Letters to the Editor.

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The Conditions of Chemical Change.

PATIENCE has its limits and modesty may be overdone. I am moved to these reflections by a three-page note in the Journal of the Chemical Society to hand to-day. I am carried back forty years, to a meeting at the society, in March 1885, when a shy Oxford graduate, a worker in Mr. H. B. Dixon's laboratory, told us practically that charcoal and phosphorus could not be burnt in dry oxygen. Actually, he told us less, as he did not then go nor has he since gone beyond his facts. That, however, was the inference to be drawn from his work, taken in conjunction with that of Cowper, Dixon, Wanklyn and others, on the effect of drought in checking chemical interactions. I there and then stated what I will now venture to term the *theory* of chemical action, chary as I am always of using the word, if there be the least reason to rest more faith in honest doubt than in a creed. In a sentence, that theory is, that chemical action, of whatever kind, is essentially electrolytic: consequently, change takes place only when the potentially interacting substances constitute an electrolytic circuit: such circuit appears always to be one of three components, of which one, necessarily, is an electrolyte. I was, therefore, able to say that hydrogen and oxygen would not and could not interact; further, that even when "wetted" they would not interact, as water was not an electrolyte: only when the water was made "conducting," by the presence of a dissolved "salt," would change be possible.

Several years later, by further exact experiments, Baker demonstrated the truth of these propositions. He developed an uncanny ability in "drying" things, and proved, in numerous cases, that the conditions I had laid down were essential to the occurrence of change. Others have verified some of his observations. About three years ago, however, Coehn and Tramm questioned his work and stated that hydrogen and oxygen not only interacted, under the influence of the light from a quartz-mercury lamp, but at the same rate whether the mixture were dry or moist.

Talking the matter over with Baker, I said: "Yes, as interaction takes place in a liquid film, at the surface of the containing vessel, ultra-violet light, at a low temperature, may well have a greater effect than heating, in bringing about change. Persevere in cleaning and drying the vessel and its contents and action will be stayed."

Baker now reports, that in a quartz tube which had been dried during twelve weeks no measurable action was observed during an exposure of thirteen hours to a quartz-mercury lamp at a distance of 2 cm., although action took place in tubes less thoroughly "prepared."

Baker and I have wandered all but alone these forty years in an arid wilderness, athirst for recognition and sympathy. I feel that we must now compel this, and that we have the right to challenge all and sundry, chemists and physicists alike, to consider the grounds of their belief, if they have one. When the history of our time comes to be written, nothing will appear more striking than the strange psychology of the "scientific" worker so-called: particularly the way in which he was captured by the wild Arrhenic speculation, more especially by Ostwald's dogmatic rulings. After all, he has but shown that he is human and dominated by the herd-instinct. Many may be called into "science," but few are chosen—to think for themselves and behave as reasoning beings. Still—

One must receive their nature in its length
And breadth, expect the weakness with the strength!
Sordello.

The issue I raise is of consequence at a time when so much importance is attached to the determination of ionisation potentials in gases and so much argument is based upon the values deduced—but no attention whatever paid to the conditions prevailing in the vessels used. Neglectful as is the chemist, the physicist is far worse: seemingly he has no conception of chemical cleanliness, and the chemist, in consequence, often takes leave to doubt not a few of his deductions. It was not always so—it was not so when chemistry was taught together with physics. Of late years, we seem to have sought how not to teach the various branches of science effectively—by teaching them singly and encouraging a crass specialisation, which is leading us to neglect and fail in solving the problems of real importance.

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October 1.

Haploidy in the Male Sawfly (Tenthredinidae) and some Considerations arising therefrom.

IN 1907 Doncaster published certain statements on the gametogenesis of the common gooseberry sawfly, *Nematus ribesii* Scop. ("Gametogenesis and Fertilisation in *Nematus ribesii*," *Quart. Journ. Micro. Sci.*, vol. 51 n.s., 1907), but later, in 1909, he published a correction (*ibid.* 1909. "Gametogenesis of the Sawfly *Nematus ribesii*. A Correction." NATURE, Dec. 2, 1909, p. 127). Unfortunately, he never resumed the investigation, and the problem was so left that its solution demanded work *ab initio*. Since then, nothing further has been done to elucidate the question, but work done by me on the spermatogenesis of the sawfly *Pteronidea melanaspis* Htg., together with certain breeding experiments with *Pteronidea* (*Nematus*) *ribesii*, permits certain pronouncements which throw considerable light on the subject.

Doncaster's material and mine belong to the same genus,[1] and the two species behave alike sexually in that the females, by parthenogenesis, produce males only (arrhenotoky), whilst, after insemination, they produce both sexes. These reproductive habits are likewise common to a great many sawflies, and indeed, except possibly for one species only, we may lay it down as a principle that for all bisexual sawflies in which the sex-ratio is normal (about 100 : 100), parthenogenesis results in the production of males only.[2] Any conclusion concerning *P. melanaspis*, therefore, will apply most probably to all such arrhenotokous species.

The cytological results for *P. melanaspis* are:

1. The spermatogonial chromosome number is 8 (Fig. 1);
2. The spermatocyte chromosome number is also 8 (Fig. 3);
3. There are two maturation divisions in spermatogenesis, but no reduction in chromosome number (Figs. 4 and 5);

[1] *Nematus ribesii* now is called *Pteronidea ribesii* according to Enslin's classification.
[2] The experience of sawfly workers, notably Miss E. F. Chawner, shows that the list of arrhenotokous species will be greatly increased by further work, and indicates also that all bisexual sawflies are facultatively male-producing by parthenogenesis. Occasionally a female is produced, but this occurrence raises questions which cannot be discussed here.