement, this state would have been retained due to the increase in temperature and a subsequent decrease in relative humidity and accordingly, cloud water content within the downdraft as it descends to lower altitudes. An exception is if there is an additional moisture supply (e.g., through mixing) from neighbouring regions, such as supersaturated updrafts, which can bring the downdraft regions to a saturated or supersaturated state.

Although there are studies using numerical models that have pointed out the existence of supersaturation even in downdrafts (D'Alessandro et al., 2017), a concise explanation is lacking. Furthermore, a closer evaluation of the supersaturated points shown in Figure 5 in the manuscript revealed that the supersaturation is part of several downdrafts. Thus, these features are not anecdotal and require more detailed research.

In the manuscript line 273, we have added

The measurements show that the supersaturated points in Figure 5 originate from several downdrafts from different flights. Although there are studies using numerical models that have pointed out the existence of supersaturation in downdrafts in ice clouds (D'Alessandro, J. J., et al., 2017), a concise explanation is lacking.

We provide a potential explanation for the existence of supersaturated downdrafts in the answer to the following point.

3. The suggestion that "large eddies" explain the similarity in particle size distributions between strong up- and downdrafts is vague and potentially inconsistent with the data. If the strongest drafts are most often relatively narrow, as indicated in the manuscript, then the mixing eddies that connect them should be small as well. In fact, larger eddies would typically imply longer times spent within either updrafts or downdrafts, thus enhancing, rather than reducing, differences in particle characteristics compared to smaller eddies with shorter updraft and downdraft segments. The manuscript would benefit from a clearer definition of eddy scale and a more explicit explanation of the proposed mixing mechanism.

Using an undefined length scale for the eddies here can be misleading. Therefore, we will use "eddies" (without any length scale) throughout the manuscript (lines 16, 330, 360), and add a more elaborate explanation in the discussion of these.

As the reviewer pointed out, the strongest drafts are narrower; hence, the eddy associated with them would also be smaller. However, quantifying the eddy scale from auto-correlation or similar methods is difficult due to the high aircraft speed (~ 150 m/s) and the accordingly short time spent in smaller drafts.

In the manuscript we have added in section 4.3 (line 333):

Due to the high aircraft speed (~150 m/s), resulting in short times spent in the smaller drafts, it is not possible to calculate the actual eddy scales using methods like auto-correlation, which require data for a significant period inside the drafts.

The proposed mixing mechanism involves a simple transfer of cloud particles between updrafts and downdrafts, as this "sharing" could result in similar PSDs. To clarify further, we analysed the updraft-downdraft structures during different flights and their associated PSDs. Figure R1 shows two such cases and the average PSDs related to the updrafts, downdrafts, and the area outside the draft structure. This shows that the PSDs of updrafts and downdrafts are comparable, while one could have expected differences in the PSDs from the updrafts (coming from an altitude below) and downdrafts (coming from an altitude above), c.f. Figure 7 of the original manuscript which shows the change in size distribution with altitude. On the other hand, the PSD of regions outside the draft structure differs and has lower concentrations. This could be indicative of a direct link between the updraft and downdraft that are located next to each other, as they show similarity not only

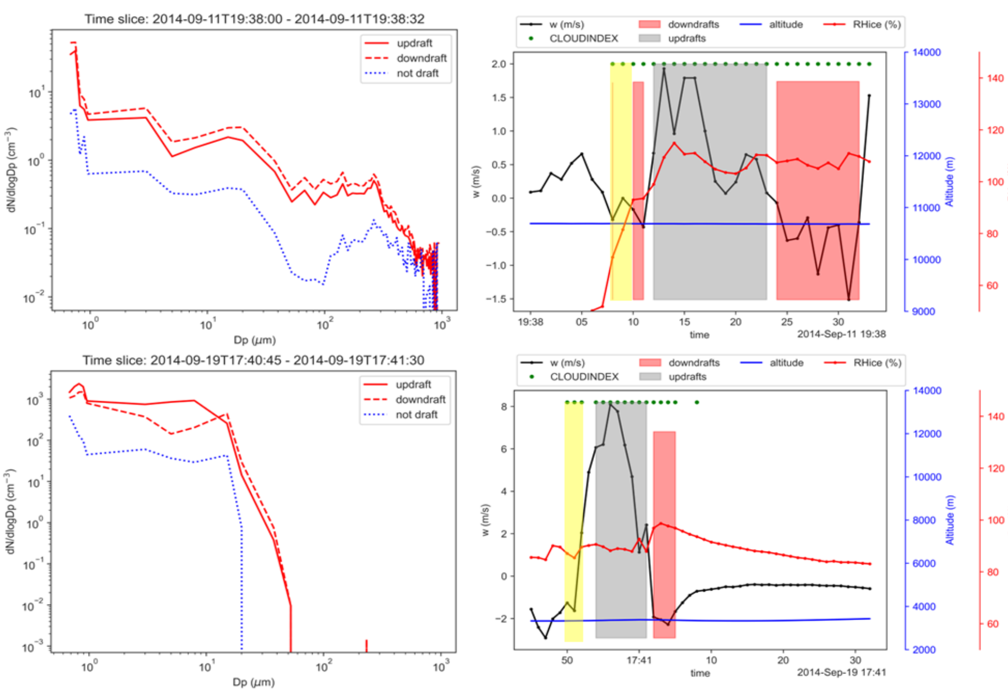


Figure R1: Particle Size distribution and time series of w, altitude, and RH of the corresponding flight segments. Similarities in updraft (grey shading) and downdraft (red shading) PSDs are visible, while those from outside the draft structure (yellow shading on the right) exhibit lower number concentrations.

in the PSDs but also in e.g. relative humidity as shown here.

4. While the title and conclusions emphasize downdrafts, the figures and analyses give comparable attention to updrafts. The manuscript might be better positioned as a study of upper-level cloud properties, particularly of anvil regions, focusing on microphysical

structure and variability, rather than attempting to infer cloud dynamics from limited information.

While the original aim of the study was to elucidate the downdrafts, we agree, that in the manuscript as provided, the title might be misleading in that regard. Therefore, we chose to revise the title to: *Upper-level characteristics of updrafts and downdrafts in tropical deep convective clouds.*

We also paid attention to this comment while rewriting the introduction.

5. The manuscript would benefit from thorough professional proofreading. There are frequent issues with article usage, and sentence structure overall, as well as redundant phrasing, which at times reduce the clarity of the scientific argument.
   Along with the significant revision, we have carefully checked and corrected the manuscript for grammatical errors and redundant phrasing.

Minor Comments:

The introduction is overly long and lacks a clear narrative structure. It moves back and forth between studies without establishing a coherent line of reasoning. In several places, relatively recent studies are cited to explain long-established mechanisms, which can be misleading. A more concise and focused literature review is recommended.
We completely rewrote the introduction in order to provide a clearer storyline, as also stated at the beginning of our reply.

Figures 1 and 2: It is unclear whether these show one-dimensional PDFs at each height or joint PDFs as a function of height and draft diameter/mass flux. If they are 1D slices, please show the number of observations per height bin.
The PDFs shown in Fig. 1 and Fig. 2 represent joint PDFs in height and draft diameter/mass flux. The density values are calculated based on the bin width of both variables in x and y axes. We have clarified this in the text and the figure caption in the revised manuscript, accordingly.

Confidence intervals or error bars should also be added to the mean and percentile curves in panels b.

We thank the reviewer for the suggestion. We have added the confidence interval for mean and percentile curves in panel b of Figures 1 and 2. Additionally, we have modified the number of drafts in Figure 1b to draft fraction, as suggested by the editor.

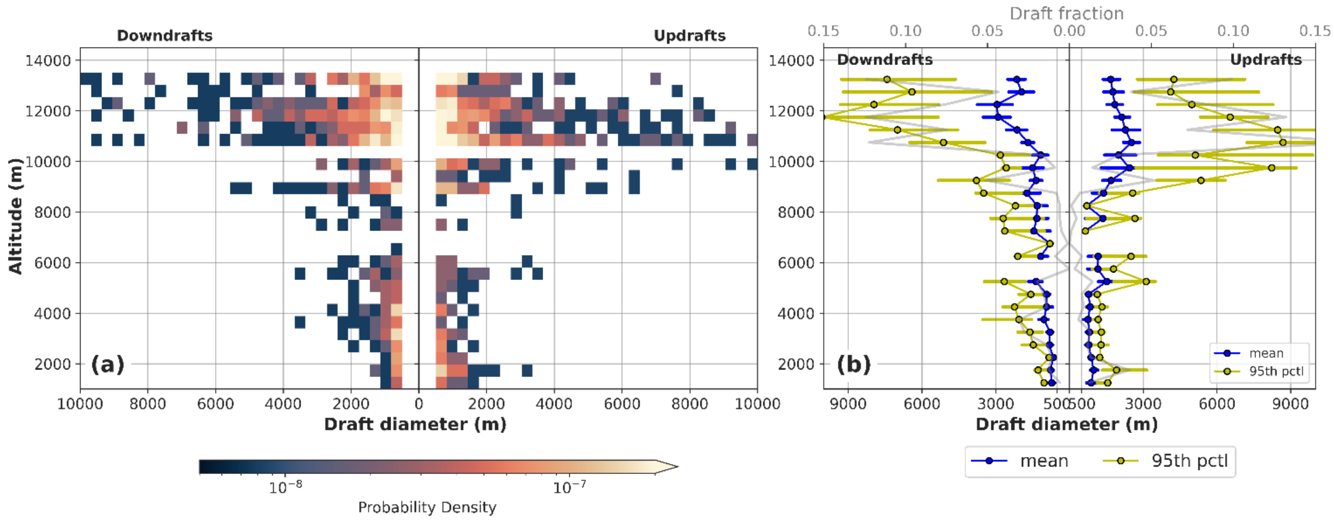


Figure 1 (revised) : Altitude-wise draft diameter statistics of all in-cloud drafts. (a) Joint Probability Density Function of Draft diameter and altitude (b) mean (blue) and 95th percentile (yellow) values of diameter of drafs. 90% confidence intervals for mean and percentile curves are indicated by the error bars.

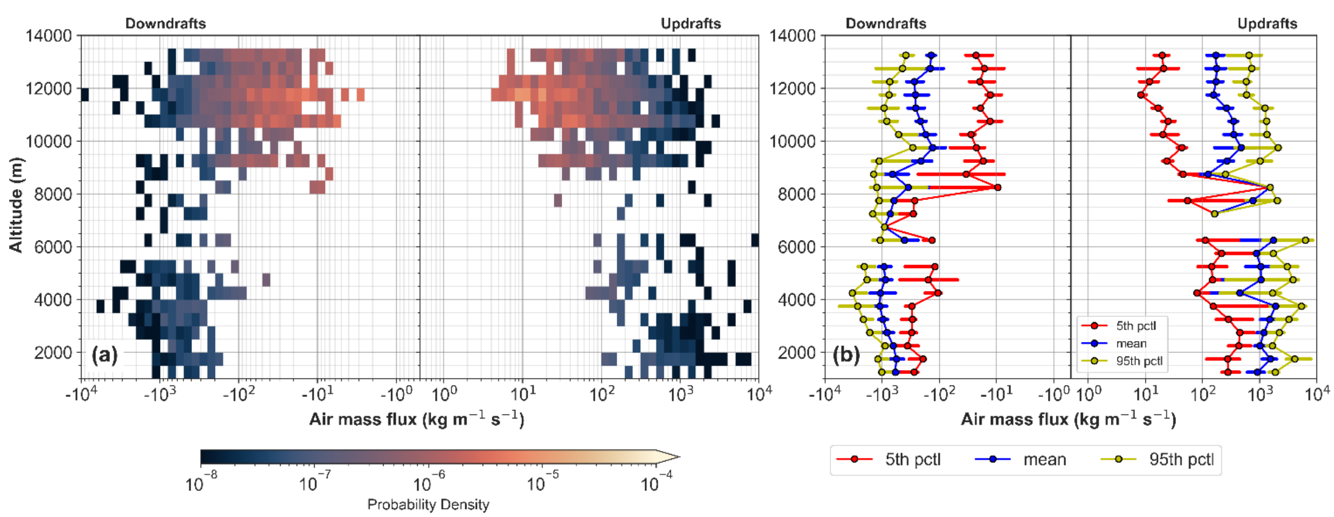


Figure 2 (revised) : Altitude wise air mass flux statistics of all in-cloud drafts (a) Joint Probability Density Function of Air mass flux and altitude (b) mean (blue), 5th percentile (red) and 95th percentile (yellow) of air mass flux. 90% confidence intervals for mean and percentile curves are indicated by the error bars.

Figure 5: Consider using a heatmap or 2D histogram to show point density more clearly. The current scatterplots suffer from significant overlap of data points, making it difficult to interpret the underlying distribution.

We have revised the Fig. 5 from scatter plot to a 2D histogram to make it more comprehensive.

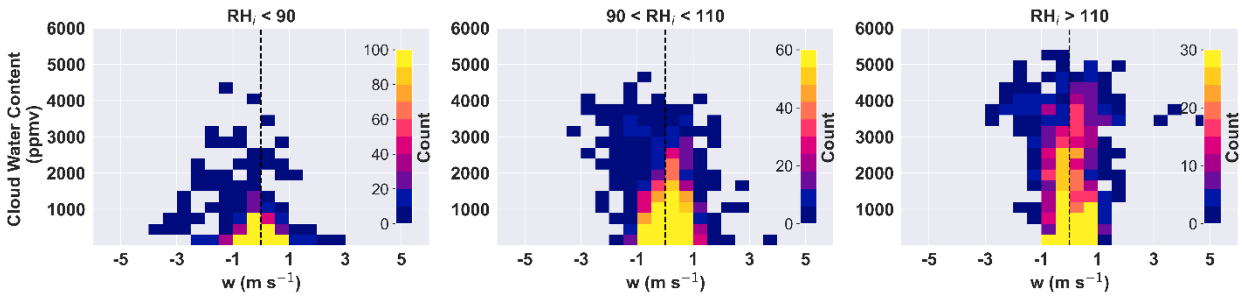
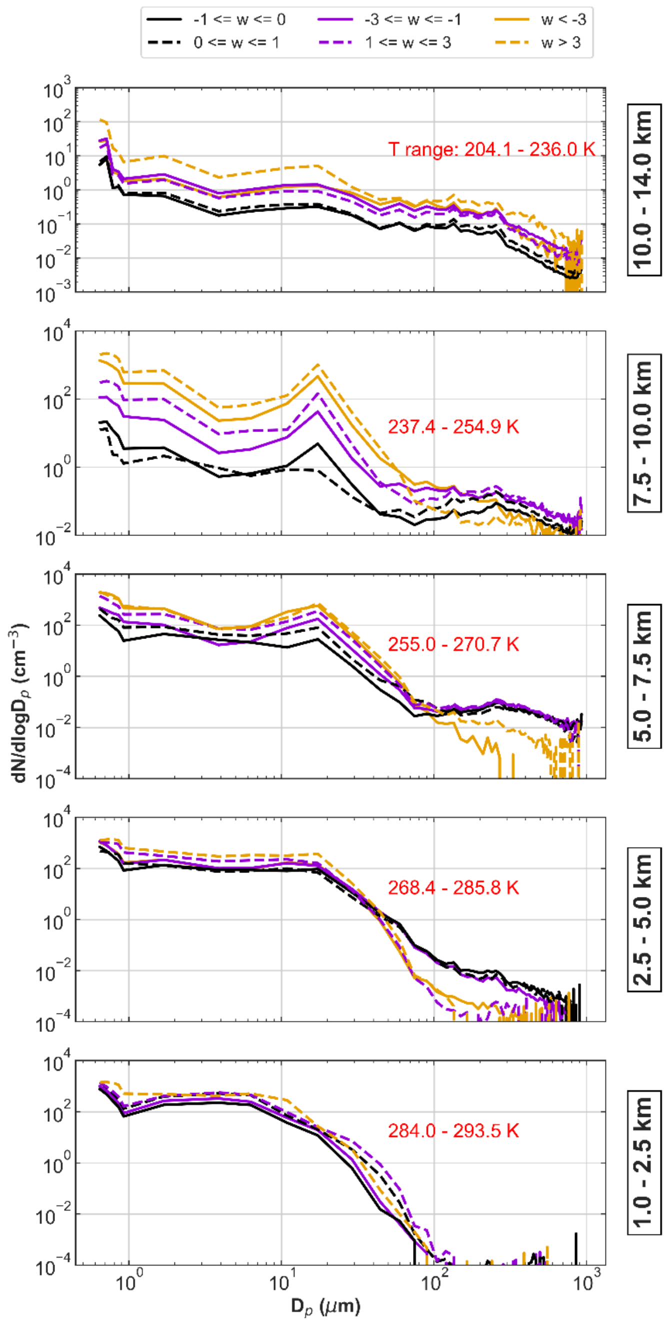


Figure 5 (revised) : Histogram of Cloud water content (ppmv) versus vertical velocity (m/s) for upper-levels (10-14 km) in regimes based on RH_{ice}. (a) Subsaturated (RH_{ice} < 90%), (b) Transition/Intermediate (90% < RH_{ice} < 110%), and (c) Supersaturated (RH_{ice} > 110%).

Figure 7: The green and blue shades are hard to distinguish, please choose more contrasting colors.

We have revised the colors in Fig. 7 to improve the readability also keeping in mind colour vision impairments.



6. how do varying sample sizes in each vertical velocity and height bin affect the calculated distributions? Are the results statistically robust across all w-z bins? Some quantification of uncertainty or sampling error would be helpful.

Fig. R2, shows the number of observations used in calculating PSD at different altitudes and vertical velocity bins. The number of observations for the strongest vertical motions is generally lower. This is expected, as it is difficult to sample the strongest parts of the convection with aircraft.

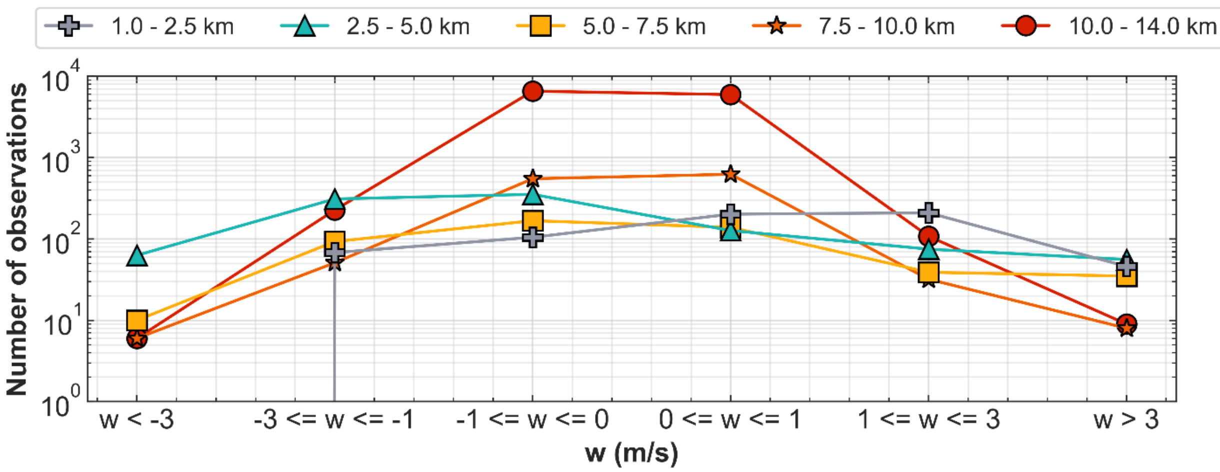


Figure R2: Number of observations in each vertical velocity bin at different altitudes.

References

D'Alessandro, J. J., Diao, M., Wu, C., Liu, X., Chen, M., Morrison, H., Eidhammer, T., Jensen, J. B., Bansemer, A., Zondlo, M. A., & DiGangi, J. P. (2017). Dynamical conditions of ice supersaturation and ice nucleation in convective systems: A comparative analysis between in situ aircraft observations and WRF simulations. *Journal of Geophysical Research: Atmospheres*, *122*(5), 2844–2866. https://doi.org/10.1002/2016JD025994

Knupp, K. R., & Cotton, W. R. (1985). Convective cloud downdraft structure: An interpretive survey. In *Reviews of Geophysics* (Vol. 23, Issue 2, pp. 183–215). https://doi.org/10.1029/RG023i002p00183