THE KANSAS EXPERIMENTS: LOOKING BACK 50 YEARS

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1. Introduction

A surface layer experiment conducted in 1968 by our group at the Air Force Cambridge Research Laboratories (AFCRL) in Bedford, Massachusetts over a very flat open site in southwestern Kansas has come to be regarded as a milestone in atmospheric boundary layer research. Using tower-based sonic anemometry and computer-controlled data acquisition, new for that era, we collected wind and temperature turbulence data which micrometeorologists had been waiting to see for over a decade. We needed the data to better understand the role turbulence plays in the transport of momentum, heat and water vapor in the air next to the ground. Our findings have been discussed extensively in the literature. An experiment of this complexity never happens overnight. It took us several years to get there. We had our share of lucky breaks and mishaps. Here is a brief account of how it all came together in 1968.

2. Background

In the fall of 1961 Duane Haugen took on the leadership of the Boundary Layer Branch at AFCRL. That group had been active in atmospheric boundary layer research for nearly a decade. Haugen had been deeply involved in developing, for the Titan II Intercontinental Ballistic Missile Program, meteorological observation networks and diffusion prediction models capable of predicting where the toxic propellant would go in the event of an aborted launch at their test sites. The models were designed for two sites, one in populated Cape Canaveral and the other at Vandenberg Air Force Base in California. That same fall I joined the group, having just finished my Ph.D. at the University of Washington. Working with my advisor, Joost Businger, I had developed and field-tested a continuous-wave sonic anemometer-thermometer that could potentially replace existing slower hot-thermistor and bivane systems in future boundary layer experiments. Haugen's and my overlapping interests in field observations set in motion events that got us to Kansas in 1965.

Two attempts in the 1950s had brought together most of the active micrometeorologists in the U.S. and their instruments to a single site in O'Neill, Nebraska (Great Plains Field Program 1952 and Project Prairie Grass 1956, organized by Geophysics Research Directorate of the U.S. Air Force). These attempts failed to answer the main question of the day: how valid was the assumption of constant flux in the lowest 10 to 20 meters of the atmosphere, often referred to as the surface layer. This assumption was central to formulations of turbulent transport in that layer. None of the participating groups could measure wind and temperature fluctuations with sufficient accuracy to verify the premise. Haugen's success at Cape Canaveral and Vandenberg AFB depended on his use of a Packard Bell computer, primitive by today's standards, to acquire and process data from sensors in real time. By 1963 faster solid state computers and working sonic anemometers put answers to the constant flux and other questions within reach. Also, the new theories coming out of the USSR needed verification. The -5/3 power law proposed by Kolmogorov for the inertial sub range of velocity spectra headed the list; Monin-Obukhov similarity with its promise of universal relationships in the surface layer was next. But such an effort would be costly, and the group did not have the funds to proceed.

Their prospects changed overnight in 1963 with a phone call from the Titan II office, carrying the news of a fund transfer of a million dollars to AFCRL to be used for any research project of our choosing. It was clearly a thank you for Haugen's work. This enabled us to purchase a state-of-the-art silicon transistor-based computer with 16K core memory, newly on the market (Scientific Data Systems, 920), and to build around it a data acquisition system we could take anywhere to collect and record atmospheric data. There were the usual procurement hurdles, but we persisted and took the time to do the system integration ourselves. I did much of the hardware assembly and Haugen the entire software. The computer, all its peripherals and the sensor electronics, fitted easily into racks in a 40-ft van designed for transport on the highways (Figure 1). We called it our Mobile Micrometeorological Observing System (MMOS). The summer of 1965 was the target date for its field test.

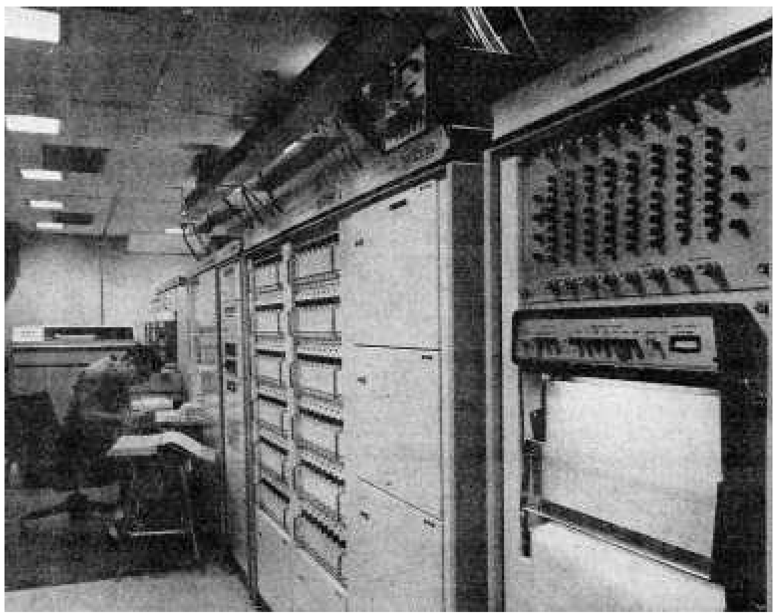

Figure 1 - Interior of the data acquisition van with Duane Haugen at the console.

Meanwhile, the search was on for a suitable site for our experiments. It had to be very flat and very open. The Nebraska site of the earlier experiments was quickly rejected as being not flat enough. Haugen found just the right spot in the wheat fields of southwestern Kansas near the village of Kismet, not far from the Oklahoma panhandle. It was an exceptionally flat square mile with no trees or buildings within miles. But he ran into opposition from the aging patriarch of the family that owned the land who nursed an animus toward the federal government, not shared by his heirs. His sudden death by stroke moments before Haugen was to sign an agreement for a less desirable plot led to negotiations that granted us use of the site. It was a fortuitous turn of events for the group because there was nothing else as good in that part of Kansas. The agreement with the family gave AFCRL access to and use of a small plot of land in the center of the mile square. There we could bring in power and phone lines, set up equipment, and conduct experiments in the summers, right after harvest. (Figure 2)

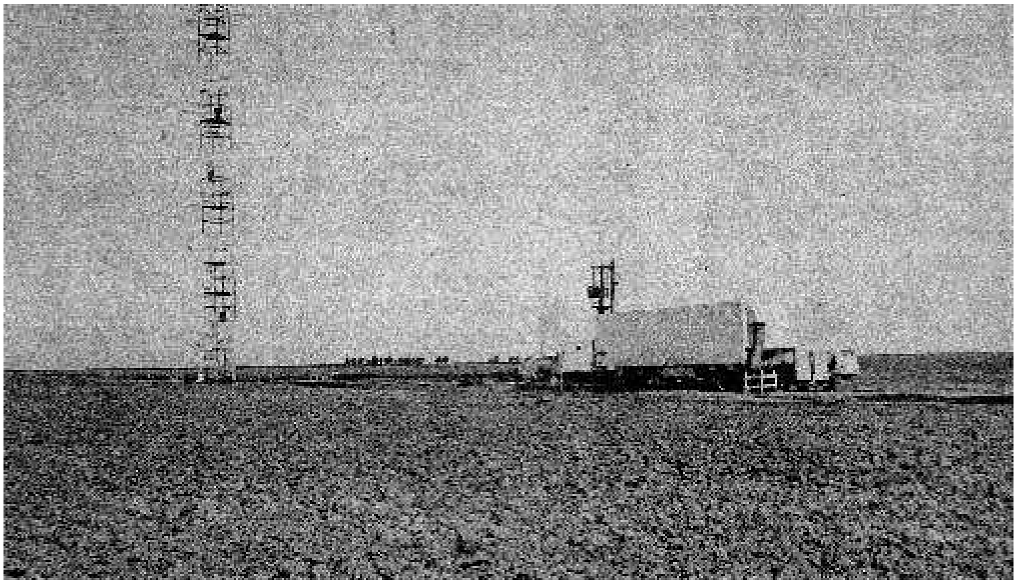

Figure 2 - The experiment site in 1965 showing the tower and data acquisitions trailer as seen from the south.

Site preparation started in earnest the summer of 1964. A 32-m tower was erected and cables were laid to bring the sensor outputs from the tower to the data acquisition van. Eight booms were installed on the tower pointing south, the prevailing wind direction in the summer. The booms at 1, 2, 4, 8, 16, and 32 m were for the slow-response wind and temperature profile sensors, and the 5.66 and 22.6-m booms for the fast-response flux measurements. The sensors were being made by local contractors. The cup-vane anemometers needed for the wind profiles would maintain accuracy down to very low wind speeds. Shielded aspirated platinum resistance elements in Wheatstone Bridge circuits would provide the needed 0.01° C accuracy in the temperature difference measurements between successive levels; actual temperatures would be measured only at 2 m and 32 m. (Figure 3)

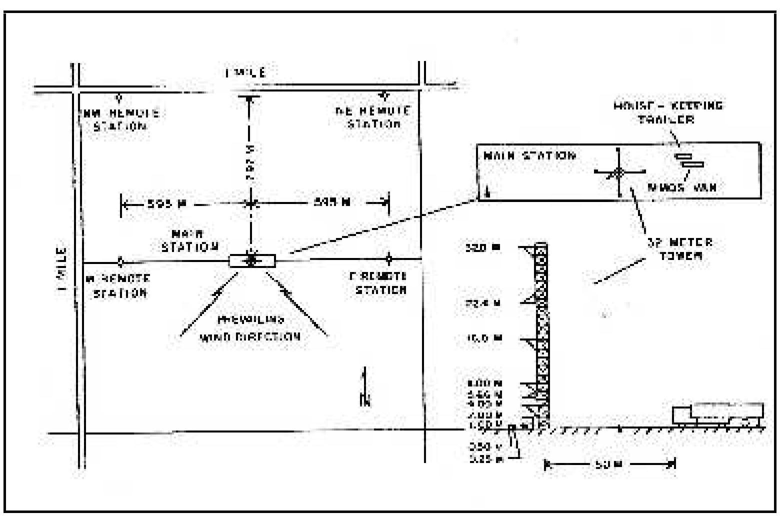

Figure 3 - Plot plan of the experiment site.

The two sonic anemometers we needed for the flux measurements were developed by Bolt, Beranek and Newman (BBN) in Cambridge, Massachusetts. Building on the success of my continuous wave approach, they built a smaller 3-axis version of my instrument with solid-state electronics and weather-resistant transducers. The pulse approach, tested earlier by Verner Suomi of the University of Wisconsin during Project Prairie Grass, had failed because of instability in the received pulses. The phase of a continuous wave proved easier to track than the unsteady envelope of a pulse-train in a turbulent environment.

BBN's probe had its two 15-cm horizontal sonic paths set 120 degrees apart to accommodate possible wind direction shifts during a typical 1-hour run. The 20-cm vertical path was centered and set slightly forward of the horizontal paths. The frame was designed to hold, near the back, a fast-response fine platinum-wire temperature probe developed by Cambridge Systems Inc. in Newton, Massachusetts. On the tower the two BBN probes would be mounted at the end of booms designed to be pivoted to point into the winds during runs, but to swing back into a small anechoic boxes (needed for zero-wind calibration) between runs. (Figure 4)

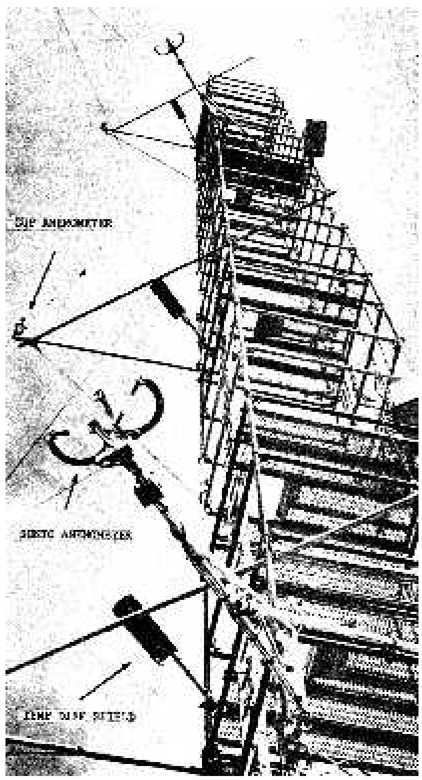

Figure 4 - Instrumentation on the tower in 1965. The two sonic anemometers on the tower at the ends of the booms were designed by Bolt, Beranek and Newman.

3. Early Attempts

Our first field test in the summer of 1965 was a full-scale operation with all the sensors and the data acquisition system in place and a field crew of eight ready to work long shifts. Haugen assembled, on-site, the machine language program for sampling the analog and digital outputs from the sensors, preparing statistical summaries for printout every 15 minutes, and recording all the data from each 1-hr run on a single digital magnetic tape.

The slow-response sensors were sampled at 1-Hz rate and the fast-response sensors at 10-Hz rate. Two types of observations were conducted. The first was a series of long (12-22 hrs) continuous runs with only the slow response sensors to determine the surface roughness and horizontal homogeneity of the site. Only the 15-min summaries were saved from these runs. The second set of observations was recorded in 1-hr segments with both profile and fast-response sensors operating. Fourteen runs were made, twelve under unstable and two under stable conditions. They were flawed because the sonic anemometers failed in the hot Kansas sun. The carefully handcrafted transducers, tuned to the individual frequencies in each channel, drifted with changing temperatures, rendering the readings worthless. The transducers needed to be fixed.

An unexpected side benefit was the presence in Kansas of visiting scientists. Joost Businger from the University of Washington was there along with Yasushi Mitsuta from Kyoto University and Y. Kobori from Kaijo Denki, an instrument manufacturer near Tokyo. The latter two were interested in demonstrating their approach to sonic anemometry and had brought with them a few one-axis versions. Mitsuta, as head of the Disaster Prevention Research Institute at Kyoto University, was initially searching for a wind device that would operate in high winds. He found the continuous-wave approach limiting in range and subject to zero drift. He and Kobori got their pulse system to work by using highly damped transducers and driving them hard. Their instruments performed well and by 1966 we were seriously considering getting Kaijo Denki to build our future sonic anemometers.

We were back at our Kansas field site in 1967 with improved BBN sonic anemometers. The redesigned transducers performed better and we were able to get some useful data. Data sampling was in progress when a dust devil went right through the tower. That and some near-ideal passages of convective plumes provided enough material for a study of their structures by Kaimal and Businger (J. Appl. Met. 1970). Operations came to an abrupt halt before we could start serious data gathering. A severe thunderstorm with golf ball sized hailstones destroyed all our cup anemometers, and water flowed in from all sides, converting the site into a lake. The Kansas mud made driving next to impossible within the square mile. Our 1967 effort was totally compromised. The only consolation was we knew we would be back in 1968 with better sonic anemometers.

BBN's 120-deg array, however, caught the attention of John Wyngaard, who joined the group in 1967. He had just finished his dissertation under John Lumley at Pennsylvania State University. He explored theoretically the spectral consequences of the 120-deg angle and the separation distance between the horizontal axes. He found the distortions they created to be serious at the high-frequency end, but greatly reduced when the horizontal separation between mid-points of the axes was set at 0.6 times the path length. This became a factor in the design of the 3-axis sonic anemometers Kaijo Denki was building for AFCRL. The three units arrived in time for the 1968 Kansas Experiment. These were mounted at 5.66, 11.2, and 22.6 m at the end of booms on precisely machined antenna rotors that could be controlled from inside the data acquisition van.

4. The 1968 Experiment

The summer of 1968 would turn out well. We had good weather, predictable southerly winds and working instruments. We also had more sensor signals to deal with. The fetch was wheat stubble all the way to the southern edge of our designated square mile.

The three Kaijo Denki sonic anemometers came also with sonic temperature outputs. The Cambridge Systems platinum-wire probe was fitted just behind the sonic paths--a wise move as it turned out because the sonic temperatures were serious degraded by cross-wind contamination. The wind profile measurements were made at 2, 4, 5.66, 8, 11.3, 16, 22.6 and 32 m; the mean temperature measurements were made at the same levels as before and dew-point hygrometers were installed at 4, 8, 16 and 32 m. The fast-response sensors were sampled at 20 Hz and the slow-response sensors at 1 Hz.
In addition we had three DISA hot-wire anemometers attached to the base of the sonic probes to provide an independent measure of the dissipation rate of turbulent kinetic energy, normally calculated from the inertial sub range of velocity spectra. The hot-wire outputs had to be differentiated, low-pass filtered and recorded on analog magnetic tapes. Wyngaard conceived and coordinated this aspect of the experiment.

Also present were several international participants. Frank Bradley from Australia came with two 1-m diameter drag plates capable of directly measuring surface stress. Niels Busch and Soren Larsen came from Denmark with a fast-response hot-wire bivane probe that made independent measurements of wind turbulence alongside the 5.66-m sonic anemometer. We were also joined by Businger and Shelby Frisch from University of Washington with a small aircraft instrumented to observe convective plumes.

Haugen, who directed this experiment, compiled the software for data acquisition, processing and storage of the data. It had to be extremely efficient to work around the memory limitation of the computer and the storage capacity of 12-inch diameter magnetic tape reels.

The observations were made in 1-hr segments. These segments could never be contiguous because some time was needed between runs to re-orient the sonic anemometers for best wind exposure, and to check the scaling factors and calibrations of all the sensors. We collected 54 runs in all, out of which 32 seemed complete enough for analysis. These included 14 under unstable, 6 under near-neutral and 12 under stable conditions.

5. Data Analysis

The data kept us busy for the next three years. Businger joined us at AFCRL for a year as a Senior National Research Council Fellow to take part in the analysis effort. Working with members of our group, he came out in 1971 with the first definitive paper on flux-profile relationships. Monin-Obukhov similarity emerged as the most appropriate framework for viewing surface layer turbulence. The very small scatter in the data plots was particularly reassuring. The assumption of a constant-flux layer above the ground seemed to hold, but only when viewed over suitably long averaging periods, an hour at least, for the very unstable runs.

Meanwhile I was examining the spectra of wind components and temperature looking for signs of consistency. I found that the spectra, when plotted along surface layer coordinates, reduced to a set of universal curves that converged to a -5/3 slope in the inertial sub range but spread out systematically as a function of stability at lower frequencies. The exceptions were the daytime spectra of horizontal winds and temperature which seemed to follow a different scaling law (the boundary layer depth, as it later turned out). The co spectra of stress and heat flux showed a different slope in the inertial sub range. Wyngaard and Cote developed a co spectral similarity theory that provided justification for the -7/3 power law the data were showing. The stability dependence of the co spectral curves was also consistent with their similarity predictions.

With the hot-wire data Wyngaard was able to confirm Kolmogorov's original hypothesis that the smallest scale structure of turbulence is universal and depends only on dissipation rate and kinematic viscosity. The Kansas data were sufficiently complete to allow Wyngaard and Cote to carry out the most thorough analyses of the budgets of turbulent kinetic energy and of the fluxes of momentum and heat. This, in turn, provided the first experimental basis for second-order models of the atmospheric boundary layer. (In their 1990 article in Boundary Layer Meteorology, "The Kansas and Minnesota Experiments," Kaimal and Wyngaard offer a more complete description of the 1968 experiment, including problems resolved in the data preparation phase, details of visitor participation, and an account of all the studies and papers that came out.)

6. Conclusions

The success of any experiment rests on two factors: the universality of the results and their duplication by other experimenters. The 1968 Kansas Experiment seems to have satisfied both requirements. It has provided parameterizations of surface exchanges that are still used today in meteorological models on all scales. It taught our Boundary Layer group a great deal about collecting and processing atmospheric data. Building on this experience we moved on to our next major undertaking: a study of the entire atmospheric boundary layer over a very flat site in northwestern Minnesota, which we did in 1973. We were fulfilling a dream that started twenty-one years earlier over the plains of Nebraska. At this 50-year mark it is important to recognize Duane Haugen's singular role in bringing together the Kansas experiments. It was his vision, hard work and persistence that got us there. The alternative would have been a fragmented approach with smaller research groups, as in the past, and a much longer lead-time for the results to emerge.

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