I studied statistically simulated Poisson Dirichlet tessellations, in particular the point process of vertices of cells. Surprisingly we found that the corresponding second-order product density $\rho(r)$ has a striking form: it seems to be true that

$$\lim_{r\to 0} \rho(r) = \infty,$$

or, at least, $\rho(0)$ seems to be very great. Usually, such behavior of a product density is an indicator of a high degree of clustering. By visual inspection of some simulated tessellations we found that clusters of vertices in the usual sense of the word are not typical for these tessellations, but there appear frequently very short edges (of otherwise "normal" cells) or pairs of vertices very close together.

With respect to statistical shape problems related to "landmarks" in the sense of Bookstein (1978, 1986), I should like to ask the following question. Imagine three nonintersecting circles in the plane. Take a random point in each of the circles, for example uniformly or with respect to any distribution. Form the triangle having the three points as their vertices. Is it possible to give the corresponding shape density?

ADDITIONAL REFERENCES

BOOKSTEIN, F. L. (1978). *The Measurement of Biological Shape and Shape Change. Lecture Notes in Biomathematics* **24**. Springer, Berlin.

BOOTS, V. N. (1987). *Voronoi (Thiessen) Polygons.* GeoBooks, Norwich.

HANISCH, K.-H. and STOYAN, D. (1984). Once more on orientations in point processes. *Elektron. Informationsverarb. Kybernet.* **20** 279–284.

OHSER, J. and STOYAN, D. (1981). On the second-order and orientation analysis of planar stationary point processes. *Biometrical J.* **23** 523–533.

STOYAN, D., KENDALL, W. S. and MECKE, J. (1987). *Stochastic Geometry and Its Applications.* Wiley, Chichester.

Rejoinder

David G. Kendall

It is appropriate that Professor Bookstein should open this discussion in view of the importance of his work and the great influence that this has had through his own presentation in *Statistical Science* and his earlier 1978 monograph. I was already deeply involved in shape theory when I first read the latter, but did not at that time foresee how closely our two different and differently motivated approaches would converge. It is all the more valuable, therefore, that he has generously taken the time and trouble to survey their current interactions and differences of emphasis. His remarks will deserve careful study.

Professor Small's contribution is full of wise insights, and novel suggestions are made that I shall think about deeply. "Projection-pursuit" viewing of higher dimensional shape manifolds may well be a reality a few years from now. My current practice, not so technologically ambitious, is to try to understand these spaces as thoroughly as possible, and then to seek dimension-lowering projections that retain the important information and make it visible in a helpful way. One example of such a procedure will be found in my contribution to the discussion on Bookstein's 1986 paper referred to above. Of course I agree with the remarks that he and others have made about the advantages of having a variety of visual displays available. I recall that Kipling wrote a fine poem on a similar topic many years ago.

Professor Mardia's contribution was a shock to me because I did not expect to see so beautiful a solution as that found by Mardia and Dryden to the important problem they have studied. It makes one ask, why is it so beautiful? What has happened to all the horrible noncentral χ^2's? Of course the Gaussian distribution never ceases to spring surprises on us. I discussed Mardia's remarks with Wilfrid Kendall, and it occurred to us that a dynamic approach might at least "explain" what lies behind such a nice formula. So here are a few remarks intended only to illuminate the anatomy of the problem.

To start with it will be necessary to change the notation a little. We identify Mardia's κ with $s_0^2/(4c^2t)$, where c is a diffusion constant, t is the time elapsed during the interval considered and s_0 is a linear measure of the size of the triangle $\Delta_0 = (A_0, B_0, C_0)$ at the beginning of that time interval. The Mardia-Dryden formula then gives the law of distribution of the shape at the end of the time interval when we know what the shape was to start with. Notice that in this formulation it is no longer necessary to exclude $A_0 = B_0 = C_0$ as a possible initial shape, for then $s_0 = 0$, and this makes $\kappa = 0$, and then the Mardia-Dryden formula tells us that the distribution of size at the end of the interval is uniform over the sphere, as it ought to be.

More generally let us write $\zeta(t)$ for the shape of $\Delta_t = (A_t, B_t, C_t)$ at time t, this being undefined at

$t = 0$ if the size s_0 is then zero. We let the points A, B and C perform independent standard plane Brownian motions with no drift and with diffusion constant c, starting at A_0, B_0 and C_0, and we look at the situation after a positive time t has elapsed, when we have a new labeled triangle $\Delta_t = (A_t, B_t, C_t)$ with size $s(t)$ and shape $\zeta(t)$. Their problem was to find the law of distribution of $\zeta(t)$ for this given $t > 0$ when $\zeta(0)$ is given.

It is actually easier to think first about the stochastic motion performed by $(s(t), \zeta(t))$ on the size and shape space, which we know to be a cone with $\Sigma_2^3 = S^2(½)$ as unit section. The vertex of the cone corresponds to the situation $A = B = C$ (almost surely only possible when $t = 0$). This combined size and shape process is known to be a diffusion, and it is a skew product factoring into a Bessel-type process on the generators (for size), and driftless spherical Brownian motion on the (spherical) cross sections (for shape), that spherical Brownian motion being described at a random rate $d\tau/dt$ inversely proportional to the current squared size s^2 of the triangle. This decomposition was hinted at in my 1977 note, and happily we now have a thorough analysis by W. S. Kendall (1988) in *Advances in Applied Probability*.

The formal solution to the dynamic form of the Mardia-Dryden problem is thus to write down the law of distribution of $\beta(\tau)$ for given $\tau > 0$, where β is spherical Brownian motion on the sphere Σ_2^3 starting at $\zeta(0)$, and then for given t to integrate out the dependence of $\beta(\tau(t))$ on the elapsed random-clock-time $\tau(t)$, using what we know about Bessel processes.

The result, for a fixed $t > 0$, ought to be the Mardia-Dryden law when reexpressed in the new notation. In fact it turns out that this diffusion approach really does work, and I have now pushed it through to get a stochastic calculus proof of the Mardia-Dryden result. Details will appear elsewhere, and perhaps will suggest higher dimensional generalizations.

I am grateful to Mardia for taking up my remarks about Central Place Theory (which, until the statisticians began to interfere, did not seem to have much theory in it). I hope that we can follow up some of his suggestions together.

I am delighted that one of my collaborators of long standing has chosen to write on the general philosophy of the use of computer algebra in stochastic science. I have found it helpful to match complicated calculations with parallel simulations, in order to cover the risk of not detecting gross errors (say extra factors of 2), and because such a practice can alert one to aspects of a problem that have been overlooked. With the arrival of computer algebra we have a second such "automatic colleague" skilled in the development of asymptotic formulae, able to tell at a "glance" whether two monstrous expressions are indeed equivalent, able also (with some persistent and inspired prodding) to "simplify" expressions and so forth. Wilfrid Kendall has now taken us a stage farther along that road, pointing out that computer algebra can be trained to be a good sniffer-out of unsuspected structures, although emphasizing that like all good colleagues it will make its most fruitful discoveries when supplied with well chosen hints. And if the hints turn out to have been unhelpful one can try again, knowing that computer algebra can wipe out the past at will, and so need not be prejudiced by false starts. What can be done when computer algebra is teamed up with a human expert is well shown in his research into shape diffusions, and we are lucky that he has documented the "learning" route with such helpful comments, and so supplemented in a most valuable way the necessarily terse style of the manuals.

Professor Watson rightly reminds us of the antiquity of geometrical stochastics in the sense that some specific problems ante-date its present general formulation. One needs to keep a sense of proportion when writing the history of a mathematical topic. Sometimes one is left wondering whether anything at all is really new. But this is an over-reaction; the problems have always been there, and have provoked reactions from time to time, but in most cases it has taken decades if not centuries for the language to have been developed in which to pose such questions in their natural form. Thus, Woolhouse in 1863 calculated for arbitrary a and b the chance that three points iid uniform in a rectangle of sides a and b will be the vertices of an acute-angled triangle. At first this seems like a demonstration that what we here call shape theory existed 125 years ago. But that this is a mistaken view is made clear when one notices that the article just quoted forms one of a series of papers by various writers including a "proof" that three points taken iid "uniformly in space" have a zero chance of forming an acute-angled triangle! Of the numerous valid variants of that improper problem today, the most attractive is: three Brownian particles set out at time $t = 0$ from a given point in the plane; show that at any given later time t there is a chance ¼ that they form the vertices of an acute-angled triangle. The proof takes just one line—but it makes use, implicitly or otherwise, of several branches of mathematics developed during the last century.

I am relieved that Watson finds shape theory difficult. So do I, and until his reassuring remarks I thought that this was merely a reflection of my own antiquity. He will be happy to learn that the first (a basic part) of the forthcoming book will be really elementary, being based on a lecture I have to give to school children later this year. (Of course "elementary" does not necessarily mean "uncomplicated.") I agree that it is helpful to have several different sets of shape coordinates, because different sets are convenient for different purposes. But two basic facts should not be forgotten. If we wish to keep to a metric directly related to the procrustean distance, then there is no question about it; we have to use the spherical representation for three points in two dimensions, and the appropriate complex projective representation for k points in two dimensions. If on the other hand we are primarily interested in (say) the possible occurrence of "hot spots" in the distribution on shape space, then we can use any convenient representation diffeomorphic (but not necessarily isometric) with the standard one provided that the "null" distribution as seen in a plotted simulation looks sufficiently nearly uniform in the relevant region. To indicate something of the variety of possible visual displays, here (Figure 1) is a collection of triangle shapes each one of which sits at its proper position on the sphere $S^2(\frac{1}{2})$, and here (Figure 2) is the distribution on that shape space of a large iid sample from a plane Gaussian law with $\sigma_2 = 5\sigma_1$. In each case the picture is to be interpreted as a three-dimensional one, and the viewer must bear in mind that, to the untutored eye, a uniform law on the surface of a sphere looks as if there were an enhanced density near the rim of the sphere. This reminds us that a proper education of the eye is essential to good practical geometric statistics. In this example the "hot belt" around the collinearity locus is easily recognizable, and is a genuine (and scarcely surprising) consequence of the fact that we made $\sigma_2/\sigma_1 = 5$.

I am fascinated by Watson's problem of the cubic with random complex coefficients $c_r = a_r + ib_r$ where all a's and b's are iid Gaussian. How about starting with a nice large simulation, the shape of each triangle-of-roots (z_1, z_2, z_3) being displayed on $S^2(\frac{1}{2})$ as above? It might be worth plotting separately those root triplets having different graded values of the size

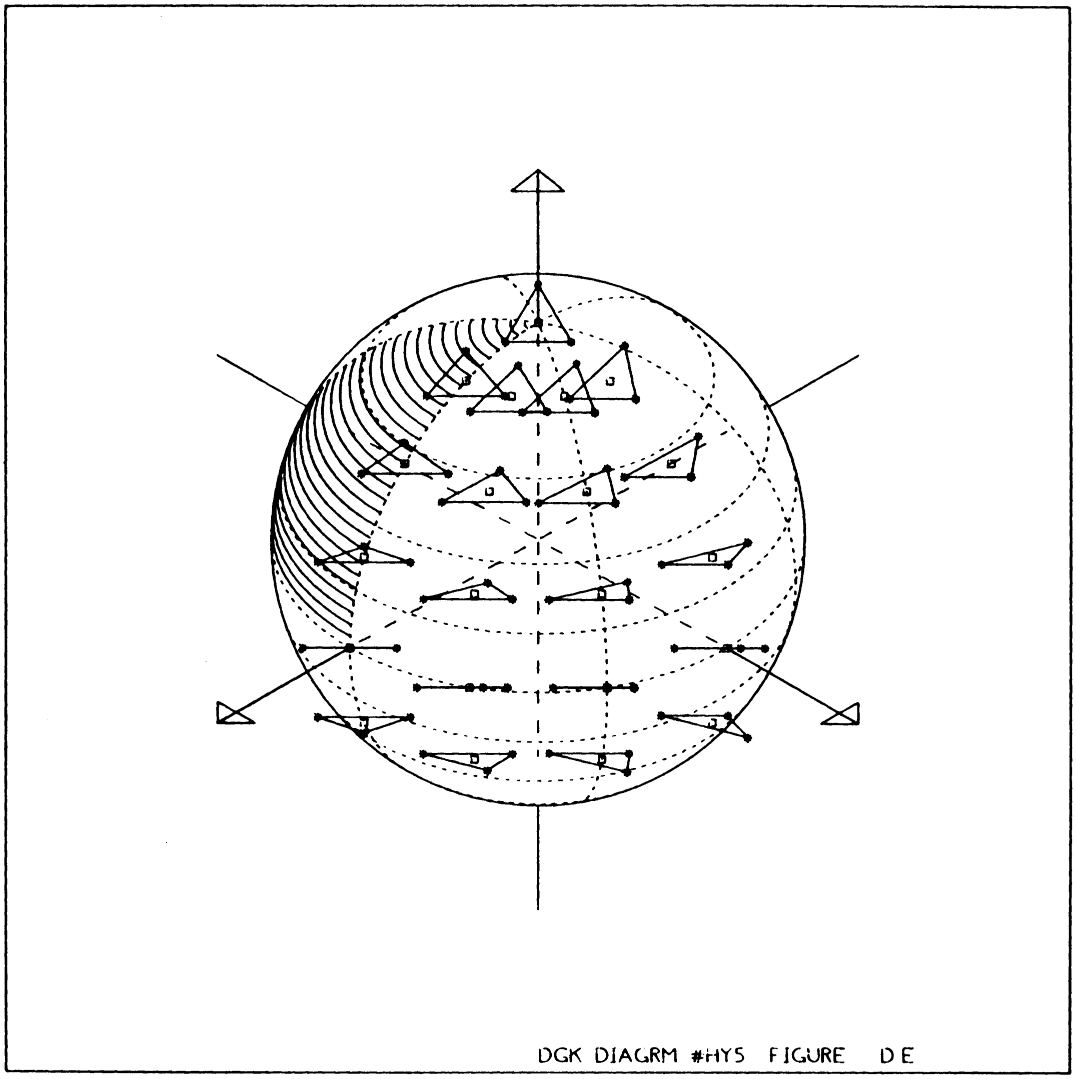

FIG. 1. *Some triangle shapes at home in the shape space. The shaded half-lune is the customary basic region.*

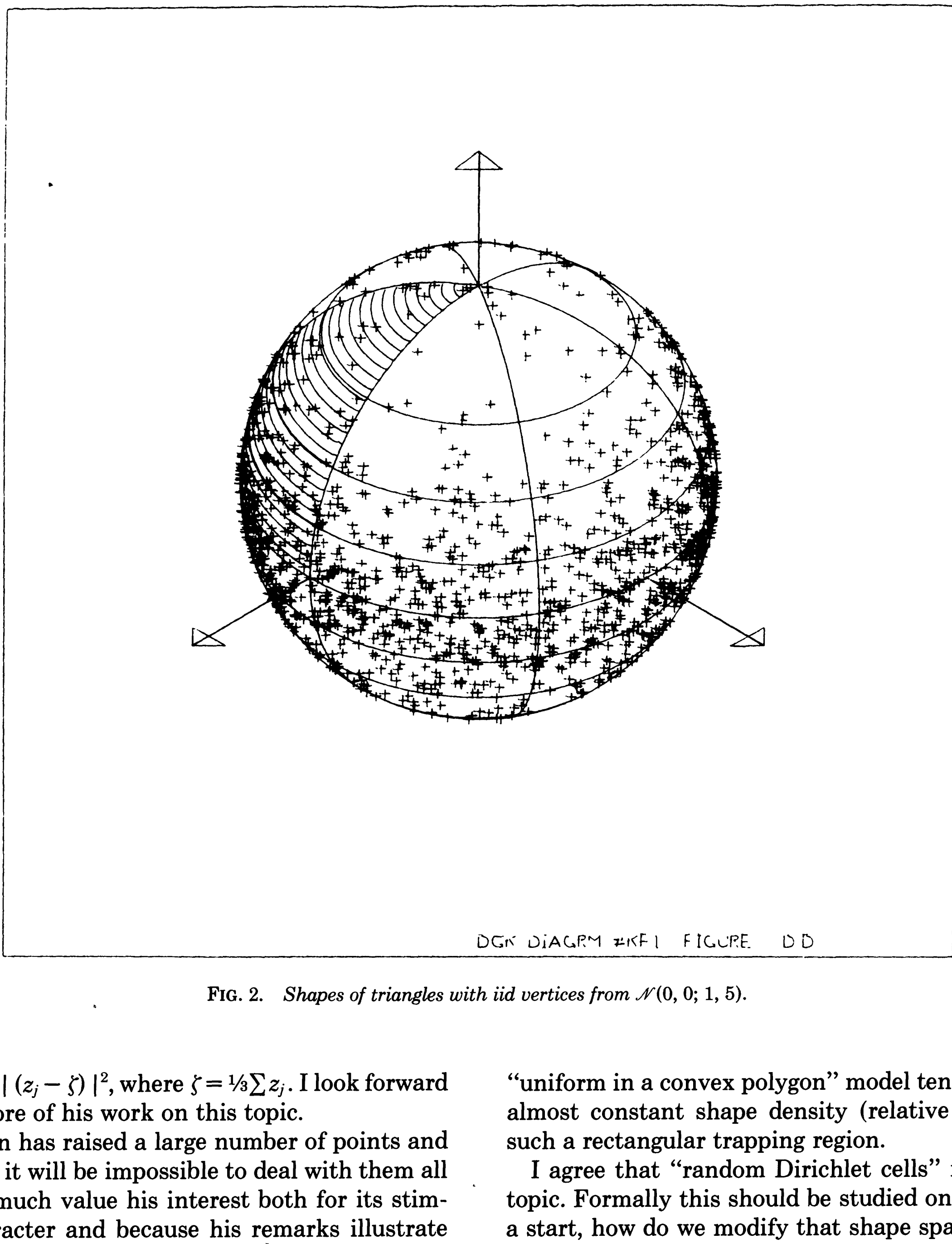

FIG. 2. *Shapes of triangles with iid vertices from $\mathcal{N}(0, 0; 1, 5)$.*

variable $\sqrt{\sum} \mid (z_j - \zeta) \mid^2$, where $\zeta = \frac{1}{3}\sum z_j$. I look forward to seeing more of his work on this topic.

Dr. Stoyan has raised a large number of points and queries, and it will be impossible to deal with them all here, but I much value his interest both for its stimulating character and because his remarks illustrate well the splendid work in stochastic geometry by Stoyan and his colleagues in the DDR. Solutions to some of these problems using the powerful techniques of stationary point processes will certainly be useful, although we must of course remember that few things in life are really stationary. With regard to his suggestion about the rhomb, my preference is for a "trap" such as is shown in Figure 3. This is designed to catch near collinearities although excluding those that are better described as near coincidences (of two of the three vertices). It can conveniently be employed with the (x, y)-plots used by Huiling Le and myself, particularly because from her work we now know that the "uniform in a convex polygon" model tends to give an almost constant shape density (relative to *dxdy*) in such a rectangular trapping region.

I agree that "random Dirichlet cells" forms a rich topic. Formally this should be studied on CP^∞, but as a start, how do we modify that shape space so that it contains only the shapes of the convex labeled polygons? It seems worth remarking that the Dirichlet cell is more nearly dual to the whole collection of Delaunay cells that meet at a point—and as I have remarked in the paper, we know little about that at present. Another approach would be to condition the shape of the Dirichlet cell on the number of its vertices.

For the last problem proposed in Stoyan's contribution one should point to the work of Mardia and Dryden referred to elsewhere in the discussion and in this reply. It appears that the switch from disk distributions to Gaussian ones simplifies the question

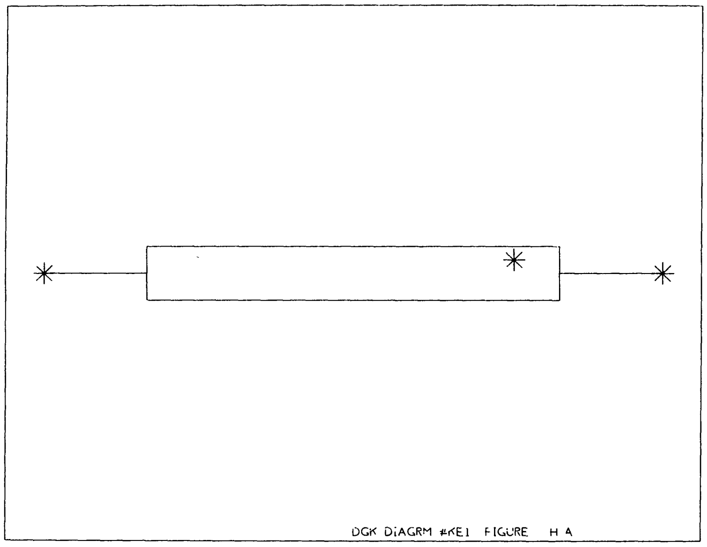

FIG. 3. *A trapping region for collinearity testing (not to scale).*

dramatically, and perhaps this alternative model would be equally appropriate for his purposes.

Finally I should like to add a special word of thanks to Morris DeGroot for his invitation to me to write this paper, and for his conviction, now splendidly vindicated, that it would generate a lively and interesting discussion.

ADDITIONAL REFERENCES

BOOKSTEIN, F. L. (1978). *The Measurement of Biological Shape and Shape Change. Lecture Notes in Biomathematics* **24** Springer, Berlin.

WOOLHOUSE, W. S. B. (1864). Problem 1361. Math. questions and their solutions. *The Educational Times.* 131–137.