Comments on "Aluminum Oxide Hygrometer: Laboratory Performance and Flight Results"

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This letter concerns itself with the article "Aluminum Oxide Hygrometer: Laboratory Performance and Flight Results" (*J. Appl. Meteor.*, 5, 878–886). We have been closely associated with this work over the past few years, and because of our technical familiarity with it, and our disagreements on the significance of the data, we feel an obligation to alert the meteorological community to certain aspects of this sensing technique.

There are two basic parameters of this sensor which we feel are not objectively reported and for which there appears to be adequate information available to reasonably reach conclusions which are quite disparate from those in the article. These two basic areas are time response and "temperature coefficient."

Regarding the response time, the author first presents some of the data obtained in the "two-pressure" chamber and then attempts to dismiss these data on the claim of wall contamination. This claim does not appear valid since the chamber was designed to avoid this and tests performed in it on the carbon element (ML-476) at −20C yield an order of magnitude shorter response time than the Al_2O_3 element. A description of the chamber's operation and of the tests performed on the carbon element can be found in an article by Marchgraber and Grote (1963). It should be noted that the different response time with increasing and decreasing humidity is a property of the sensors and not the chamber. Further evidence in support of the two-pressure chamber test results can be obtained by applying the theoretical work of Glueckauf (1947) concerning response time effects due to the need of this type of sensor to diffuse moisture through an aerodynamic boundary layer. Using this method, one obtains good quantitative agreement between the calculated ratios of the response time at the various ambient temperatures and the experimental data.

After discussing the evidence from the two-pressure chamber, the author then refers to tests performed in his calibration system (Fig. 1 of the article) and states: "These results show the element time constant at ambient temperatures as low as −61C to be approximately the same as observed at room temperature." Elsewhere in the article, this room temperature lag was cited as being on the order of 2 sec. On the other hand, the author mentions that when using this system for calibration purposes, they evacuate it to allow the chamber to reach equilibrium more rapidly. It soon becomes evident that these "response tests" performed in the calibration system must have been run at reduced pressure. Since the time constant, according to Glueckauf, is inversely proportional to pressure, the enhancement of a couple of orders of magnitude of the "measured time constant" could be achieved in this manner. It should be noted that although the pressure effect works to the sensor's advantage in the stratosphere, the magnitude of this enhancement is not sufficient to overcome the lengthening of the time constant due to the reduced temperatures. This is especially true in the lower stratosphere where time constants of several minutes might be expected.

The second major issue is whether the element exhibits a temperature coefficient. This term "temperature coefficient" has been used by the author to mean whether the element senses relative or absolute humidity. Actually, there are two effects which can be termed "temperature coefficient", the problem of absolute vs. relative humidity and the problem of whether the element is in temperature equilibrium with the air being sensed.

It is curious to note that, whereas the two-pressure chamber tests were designed for time response testing and not for relative humidity effects, the article rejects the time response testing but claims the independence of the element from relative humidity effects has been verified. It is difficult for us to understand this verification since, in some instances, the elements actually changed calibration during the test sequence. In connection with this, the reader might wish to scrutinize more closely Fig. 3 of the article to determine whether he agrees with the author on there being a negligible calibration shift.

The problem of whether the element senses relative or absolute humidity has been discussed before and both Jason (1963) and Miyata and Watari (1963), who have investigated Al_2O_3, find that it has a relative humidity dependency. We, ourselves, have done some testing of the elements produced by the author's firm and find a relative humidity dependency. We do not as yet consider these studies definitive. Further testing is now being performed, and the results will be made available when completed.

In connection with sensing absolute humidity, it should be noted that whereas the author says the element senses absolute humidity, the calibration curve, Fig. 2 of the article, is plotted in dew point vs. impedance. Dew point is not an absolute humidity indicator and corrections of about 3C at the lower dew points would be necessary to make measurements taken at saturation and very low relative humidity (less than 0.01% relative humidity for 700 kΩ impedance point) comparable.

If we then use the author's logic concerning the flight data but start with the opposite premise, i.e., a long response time, the only reasonable explanation for the detail indicated in the stratosphere would be that the element was responding because of a "temperature coefficient."

There are several other items with which issue might be taken. For example, the accuracy of the Cambridge System's device which the author doubts is directly traceable to National Bureau of Standards tests. Also, the quotes of Ballinger are made to appear applicable only to dew-point sensors which was not the intent of his work. The low mass transfer rates for water vapor in the stratosphere restricts the response of all direct sensing instruments, which includes the Al_2O_3 element.

In conclusion, because of the basic problems associated with the sensor itself, the incomplete state of development of this sensing technique and the unusual meteorological data obtained with it (increase of some 200 times in mixing ratio above the tropopause), it would appear that the original concept of these flights as "proof of principle gear" should be retained. The data should be scrutinized to obtain sensor information instead of being presented to the scientific community with conclusions concerning temporal and spatial variability of water vapor in the stratosphere.

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