

after either 200 or 500 r. The approximate stages of spermatogenesis sampled in spermatozoa ejaculated after one to six weeks are shown in Table 2, which extends the calculations given above. With respect to Table 2, it must be emphasized, first, that this timing combines data from normal animals, and from animals irradiated up to 100 r.; second, that the timing of stages for the centre of weeks departs from the standard procedure in radiation genetics of scoring by full weeks after treatment.

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I SHOULD first like to congratulate Drs. Sirlin and Edwards on their excellent summary of the cytological data on the timing of spermatogenesis in the mouse. Of special relevance to my work with 200 r. is their reference to Oakberg's recent results after 100 r. There is still, however, a paucity of results on the time spent by the sperm on its journey through the epididymis and vas deferens, an interval which seems likely to be very easily modified by various factors, particularly by the pressure exerted from behind by further mature sperm leaving the seminiferous tubules. This would mean that destruction of the spermatogonia by irradiation would cause a sluggish passage through the epididymis four weeks later, when there was no sperm leaving the testis.

Nevertheless, the cumulative cytological evidence is such that it seems necessary to modify my original conclusions and assume that the sperm sampled during weeks four to seven had been irradiated as spermatocytes, not as spermatids. The peak in the third week would then after all be due, as claimed by Drs. Auerbach and Slizynski<sup>1</sup>, to hypersensitivity of the spermatids. This is like *Drosophila*, though mouse spermatids show only twice the sensitivity of the sperm, whereas my estimate for *Drosophila*<sup>2</sup> shows a ratio of 10:1.

If the identification of the spermatogenic stages represented in the sperm sample of each week is taken as settled, the most important question becomes the origin of the dominant lethals from the irradiated spermatocytes, sampled in weeks four and five, and probably extending into weeks six and seven owing to the sluggish stream through the epididymis at this time.

My genetical timing of spermatogenesis<sup>3</sup> had been based on the *a priori* assumption that viable sperm carrying dominant lethals could not be produced from chromosome breakage in spermatocytes. One has to admit that there is no experimental evidence

on this point. In *Drosophila*, where one might expect such evidence, the spermatocytes are alleged to be killed by irradiation<sup>4</sup>, and in any event are of such short duration that their detection would be difficult in sperm samples.

It remains true, however, as observation of irradiated testes will show, that a high proportion of the products of meiosis immediately following irradiation are grossly abnormal, either as larger-than-usual pro-spermatids (restitution nuclei?) or as misshapen and apparently impotent cells at later stages of spermiogenesis. Probably most of these abnormalities are accompanied by chromosome unbalance, so that many of the dominant lethals induced in spermatocytes cannot survive as effective sperm. If the dominant lethal incidence in matings of weeks four to seven is as high as in the first two weeks, the primary incidence must be higher. Thus spermatocytes must be mutagenically hypersensitive as well as spermatids. The quantitative estimation of their sensitivity can only be made with a type of mutation not subject to germinal elimination and obtained at a dose lower than 500 r., which sterilizes the spermatocytes.

The outcome of this very useful discussion seems to have been that what I at first regarded as a genetical method of timing spermatogenesis has turned out to be a method of studying mutation in spermatocytes.

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- <sup>1</sup> Auerbach, C., and Slizynski, B. M., *Nature*, **177**, 376 (1956).  
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### The Infra-red and the Bloch-Nordsieck Divergences

IN recent years physicists have become so accustomed to divergences that the divergence of the number of emitted photons, which was found by Bloch and Nordsieck<sup>1</sup>, is considered quite a normal state of things. The reason is that the total energy carried off by these photons and the transition probability remain finite.

However, I think that the appearance of an infinite quantity in a physical result is always unsatisfactory, and that it indicates that too rough a schematization of reality was made at some stage in the theory. In fact, in a previous paper<sup>2</sup>, I have studied, in the Bloch-Nordsieck approximation, the number of photons emitted by an arbitrary classical system of charges, and I have shown that this number is always finite, provided the charges are confined within a finite region of space. Therefore, in every experiment which can actually be made, no divergence of the number of emitted photons appears, and the divergence found by Bloch and Nordsieck has its source in the particular problem studied (transition between two definite momentum states of an electron), which corresponds to an experiment that cannot really be made, because an infinite region of space is needed. This result may also be expressed in the following way. The energy per unit of frequency-range of the radiation emitted from a system