CORRESPONDENCE

Comments on "A Revaluation of the Kansas Mast Influence on Measurements of Stress and Cup Anemometer Overspeeding"

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20 April 1981 and 11 November 1981

Attention is drawn to *Journal of Applied Meteorology* readers of the above paper by Wieringa (1980) appearing elsewhere, but which related directly to previous papers and correspondence appearing in this journal (Bernstein, 1967; Kaimal and Haugen, 1969; Izumi and Barad, 1970; Kaimal and Haugen, 1971; Hyson, 1972; Gill, 1973; Lo and McBean, 1978) and in several other journals. Wieringa has done a most commendable job of reexamining the Kansas mast data and trying to pinpoint the probable cause as to why the Kansas mast measurements of stress differ so markedly from those of other scientists; and why the measurements of mean wind speeds by sonic and cup anemometers on the same tower and at the same height did not agree.

In trying to determine why cup anemometers on one set of booms differed by 8–16% from those of sonic anemometers on another set of booms at the same heights (5.66 and 22.6 m), he observed that in one published paper (Haugen *et al.*, 1971) a photograph of the tower (Fig. 1) showed that bulky boxes were located at these same levels within the tower. Thus, the tower arrangement was not adequately described by the simple line drawing shown in Fig. 2 (Izumi and Barad, 1970), but is better approximated by Fig. 3 (Wieringa, 1980). He deduced that these boxes could cause some increase in apparent wind speed at the cup anemometer locations for winds from the 150–180° sector, while at the same time, there would be some reduction in speed at the sonic anemometer locations. He also deduced that for southerly air flows the air passing the sonic anemometers would probably have a slight upward direction, as would be suggested by Halitsky's wind tunnel testing, a schematic of which is shown in Fig. 4 (Halitsky, 1968). It should be noted here that in the planning stages of this experiment (1966–67) there appears to have been no published data or graphs similar to Fig. 4, that would have alerted the AFCRL scientists to expect an upslope flow of a degree or two at the sonic anemometer location for southerly winds—only concurrent and subsequent studies by Halitsky (1968) and Cermak *et al.* (1966, 1968) showed this potential error. If these scientists were to conduct a similar study today, I am sure they would reduce the horizontal wind blockage of essential components left on the tower to say one-fourth or even one-eighth of that shown in Fig. 1.

In this writer's opinion Wieringa has probably detected the physical reason why neither the cup nor

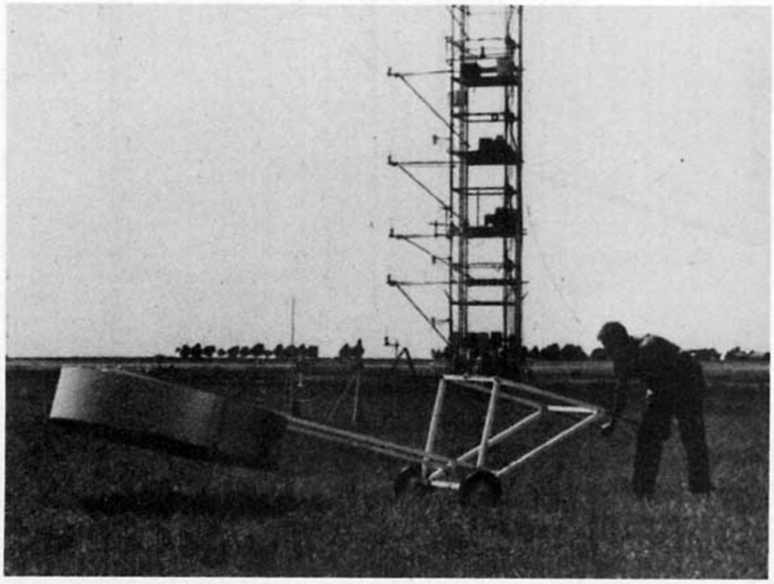

FIG. 1. The lower part of the Kansas mast as seen from the east-southeast, with a drag plate calibration cover in the foreground. The four booms shown are those at 2, 4, 5.7 and 8 m. (Wieringa's Fig. 1.)

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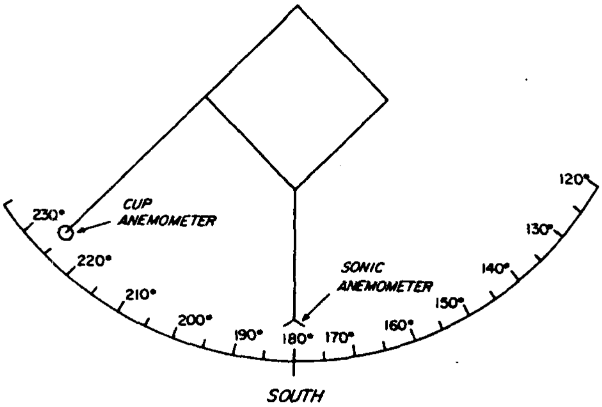

FIG. 2. Horizontal cross section of micrometeorological tower near Sublette, Kansas. (Izumi and Barad's Fig. 1.)

sonic anemometers were measuring the free-stream wind past the tower. Since the two types of sensors were on differently oriented booms and at somewhat different distances from the bulky obstacles, the two sensors at either elevation would not be receiving the same wind speed and direction simultaneously. By his careful, searching analysis of the published data Wieringa has developed the probable difference in wind speed (as a percentage) at the cup anemometer location versus the sonic anemometer location for each 10° of arc from 150 to 220° azimuth. Under stable conditions this varied from +8 to +13%. Under unstable conditions this factor varied from +9 to +17%. From these observations one might conclude that the probable overspeeding of the cup anemometer in going from a steady wind to a turbulent

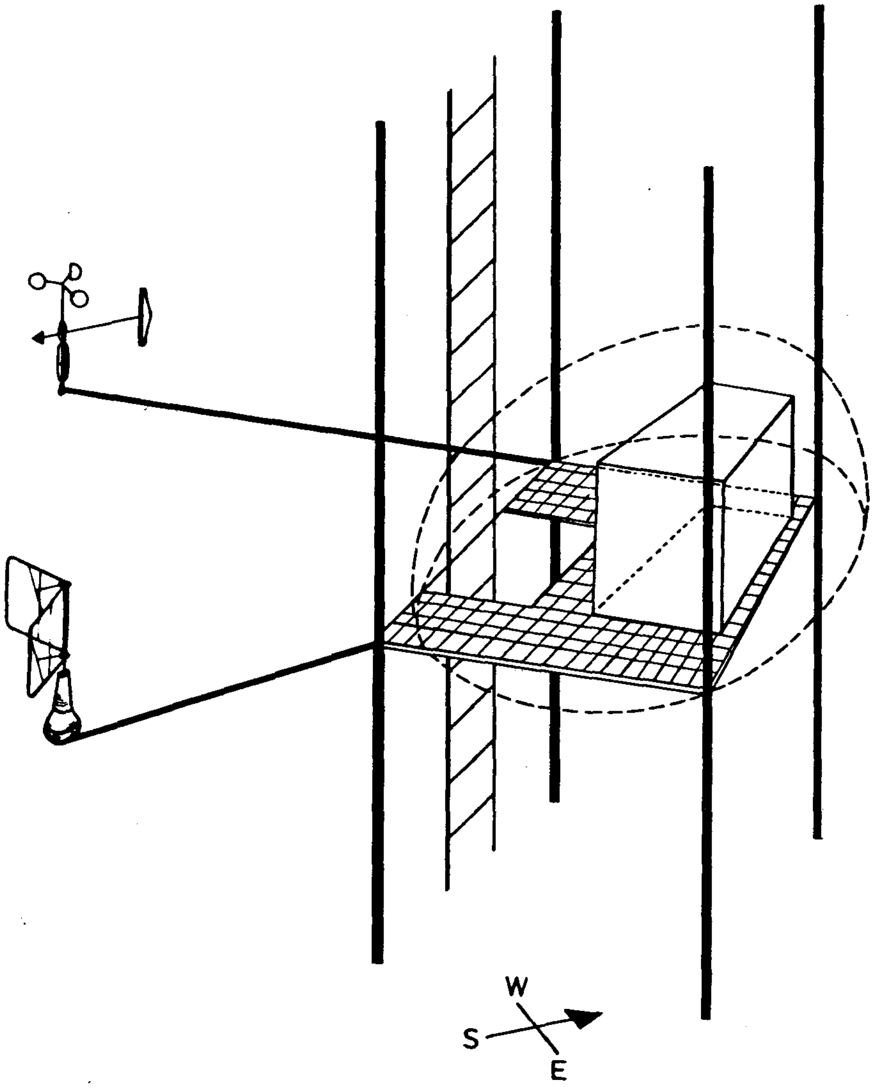

FIG. 3. Basic structure of the 5 and 22 m levels on the Kansas mast. The drawing is to scale, including the anemometers on the left. A typical potential-flow model sphere is indicated by the dashed lines (upper half only). (Wieringa's Fig. 2.)

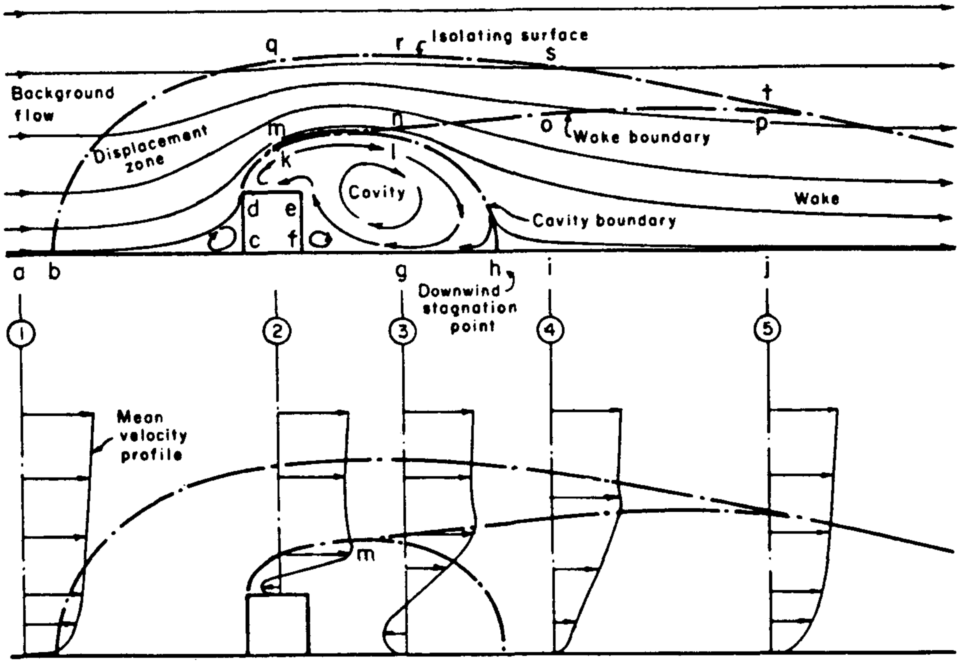

FIG. 4. General arrangement of flow zones near a sharp-edged building. (Halitsky's Fig. 5.17).

wind would be 1-4% (9-8 to 17-13%), instead of the 8-16% suggested by Izumi and Barad (1970). Wieringa, both in the text (pp. 411-428) and in the Appendix to his paper (pp. 428-430) examines quite carefully Izumi and Barad's (1970) evaluations of the overspeeding of the cup anemometers both at the Kansas site and the Bedford site. He concludes, "Reanalysis of the wind data shows that the cup overspeeding factors were ~4% less than those originally derived by Izumi and Barad." This writer (Gill) maintains the position given in his 1973 paper, "Repeating, it is not possible for the 3-cup anemometer to have an overrun of 8% or more in a nongusty wind if the instrument is working properly and has been properly calibrated and exposed. From my analysis of Izumi and Barad's paper the B. and W. model 170-41 3-cup anemometer probably over-indicates the mean wind speed by an amount from 0-6% depending on the intensity of turbulence."

As mentioned above, Wieringa deduced that likely there was an upflow component of the wind at the sonic anemometer locations for southerly air flow due to the presence of the boxes. For his simplified obstacle shown in Fig. 3 he computed a probable upslope wind of 1-2°. He made an estimation of the effect of this locally induced upslope wind and of the errors in wind speed measurement on the calculations of the vertical momentum flux. He concluded that both of these had the effect of significantly reducing the measurements of the vertical turbulent momentum flux. Since this is an area outside of my expertise, I am not in a position to judge the validity of his argument.

Kaimal and Haugen (1969, 1971) contend that vertical flow sensors should be aligned within 0.1° of the true vertical—a very difficult specification to achieve, and maintain, on all but the most rigid towers with reinforced booms. Dyer *et al.* (1970) contend that a 1.0° accuracy is a lot more reasonable, and has a chance of being achieved. Many precision potentiometers used in wind direction work have 3000 or fewer turns for 350° of winding, i.e., about 10 turns per degree. In normal use the moving contact usually bridges 3-5 turns of the winding, so that the sensitivity of the transducer is 0.3-0.5°. Typical linearity of such precision potentiometers is ~ ±0.5%, i.e., ~ ±1.8°. Combining these limitations of the transducers with a possible 1-2° upslope wind, as suggested by Wieringa's computations, we must conclude that trying to align the sensor within 0.1° of the vertical is scarcely warranted—within ±0.5° of the vertical is more reasonable and more likely to be attainable.

From the vantage point of "hind sight" any of us planning to conduct similar high quality studies from tower supported instruments will want to minimize the size and interference potential of booms, tower structure and auxiliary components. For some installations a carefully conducted wind tunnel study of an accurate scale of a tower section (complete with platform, ladder, pipes, boxes, booms, etc.) may be justified. Such a study (conducted by a competent group) should determine the probable speed, direction and elevation angle errors likely to be found at the sensor locations for winds over the range (in speed and direction) expected at the tower site.

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