been shaken in this opinion by the lack of a single example of such a rope device in the wealth of well-preserved Egyptian artifacts; by its absence from any tomb paintings or other paintings; and by the failure of any extant Egyptian mathematical writings to describe such a device or even to intimate its existence. Did the megalithic men leave any direct evidence that they understood Pythagorean triangles? No.

Determining the mathematical capabilities of the megalithic builders by examination of the stone rings is comparable to deciphering an unknown language from ancient inscriptions. There is no evidence that the mathematics of the megalithic builders is similar to the mathematics of the Greek tradition which still forms the basis of our mathematical training. Without a mathematical Rosetta stone, all attempts at understanding the megalithic mathematics are merely conjectural. Discussing decipherment of unknown languages, Johannes Friedrich gives an incisive warning (*4*): "I must state once again the fact, self-evident and trite as it may be, that the decipherment of any unknown script or language presupposes the availability of some clue or reference; *nothing can be deciphered out of nothing.* In those cases where one has absolutely no possibility available to link the unknown to something known, the amateur can give free rein to his imagination, but no real or lasting result can be accomplished."

I agree with Cowan that "perhaps much remains hidden in these remarkable sites." It is to be hoped that archeological investigation will reveal real evidence of the motivations and methods of the builders. Until such evidence is uncovered, I will continue to look upon such exercises as Cowan's as amusing games, and to view with admiration and awe the megalithic men who conceived and built the sites for their own personal, and still unknown, reasons.

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References and Notes

1. T. M. Cowan, *Science* **168**, 321 (1970).
2. See, for example, A. Thom, *Megalithic Sites in Britain* (Oxford, London, 1967).
3. I am a practicing geometer, and I find quite a few mathematical things more intuitively satisfying.
4. J. Friedrich, *Extinct Languages* (Philosophical Library, New York, 1967), p. 151.

9 July 1970

Fungal Archimedean Spirals

Bourret *et al.* (*1*) presented examples of beautifully regular spiral and concentric patterns of zonation in plate cultures of two fungi. Especially interesting is their figure 2, which shows that the space between bands of spores is constant even when a double spiral arising from two spores is present. As Bourret *et al.* point out, in double spirals every other band originates from one of the two mycelia and the spacing between these alternate bands is twice that between bands of a spiral originating from a single spore. If the banding is an expression of an endogenous rhythm, then in the case of double spirals, the period of this rhythm is exactly twice that in single spirals. This seems extremely unlikely. The constancy of the spacing between bands points to their origin in changes in the medium resulting from mycelial metabolism.

Banding patterns and concentric rings are known in bacterial cultures and are considered to be the result of progressive exhaustion of single components in the medium and chemotaxis in the chemical gradient thus produced (*2*). A similar explanation, that invokes changes in concentration in the medium which affect sporulation might account for the banding patterns of *Nectria cinnabarina* and *Penicillium diversum.*

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References

1. J. A. Bourret, R. G. Lincoln, B. H. Carpenter, *Science* **166**, 763 (1969).
2. J. Adler, *ibid.* **153**, 708 (1966).

6 April 1970

The colonies that produced double spirals as described in our report were not composed of "two mycelia," but of one mycelium which arose from a single spore, not "two spores." Thus, in the case of the colony producing a double spiral, all the bands are produced by one mycelium. When viewed along a radial transect, the period of the rhythm is the same as in the mycelium that manifests the rhythm as a single spiral. Whether this or any other biological rhythm is entirely endogenous has not been resolved, but the experimental evidence to date seems to justify our use of the term.

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7 August 1970

Somatic Cell Mating in Frogs

Volpe and Earley (*1*) have proposed that hybrid cells can be demonstrated in bone marrow cultures of diploid-triploid parabiotic chimeric frogs. Their evidence is the observation of two pentaploid metaphases in cultures from one of the 22 individuals examined; the analysis of one of these is presented as figure 2 in their report and is here reproduced (Fig. 1). If a curved line of regular contour is drawn as shown in the figure, it divides the chromosomes into two groups: A ($2n = 26$) and B ($3n = 39$). These two chromosome groups are euploid sets. The group of origin of the homologs numbered 1 through 5 in the photokaryotype may be determined from the intact spread: (1) AABBB, (2) BBBAA, (3) BBABA, (4) BBAAB, (5) BBBAA. The order given is that shown in the lower part of the figure. The five examples of chromosome 10, which bears the secondary constriction, have the following origins: AABBB. The small chromosomes 6 to 9 and 11 to 13 have not been analyzed in this way, but of these 35 chromosomes, 21 are found in group B and 14 in group A.

It is therefore reasonable to conclude that groups A and B are diploid and triploid metaphases that lie close enough together on the slide to produce an artifact. Such instances of interference can usually be recognized by differences in staining or degree of compaction between chromosomes of the two groups, but in this case the two groups are remarkably similar. One reason for concluding that a chromosome spread is a hybrid metaphase is the random position within it of chromosomes of differing origin. Since this "pentaploid" is so clearly an artifact, the claim for cell hybridization, which is the basis of the report by Volpe and Earley (*1*), is not supported by the cytological evidence they present. It would be of great inter-

Fig. 1. Chromosome spread presented as a pentaploid by Volpe and Earley (*1*). A line has been added which clearly separates the chromosomes into a diploid group A and a triploid group B. Lettering over the photokaryotype shows the origin of three homologs from B and two from A in each case analyzed.

est if they would present the karyotype analyses of the haploid cells also observed in their study.

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Reference

1. E. P. Volpe and E. M. Earley, *Science* **168**, 850 (1970).

25 May 1970

Freed acknowledges that an artifact created by two juxtaposed metaphase spreads can be "recognized by differences in staining or degree of compaction between chromosomes of the two groups." Assessing the features of the pentaploid plate illustrated in our report (*1*), Freed allows that the two groups—the diploid and triploid components—are *"remarkably similar"* (italics ours). The remarkable manner in which the chromosomes of the two sets match in a common pattern, we believe, cannot be dismissed as merely coincidental.

A searching question to be asked by an interested but impartial observer is: What is the probability of bringing together two cells in precisely the same stage of mitotic activity such that the members of the two sets of chromosomes are contracted identically? A companion, perhaps more incisive, question is: What is the probability of a diploid set and triploid set being positioned so perfectly to each other, or becoming so closely knit, without a single member of the diploid complement of 26 chromosomes either touching, or coming to rest upon, any one member of the triploid complement of 39 chromosomes? Each probability is extremely small; the two taken together comprise a chance phenomenon that would be incredibly rare.

The exceedingly rare event of perfect apposition of two uniformly contracted discrete sets of chromosomes must be weighed, admittedly, against the slight chance that the chromosomes of a given set would be aligned along the metaphase plate in such a way as to pass to one side (as delineated by Freed) when the chromosome spread is prepared by the investigator. This latter low probability would be a source of immense concern if it were not for two considerations. The first consideration is that the other (or second) pentaploid metaphase mentioned in our report (*1*) shows a more random distribution of the chromosomes of diploid and triploid origin. The second consideration is that the so-called complete diploid complement (set A) delimited by Freed is, in fact, an irregular, or quasi, complement. Freed's analysis covers only a select number of chromosomes, specifically Nos. 1 through 5 and No. 10. When the analysis is carried to completion, it is apparent (Fig. 1) that the smaller chromosomes (Nos. 6 through 11, excluding 10) do not follow the neat scheme suggested by Freed for the larger chromosomes. Attention is directed especially to the No. 7 chromosome, all five of which fall into Freed's triploid arena (set B).

The foregong discussion is not fatal to Freed's position. In the final analysis, neither we nor Freed have escaped from reliance on direct observation. We still

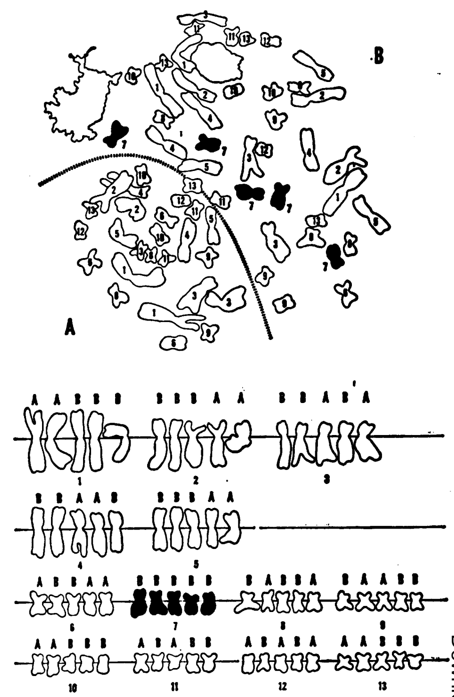

Fig. 1. The identification of the individual chromosomes of the pentaploid spread in the study by Volpe and Earley (*1*). It is not possible to assign all the appropriate homologs to either the diploid set (A) or to the triploid set (B) demarcated by Freed. The No. 7 chromosome is highlighted to reveal that all five homologs fall in the triploid group (B) delimited by Freed.

cannot elude the fundamental limitation of the observational method—namely, the subjective comparison of phenomena. We can hope, however, that in due course we can verify or disprove our ideas as more data accumulate or as those data that we do have are better understood.

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Reference

1. E. P. Volpe and E. M. Earley, *Science* **168**, 850 (1970).

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Composition of Avian Urine

Folk's approach to the study of avian excreta is original and refreshing (*1*). He rejects the idea that nitrogen is excreted by birds mainly as uric acid or urates, but this conclusion is not warranted by the data he presents. His observations were of the white powdery component of the dried excreta; but it is not proper to call this material urine, as Folk does, without also pointing out the likelihood that the urine was mixed with fecal material and intestinal fluids after it entered the cloaca from the ureters and before it had been voided (*2*). The urine from which this white powdery material is partly derived is a

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