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This document describes the operation and measurements of the Integrated Surface Flux Facility (ISFF) during the Carbon in the Mountains Experiment, Niwot Ridge, Colorado in Spring and Summer, 2004.

Pine tower for CME (view from Aspen tower)

MEASUREMENT SITES

Table of Sites

Station#	Name	Latitude (deg N)	Longitude (deg W)	Elevation (m)
1	Willow	40.035888178°	105.552138222°	3089
2	Pine	40.035931579°	105.547123249°	3051
3	Aspen	40.034959920°	105.542495110°	3012

Instrumentation

Layout of all sensors on the 3 towers in PNG and PostScript formats.

The instrumentation consists of:

- Sonic anemometers, a mix of:
  - Campbell CSAT3 used for scalar fluxes at 2m and 17 or 30m AGL,
  - ATI-K or NUW probes used for mean wind and heat and momentum flux profiles,
  - Gill Instruments, model R2 also used for mean wind and heat and momentum flux profiles,
  - One Gill Instruments WindSonic for evaluation at 6m at Willow,
- Fast-response hygrometer, Campbell KH2O ~25cm behind each of the CSAT3s
- Vaisala 50Y Humitters to measure air temperature and RH profiles at 1, 2, 6, 10, and 17 or 30m AGL; in NCAR aspirated radiation shield
- Barometers, Vaisala PTB220B, with a single-disk static pressure port
- Net radiometer, REBS Q*7 near the top of each tower
- Photosynthetically Active Radiation (PAR) sensors, Licor Li-190 also near the top of the tower, uplooking (but some shading may occur)
- Infrared surface temperature sensor, Everest 4000.4GL at the top and some intermediate levels on the tower, looking down at an elevation angle of 45 degrees.
- Soil temperature array to measure horizontal and vertical variation for use in modeling CO2 respiration. (See CME Tsoil document for details.) Here are the Tsoil "mote" layouts at the willow, pine, and aspen sites. (Plots courtesy Sean Burns.) The locations are in the

following table. Altitudes marked with "*" are about 1m too high (the antenna was raised to obtain a good signal).

Site Name	Mote	Latitude (deg N)	Longitude (deg W)	Elevation (m)
Willow	20 - A	40.035590072°	105.552205907°	3087.5
Willow	21 - B	40.035842812°	105.552604226°	3089.6
Willow	22 - C	40.035827942°	105.551906203°	3087.8
Willow	23/18 - D	40.036230728°	105.552300276°	3090.9
Pine	12 - A	40.035984774°	105.546927702°	3051.9*
Pine	13 - B	40.036033647°	105.547040741°	3054.9*
Pine	15 - C	40.035881324°	105.547350065°	3054.7
Pine	25 - D	40.035733200°	105.547148042°	3051.9
Aspen	14 - A	40.034784571°	105.542222294°	3012.6
Aspen	16 - B	40.035225894°	105.542552246°	3017.3*
Aspen	24 - C	40.034861897°	105.542628565°	3014.9
Aspen	26 - D	40.034980507°	105.542750096°	3018.6

CO2 sensing is different at each tower.

- Willow has a "HiLo" system, switching between sampling from either 2m or 17m every 5 minutes, connected to a Li6251 analyzer (CO2 only, modified to have serial output of co2, T, and P in 0.1mV). This system also switches to calibration cylinders every 9 hours. All are valves controlled by a relay board inside the ndaq.
- Aspen also has a "HiLo" system, this time sampling at 2m and 30m, controlled with on the same timing by the ndaq. It uses a Li7000 analyzer (without a Nafion dryer, since the flow rate is too high).
- Pine has an Li7500 at 1.5m, which is used with the CSAT at 2m to compute CO2 fluxes.
- Pine also has the Hydra system, with inlets at 1, 2, 6, 10, 17, and 30m for vertical profiles, and with inlets at 1m height on a horizontal transect every 20m from 20m to the N to 180m to the S of Pine.
- Willow and Aspen both have AIRCOA systems to measure vertical profiles of CO2.

DATA COLLECTION NOTES

This was the second use of the new PC104-based data systems. In general, they performed well. Issues are:

- Several serial ports on the 8-port (Emerald) cards were bad and necessitated reassigning channels from the planned configuration.
- We never got RS485 working for the Gill R2 and had to use an external adapter.
- After lighting hits on aspen (which killed the interface and CPU boards), we started using external optoisolators on the 4 CPU board serial ports!
- During the program, we changed relay control for hydra to use the new data system code, rather than an external process driving the serial port.

Bigger issues involved data network:

- Initially, the wireless network was weak. Remounting the omni antenna on the CUFF tower away from the tower structure solved this problem.
- The Breezecom wireless network adaptor at Aspen died near the beginning of Aspen's deployment (perhaps due to lightning). We never could get a consumer-quality D-link adaptor to function as a replacement. As a workaround, we used fiberoptic cable to the CUFF trailer for this tower.
- The Breezecom adaptor at Pine crashed a lot several weeks into the project (during the SF6 deployment). The fix was to power it directly from AC using its power "cube" -- not through the Aster Power Distribution Box power supply.
- The Breezecom adaptor at Willow became weak late in the program. We changed to a higher-gain antenna, which helped, but still experienced some level of data outages during the last few weeks.

Sonic anemometer

Height.Site	Boom Angle (deg rel N)	Actual Height (m)
1m.w	179.7	1*
2m.w	179.9	2*

6m.w	178.3	6*
10m.w	178.1	10*
17m.w	178.9	17*
1m.p	158.4	1.39
2m.p	159.3	2.22
6m.p	161.0	5.82
10m.p	160.1	9.49
17m.p	162.2	16.77
30m.p	163.5	29.69
1m.a	182.9	1.18
2m.a	182.7	2.26
6m.a	172.1	6.43
10m.a	182.0	10.28
17m.a	184.7	17.62
30m.a	186.5	30.54

* = Not measured during operations, but should have been close due to the tower/clamps that were used at willow.

The tops of timbers used for the base of the willow and aspen towers were used to define "0" height. For the pine tower, a point on the ground directly below the 1m sonic was used (since the terrain sloped significantly.)

Sensor issues are:

- 2m.w ran at 20sps rather than 30sps until 20 Jun.
- 17m.w died 30 Jun 17:51 GMT (rain in cable); was revived 14 Jul; died again (serial port problem); was fixed 27 Jul.
- 6m.a gave garbled data initially; fixed 3 Aug with new RS485-to-RS232 converter.
- 1m.a died (rain); fixed 5 Aug.
- 30m.a died (why?); "fixed" 3 Aug

None of these problems should affect the data that were archived, so no action has been taken.

Other issues requiring some action were:

- All ATI-K sonics were operated with the "shadow correction" turned on, using a maximum correction of 0.20, except for 17m.a which was set to 0.16. The 17m.a has been corrected to 0.20 as well.
- The 1m.a NUW sonic anemometer reported path winds, rather than orthogonal winds (our mistake). They have been rotated in the final data set.
- The 1m.p, 6m.p and 10m.p were noisy starting ~22 Jul. They were "fixed" by swapping 12 Aug, but problems at least at 10m recurred. It is likely that water penetrated into the transducers. Our despiking routine was unable to fix this noise since it persisted for long periods. A simple test that wind speed and sigma-w was less than at 17m was used to remove most of these cases.
- In the process of working on the above 1m.p problem, the sonic temperature developed a large (-7 C) offset. This has been added back to the data.

Naturally, there are various rain-related problems. Many are dealt with by our despiking routine, but there clearly are cases (19 Aug) when the statistics are affected. These have not yet been edited from the data set.

Krypton hygrometer

Issues are:

- 30m.a data have "zero" values until serializer swapped 31 Aug. Our standard processing removes near-zero values from whatever source (primarily precipitation), so these zeros will be ignored.
- Typical rain spikes/dropouts. Now removed -- see above.
- 2m.w data dropouts. Should also be removed by our code -- see above.
- Typical removal of cleaning events (not too many, since they didn't scale up very fast). These have been edited from the data set.

Fast-response CO2 sensors (HiLo's + open path)

As mentioned above, their were two types of co2 flux sensors used during CME04. Willow and Aspen had "HiLo" systems that used a large pump to bring air from inlets near the CSAT sonic anemometers down to a closed path analyzer at the tower base. A valve cycled sampling each inlet for 5 minutes and occasionally calibration gases. Thus, 5-minute average flux values are available from each level every 10 minutes.

Pine had an open-path co2 sensor at 2m which ran continously. (No co2 flux measurement was made at 30m at Pine, since this should have been nearly the same as at the CUFF tower.)

These data have been organized in the NetCDF files as one set of co2 measurements (co2, co2'co2', w'co2', etc.) made at 2m height dimensioned by the 3 tower sites. The upper-level co2 measurements are reported as a one-site at 17m (Willow) and one site at 30m (Aspen).

Issues are:

- Aspen Hilo ran for 2 days (29-31 July) with incorrect settings (including sample rate only 0.2 Hz). These data will be ignored.
- Pump flow rate at aspen (filtering through tube)
- Variable delay time (need to look at cross-correlations)
- Only one cal gas sometimes
- Change from differential to absolute mode at willow
- WPL corrections for Li7500 at pine?

See the following discussion for the treatment of these issues.

Willow HiLo
Willow's HiLo was reasonably straightforward. It only had 4 configurations:

- 18 Jun - 29 Jul: Running in absolute mode with no calibration gases.
- 29 Jul - 2 Aug: Running in differential mode with calibration gases. However, the analyzer signal was offscale during this entire period, so no data were collected. Also, the reference gas had a leak and ran out in these 5 days.
- 2 Aug - 31 Aug: Back in absolute mode using calibration gases. The Scott Marin HiCal cylinder was noted to be empty by 26 Aug, but calibrations appear to be okay through 31 Aug when this cylinder was changed. Starting 21 Aug, the calibration cycle was changed from 9 hours to 19 hours to conserve cal. gas.
- 31 Aug - 5 Oct: Same as before using an Airgas cylinder for HiCal.

The dessicant/scrubber used for the reference cell was never changed, however it was always used cycling through the reference cell and should not have been depleted.

Since the Li6251 appears to measure CO2 density, we convert the calibration cylinder mixing ratios to density using the measured Li6251 sample cell pressure and temperature. A plot of all the data are shown here. There appears to be an offset shift in the Li6251 at the time of the HiCal change, though I can't remember any action that should have induced this offset. The lines shown are eyeball fits to the two offsets, adjusted to make the following plot fit well. We check this fit by plotting the difference in CO2 mixing ratio between HiCal and LoCal. This plot shows pretty good agreement with the horizontal lines representing the cylinder concentration differences. Note that this plot is generated using one gain for the analyzer for the entire experiment and the same offset is used for both HiCal and LoCal. Thus, the analyzer was relatively stable. The differences between the Scott Marin cylinders are better behaved than between the Airgas and Scott Marin cylinders. Also, a drift is seen in the last 1-2 weeks which could indicate the AirGas also becoming empty. Since the gain appears to be constant in these calibrations, we will apply it to the June/July data when calibration gases were not available.

Lag times have been measured in the lab and determined by cross-correlations using field data. The inlet tubing was 19.5 m long with 3/8-inch ID. The flow rate through each tube was 39 lpm, producing a delay of 2.1s. For the lower ambient pressure at Niwot Ridge, the regenerative blower that was used is specified to have a higher flow rate, thus the delay would be 1.8s.

The in-field cross-correlations between sonic temperature and co2 ranged from 2.5 to 6s. However, many of these estimates had low correlation coefficients. Lag times computed from high-correlation cases (near local noon) were:

```
    MST         correlation     lag(2m/17m)
 8 Jul 1300      -0.8/-          2.9/-
15 Jul 1000      -0.9/-0.9       3.0/2.7
18 Jul 1200        - /-0.8        - /2.6
 8 Aug 1100        ? /-0.7       3.1/2.9
 9 Sep 1200      -0.8/-0.6       3.0/2.8
 3 Oct 1300      -0.6/-0.5       2.9/2.8
```

Thus, as expected if the pump were running similarly, the lag times appear to be approximately constant at 3.0s for the 2m inlet and 2.8s for the 17m inlet. The reason for the 0.2s difference between these inlet lags is not obvious. The 2m tubing was mostly coiled, whereas the 17m was straight, so we assume that the coiled tubing had somewhat more drag.

Also, a lag of 2.8 or 3.0s is signficantly larger than the calculated value of 1.8s. The calculation didn't take into account lags in the analyzer itself, though 1.0s seems rather large. We'll try to measure this component of the lag.

Lenshow and Raupach also allow us to estimate the filtering effect of this length of tubing. For the flow rate of (39 lpm)(700mb/840mb), U = 7.6 m/s and for temperature of 10 C, Re = 5100, which is turbulent. Thus, they predict a half-power frequency of 2.8 Hz. This will cause some of the flux to be lost, but we will not attempt a correction here.

Thus, the data will be processed simply using a constant analyzer gain and a constant delay time for each inlet. We will convert the measured CO2 density to CO2 mixing ratio using measured analyzer cell pressure and temperature.

Aspen HiLo

This system had two major problems -- the Li7000 calibration was quite bad until recalibrated in the field on 23 Aug and the main blower was too weak until changed 22 Sep. A chronology of major events is:

- 30 Jul: Started, but 30m sonic was removed
- 2 Aug: 30m sonic replaced; Ref gas flow lowered
- 23 Aug: Recalibrated analyzer
- 31 Aug: Replaced Ref gas about the time that it ran out
- 22 Sep: Replaced blower with pump; replaced HiCal gas (but had run out about a week before)

The data from 30-31 Aug when the reference gas ran out have been removed.

The Li7000 reports CO2 mixing ratio, but like the 6251 is fundamentally a density-measuring device. Just to be safe, I've worked up the calibrations as density. It was necessary to break the data into 4 periods to obtain reasonable calibrations:

- 2-23 Aug: The initial calibration before the field recalibration. The gain was found to be 0.26 (only 1/4 of expected response to cal gases).
- 23-31 Aug: The change of reference gas appears to have changed the calibration (more than just the expected change in offset). The gain went up to 0.66 (but still rather far from 1.0).
- 31 Aug - 23 Sep: The change of intake blower also appears to have shifted the calibration, even though no change of analyzer was noted. Gain = 0.73.
- 23 Sep - 6 Oct: The last period, gain = 0.87 (getting closer to 1).

These fits are shown in a scatterplot and the difference in cylinder amounts in another plot. This difference plot still shows a drift from 10-13 Sep, which I assume are bad HiCal values prior to the cylinder completely running out. (Values are missing completely from this plot from 13-22 Sep, when the Pcell values indicated that HiCal was empty.)

After these calibrations, a plot of the CO2 densities 30m-2m generally are as expected. However, from 6-22 Sep, there is a step change in differences apparent from the daytime data. My guess is that this shows the failure of the regenerative blower which we finally fixed by replacing it. Alternatively, this plot could indicate a problem with the system after the new pump was installed. Indeed, the 2m data after the pump change are somewhat strange, with daytime lags being impossible to determine (see next paragraph), however this also could be due simply to changing photosynthesis.

In general, the lag correlations were as expected:

```
    MST         correlation     lag(2m/30m)
 8 Aug 1100      -0.7/-0.9       5.6/4.7
25 Aug 1300      -0.7/-0.8       5.3/4.7
 9 Sep 1200      -0.2/-0.6        - /4.6
20 Sep 1100      -0.4/-0.7       5.1/4.4
23 Sep 2300      -0.9/-0.5       3.0/3.1
26 Sep 1200      -0.2/-0.9        - /3.1
29 Sep 0600      -0.8/-0.8       3.3/3.1
 3 Oct 1300      -0.3/-0.9        - /3.1
```

Note that the correlation coefficients were quite small for the 2m data during the day starting in September, making the time lag impossible to find. However, the nighttime data still had large correlation coefficients with a stable lag. Also note that the lags have a step change at the time of pump replacement, as expected. Finally, note that, like at Willow, the 2m lag is larger than the 30m lag. The data have been processed using constant lags of 5.6 and 4.7s for 2m and 30m, respectively, prior to 23 Sep and 3.3 and 3.1s afterwards.

Pine Open-path

No particular problems were noted with this sensor, other than expected bad values during rain. Removing data when the krypton hygrometer reads a low voltage (below 0.01V) cleans up the vast majority of these cases. The remaining outliers were mostly removed by applying a simple range check (440 < co2 < 638).

Even after this filtering of the data, the computed fluxes were different from those computed using the HiLos. This difference may be real, since the 2m level at Pine would be expected to be heavily influenced by the soil directly below. However, this also may be due in part to the sensor placement (see photo), where the Li7500 was 23cm in back (North) and 53cm below the center of CSAT3 array. According to "How Close is Close Enough?" (Kristensen et al.), the vertical displacement should have only a small effect on the fluxes, however their result would not necessarily apply within a canopy. The horizontal displacement will have a time lag, but this would be difficult to account for in data processing, especially with the light and variable (temporally and spatially) winds within the canopy. For now, the data have been processed with no lag.

To determine the approximate magnitude of the effect of displaced sensors on fluxes in this environment, we can compare h2o fluxes from the Li7500 and krypton hygrometer (which was only 20 cm below the sonic array). A scatterplot shows that many of these Li7500 fluxes are about 90% of the krypton fluxes, which is in general agreement with Kristensen et al., however there is a lot of scatter -- including many cases with near zero krypton h2o flux, but significant Li7500 h2o flux. If the fluxes were well behaved (i.e. only attentuated by the displacement), the flux could be calculated using a Modified Bowen Ratio method, e.g. w'co2' = w'tc' (sigma_co2/sigma_tc).

Hydra

These data have been processed by Sean Burns and are now available here. Some issues were:

- Our data system dropped the cell pressure and temperature measurements until corrected on 3 Aug. Prior calibrations will have to use a surrogate measurement. [It should be possible to use the average Pcell/P_ambient from later periods. Also look at Tcell vs. T_ambient, though this is more iffy.]
- Hydra control changed from "rserial" to "ndaq"
- Process calibrations
- Sort by location/label

T/RH

These were mounted at nearly the same heights as the sonic anemometers at pine and aspen. At willow, they were approximately 0.5m lower than the sonics. On 28 June, the 1m TRH at willow was moved up from 0.5m to 0.95m.

Laboratory post-calibration results for these sensors are described in a temperature postcal report and a humidity postcal report. Temperatures all agreed to within 0.05 C and humidities to 1% (except for one sensor that was 2% off.) Nevertheless, RH data reported during saturated conditions ranged from 97-105% and T.6m.a appeared to have a bias of +0.2 C on 21 Sep. No adjustments have been made to the data set (yet).

Noise was seen on the data from several sensors. Alternate data samples differed by 0.1 C typically. Lab testing was unable to duplicate this problem and field testing was unable to identify either the source or the reason for susceptibilitiy to this noise. (A change of power supply appeared to reduce, but didn't eliminate, the noise.) For 5-minute average statistics, the noise would be averaged out if it were symmetric, but this is not known. Thus, values for these sensors [LIST] could be off by as much as [LIST].

Other issues are:

- 2m.w initial sensor had +3 C T bias during first day that has been removed from the data.
- 1m.w sensor had CPU problem and went offscale 3 times. These data have been removed.
- Later, 1m.w fan blade broke. Was replaced 20 Aug. I've removed about a day of data prior to this, but the failure is not obvious.
- 6m.p had corrosion which stopped the fan. About 3 weeks of data were bad until this problem was discovered. These have been removed.
- 6m.a was moved to 2m for 2 days in Sep to diagnose the TRH noise problem. These data have been removed.
- A few other bad periods on 2m.a have been removed.

Barometer

NO PROBLEMS. (There may be some initial data at each tower with pressure port disconnected.) (There are some "aspen" data on ~20 June which are bogus.)

Radiometers

Rnet/PAR. Issues are:

- Willow Rnet/PAR acquired through a T/RH until 12 Aug. Calibrations were set to one value until 21 Jun, then changed to report mV.
  - Use less-rounded PAR calibration during first period; correct calibration during second period.
  - Apply correct calibration to Rnet during second period; apply different day/night gains.
  - Subtract offsets (source unknown) from data through T/RH. The offset has been determined by eye matching later data and those from Pine. BECAUSE OF THIS, THE Willow Rnet/PAR DATA SHOULD ONLY BE USED QUALITATIVELY UNTIL 12 Aug.
  - Remove data during configuration changes and one spurious value manually.
- Aspen's logger used the wrong calibration for PAR until corrected on 10 Aug. The statistics have been corrected.
- These were placed on the north side of the Pine and Aspen towers and thus were sometimes shaded each day during the period ~11:30-14:30 until the booms were moved to the south side of the towers on 3 Sep (15:40 and 13:20, respectively). Even then, the radiometer's boom's upper guy wire shaded PAR for 20 minutes in the late morning. The angles of these obstructions were estimated by calculating the solar azimuth angle during shading times observed during the few clear-sky days. Then, using an ephemeris, the times that the sun was at these angles was calculated and all data with the solar azimuth from a blocking structure were removed from the data. This is somewhat crude since it treats shading as a 2-, rather than 3-, dimensional problem, and removes some valid data with light shining through the relatively open structure of the tower, but does remove the majority of shaded cases.

- SF6 tarps underneath Rnet at pine. Won't be handled in the data. They were up only during the Washington State Univ. data collection period (23-30 July).
- A few cleaning events?

Tsfc

Issues are:

- Some data taken in mV. Gain of 0.1 now applied to data.
- 10m.p jumped low 23-25 Aug. Data deleted.
- 2m.a filled up with rain early on, then replaced. Bad data deleted.

Tsoil sensors

Laboratory post-calibration results for these sensors are described in a Tsoil postcal report.

Issues are:

- 19 of the 72 probes were broken for some of the time. 3 were broken for the entire program. All but 2 of these outages were due to cables that were bitten by rodents or uprooted by larger animals -- another 2 were unrepairable. (The rodents carried the probes away!) All of the data while these probes were misreading have been filled with missing value flags.
- A significant number of temperature spikes are in the recorded data. They tend to be the later samples in the message, but shouldn't be caused by the radio transmission since only checksum-passed data were saved. (We can't check this because the full checksum was not archived.) These spikes appear to be "bit errors", but this has not been completely investigated. The final data set has these signals despiked, rejecting data with sample-to-sample (every 30s) differences greater than 0.5 C or sample-to-1 hour mean differences greater than 2.0 C. Some obviously bad data survived even this despiking and have been manually deleted.

Detailed Information about the Tsoil Implementation.