CORRESPONDENCE.

To the Editor of the JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY.

DEAR SIR,

COMPARISON OF VARIOUS METHODS FOR DETERMINING FLIGHT SPEED AT GREAT ALTITUDES.

J. of the R.Ae.S., Jan., 1944. Translated from Luftwissen, Sept., 1942.
Written by R. Schmidt. Translated by L. J. Goodlet.

The three methods compared in this article are:—

1. Dornier air log.
2. Dynamic pressure.
3. Ground calibration by gyro-stabilized vertical sight telescopes.

The last method is used for calibration only, but may be used as a basis for comparison with the other two.

Schmidt correctly points out that dynamic pressure measurement has the following disadvantages:—

1. Interference of obstructions with static pressure.
2. Difficulty of calibration.
3. Effect of Mach number.
4. Air temperature errors.
5. Elastic pressure capsule errors due to hysteresis and temperature.
6. Inaccuracy of mean values.

He then claims to prove that the Dornier air log is more accurate than the dynamic pressure method and more accurate than the ground contact method (by which he calibrated the log). This after only four calibration runs! And these at low level and quite short (8 km.). The method by which he attempts to prove this is a masterpiece of confusion.

He first introduces the idea of the quadrangle of error without explaining its use. However, we must remember, even if Schmidt forgets, that the quadrangle of error is a measure of the consistency of the speed of the flight and has *nothing* to do with the instrument. Both the distance and the time are measured separately from the instrument and a small closure of the quadrangle of error is a good reflection on the pilot and not on the speed indicator. Yet it is on this basis that Schmidt proves the superiority of the Dornier instrument. His method of work is quite confused. He describes it as follows:—

> "The air log is towed on a 30 m. cable and its revolutions recorded by a counter on the aeroplane. At the same time, a stop-watch is intermittently operated, allowing the number of revolutions over given intervals of time to be counted. The instrument errors are exceedingly small; the rotor pitch is about 4.4 m., and at 130 m./sec. airspeed the turning speed of the rotor is about 30 rev. per sec. If the stop-watch timing accuracy is within 0.1 sec. the error in determining the distance flown will be $\Delta S = 30/10 \,.\, 4.4 = 13$ metres, or a negligibly small variation of 0.00065 of the total measuring length of about 20,000 metres."

Now this stop-watch error is for each reading, and if the watch is "intermittently operated, allowing the number of revolutions over given intervals of time to be counted," then this error is not for the whole 20,000 metre course, but for each stop-watch reading. Or, as is more likely, the stop-watch may not be operated over given intervals of time at all, but over the whole course. If this is so, why does he say the opposite?

Schmidt goes to great length to point out the fact that, as the log integrates the readings for a run, while a pressure device gives only discreet values that must be averaged and summed arithmetically, the log therefore is more accurate in calibration. He neglects to mention that if he wants to read velocity in the ship he will have to calibrate a gauge with discreet velocity readings against the speed of the rotor, a process which is hardly more accurate than photo-observer results followed by integration on a pressure device.

On page 12 Schmidt says: "Owing to the insignificant frictional losses, the air log is almost completely unaffected by differences in altitude." On page 18 he contradicts this and then follows with an alleged proof of his point (note the passages in italics):—

> "The effective pitch of the log rotor tends to increase with altitude, owing to the friction in the rotor bearings. The relative importance of the frictional effect will obviously vary inversely as the turning moment exerted by the airflow; which, however, is proportional to the air density, *i.e.*, at $\rho=0$ the rotator ceases to function, and the apparent pitch becomes infinitely great. If the ratios of log pitch at varying altitudes to log pitch at the ground (4.405) are plotted against the air density (Fig. 5), the log altitude correction factors are obtained; it must be remembered, however, that the log pitch is affected by the same errors as the airspeed. Plotting the ratio of altitude pitch/ground pitch against air density produces the curve shown in Fig. 5. The zone of scattering is composed of two sets of values—scattering of the individual values at altitude, and scattering of the reference ground value; obviously, therefore, *there is no relationship between altitude and range of scattering—the errors of airspeed measurement are therefore accidental and not related to the altitude.* On the other hand, the dependence of effective log pitch on altitude is unquestionable, and there is no reason to assume that *the relationship between effective log pitch and air density is other than steady.* Consequently, the fair curve for the log pitch must be drawn steadily and always within the range of scattering of the measured values; which leads *to the conclusion that the scattering of these values is almost exclusively due* to *errors inherent in the method of quadrilateral test flights, and not to any unsteadiness in the running of the log rotor, i.e., air log readings are still far more accurate than the results of measurements by means of quadrilateral calibration flights; unfortunately, no more accurate basis of comparison exists,*"

which, as far as this writer can see, is a complete "non-sequitor."

He next shows the "inaccuracies" of the dynamic pressure method by showing what is really the deviation from the mean, uncorrected, is greater for the dynamic pressure method, than the deviation from the mean (which may have actually really existed and is no indication of error), corrected by an arithmetical process, of the log.

The implications of this distorted presentation of data are, in this writer's opinion, unjustified. Somewhere may possibly be a proof of the validity of his method, but Schmidt has certainly not presented it and on the face of it the article seems to be a series of unproven claims.

To illustrate his method, Schmidt shows how the log worked for a series of tests of airspeed with the propeller having:—

(1) Polished blades on suction side.
(2) A coating of anti-glare varnish.
(3) Varnished and sanded—

a series in which extreme precision is needed. His results show 12 relatively consistent points; that is, yielding three distinct straight lines. This requires a consistency of approximately 2 m.p.h., which is about what he obtained. Again, we are talking about *consistency* in tests of this type, and experimental tests used for this purpose—and not of absolute accuracy. His results on this test are no proof of the accuracy of the instrument. Furthermore, a dynamic pressure indicator on a test of this type would match these results for *consistency*. Moreover, his errors apparently did not include the instantaneous readings so necessary to the pilot, but was the much more accurate method of taking the integrated log reading. The other disadvantages, such as inconvenience in flight manœuvres, descent and retraction, are not mentioned. All the transmitting difficulties, so necessary in flight, are overlooked in these runs, where the sum of revolutions could be used.

As far as this writer can see, the author's claims for the Dornier log have not been justified.

The possibility of substituting a log, or trailing bomb, for a pressure indicator, as far as experimental test work is concerned, has a certain amount of interest and validity. It may well prove fruitful to investigate this field either by experimentation or reference to other articles, but Schmidt's article is of no use whatsoever. His remarks on the functioning of the depressor vanes may at best be of limited value in the design of such logs.

Yours faithfully.

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