

range in unbuffered solutions are directly related to the changes in the directional frictional effect; (2) apparently all known anti-felting treatments cause diminution in the directional frictional effect, although it is possible that the ability of some of these reagents to increase the resistance to deformation of the fibres contributes to their efficiency by rendering fibre entanglement more difficult, and (3) the low felting power of wool in non-aqueous media is due to their apparent incapacity to enhance the directional frictional effect.

It would appear, therefore, that the peculiar frictional properties are a principal, if not the principal, governing factor in the felting of wool fibres. The present evidence also suggests that their surface characteristics are of a more complex nature than hitherto supposed.

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<sup>1</sup> Speakman and Stott, *J. Text. Inst.*, **22**, T339 (1931). Speakman, Stott and Chang, *J. Text. Inst.*, **24**, T273 (1933). Speakman, Menkart and Liu, *J. Text. Inst.*, **35**, T41 (1944).

<sup>2</sup> Monge, *Ann. Chim.*, **6**, 300 (1790).

<sup>3</sup> Martin, *J. Soc. Dyers and Col.*, **60**, 325 (1944).

<sup>4</sup> Whewell, Rigelhaupt and Selim, *Nature*, **154**, 772 (1944).

<sup>5</sup> Barr and Speakman, *J. Soc. Dyers and Col.*, **60**, 335 (1944).

<sup>6</sup> Liu, Speakman and King, *J. Soc. Dyers and Col.*, **53**, 183 (1939).

### Röntgen Centenary

PROF. J. A. CROWTHER in his interesting article on "Röntgen" writes as follows: "The use of X-rays in the treatment of disease has scarcely made such satisfactory progress as its use in diagnosis". With Röntgen's first skiagram, radio-diagnosis was born; but what *a priori* reasons were there for supposing that X-rays would have any therapeutic value? None at all I think, yet some courageous few entered the field of exploration, and can anyone say that the results have in the circumstances really been unsatisfactory? Thirty-five years ago, all the beds in the Cancer Wing of the Middlesex Hospital were occupied by inoperable cases of cancer; there was no treatment except an almost superhuman kindness. In 1939 there was not one among the 92 patients in those wards who was not receiving active treatment, and for the great majority of them the treatment was by means of X-rays and radium. Though it cannot be claimed that these agents are a cure for cancer, the development of radiotherapy can scarcely be called unsatisfactory.

Prof. Crowther says later on in the same article, "The action of X-rays on tissue cells, whether healthy or diseased, is, it must be understood, always destructive". But is it so? Are the hamostatic action of X-rays, their power of producing a lymphocytosis, their resolution (not destruction) of scar tissue, their action in regenerating bone, their excitation of the bone marrow to unusual activity, their temporary hold-up of mitosis, their enhancement of the mutation-rate, are these all to be labelled as destructive?

Treatment by means of X-rays and radium is a subject in which we do well to think in a rather less restricted manner than Prof. Crowther's article suggests.

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PROF. RUSS does well to be jealous on behalf of a subject which he has made peculiarly his own. I can assure him, if assurance is needed, that my admiration of the magnificent work both in the way of treatment and research which has been, is being, and will with ever-increasing success continue to be, done in our radiotherapy departments is no whit the less than his own. I am only surprised that anyone who has read carefully the whole paragraph of which Prof. Russ quotes the opening sentence should doubt it. "Unsatisfactory" is Prof. Russ's word not mine.

As regards my poor word "destructive" with which Prof. Russ quarrels, it all depends on whether one considers the immediate action of the radiation, or the long-distance results of the action. To take only the case of the production of mutations, which is one which we are just beginning to understand, the primary action of the X-radiation is to break a chromosome chain at two different points. This, of course, gives Nature its chance to arrange the pieces in a different order; nevertheless, the breaking of a chain is essentially a destructive action.

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### Problems of Nomenclature

I HAVE read with much interest the discussion in *Nature* of December 30, p. 812, and wish to add a few suggestions to those there given. In the first place, it is highly desirable to have a single code of nomenclature for plants and animals. The problems are the same in both cases, and the existing codes are so much alike that a very moderate amount of revision would be necessary to secure uniformity. There are two matters which cause confusion, and should be dealt with.

(1) *Nomina seminuda*—names which have been introduced in an informal manner, without proper descriptions, but have been taken up because they could be interpreted in the light of subsequent researches. It would be a dangerous policy to rule that names poorly supported by descriptions should be rejected; but when there is nothing which will distinguish the species, and only subsequent studies of the fauna indicate by the locality what was referred to, the name should be rejected.

(2) Names proposed as of lower than specific rank. It should be ruled that subspecific names have the same validity as specific; that is, if a form is proposed as a subspecies, but later raised to specific rank, the subspecific term should be used. This is the usual practice, at least in zoology, but the botanists have mixed subspecies and individual variations under the designation 'variety', and it is not always easy to determine what the author had in mind. It would be well to take the lists of so-called varieties, and separate those names which were really intended for what zoologists call subspecies, rejecting the others as invalid for use as species names.

In such ways a good many really needless changes might be avoided. A very desirable reform in botanical writings is the dropping of the name of the author of the combination in ordinary references to plants. Such names are scarcely ever cited in zoology, and I cannot recall an instance in which their omission has caused any inconvenience. The botanical practice wastes a lot of printer's ink and paper.

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