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

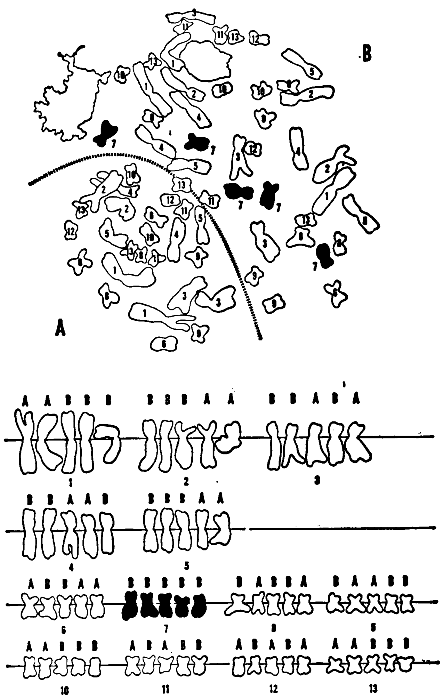

Fig. 1. The identification of the individual chromosomes of the pentaploid spread in the study by Volpe and Earley (*1*). It is not possible to assign all the appropriate homologs to either the diploid set (A) or to the triploid set (B) demarcated by Freed. The No. 7 chromosome is highlighted to reveal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

16 July 1970 ■

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 complex and variable mixture of water and organic and inorganic materials. Organic constituents include uric acid, urea, ornithuric acid, creatine, and mucoid substances (*3*), while inorganic constituents include Na, K, Mg, Ca, Cl, and phosphate (*3, 4*).

Folk's observations are consistent with this known chemical complexity of avian urine, and he is correct to point out that the white powdery part of the dried excreta may not consist mainly of uric acid and urates. He is not correct to deny, on the basis of the data reported, that the white crystalline material suspended in the watery part of avian excreta consists largely of uric acid and urates. The observation that 20 to 90 percent of the urinary spherules dissolved in weak acid and immediately recrystallized to form crystals resembling uric acid tends to support the idea that a major part of the urinary solids is indeed uric acid.

Folk's data do not contradict the statements in the literature that 50 to 80 percent of urinary nitrogen is excreted in the form of uric acid (*3*), nor do they contradict theories based on this fact.

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References

1. R. L. Folk, *Science* **166**, 1516 (1969).
2. E. Skadhauge and B. Schmidt-Nielsen, *Fed. Proc.* **24**, 643 (1965); E. Skadhauge, *Comp. Biochem. Physiol.* **23**, 483 (1967); ——, *ibid.* **24**, 7 (1968); B. R. Nechay, S. Boyarsky, P. Catacutan-Labay, *ibid.* **26**, 369 (1968).
3. P. D. Sturkie, *Avian Physiology* (Comstock, Ithaca, N.Y., ed. 2, 1965); C. L. Prosser and F. A. Brown, Jr., *Comparative Animal Physiology* (Saunders, Philadelphia, ed. 2, 1962).
4. E. J. Willoughby, *Comp. Biochem. Physiol.*, in press.

17 February 1970

1) The white material I have examined is the solid that forms the bulk of avian urine and is indeed the main form of excreted nitrogen [see many references cited in my original report (*1*)]. To be sure, it is suspended in water and some intestinal fluids, otherwise evacuation would be awkward. I have been very careful not to sample fecal material, which can easily be distinguished. Although bird feces are an interesting petrographic study in themselves, as a geologist I do not feel I can crane my neck out any further into this distinct field.

2) The white material is alleged by biologists to be mainly uric acid. It is not, because the tiny spheres are soluble in dilute acid (uric acid is insoluble in acid); it is not, because the spheres do not give the x-ray diffraction pattern of uric acid. The cardinal virtue of this technique is that it "tells it like it is" without altering the real nature of the material. It is simply ducking the question to repeatedly cite older authoritarian works that have never used x-rays. One should not be gullible or swallow any data coming from wet analysis of this highly reactive material. I suggest that biologists emulate the work of Denning *et al.* (*2*), who verified the presence of uric acid in a saliva stone by both x-ray and petrographic methods. I have never rejected the idea that the spheres may consist of mixed urates—Willoughby to the contrary—and am only arguing against insoluble uric acid per se as an important constituent.

3) I cannot understand the logic of Willoughby's second paragraph.

4) A reprint recently sent to me by Gibbs (*3*) reports very interesting experiments that offer a flicker of hope toward solving the mystery. He found that warming fowl urine caused the contained solids to dissolve, and he also made uric acid gels. Finally, Gibbs found that fowl blood "contains uric acid in a specially soluble form." I believe that Gibbs's "uric acid" was some other acid-soluble nitrogenous compound (perhaps the same one that is actually secreted by the kidney and excreted by the cloaca), and this explains its curious behavior.

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References

1. R. L. Folk, *Science* **166**, 1516 (1969).
2. R. M. Denning, I. M. Blatt, W. M. Mikkelsen, *Ann. Otol. Rhinol. Laryngol.* **67**, 1022 (1958).
3. O. S. Gibbs, *Science* **70**, 241 (1929).

19 March 1970

Fluid Pressure Variations and Prediction of Shallow Earthquakes

Evidence has accumulated recently showing that shallow earthquakes can be triggered by increases in fluid pressure induced by man. An implicit corollary seems to follow: tectonically induced increases in fluid pressure probably constitute an important mechanism for triggering shallow earthquakes, as suggested by Nakamura (*1*). It may be possible to predict where and approximately when some earthquakes are likely to occur in seismically active areas by continuously monitoring variations in fluid pressure in deep wells.

In 1965, Evans (*2*) showed that there was a spatial and temporal relationship between a series of 710 minor earthquakes and the injection of waste water into fractured basement rocks through a deep well located at Rocky Mountain Arsenal, northeast of Denver, Colorado. Evans postulated that the earthquakes were triggered because the fluid pressure at depth was increased sufficiently to reduce the frictional resistance of the basement rocks to faulting. Subsequent investigators confirmed Evans's observations (*3*) and concluded that the release of stored tectonic strain was triggered by the injection of fluid into the basement rocks (*4*). Although this conclusion continues to be questioned (*5*), several other instances have been cited recently in which causal relationships between increases in fluid pressure induced by man and earthquakes seem clear (*6*). In some cases the increases in fluid pressure have been caused by the injection of fluids into deep wells, and in other cases by rising reservoir levels.

Significant tectonically induced variations in fluid pressure are commonly associated with earthquakes and are manifested by increased or decreased flow rates of springs and creeks and by fluctuations in the production rates of oil, gas, and water wells. In general, however, it has not been possible to establish exactly when the fluid pressure variations occur with respect to the time of the earthquakes, except in a few cases in which flow rates or well-casing pressures were measured continuously (*7*).

An interesting example in which tectonically induced increases in fluid pressure may have triggered a major aftershock is suggested by the behavior of oil wells in the Tejon Ranch area, about 7 miles (11.2 km) from the epicenter of the main earthquake in Kern County, California, in 1952 (Richter magnitude = 7.7). Casing pressures increased to as much as ten times the