Dr. Bisset's criticisms of the techniques developed and utilized in these studies[1-3] may perhaps best be answered by considering each aspect of the procedure separately.

(1) The procedure includes acid hydrolysis of the cells being processed in the same manner as that used in the Feulgen stain. The stain used is either thionine or methylene blue *A*, both of which in the presence of sulphurous acid form complexes which react specifically to aldehydes in the same manner as the Schiff reagent, except that no leucobase is formed by the dye in the presence of sulphurous acid. In the case of thionine, by carefully balancing the amount of sulphurous acid added, it is possible to obtain a thionine–sulphurous acid complex and still maintain a percentage of unreacted thionine which is capable, in this mixture, of acting as a basic dye and staining other cell elements; in other words, is capable of acting as a counter-stain. It is because of the excess of this thionine that it has been possible to stain elements within the nucleus other than the chromosomes, such as the centrioles and the spindles. Similar results can be obtained by counter-staining Feulgen or azure *A*-SO_2 stained preparations with fast green.

(2) The dehydration procedure utilized involves the instantaneous freezing of the preparations in absolute alcohol at temperatures below — 50° C. Under these conditions no distortion of the cell occurs, due either to collapse of internal structure or by the formation of large ice crystals. Freezing-dehydration techniques, utilizing vacuum apparatus, have long been used in critical cytologic work. In the present procedure the water is removed from the cell in the frozen state directly into solution in the alcohol, and as this proceeds the cell is hardened by the alcohol. Cytochemically, it seems unlikely that these procedures should be subject to criticism.

As for the errors of interpretation which Dr. Bisset is ascribing to us, it seems proper that the evidence should be presented so that the photographs, which are included with the original papers, may speak for themselves. Reference should be made to these photographs by those interested. It might well be that if the criticizer were to apply himself diligently to the methods which he criticizes, and were to compare these new procedures with the older techniques which he uses, he, too, might be convinced of their superiority. Students at the Cold Spring Harbor Summer Session, 1951, under the direction of myself and a qualified assistant, produced entirely acceptable preparations when the procedure was followed in detail.

Concerning the criticism of the mitotic process as described in cocci, it should be pointed out that the nuclear activity as described by myself[4] is distinct from cross-wall formation, which has been demonstrated to occur by means of a cell plate in cocci, as in the onion root tip. If the structures which Dr. Bisset considers to be subjective are actually subjective, it is remarkable that it is possible to photograph them. It should be pointed out further in connexion with the cocci that, on the basis of the techniques used, I consider cocci to be unicellular organisms, and each cell to be uninucleate.

Dr. Bisset is in error when he states that the interpretations which my associates and I[5,6] have presented are errors common to the past, because only two examples occur in the literature to our knowledge in which mitosis has been specifically described in bacteria. The first of these, by Vedjovsky[7], has been criticized, not because of its validity in terms of observation, but because the organism that Vedjovsky studied was probably not a bacterium, but a protozoon. The second and more recent work by Chance[8] using special techniques, gives what appears to be an unequivocal demonstration of mitotic spindles in bacteria. The reader might also refer to Fig. 21, left, of Bisset's recent book[9] for a passable example of a mitotic spindle in a bacterium.

Finally, Dr. Bisset considers the observations presented for the occurrence of fusion tubes in *Bacillus megatherium* to be, in his opinion, "equally baseless". He considers that the conditions under which such fusions are observed to occur are abnormal for this organism. It is perhaps worth while to emphasize that the factor which apparently induces this condition occurs in a sub-fraction of Cohn's Fraction 3 of human serum as well as in casein, and that Braun has similarly demonstrated the re-direction of biochemical pathways by similar blood fractions in other organisms. In addition, it should be pointed out that the work of Hawker[10] and Buston and Basu[11] in Britain, and the work of Cantino[12] in the United States, indicate that sugar metabolism related to the occurrence, growth and sexual reproduction in certain fungi involves complex biochemical problems which are just beginning to be evaluated. The present work on *Bacillus megatherium* tends to concur with such studies of Hawker and others, and it might be better for Dr. Bisset to allow the evidence to accumulate and be evaluated before it is condemned.

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"The Liquid State"

The informed and generally favourable review of the second volume of my book, "An Advanced Treatise on Physical Chemistry", in *Nature* of January 12, p. 46, remarks on the absence of a treatment of the electrical properties of liquids. Optical and electrical properties both of liquids and solids have been deferred to a later volume. It may also be mentioned that the theory of liquids is taken up in relation to properties in parts of the book other than the first section, which might not be clear from the review.

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