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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Lagrangian matches between observations from aircraft, lidar and radar in an orographic warm conveyor belt" *by* Maxi Boettcher et al.

Anonymous Referee #1

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The authors present the results of a unique and ambitious observational experiment aiming to sample air on its path within a warm conveyor belt airmass several times over the timescale of 1-2 days that the WCB exists. Although Lagrangian experiments have been conducted before, this is the first time to my knowledge that a deliberate tracer release has been used to test beyond doubt whether or not the same air is intercepted on later occasions over a day later in a WCB. In contrast with ground-based tracer release (such as ETEX in 1994) where tracer measurements later were sparse because the network of measurement sites were in the BL, but most of the tracer left the BL, the tracer release in T-NAWDEX-Falcon extended throughout the depth of the boundary layer (from a light aircraft) and the interception was made in the

upper troposphere using an aircraft directed to forecast interception locations. In the ITCT-Lagrangian 2004 experiment strong evidence that multiple air mass interceptions were made across the North Atlantic was established, but these were not verified using deliberate tracer release and so some degree of uncertainty remains.

The tracer interceptions occurred at times along the two downstream flights consistent with the calculated trajectories from the release flight track. An ensemble of analyses (from the ECMWF EDA) was used to account for uncertainty in the resolved wind fields used to calculate the trajectories. A complexity was that the calculated WCB trajectories from the small release area grouped into two coherent branches crossing the Alps in different locations, but the tracer measurements are consistent with the existence and path of these two branches. The difference in trajectories relates primarily to the altitude of release. Although the authors appear disappointed in conclusion that the WCB air masses tagged with tracer release were intercepted on the edges, I think it is remarkable that it was achieved at all in such a complex flow with multiple branches over a major mountain range with active precipitation and strong vertical motion. To my mind, it verifies that the horizontal paths and even vertical motion of trajectories calculated from analyses must have a close resemblance to the actual path of air. However, Fig.9b illustrates how air from a relatively small volume is strung out along a very long band in the WCB (in this case spanning across Germany from SW to NE). Since there must be very substantial dilution of the tracer through mixing in this environment with strong shear dispersion, it is also impressive that the detection limit is so low that the tracer can be measured and unambiguously attributed to the release.

The authors use the Lagrangian matches with air sampled above two ground-based profiling sites to examine the time history of water in all its phases along the WCB. This is the second major novel part of the investigation. It is found that water vapour is over-estimated in analyses within the BL at the origin of the WCB. However, the sum of simulated ice and liquid water content is consistent with observations on the flight track.

I recommend publication subject to minor revisions which clarify the Lagrangian connections on all the figures. Also, in the summary the authors refer to the difficulty with planning the experiment so that the tagged WCB air could be intercepted. A major uncertainty (I suspect the greatest uncertainty) relates to the use of forecast trajectories to direct the aircraft. The importance of this uncertainty could be estimated by showing the forecast trajectories from the release flight track (using the same lead time for forecast winds as used in conducting the experiment) and comparing them with trajectories calculated using analyses (as shown in Fig. 9b and 10). The consistency between the analysed Lagrangian match trajectories and tagged tracer measurements is very impressive – all the more so if the paths of forecast trajectories turn out to be less consistent with the measurements.

Specific comments and revisions

Section 1: Should be more precise on the novel aspects of this experiment. For example, tracer release experiments have been attempted over long-range (>1000 km) using release and a network of measurements within the boundary layer (e.g., ETEX in Van Dop et al, 1998, Atm. Env., 32, 4089-4094, doi.org/10.1016/S1352-2310(98)00248-9.). This section misses references to this work. However, I believe the T-NAWDEX-Falcon experiment is the first time that release and interception have been attempted using aircraft with a long time, distance and altitude difference between them.

l.47: Similarly, it is stated that “this is the first study that describes Lagrangian matches between humidity measurements in a WCB”. However, this was done in the ITCT-Lagrangian experiment in terms of specific humidity (and theta-e) only, but not in terms of liquid and ice cloud condensate and without verification from deliberate tracer tagging. There are also quasi-Lagrangian experiments that have attempted to follow the evolution of clouds over shorter range (using airborne cloud microphysics measurements, e.g., ASTEX). Finally, there are aircraft experiments that have examined transport within WCBs using trace chemical measurements, but without the benefit of measurements in a Lagrangian frame. For example, Bethan et al, 1998, J. Geo-

phys. Res., 1031(D11), 13413-13434, doi: 10.1029/98JD00535. A rather similar case in terms of a WCB running across France, Switzerland and Germany was examined in the EXPORT experiment (Purvis et al, 2003, J. Geophys. Res., 108(D7), 4224, doi:10.1029/2002JD002521). Some reference to these observational studies is required and their limitations compared to your approach in the T-NAWDEX-Falcon experiment.

Figures 4 and 5. Labelling the Lagrangian matches on figures. Although you do not introduce the notation until Section 4.2, it would be very useful to label the locations of matches T1 and T2 on these plots (especially the top panels) and then explain the triple “Lagrangian matches” later.

Figures 4 and 5. Relative error in Qv is not defined. I would have assumed 100*(Q_EDA-Qv)/Qv. However, in both figures it seems to vary about 100% and the 100% line is marked. So is the quantity shown actually 100*Q_EDA/Qv ?

l.446: The physical picture associated with the radar section in Fig.8b is not clear to me. You point out the WCB trajectories (red dots) in the western half (west of 8.8E) and the region with less precip and non-WCB trajectories (blue dots) in the middle. However, why is there this region with less precipitation and what is the heavier precipitation east of 9.0E associated with? This is no longer the WCB air mass? Can you explain why these structures are there?

Fig. 8c: Many of the black contours in this figure stop in the middle. Why is that? I don’t understand what they represent.

Figures 9 and 10: The 600 hPa ascent criterion used to label trajectories as WCB trajectories is having rather a large influence here, although it is arbitrary. You can see from the lower panel of Fig.9a that all forward trajs from the release track ascend a long way, but the majority less than 600 hPa. However, these ones start from a higher altitude since the release track goes up to 750 hPa. Indeed, it looks as though all the trajectories from this release track reach a pressure level of about 300 hPa and the variation is Dp is mainly to do with the altitude of the release aircraft. Therefore it seems likely that the whole release track is within the WCB airmass and is all destined to reach a similar outflow level in 48 hours time. So the "WCB probabilities" shown in the Fig.10 cross-sections must naturally only identify the upper flank of the WCB since the ascent criterion is so strong. The fact that the "tracer probability" is high in gaps between the WCB probabilities is not especially relevant other than indicating that the tracer was released into air that travelled beneath the upper flank of the WCB (and therefore presumably nearer the middle of the air mass).

l.530: "technical issue concerning the manual time adjustment of the device cannot be completely excluded". The issue is not explained. Do you mean to say that the position of the samples on the time axis in Fig.10a is uncertain? If so, by how much? It looks to me that it cannot be much since the tracer is detected in the Mediterranean WCB airmass on the aircraft ascent and descent and also the first detection at 400 hPa and above coincides with Lagrangian match T1 and entry into the upper flank of the WCB. Also, the detection on Flight IOP2c is coincident with trajectory match T2. Surely this cannot be chance?

Fig.10: It would be good to label points T1 and T2 on the cross-sections to help connections back to earlier figures.

Fig.10 interpretation: After 08:54 (Lag match T1) the aircraft flew in the upper flanks of the WCB and back trajectories from the flight track went back to the Atlantic (Fig.6a). Despite this origin, tracer was detected. However, forward trajectories from the tracer release (Fig.9b) follow almost the same horizontal path over Germany just beneath the trajectories of Atlantic origin (Fig. 10a). This behaviour is seen clearly in Fig.2b where all "WCB trajectories" are shown. There seem to be two possible explanations that are not mutually exclusive:

A) The trajectories are calculated following the resolved, laminar flow represented by the analyses and the outflow layers are shallow with trajectories from different origins

coming very close. Vertical mixing by sub-grid scale motions would be expected and this maybe responsible for mixing the tracer upwards into the upper flank of the WCB (where the aircraft interception is).

B) Net ascent is under-estimated by the trajectory calculation using the analyses and the air of Med origin reaches a slightly higher level where the aircraft was flying.

Note that Lagrangian trajectory match T2 has an excellent match with the one elevated tracer sample on IOP2c. A similar vertical mixing argument to (A) was presented in Purvis et al (2003) to explain the measurements of short-lived hydrocarbons above the upper flank of the WCB calculated using trajectories. In that summer case, embedded convection in the WCB was important, giving rapid vertical mixing, while in your case the radar observations convincingly demonstrate that this WCB was not convective and vertical mixing would be expected to be slower.

This merits discussion in the conclusions. Given the very large shear dispersion of the tracer gas along the WCB (Fig.9b) and turbulent vertical mixing, it is very strong evidence that the trajectory calculations are a good representation of transport in the atmosphere since tracer was detected in the Med WCB on IOP2b and at T2 in IOP2c and was also detected in the upper flank of the WCB near T1 on IOP2b which must have been at the leading edge of the advancing tracer. One question that is not addressed is the dilution of the tracer. The release amount must be known (in kg) and it was distributed along a track (evenly?). It is detected with mixing ratio of the order $\sim$100 ppqv. So it must be possible to estimate approximately the volume of air containing the tracer (at 0900 15 Oct) and what this implies for the average depth of the tracer layer if the horizontal extent is given by the black dots in Fig.9b. Is this estimate consistent with the vertical range of the blue tracer probability in Fig.10? I think this would be important to deduce if mixing and explanation (A) above can account for the observations. It would be fascinating to know this average tracer depth estimate.

l.570: As argued above, the fact that the tracer probability maximum lies below the

“WCB probability” maximum is to be expected given the high threshold used on trajectory ascent to define WCB trajectories. So this does not indicate a failure of the experimental methodology. The tracer is in the WCB.

l.571: In the forecast methodology used for targeting the WCB an ascent criterion was used to isolate a subset of WCB trajectories. So, in order to distinguish the factors resulting in the greatest uncertainties it would be necessary to examine the forecast trajectories. I suggest you calculate forward trajectories from the release flight track using forecast winds (with the lead time used at the time) and compare the results with Fig.9b and Fig.10. Was the Falcon flight track above the maximum tracer probability obtained from forecasts (as it is using analyses)? Is the mismatch associated with forecasting the winds?

Technical corrections

Title: I am not sure that “orographic warm conveyor belt” is accepted terminology. I suggest, “Lagrangian matches between observations from aircraft, lidar and radar in a warm conveyor belt crossing orography”

l.6: “wind fields of the ECMWF ensemble data assimilation system were used” is not specific enough. I suggest “an ensemble of wind fields from the global analyses produced by the ECMWF Ensemble Data Assimilation (EDA) system”.

Fig.2: The panels are small with a lot of white space around them. I looks like 2x2 panels should work but please expand figure panels to fill the page column width.

Fig.3: There are a lot of details in the panels, but much too small to see (especially panel b with the flight tracks overlain). I am not convinced that panels a and c are needed. I think it would be better to present only panel b, much larger with key locations of ground stations and flight tracks marked.

l.407: “ascended much further WEST compared to the rest of the WCB”?

l.428: Correction: Should be referring to Fig. 8a (not Fig. 7a).

l.431: the yellow hatching is on Fig. 8a.

l.438 and Fig.8b: The red asterisk associated with T1 is very hard to spot. Please label this “T1” within the figure panel. Similarly, label the red asterisk associated with T2 in Fig. 8c.

l.456: Trajectory crosses Montpellier (LEFT grey bar in the middle panel of Fig. 7b).

Fig.7: The colours used for IWC and RWC are both blue and similar. They can be distinguished in the cross-sections but it is hard to tell them apart in the Qc graphs. Perhaps change one to a more distinct colour?

l.512: “In the evening of 14 October”

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