The Nature and Function of Golgi Bodies.

PROF. GATENBY says (NATURE, Mar. 24): "Prof. Walker's original position was that *both categories of the cytoplasmic inclusions* [mitochondria and Golgi bodies] *are artefacts*. In his printed paper he merely claims that the Golgi bodies alone are artefacts." My "position" was, continued to be in my "printed paper," and still is, that *some of* the bodies claimed as mitochondria in fixed material are artefacts; and that those among the appearances claimed as Golgi apparatus, the presence of which in the cell cannot be accounted for without assuming the existence of this peculiar "organella," as Prof. Gatenby calls it, are artefacts. Thus it would seem that either he has not read what I have written, or that he is misquoting what he has read.

Both Prof. Gatenby and Dr. Ludford make much of the constant positions taken up by the Golgi apparatus in similar cells. Our knowledge of the chemistry of the cell is so limited that it is impossible to explain in many cases the position taken up by the separated lipins in different cells. I have, however, nearly ready for publication, an account of conditions in which my Golgi bodies may be made to take up a constant position in relation to the artificial nuclei in my mixtures, conditions which may well be produced by the nucleus of the living cell in certain circumstances.

I believe that all microscopic observations upon fresh cells removed from the living body of the multicellular animal should be accepted with caution. Changes must begin at once even under the most favourable conditions. It does not appear to me that the technique of the demonstrators of the Golgi apparatus "takes into account our biochemical knowledge of the solubilities of lipoids and other subtle cell bodies" as Prof. Gatenby says they do. The facility with which the colloids of the cell, more particularly perhaps the lipins, may be separated from each other and from the water with which they are associated, seems to be entirely ignored. Temporary separation might well occur under normal cell conditions, which would be rendered permanent by certain abnormal ones.

The formation of artefacts has been much neglected during recent years. They constitute perhaps the most dangerous pitfall for the cytologist. I cannot enter into the theory and practice of fixation and other processes, but for Prof. Gatenby's information would point out that maceration of material, among many other modes of treatment, is sure to distort the cells and their contents very seriously. What Prof. Gatenby would have us believe is "modern cytological technique" seems to be largely confined to those who demonstrate what he calls "cytoplasmic inclusions" and to neurologists.

If I were, as I certainly am not, the only cytologist living who did not believe in the "Golgi bodies," I fail to agree with Prof. Gatenby that this in itself would be a proof that I was wrong. Much greater men than he or I have occupied that invidious position in the past, and have proved right in the end.

May I point out that I still seek in vain for even a hint as to the function of the "Golgi bodies," which are assumed to exist as definite structures in all the cells of all animals and many plants. Also I have failed to extract information as to what becomes of the lipins in the cells that are treated by the "Golgi apparatus" methods.

I did not write of the archoplasm but of structures contained in it, and pointed out that these had been found in every animal investigated. Dr. Ludford's definition of the Golgi apparatus as a specialised area in the cytoplasm does not seem to agree with Prof. Gatenby's "batonettes." I would refer them both to D. Tretjakoff's paper, *Zeits. f. Zellforschung u. Mikro. Anat.*, Abt. B, 7 Band, 1 Heft., Feb. 1928.

Prof. Gatenby's naïve attitude that the value of his own interpretation of observations is increased by frequent repetition, and that he has reached the limits of cytological technique, reminds me of the Bellman in "The Hunting of the Snark."

> "Just the place for a Snark! I have said it thrice:
> What I tell you three times is true."

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Mar. 23.

The Inner Photoelectric Effect with Silver Halides.

CONSIDERATION of the energy steps in the Born cycle for calculating the lattice energies of the silver halides leads to the following conclusions:

1. The photolysis of solid silver halide might occur *directly* to give halogen and metallic silver. This requires only a quantum at 8000 A. or longer, equivalent to the heat of formation of the halides from the elements.

2. This photolysis might occur *indirectly*, by way of separation of electrons from halide ions, followed by acceptance of the electrons by silver ions. This course would require (1) a quantum at $\lambda = 1300$ A., or beyond, to disrupt the lattice, then a quantum at $\lambda = 3000$ A., or beyond, to liberate electrons. In subsequent reactions, of electron acceptance, etc., energy might be freed, either as radiation quanta or by radiationless collisions, so that the final difference of energy equals the heat of formation. However, in this case (2) primary absorption of two quanta appears necessary, at thresholds much higher than those known to be operative in the photochemical decomposition of the silver halides.

Obviously, the contradiction might be simply dismissed by abandoning the view that the photolysis involves the intermediate liberation of photoelectrons from halide ions—a hypothesis suggested independently by K. Fajans and by the writer. But the phenomena of photoconductance definitely point to a relative freeing of photoelectrons, and Dr. F. C. Toy has shown (NATURE, Sept. 24, 1927) that the discrepancy between the photoconductance absorption spectrum of the silver halides and the spectrum photographically active is removed by consideration of the thickness factor. Moreover, recently Dr. W. Vanselow and the writer have obtained independent evidence of the liberation of photoelectrons, *concomitant with the production of free halogen*, in a study of the photopotentials of silver–silver halide electrodes in electrolytes.

The difficulty of the energy quanta required cannot be dismissed therefore. The experimental evidence obtained with these cells has led to a modified theory of the inner photoelectric effect which is consistent with the writer's orientation theory of photographic sensitivity and latent image formation. By considering that both the lattice energy and electron affinity are lowered at interfaces, particularly at true interfaces with conductors, it seems possible for the inner photoelectric effect to occur at the lower wavelengths in question.

An interesting corollary is a possible relation of this to the theory of E. A. Baker (NATURE, May 7, 1927, p. 685; *Proc. Roy. Soc. Edin.*, **47**, 34; 1927), that "*two* quanta are concerned in photographic action, and that the two must be absorbed within a short interval of time, giving the effect of two distinct absorptions when the exposure is short, and of simultaneous absorption when the exposure is long. This indicates