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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An overview of the HIBISCUS campaign" *by* J.-P. Pommereau et al.

Anonymous Referee #1

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(Citation from the paper is given in italic). The present paper aims at providing an overview on the HIBISCUS campaign held in South Brazil during early 2003 and 2004. Such a framework paper is certainly necessary to set the stage for a series of papers, with the intention to be published in a follow-on special edition of a scientific journal. In that respect the listing of participating instruments and involved methods appears to be valuable and complete but to a certain degree exhausting (in particular with respect to instruments&methods of which results are not really contributing to the likely objectives of the HIBISCUS campaign). Major concern, however comes with the second and more important part for this overview paper, i.e. (a) the discussion of the campaign objectives (section 2), (b) the stage setting to locate the probed air masses encountered in the various climate zones, and (b) the internal consistency of the results&conclusions (see below). The present version of the paper does not yet meet these issues satisfactorily. The latter criticism is most crucial since one objective of the HIBISCUS campaign was to unravel the relative importance of various mechanisms leading to irreversible transport of tropospheric air masses across the subtropical and tropical tropopause. The paper as it stands appears to be written in a great hurry with little time spent on internal discussions with the coauthors on the papers outline&contents (see the section minor comments below). This conclusion is well documented by the 'copy and paste' style of the paper, which is e.g., reflected in the variable English in which the various subsections are written and further, by the communication I had in the meantime with some co-authors. The paper also misses the necessary care in technical details to be accepted as it stands (see the section minor comments below). Therefore I only can recommend the paper for publication if the deficits mentioned above are completely removed.

Major comments:

The major concerns are twofold:

(1) Stage setting (sub-tropics/extratropics vs tropics); for an orientation see the study of Holton et al., 1995): In past studies different mechanisms have been suggested for the transport of air masses from the troposphere into the stratosphere (e.g., (a) ascent within the Brewer-Dobson circulation (Brewer, 1949) (b) convective overshooting (e.g., Danielson, 1982, Dessler, 2006, Hanisco et al., 2007), (c) meridional mixing across the subtropical jet of TTL air into the extra-topical lowermost stratosphere (e.g., Hoskins, 1991; Levine et al., 2007), or (d) more modern variants of some of these hypothesis, i.e. ventilation of the TTL by deep convection and further slow ascent above the level of zero heating (e.g., Sherwood and Dessler, 2000) or by gravity waves et cetera ..). In this context most important is that the various transport processes may show strong latitudinal (climate zone) dependencies. Therefore, when these hypothesis are going to be tested in field experiments as e.g. during HIBISCUS, it appears necessary to locate properly the individual soundings with respect to the different climate zones (or dominant atmospheric transport regimes) encountered. The need for this stage setting is particularly evident (and difficult) for the HIBISCUS activities, since depending on the

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location of the SACZ, some of the balloon soundings were obviously conducted north of the SACZ, i.e. (a) within in the tropics with a chance of having probed the TTL, (b) south of it, i.e. in the extra-tropics, where certainly the subtropical lowermost stratosphere was probed, or (c) with the SACZ overhead i.e., right within the boundary between the tropics and subtropics, where eventually meridional mixing of UT/LS air due to the action of the subtropical jet was encountered. How necessary this self-location is and how well this can be done, has recently been demonstrated, for example, for data gained during the TROCCINOX campaign (which was somehow related to the HIBISCUS activities) (e.g., Huntrieser et al. 2007, ACPD, 2561). Such an attempt is not at all made in the present paper (e.g., by attaching corresponding climate zone labels to the individual balloon flights as given in the Table 1 - 5), with the consequence that the reader is totally left alone to decide what observations are made in what climate regimes, and what the possible conclusions are. In order to further demonstrate this problem, some citations from the paper are added:

(a) Abstract: But the project also provides clear indications of strong episodic up draught of cold air, short-lived tracers, low ozone, humidity and ice particles across the lapse rate tropopause at about 15 km, up to 18 or 19 km at 420-440K potential temperature. (b) Section 6.2. The amplitude and the altitude of the cooling of the TTL (was the TTL always probed ?,) by overshooting of adiabatically cooled air over land convective systems has been studied from the four daily sonde ascents carried out between 30 January and 22 February and the 13 ozone sondes between 10 February and 6 March (c) Section 6.2.1: Moreover, the potential temperature level up to which the cooling could be observed, 440 K during the highest convective periodĚ. (d) Section 6.2.2: Only above 400K was there a noticeable drop-off in the long-lived tracer-mixing ratio, indicating stratospheric aged airĚĚ (e) Section 6.5.2: The overall measurements suggest a complex picture with indications of both convective uplift of moist air from the lower/middle troposphere to the TTL region (5 ppmv up to 18 km, 400 K), and the presence of dry layers linked to stratospheric intrusions. (f) et ceteraĚ

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In effect, the paper contains too many unproven statements (for another example see below), which cannot be reconciled with (a) the papers content, (b) other studies undertaken within HIBISCIS to which the paper refers to, or (c) studies by third authors in the elsewhere published literature.

(2) Convective overshoots to 440 K (see section 6.2.1): In the paper it is stated at several places that convective overshoots to up to 440 K were observed. Even though more and more reports on ventilation events of the (mid-latitude) UT/LS by convective overshoots appear in the published literature (e.g., Hanisco et al., GRL, 34, L04818, 2007, and references therein), I doubt that the present paper provides sufficient evidence that such overshoots reaching the level Q = 440 K were actually observed. Being more specific, my concerns are twofold: (2a) A drop in air temperature above convective overshoots can hardly serve as an evidence that air masses processed in the convective event actually reached the Q = 440 K level, unless not short-lived tracers are detected in these air masses (which was not the case, see section 6.6.2). Rather, the observed decrease of the air temperature (4 K) above such convective events may serve as evidence that the convective cells may be vertically emanated gravity waves (which does not necessarily mean that air from the lower troposphere was transported to the stratosphere). In fact this possibility for cold T's (and cirrus cloud) was suggested by one of the coauthors and probably overseen by the main author when copy pasting the text of the manuscript (in sections 6.2 and 6.6). (2b) I performed my own calculations on the surface conditions necessary to trigger convection up to Q = 440 K. If there were no other energy input (e.g. solar energy being absorbed by 'dirty' boundary layer air) to the air masses affected by convection other than the sensible and latent energy available at the surface, then convection from the troposphere to Q = 440 K would require a surface temperature of at least Ts = 41o C at a 100 % water vapor saturation. I do not see how this condition can happen on Earth (fortunately). Contrarily, I'm fine with the suspicion that, the warm tropical/subtropical continents being watered by first precipitations events at the end of the dry season are potentially more prone to initiate convective overshoots than e.g., surface air over the tropical oceans. More-

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over my calculations show that convection reaching Q = 400 K requires much more favorable surface conditions (Ts = 36o C and a 100 % water vapor saturation). In fact the measurements of two cooperating groups (from Cambridge and CNRS 'Durry et al.,') probably provide the best evidences that convective overshoots up to the 'already enormously' large altitude of Q= 400 K were observed.

Minor comments:

(I) Contents:

- Section 2 (and generally throughout the manuscript): The references provided in section 2 reflect a certain ignorance for considering what is already known from published literature, e.g., However, their presence although suspected, is largely unknown Ě. and Ě The vertical distribution of several important radicals (NO_2, CH_2O, BrO, IO and OClO, if present), as well as ozone potentially formed Ě please have a look into the new WMO report (2007, www.wmo.ch), and the references cited therein !

- Section 2: Moreover, the potential temperature level up to which the cooling could be observed, 440 K during the highest convective period, indicates that overshoots could penetrate relatively deeply into the lower stratosphere (see the comment above).

- Figure 2 shows the frequency of days when storms penetrated through the tropopause during February for the 7-year period (1996-2002) of the month of February, in comparison with Febraury 2004. Although it is perceived that the convective activity during the campaign period was slightly below average, the number of days when storm turrets reached the lower stratosphere was amongst the highest in 8 years of observations, viz. 58.6% of the month. This statement can be hardly accepted as it is, since (a) it is not explained how the turrets tops were detected - in fact the figures 4, 6, 9, and 11 do not support this statement, and second it is rather unclear how cloud RADARs can reliably detect cloud top due to their ˜ R6 sensitivity - and (b) the used method of cloud top detection is not published elsewhere in the peer-reviewed literature.

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- Section 4.1.1: One of each of the pair of detectors is fitted with a polarising filter to obtain information on particle shape. I do not see how the polarization the backscattered beam polarization may provide information on the particle shape, but rather I think the physical phase is meant here.

- Section 4.2.2: Ě.the spectral range 400-1000nm is extended toward the near IR for the measurement of stratospheric water vapor around 760 and 940nm and O2 (temperature) around 780 nm; The wavelengths provided for water vapor detection (760 nm) and oxygen (780 nm) can not be correct. It guess the correct wavelength are 780/760nm for both instruments.

- Section 6.1.1: Ěthe ECMWF operational temperatures show a systematic cold bias of 0.9K and the easterly zonal winds are too strong by 0.7 m/s. Please check your statement with the results of Borsche et al., GRL34, L03702, doi:10.1029/2006GL027918, 2007).

- Section 6.2: Despite this, we found the tropical trajectory errors to be surprisingly small. Based on own experience this statement can't be true in the generality expressed by the sentence e.g., at the turn around level when the QBO phase moves downward through the lower stratosphere.

- Section 6.2.1: The large amplitude of the cooling in the afternoon, coinciding with the phase of maximum development of diurnal convection Ě this can not be true given (a) the statements in section 6.4 (The picture also shows the increase of kinetic energy close to the inertial frequency on both flights, which is the signature of the ubiquitous presence of inertia-gravity waves) (b) in section 6.6. (Among the results is the frequent detection of anvils up to 14km and ultra-thin cirrus at the lapse rate tropopause around 15 km above convective systems, but not higher) and (c) the comment put forward above.

- Section 6.5.3: ĚĚ. their accuracy (2 ppm) is limited because of the broad spectral resolution measurements in a spectral band where strong narrow H2O lines are satu-

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rated. Are these optical H2O measurements published elsewhere in the literature ? Did you consider (a) a proper curve-of-growth and (b) the concerns regarding the SAOZ FOV, which is eventually sensitive to sun light being scattered from aside high-altitude clouds and thus become contaminated by tropospheric water vapor absorption.

- Section 6.6: ĚĚis the sporadic presence of small particles up to 19 km in significantly sub-saturated regions, Ě. refer here to Popp et al. (Atmos. Chem. Phys., 6, 601-611, 2006).

- Section 6.7: But since, according to model simulations, its amplitude would not be enough, it would require also an advection of Bry rich air suggesting higher amount of Bry in the lower stratosphere at equatorial latitude than in the mid-latitude summer. Even this process is in principle possible, it has shown to be not important for stratospheric halogens on a global scale (see e.g., WMO-2007).

- Section 6.7.2: Another conclusion is that the full NOx chemistry must be taken into account in the CTM models for comparisons with solar occultation measurements. This fact is known since long time (early 1980th, e.g., Roscoe et al.,), and this effect has been taken into account in any reliable comparison study (e.g., Butz et al., Atmos. Chem. Phys., 6, 1293 -1314, 2006 and refs therein). Therefore, reference to these earlier studies appears necessary because this conclusion is far from being original for the present study.

- Section 6.7.4: The photolysis rates calculations were extended in wavelength with more cross sections and quantum yields,Ě.. reformulate that sentence !

- Section 6.8.2: change from University of Heidelberg to Ě. Universities of Bremen and Heidelberg

- Section 7: But the project also provides clear indications of strong episodic updraught of cold air, short-lived tracers,Ěwhich ones ? Provide information on the gases here, and a citation of the relevant paper !

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- Section 7: Regarding water vapor, strong humidity uplifts across the lapse rate tropopause have definitely been observed over deep convective cells. No convincing statement is put forward in the paper that this conclusion (definitely) appears to be justified.

- Section 7: ĚĚwater vapor distribution in the TTL differs significantly from that currently accepted. No convincing statement is brought forward in the paper that this conclusion appears to be justified.

(II) Typos:

- page 2399: in comparison with February 2004 instead of 'Febraury 2004'. - Section 6.1: Change Hibiscus to HIBISCUS

(III) Problematic/wrong use of the English:

- Section 2: There are also many open questions in the tropics regarding aerosols and clouds. (Are the people living in the tropics asking these questions ?)

- Section 3.3: Although two MIR failed, the two MIR-SAOZ flights, including that of the SAOZ new near IR H2O version, were successful, but the flights were relatively short, only 9 and 10 days, respectively, before falling above hurricanes in the Coral Sea and NW Australia, showing again that a lighter version of the Inmarsat was required for longer duration.

- Section 6.5.2: Above these levels, the water vapor mixing ratio ranges between 3.5 and 5 ppmv, which is comparable to that observed at mid-latitude at the same potential temperature levels in late spring for the lower value and autumn for the highest ?

- Section 4.1.3: To avoid contamination by out-gassing from the balloon or the payload only data recorded during the slow night-time descent of the balloons are considered in the TTL and the UT, continued by those during parachute descent in the lower troposphere.

- Section 4.1.3: ĚĚgives information on the shape of the sampled aerosols, and in some cases a coarse estimation of their dimensions as well à gives information on the shape of the sampled aerosols, and in some cases a coarse estimation of their sizes as well.

- Section 4.1.13: Ě..the first looking downward within a 180o FOV and the second upward within a 30-80o annular FOV to avoid reflection of lightning flashes on the balloon or pieces of the payload. The second half of the second phrase is somehow 'cryptic'

- Section 5.1.1: The two flights were performed over the SACZ ???? Ě change to .. 'above'

- Section SF-4 flight: Cloud tops during the early afternoon were mostly $<$14 km, but a few cells penetrated through the tropopause. This statement is senseless without saying (a) where the local tropopause was and (b) how cells exceeding the local tropopause have been detected. Contrarily a similar statement in the section 'SF-3 flight* appears more reasonable, i.e., Ě The temperature profiles show a lapse rate tropopause near 15 km, a cold point of -78o C at 80 hPa, topped by an almost isothermal TTL up to 18 km.

- Section 6.1.1: The trajectories are generally more accurate than in the tropics (compared to what !!!!), but for one balloon many of the calculated trajectories end up on the wrong side of the tropical barrier and this leads to large trajectory errors.

- Section 6.1.3: In another effort, the data of past CNES technical and scientific balloon flights, have been searched for. This is certainly not true in the generality expressed by the sentence e.g., data from past tropical balloon flights were recently distributed by CNES.

- Section 6.2: Despite this, we found the tropical trajectory errors to be surprisingly small ĚĚ change toĚ Despite this, we found the errors for calculated air mass trajectory in the tropics to be surprisingly small

- Section 6.5.2: More surprising is the superpositionĚ.. change to Ě More surprising is the layeringĚ..

(IV) Table&Figures:

- With respect to what was said above, I recommend the inclusion of a new column (or info) into Tables 1 to 5 reagrding the type of air masses probed by the balloons and/or sensors. A possible criteria for ordering could be: (1) tropopause height ? km; (2) PV at tropopause height, and (3) then one of three launch criteria: (a) subtropical UT/LS, (b) SACZ UT/LS, and (c) tropical UT/TTL/LS. For useful criteria into what air masses the sensor were launched into please also check Huntrieser et al., (2007, ACPD 2561) section 3.2.

- The incomplete dates given in Table 3, 4 and 5 are for what year (2004 ?).

- Figure 2: Please refer in the text to where the data are from and how they were inferred ? Also if you use a PDF for the penetration probability, to what area refers the data shown in Figure 2 (I guess the range covered by the RADAR)?

- Figure 3: the labels on the y-axis e.g., DT and DZ are not explained

- A legend to the color coding (emission temperatures?) of the GOES data shown in figures 5, 6, 7, 8 and 10 is missing !

- Figure 12 is far too small to be deciphered even when using magnifying glasses

- Enlarged Figure 14

- The notation ‘A-2 slope’ can not be understand if not explained in more detail in the text or in the legend.

(V) Acronyms:

- For some acronyms (EOLE, TTL, SDLA, TDLS, LES, LEM, BRAMS, NWP Ě et cetera) explanations are missing. - The acronyms LES and LEM in section 6.5.5 and section 6.6 are obviously used for the same tools (local eddy simulation), therefore it is not helpful to give them different names in a single paper

(VI) A quick check through the list of references reveals a substantial amount of errors, which needs to be corrected for.

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