GLOBAL ORTHOTENY

New Pitfalls

by Dr. Donald H. Menzel

Aimé Michel, in the May-June, 1963, issue of the *FLYING SAUCER REVIEW*, briefly announced that "local" Orthotenic alignments previously discovered could be extended so as to constitute great world circle lines. Dr. Donald Menzel now attacks this concept and also criticises the further developments in orthotenic research surveyed by Jacques Vallée in the November-December, 1963, issue. Jacques Vallée replies in the article that follows Dr. Menzel's.

JACQUES VALLEE has presented some more statistics designed to provide further evidence in favour of the Straight-Line Theory. The study purports to derive formulas for calculating the great circles supposed to represent global orthoteny. Let T be the longitude at which the great circle intersects the equator and u the inclination of that circle to the equator. Then a point on that circle, with longitude L_i and latitude Φ_i, conforms to the equation.

$$\sin(T - L_i) = \cot u \tan \Phi_i \qquad (1)$$

This equation has two unknowns, T and u. Hence two points, giving two equations, serve to determine the great circle.

If, instead of 2, we have N points lying on or close to the line, the extra N-2 equations are redundant. Alternatively, we may find some way of averaging the points to derive the best possible great circle. Vallée applies the method of Least Squares for this purpose.

He makes the following substitutions:

$$\left.\begin{aligned} x_i &= \frac{\tan \Phi_i}{\cos L_i} \text{ and } y_i = \tan L_i \\ A &= -\frac{\cot u}{\cos T} \text{ and } B = \cos T \end{aligned}\right\} \qquad (2)$$

Then equation (1) becomes:

$$y_i - A x_i - B = t_i \qquad (3)$$

where t_i is the error if x_i and y_i do not lie exactly on the great circle.

This formula, however, gives artificially high weight to points near $L_i = 90°$, for which both the tangent and the reciprocal of the cosine go to infinity. I am sure that Vallée did not intend to give undue weight to the U.S. observations. His equations also give unduly high weight to observations from high latitudes.

To avoid both pitfalls, I should proceed as follows. Let

$$\sin \Phi_i = a_i;\ \cos \Phi_i \cos L_i = b_i;\ \cos \Phi_i \sin L_i = c_i$$
$$-\sin T \tan u = X;\ \cos T \tan u = Y \qquad (4)$$

Then, the equivalent of (3) is:

$$t_i = a_i + b_i X + c_i Y, \qquad (5)$$

and the sum of the squares of the errors becomes

$$S = \sum_{i=1}^{N} t_i^2 = \sum_{i=1}^{N} (a_i + b_i X + c_i Y)^2 \qquad (6)$$

Differentiating, to get the minimum of S, we have

$$\left.\begin{aligned} \frac{\delta S}{\delta X} &= 2 \sum (a_i + b_i X + c_i Y)\, b_i = 0 \\ \frac{\delta S}{\delta Y} &= 2 \sum (a_i + b_i X + c_i Y)\, c_i = 0 \end{aligned}\right\} \qquad (7)$$

We thus get two simultaneous equations to solve for X and Y, as follows:

$$\left.\begin{aligned} X \sum b_i^2 + Y \sum b_i c_i + \sum a_i b_i &= 0 \\ X \sum b_i c_i + Y \sum c_i^2 + \sum a_i c_i &= 0 \end{aligned}\right\} \qquad (8)$$

These equations apply for any value of the latitude or longitude. A slightly different set will be necessary when the inclination is nearly 90°. These equations are certainly preferable to those given by Vallée. However, the applicability of least squares to the problem is somewhat doubtful. For least squares to work, the errors, t_i, must be truly random. We have no assurance that this is so. For example, a random distribution would result if we used the line as a target and established the stations by throwing a dart. Nevertheless, as I have previously noted (earlier article), the global orthotenists will get the shock of their lives when they use these equations in a truly global sense.

For a short arc like the Bavic line, the equations are not sensibly different. But Michel has claimed that certain sightings in Brazil, Argentina, New Guinea and elsewhere are extensions of the Bavic line. I predict that the errors will be enormous when one tries to put a great circle through all the sightings.

Vallée further states that the distances between selected stations, divided by selected integers, give approximately the same figure. This new claim, in my opinion, is no more convincing than the other orthoteny "proofs". Experienced statisticians well know that, when a person starts to search for such relations, he can always find them, even in a series of purely random numbers. The streets of Las Vegas and Monte Carlo are paved with the hopes of gamblers who have had similar illusions.

THE MENZEL-MICHEL CONTROVERSY

Some further thoughts

by Jacques Vallée

TRANSLATION BY G.W. CREIGHTON

The author of this article is well known to readers of the *FLYING SAUCER REVIEW*. Jacques Vallée's scientific background is considerable and varied. He is a specialist in the field of electronic computers and is therefore well qualified to survey the statistics of Orthoteny and to draw authoritative conclusions. This article was received by the Editor on April 24, 1964.

THIS article is intended to serve two purposes: first, to answer Dr. Menzel's discussion of the method we have introduced for the computation of great circles and some attempts that were made to determine a fundamental interval along one of Michel's lines. Second, to clarify some points in the Menzel–Michel controversy which fall in the field of application of a method we have just recently developed and programmed for an IBM computer and which gives new indications about the statistical significance of the lines.

The first point is a purely unemotional, rational issue. This is not a discussion about the reality of the "objects" responsible for the reports, but a dispassionate debate in which very simple facts (the position of the observers on the ground) are examined: these points can be plotted on a map, their distances measured. Besides, no important discovery is at stake: never have I presented my "interval"* as anything other than an amusing phenomenon, and I have made no attempt to prove that chance alone could not explain it: Dr. Menzel does not either.

Methods to tabulate the great circles have been developed. But at no point in our article will Dr. Menzel find any allusion to the sightings in Brazil, Argentina or New Guinea, on which we have at the present time no real information, since their co-ordinates are unknown. The method we indicated has only been applied in a systematic fashion to French sightings of 1954. When it comes to the Bavic line, it certainly gives peculiar results. Of course, all it means is that six French towns are on a straight line (more precisely, on an arc of great circle) and obey a certain distribution with a certain precision. This does not prove or disprove that a material object of an unknown nature is the cause of the observed manifestations, but, if confirmed, it would indeed be a new indication in favour of the Straight-Line Theory and this perspective makes the hypothesis proposed in Michel's book worth investigating.

Precise French sightings

In order to check the predictions made about Global Orthoteny by Michel, one should introduce a system of weights, taking into account the precision with which the co-ordinates of each sighting are known, the number and reliability of the witnesses, etc. The equations we have presented could easily be modified to allow such a system to be introduced. However, we have found the information on the basic data insufficient for such a refinement to prove of significant value. We have limited our study to precise French sightings (in which case both indeterminacies mentioned by Dr. Menzel do not have to be taken into account; besides, he is obviously aware of the fact that great circles presenting values of u or T close to 90° can be found automatically and handled through separate approximations by the computer).

The Least Squares Method

If the descriptions made by the witnesses are all to be attributed to optical illusions and errors, then we should expect the points to be distributed at random on the French territory if we neglect the influence of the density of population, as can be done in the case of Bavic, and we can make the natural hypothesis that the errors are random: the Least Squares Method is thus justified, especially with a relatively small number of points in the data vectors.

The problem of the statistical significance of the lines and networks is more important. A new approach to the problem has been programmed and tested on an IBM computer. The method will be published elsewhere in detail, but we feel we should mention it here because it casts a new light on several points in the Menzel–Michel controversy.

Dr. Menzel and his opponents seem to come to an agreement about the use of Mebane's formula in the statistics. This is certainly a mistake: Mebane's formula would provide a good approximation in the case of random distributions of a small number of points over unbounded areas, but when it comes to a real problem in which the area considered is limited with respect to the order of magnitude of the precision or probable error, and is topologically complex, these ideal considerations give only vague indications. For the actual distributions we are considering (of the order of 30 points for Poncey and Montlevicq, the two impotant networks) the figures given by Mebane's formula are off by a factor of two. This factor makes the difference between the 19 found by Michel for his 3-point lines and the 37 computed: "almost twice as many as actually observed!" writes Dr. Menzel: he will find in figure 2 the true reason for this discrepancy. The number found by Michel is really in good agreement with a random distribution of that amplitude, using a distance criterion of 2.5 kilometres.

This problem of the distance criterion is the occasion of two other mistakes in Dr. Menzel's refutation of Orthoteny. If he wishes to use arguments like "On a number of maps, the width often reaches and occasionally exceeds ten miles" he should provide the reader with a table of actual distances between the points and the mean great circles. On such a table the width of his "corridors" would be apparent. In 1961 I computed the elements of all 65 lines mentioned in the French edition of Michel's book and then, considering a catalogue of my own, based on a new analysis of the French files (independent of Michel's study) I calculated the distances of all well determined points to all 65 circles. This was a matter of some 14,000 computations of distances, and it showed that Michel had certainly not "invented" or "rediscovered" sightings after plotting the lines, and that his networks were verified with a much better precision than originally claimed by him. I had not published these results at the time because I was well aware of the dangers involved in claiming that they proved Orthoteny to be true: they proved Michel's good faith, and they proved that the lines and the networks did exist. Whether they existed as a consequence of chance or as a consequence of decisions taken by intelligent "visitors" was another problem. Most of the maps appearing in Michel's book are still true with a precision better than the nearest mile, and almost all of them are still true with a precision of 2.5 kilometers: these are the networks and the lines I have considered in all subsequent researches.

Dr. Menzel's other mistake

Dr. Menzel seems to have made another mistake on the subject of the distance criterion when, after deriving "more simply" Mebane's formula, and stating that he agrees with it, he says that Mebane had not properly defined the "corridor". His own definition does not appear more convincing.

Suppose that A, B, C and D are four sighting points, and say that they are defined with the same probable error (figure 1). "Connect the two points farthest apart—by a straight line (AC)—then draw, parallel to this line, two other straight lines—on either side of the original line—if the third point falls in this corridor we shall say that the line is straight". Now let us consider figure 1 closely. ABC, says Dr. Menzel, is straight. Then what about ABD? Should not the researcher introduce the distances between the points on the lines as well as the lateral distances due to the probable error? Would experienced statisticians give the same weight to lines of very different lengths? Should the lines drawn on an area like France be given the same treatment as lines drawn on areas of a different topology, like Italy or Great Britain? France being a spherical area, it is incorrect to consider straight lines in a rigorous computation: the geodesics are arcs of great circles; in the usual system of co-ordinates the difference between the