OBSERVATIONS WITH BUBBLE SEXTANT ON BOARD S. S. "ARUCAS" IN MAY 1933

by

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Nowadays, owing to the considerable increase in the radius of action of aircraft, the problem of making altitude measurements of heavenly bodies independent of the visible horizon as a datum line has once more come to the fore. While taking part in the work undertaken by the MARINELEITUNG in solving this question, I have been taking observations with the bubble sextant, with the friendly support of Prof. Dr. WEDEMEYER and at the instigation of the MARINELEITUNG, in the course of a voyage to the Canary Islands.

The object was to examine systematically the capabilities of the bubble sextant, bearing in mind the necessities of nautical practice in commercial navigation. But little information on the subject had hitherto been available. Shipowners had hardly concerned themselves with the bubble sextant, or rather, after a few fruitless trials had lost interest in it. On the other hand, certain admirers lay themselves out to announce disconcertingly good results while other observers have had just the opposite experience. It thus appeared necessary, in view of such contradictory results, to undertake a systematic investigation that would take care neither to neglect the practical requirements nor to be cramped by influences of a psychological nature in practice.

Theoretically, the bubble sextant solves the problem of the independence of the visible horizon; the investigation had to show to what degree it was applicable in practice. We assume a knowledge of the arrangement of the instrument and the method of using the various types of bubble sextant. (1)

The firm of C. PLATH of Hamburg very obligingly put at my disposal a drum sextant with levelling box (COUTINHO system) of their own make. I was able to do the work aboard S. S. *Arucas* of the Norddeutscher Lloyd in May 1933. I owe the exceptional abundance of material obtained to the prodigal assistance of Captain BENDER and his officers as well as of two midshipmen (holding pilots' certificates) who were on board. Thanks to the keen collaboration of these gentlemen it was possible to obtain personal appreciations at any moment and to make comparative observations with several observers.

I had already had occasion to take sights with the instrument, while the other observers had to acquire the necessary manual facility by a large number of trial observations (which were not utilised). The non-controllable conditions, both as regards the ship and the weather, were distinctly unfavourable, so that the instrument and the observers were severely tested. During nearly the whole of the voyage the sea was high; on the outward passage the ship rolled heavily at times, while homeward bound she pitched into a sea of force 4 to 6. Even in a lesser sea the ship, which was too short and which (to make matters worse) was not fully laden, particularly when homeward bound, laboured considerably. For the very reason of these unfavourable conditions, the results of the observations are particularly significant, observing that it was not a question, as it happened, of "fair weather observations".

System of representation.

After some trials, and in full agreement with Prof. Dr. MELDAU of the Navigation School at Bremen and Lieut. (retd.) MARTINI of the MARINELEITUNG, I decided on representations of long series of observations by graphs, because these, as compared with

(1) *See:* Hydrographic Review, *Vol. V, No. 2 & Vol. VI, No. 2. (I.H.B.).*

the treatment of isolated observations (or of short series of 3 to 5 measurements) are more objective and, shown in the form of a curve, enable comparisons to be made at first sight. Even treatment as a percentage according to the limits of error is only impeccably possible by this means, observing that in that case one always compares numerous measurements which have been effectively made in absolutely similar conditions. The observations were made in such a way as to take the greatest number of altitude observations possible within an interval of ten to twenty minutes ; at each "stop" the instrument was placed *without* altering the setting, so that at the next sight the image of the star and the bubble (after it had come to rest) could be brought together again by a slight movement of the screw. It might naturally be objected that in time one would acquire a certain skill in this "screwing up", capable of falsifying the general impression. As against this it may first be said that the different measurements do not follow one another at strictly equal time intervals, and also that the differences of altitude vary with the azimuth, so much so that it would hardly be possible for any degree of skill to adapt itself to these two factors. Besides, the whole question is to attain the greatest accuracy in a certain number of isolated measurements by working the screw. The series of ten to thirty different altitude measurements thus taken were compared, on the one hand, with distances of the horizon always measured simultaneously, and on the other hand, with true altitudes calculated for the different instants of observation, taking care, in the latter case, to get as accurate a fix as possible of the ship's position. When the azimuth was large, a calculation was made, for the period of observation of 10 to 20 minutes, of the true altitudes at the moment of the first altitude, of the intermediate altitude, and of the last altitude by the bubble, assuming the line of true change of altitude to be rectilinear. When the azimuths were smaller, i. e. when the path of the true change of altitude during the period of the observations was curved, the true altitudes were calculated again for intervals between these times, so as to obtain as accurate a curve as possible for the true change of altitude. The two curves were shown graphically ; afterwards the points corresponding in time to the different heights of the bubble were plotted on the curve of calculated change of altitude, so that the differences between observation and calculation could be read. The positions of the ship, azimuths, values of the isolated measurements and times, as well as their position with respect to Greenwich Mean Time, were given with sufficient accuracy to make checking always possible. In all, 30 series of observations of this kind, comprising more than 600 individual measurements, were taken by five different observers during the voyage ; a few of these were made in harbour. Nearly the same number of isolated measurements, which however were not used, had also been made by way of trial and for training. In addition we had measured a certain number of isolated altitudes which were used practically for fixing the ship's position.

Technique of the measurements.

As already stated, the ship laboured excessively at certain times, and under these conditions even the smallest degree of satisfaction could only be obtained when the azimuth of the heavenly body happened to be about at right angles to the movement of the ship , i. e. in the case of rolling one had to observe in the fore and aft line, while in the case of pitching the observation had to be taken on the beam. No observation in the direction of movement of the ship could take place, for the reason that the bubble, owing to the motion of the ship, was subjected to such acceleration that it was impossible to keep it long enough at rest, or rather to bring it to rest at all. It was occasionally possible to obtain an apparent condition of rest by a counter-movement of the instrument but, probably more often than not, this was only due to a stoppage of the acceleration (by this very counter-movement) ; the bubble, in this case, only fetched up in its real position of rest by the merest chance. The same phenomenon occurred also, though in a lesser degree, during observations at right angles to the movement of the ship, because most of the time one could not choose a place to observe from such that the motion of the ship could be compensated for altogether. So it was that, on the return passage for example, one could only take observations on the bridge at a height of 13 m. (42 ft. 8 in.) because the ship had a deck cargo on which it was impossible to obtain a sufficiently firm footing.

The observations were always made sitting in a transatlantic deck chair, but unfortunately the big wheel-house did not enable one of these to be placed amidships. The other deck chairs for passengers, with elbow rests, were not suitable because their construction was too rigid and transmitted the vibration of the ship too strongly to the observer, which did not happen with the ordinary canvas transatlantic type. Care was also taken that the chair was placed as firmly as possible so that the upper part of the body should be absolutely free to be able to compensate a little for the ship's motion. The right arm rested lightly against the body. Some people had previously recommended leaning both elbows on the guard-rail and opposing one's movement, with the instrument, to that of the ship. It was not possible to steady oneself like this, both because of the excessive vibration transmitted, and because on the guard-rail one is nearly always exposed to the wind which, even when of relatively little force, lends so much liveliness to the instruments that it is impossible to bring the bubble into the position of rest.

With the continual movement of the ship, all five observers obtained the worst results with the WOLLASTON prism, and for this reason the observations were made without prism.

In normal temperatures, the bubble is a little larger than the image of the sun, and in consequence the latter could be brought over the reflected image of the bubble. At higher temperatures the liquid in the level dilates and the bubble consequently shrinks. During the observations in the harbour of Santa Cruz, the solar radiation for about an hour was so strong that the bubble almost disappeared; the true temperature of the air measured with the «Assmann» was actually 29.4° C. (84.9° F.), while the ordinary ship's thermometer showed 37.5° C. (99.5° F.) (1). The small tubular level with which the apparatus is fitted cannot be provided with a reservoir for these cases. In bubble sextants equipped with a spherical level, one can increase or decrease the contents of the level at will by means of a screw which acts upon the elastic bottom of the latter, which allows temperature influences to be eliminated.

Result of the Observations.

Allowing for the very unfavourable conditions for observation to which attention has been called above, the limit of error of all the observations taken individually comes out at ± 5', thus giving the expectation of a mean error of ± 3' for each series of observations. This value seems to correspond to the degree of accuracy that one expects in the ordinary practice of measuring the distance above the visible horizon. This limit of error was not to be exceeded if it were to be shown that the practical use of the bubble sextant was a possibility; also, under the circumstances, no allowance must be made for the angle between the azimuth and the direction of movement of the ship. But, from the first trials, it was evident that this was impossible; consequently as a rule we allowed for the azimuth and the ship's movement, without decreasing the value of the limit of error.

Starting from this point of view, we obtained the following Table for 600 observations, of which 75 % were taken at sea:

Difference.	± 0'	*up to* ± 5'	*Mean difference of all observations.*
At sea	8 %	50 %	7.9'
In harbour	12 %	74 %	3.2'

For the computations use was made of all the materials of observation (about 30 series) originating from five different observers; let us say, further, in favour of the bubble sextant that no selection or elimination was made of bad series or values. The observations intended beforehand for training were neglected, even when the results were good. (2)

(1) *Cf.* CASTENS. — Wüstenwinde, Kaltwasser-Auftrieb u. Nautik (*Ann. d. Hydr.* 1926, *p.* 22).

(2) *Graphs and details of results are not reproduced here* (*I.H.B.*).

The big differences above 10', and even above 20' in many of the cases, which one finds in nearly all the observations taken at sea, must in my opinion be attributed almost exclusively to the damping effect on the acceleration of the bubble, as already stated above ; it has in fact often enough happened that the very readings thought to be good ones at the moment of observation have turned out to be erroneous when subsequently worked out.

It has not been possible to verify any consistency in the distribution of the large and the small errors during the determined periods of observation ; on the contrary, the good and the bad measurements appear in the various series in totally different places, which exactly confirms the incalculable influence of the acceleration.

As already stated, in addition to the long serial observations, a certain number of isolated measurements were taken of the altitude of the moon, planets and fixed stars ; the result of the computations fell entirely within the framework of the experience acquired with the serial observations.

Thus, if it is desired to make serious use of the bubble sextant in practice, one must never, even under favourable conditions, take readings singly or in threes. On the contrary it is necessary to proceed as in trials on board S. S. *Arucas*, described above. The length of the curve of observations and the value to be expected must always be a function of momentary conditions. Size and loading of the ship, amount of motion on the ship, the angle between her course and the azimuth, besides the choices of the most favourable possible position for observing, are of decisive importance. It would eventually come to stopping the engines and modifying the course to correspond with the direction of the sea. For all the times of the series of observations one would then extract from nautical tables corresponding changes of true altitude, and would compare the series of changes of true altitude thus obtained with the observations. The parts of the curve which give the best agreement should contain the most correct results, which, after taking their average, are suitable for use. The result could, if the conditions for observation have been reasonably favourable, be used with the necessary precautions to fix the position by.

The results of these observations in the *Arucas* show that it is impossible and absolutely misleading to try and establish limiting values for the use of the bubble sextant, by indicating, e. g., a figure for the force of the sea or a degree of heel.

To sum up, it may be said that the bubble sextant on board small vessels, even in comparatively fair weather, does not fulfil the conditions which nautical practice requires of a measuring instrument which must, in case of need, be accurately used at difficult moments. But on board large ships also, even under favourable conditions of loading, its capacity for use will only be very limited, since the bubble of the level, at the least motion of the ship, acquires such an acceleration that the results of the observations cannot attain the degree of accuracy upon which one is entitled to insist. Even mounting the instrument on a universal joint, similar to those used with periscope sextants in aircraft (Opitz system) cannot eliminate the acceleration. The problem of freeing measurements of the altitude of heavenly bodies from the visible horizon is not, therefore, solved by the bubble sextant ; the same applies to the pendulum sextant which is subjected to the same influences to a similar degree. Even trials with a gyroscopic horizon will finally come to grief over acceleration. This is why investigation into the abolition of the horizon as a line of reference must be pursued in another direction.

The question still remains of knowing whether, at this period of rapid development of the wireless direction finder, the problem merits all this attention. As regards the merchant service, in any case, the answer is in the negative. A new instrument can only rouse interest here if it offers real advantages over the ordinary sextant ; further, it must be as easy and simple to handle as the latter and finally must not be more expensive.

For the Navy, on the other hand, which in certain critical cases cannot always count on wireless direction finding, the solution of the problem is of capital importance, as it is also for long distance air navigation. In transatlantic flights one still makes use, in these days, of astronomical fixes by distances above the horizon, as far as the meteorological conditions and the state of the sea permit, by coming down to the very surface of the water to take observations, particularly as one does not, in fact, give the preference to the bubble or pendulum sextant which, under the entirely different conditions of movement of aircraft, give better results in their case than in maritime navigation.

The long distance flights of the future will be performed at such heights that it will no longer be possible to come down to take observations ; it is then that an absolutely sure instrument will be indispensable, which would be independent of the visible horizon and of every influence of acceleration.

The success of our work has been due largely to the friendly help and support given me by Ministerial Counsellor Prof. Dr. WEDEMEYER and by my instructor, Captain RENNER, member of the Council of Studies at the Navigation School at Bremen ; to both of these I offer my warm thanks.