I proposed for the negative stroke. Many characteristics of lightning strokes can be satisfactorily correlated by means of such a concept.

With reference to the object struck, there are probably two main types of strokes: (1) The positive stroke, in which a positively charged tongue of ionised air extends from the cloud to the object; (2) the negative stroke by an electronic dart, of which the highly ionised trail extends from the cloud to the object. In general, the initial effects of the second will be more abrupt, more deep-seated, and more rending than those of the first.

The positive tongue theory advanced by Dr. Simpson and the electronic dart theory proposed by myself are not conflicting, but mutually complementary. The apparent conflicts, in so far as they are not a mere matter of words, arose solely from misconceptions, on one side or the other, regarding the completeness of the picture being presented.

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June 9.

IN the paper referred to by Dr. Dorsey, I discussed how a lightning flash is propagated from a positively charged cloud. In that paper, of set purpose, I did not discuss how the ionisation takes place, relying on the simple statement that ionisation does take place when and where the field exceeds a certain unspecified minimum value. I did this to avoid discussion of details which were unessential to the main purpose of the paper. But as Dr. Dorsey now raises these questions they must be considered, although the correspondence columns of NATURE are not appropriate for the full discussion which they really require. I must therefore be content with a few remarks on his main points.

There can be little doubt that free negative electrons which are always present in the atmosphere play some part in the ionisation accompanying a lightning discharge; but all the evidence, in particular the sharp boundary of the discharge channel, points to this action being confined to the immediate neighbourhood of the channel. In fact there is reason to believe that it is confined to the small region at the extreme tip of the advancing channel, where alone, in my opinion, the field is sufficiently strong to cause appreciable ionisation.

With regard to branching, Dr. Dorsey says: "Our present conceptions of the nature of the discharge of electricity through gases suggests that the branches are not outgrowths from the trunk, but ingrowths to it." I wonder to whom the word "our" in this sentence refers, for personally I cannot form any such conception. To me it is quite inconceivable that the branches grow from their tips inwards, finally uniting to form the trunk. This would mean that the electrical discharge starts in the weakest part of the field and not, as one would expect, where the field is strongest, near to the charged cloud.

The second half of Dr. Dorsey's letter is of more fundamental importance. He describes darts of negative electrons which are shot out of negatively charged clouds. It is not easy to understand Dr. Dorsey's ideas from this letter alone; but by reading his other publications I have come to the conclusion that his darts are very little different from the negative discharges which I considered and rejected in my Royal Society paper. I did not consider that such negative discharges are impossible "because the mutual repulsion of the electrons would cause a continued broadening of the tip," as stated by Dr. Dorsey. As a matter of fact, the mutual repulsion of the electrons never entered my head when writing the paper, for any such repulsion is infinitesimal in comparison with the fields which exist where ionisation takes place. The negative electrons would spread out at the tip of a negative discharge, if such could form, not because of mutual repulsion, but because the field under which the electrons move is divergent at the tip.

If it is granted that ionisation takes place where the field is a maximum, then the lines of force must diverge from that maximum. It is because the electrons move inwards for a positive discharge and outwards for a negative discharge that the former is possible and the latter impossible. It is this divergence of the field of force which would prevent the formation and existence of the darts imagined by Dr. Dorsey.

G. C. SIMPSON.

### Butterfly Migration.

IN NATURE, September 5, 1925, Dr. E. P. Felt, State Entomologist of New York, in an article on the "Dispersal of Butterflies and Other Insects," said, "It is our belief that determinate flight is a comparatively small factor in promoting the spread of insects, and that in many cases this is accomplished largely by drifting with the wind, . . . There are a number of records of apparently determinate movement by butterflies, . . . These cases may represent a true migration, though this is scarcely established by available data."

In a later article, "Physical Basis of Insect Drift," NATURE, May 29, 1926, Dr. Felt continued the discussion and said, "the general tendency has been to explain any widespread movement as a migration, that is, a somewhat determinate or purposive movement by hosts of insects. This attitude is due in part to our very limited knowledge as to the movements of the upper air currents, . . . Turning to the western hemisphere, there are several records of enormous swarms of this butterfly, *Vanessa cardui*, being observed in apparent migration in southern California in 1924 and 1926, the movement being from the south-east to the north-west. One of the observers suggests that the source or the origin was either the foothills of the Sierras or the Sierras proper. There is a possibility that these swarms originated at a considerably greater distance. They may have been carried into the upper air in regions bordering desert areas considerably farther south or south-east, in much the same way as suggested for this insect in the eastern hemisphere, since we have in both extensive desert areas constantly producing convectional currents, and after a certain altitude is attained, the probabilities of extensive drift are certainly excellent."

Doubtless the movements of some insect swarms cannot be explained without the help of the winds. But what is needed, at least in the case of the California 'migrations,' before a general conclusion is drawn, is a careful accumulation of facts. In the early spring of 1926, there was a migration of the 'painted lady' through Palo Alto, California, in the Santa Clara Valley. The movement lasted several days and cannot by any possibility be explained by wind drift. Neither the origin nor outcome of this migration is known; it is not even known whether the swarm started from one area or was cumulative, picking up members as it went; but at the height of the movement the facts were so plain that they could not be misinterpreted. During the two days of March 25 and 26, 1926, I was out of doors nearly all the time and my official duties took me over an