Squawk List for Flight 1843, 11 Dec 2000

Project IMPROVE test flight

(Instruments not mentioned as having a problem are believed to have worked satisfactorily)

Flight over the coastal waters of Washington State just southwest of the Westport radar in stagnating rainband. Stratocumulus shelf extruded out toward the NE from the precipitating band allowing a good test of the liquid water measurements, while the precipitating band itself produced ample numbers of targets for the two imaging probes (2-DP and HVPS) onboard and the 1-D cloud probe.

OVERALL ASSESSMENT

All cloud-related probes appeared to work with varying degrees of success. General improvement in instrument status. For example, the 2-D Precip probe worked for the first time with the new software. However, probe did not produce useful data.

1. GPS /WINDS/TURBULENCE/AIRSPEED

GPS tans-vector system. Data OK; apparently a characteristic of this system is to only find a new lat-long every 3-15 seconds. Thus for intervals of the same time period, winds cannot be updated, nor do we show a location change. Winds and ground speed are thus necessarily constant, using the last lat-long.

MRI turbulence meter: not working.

Winds: Our own winds, using the GPS data, and Shadin heading continue to be unreliable. It appears that they may derive from a faulty heading indication. Backing out the Shadin winds leads to a difference between the heading and the actual ground track (tans-azimuth) heading of about 1.5 degrees. However, using the Shadin heading output does not appear to be the same one used for its own wind calculations. The Shadin heading that we use indicated a heading that was 5.5 deg different from the tans-azimuth ground track, and thus much stronger winds were calculated due to the larger drift angle that we used in our calcs. The error in our data for the time period investigated was in the wind speed in this case—a simple one was chosen.

The Shadin Air Computer winds: generally yielded reasonable-appearing winds in straight line flight except that they are limited to discrete values such as 2.6, 5.1, 7.4, etc, rather than a continuum of values. Due to the limitations of the GPS system, these winds are necessarily constant over several to more than 10 seconds.

BAT: Not working yet.

Rosemount TAS: Grant implemented a pressure-altitude correction for the Rosemount TAS that had been missing previously resulting in TAS that was too low for higher altitudes by 10-20 m s^{-1}. With the correction, the Rosemount TAS is a little HIGHER than the Shadin TAS. It is not clear which one is correct, but the difference should not pose a major problem for concentration caculations.

2. STATE PARAMETERS

Rosemount temperature sensor: The Rosemount-derived static temperature continues 5-10˚ C higher than both the reverse flow temperature (tstatr) and the Shadin Air Computer static temperature. It has been suggested that this is due to a problem with the wiring and/or the Rosemount sensing head. Implementing a calibration is confounded by a Rosemount temperature dependency on TAS and time —the magnitude of the temperature discrepancy diminishes from the beginning to the end of the flight. No progress yet in solving this problem.

Reverse flow temperature sensor: We still continue to see large spikes (electrical noise) in the data. These are not easily excised from the data in post analyses since some spikes, or portions of spikes are only a few degrees departure from the actual temperature. No progress yet in solving this problem.

Ophir hygrometer: The same types of noise spikes seen in tstatr also affect this instrument. After fabulous agreement with the chilled mirror system on the last flight, the Ophir degraded some in tracking the chilled mirror humidities (which are considered reliable) on this flight. Also, the Ophir did not appear to work until 2104 UTC. In-cloud humidities topped out around around 90% when we first entered a stratocumulus deck, but dropped off markedly while we remained in-cloud to values around 60%. Generally, while tracking the chilled mirror well, the Ophir dewpoint was a few degrees lower than the chilled mirror. May need to be cleaned again?

Chilled mirror: Did not work until about 2104 UTC. Looked good after that. 100% rh in-cloud.

3. CLOUD PHYSICS

DMT LWC Meter: Some response in-cloud, but overall noise bothi in and out of cloud appears to preclude retrieval of accurate LWCs.

PVM-100: “Noise spikes eliminated” stated on last flight’s squawk list was premature as it turns out. The usual, numerous noise spikes that we have been seeing since after the Barrow project returned with a vengeance as they say on this flight. Again they seem to be both random, and also are triggered when the probe is first beginning to sense LWC and at the end of cloud penetrations as the LWC recedes rapidly to zero. I now suspect the PVM-100 was simply not turned on during the clear air flight of 1842. No solution to this problem in sight. The probe worked very well in noise free regions.

However, PVM-100 LWCs, when not impacted by noise spikes, were about 70% of those of the FSSP-100 derived LWCs. It is not clear which one is correct. Further investigation will take place checking the adiabaticity of LWC values.
One possibility that remains in view of the high FSSP-100 concentrations on flight 1841, is that they were again a bit too high (100-200 cm^{-3}). However, the 100-200 value while a bit high for a maritime scnario, has been observed previously in this area under similar circumstances and so it can’t be rejected as too high.

FSSP-100: Looked good but should be calibrated due to the lack of agreement with the PVM to help determine which is the more correct. Still possible that droplet concentrations are somewhat high, but I believe this is a minor possibility at this time.

Johnson-Williams hot wire: Did not work early in flight, but showed good agreement with the FSSP-100 LWC during the last few clouds sampled after Don turned it on/got it running/took care of a severe offset problem.

DMT hot wire: No change, still broke. Noisy in and out of cloud, rather different behavior than we saw in the last few SAFARI flight wherein the noise disappeared as the LWC signal became significant. Did not appear to correlate with pressure or cabin temperature as appeared to be the case at one point during the previous flight.

2-D precip probe: Software appeared to work; images produced during the flight. Post flight analysis revealed little useful data and a HUGE amount of noise strips being produced in clear air. Sometimes noise in clear air can be due to a dirty lens. The probe should be cleaned and re-aligned. If this does not fix it, it will have to be returned to DMT/PMS for repair.

This problem, by the way, appears to be exactly the same one that we experienced near the end of KWAJEX; mountains of zero image strips, and only a very few particle images in precip compared to the hundreds expected.

1-D cloud probe: No useful data acquired. Appeared to work in cursory checks during the flight since particles were being recorded in various channels. (A software problem caused the total concentrations not to be calculated and displayed in real time or in post-processing.) However, when the probe spectra were examined after the flight, it revealed severe problems. No particles were being registered in several channels with adjacent channels recording particles. This is usually a sign that the diode array has failed. It is doubtful that a replacement array can be found for the mid-1970s manufacture probe.

HVPS: Appeared to work and data appeared to be piling up in the hard drive. However, the software to view the results of the flight and to view images in real time is not yet ready (poor Tom!!)

CPI: Not installed, being repaired at SPEC.

2-D cloud probe: Not installed, being repaired at DMT.