I succeeded at once in obtaining it, when the condensed discharge was used. This glow has many interesting properties, of which a preliminary publication seems desirable.

I believe it to be due to pure nitrogen. Lewis states that it cannot be obtained from atmospheric nitrogen, but this does not agree with my experience. I have used atmospheric nitrogen exclusively.

The glowing nitrogen is unaffected by silver gauze, which quenches the ozone glows. It is destroyed by mixing oxygen with it, but merely diluted by hydrogen or ordinary nitrogen. When acetylene is led in, a bright flame is produced at the point of confluence. This flame replaces the original glow. It has a spectrum consisting of the swan and cyanogen bands, along with others not identified. If the nitrogen glow is led over iodine a magnificent blue flame is produced, contrasting sharply with the original orange glow. With sulphur the original orange glow is quenched, but no other replaces it. The sulphur becomes hot, and a metallic-looking sublimate is formed along the tube.

The most remarkable phenomena, however, are with metallic vapours, which give line spectra when the glowing nitrogen is led over them. Sodium, potassium, thallium, mercury, zinc, cadmium, and magnesium have all yielded line spectra in this way.

Investigation is being pushed on as fast as possible, but the facts so far obtained seem to point to the production of a chemically active modification of nitrogen. It is suggested, provisionally, that the spectra are developed by the chemical union of this active nitrogen with the various metals and with iodine and acetylene. The orange glow obtained with nitrogen only would, on this view, be due to the transformation of the hypothetical active nitrogen into ordinary nitrogen. R. J. STRUTT.

Imperial College of Science and Technology,
January 30.

Singularities of Curves.

I HAVE not, at present, access to the books referred to by "T. J. I'A. B." in his letter of January 12; but he is altogether wrong in thinking that the singularity he mentions cannot be investigated by the methods explained in my "Geometry of Surfaces." An arbitrary line through the origin has sextactic contact thereat; but since the axis of x has 12-tactic contact at the origin, the latter cannot be an ordinary sextuple point, because no line through such a point can have a higher contact than septactic. The singularity is either a singular point of the sixth order or one of lower order with coincident branches passing through it, and it illustrates the necessity of drawing a distinction between ordinary multiple points and singular points. The trilinear equation of the curve can be obtained by eliminating t between $\beta=\alpha t^6$, $\alpha\gamma-\beta^2=\beta^2(t^3+t^4)$. The factor $\alpha\gamma-\beta^2$ suggests the existence of tacnodal or other branches of a similar character, and that the singularity might be transformed into a simpler one lying on a curve of lower degree than the sixteenth by using Cremona's transformation,

$$\frac{\alpha}{\alpha'\gamma'+\beta'^2}=\frac{\beta}{\beta'\gamma'}=\frac{\gamma}{\gamma'^2}$$

before applying the methods of chapter iv. of my book.

But it would have been foreign to the plan of my treatise to have introduced parametric methods when discussing singularities; moreover, the method of which the example is an illustration is only applicable to unicursal curves, whereas my own methods are independent of the deficiency. For example, the various singularities the point constituents of which are nine nodes could not be investigated by means of a unicursal curve without complicating the problem by introducing additional nodes isolated or in combination sufficient in number to reduce the deficiency to zero; and this might limit the generality of the investigation, for when the nodes exceed a certain number they are not arbitrarily situated, but lie on one or more dianodal curves. A. B. BASSET.

January 14.

MR. BASSET now admits that he has seen neither Zeuthen's two papers of 1876 nor Jordan's book of 1893, thus practically acknowledging the accuracy of my criticism—that the treatment of singular points in his "Geometry of Surfaces" is incomplete. With this admission from Mr. Basset the matter ends, so far as I am personally concerned.

But I must enter a protest against Mr. Basset's inference that the methods of Zeuthen and Jordan are only applicable to unicursal curves; since Mr. Basset has not read the work in question, his only reason for this statement is the fact that the example in my first letter happens to be a unicursal curve. This example was made up so as to provide a simple illustration of the general methods; but these methods hold good for curves of any deficiency.

It is absurd to suggest that parametric methods cannot be used for any algebraic curve; of course, the coordinates are expressed in the form of infinite series (convergent near a particular point of the curve) instead of terminated series. Mr. Basset's objection to using parametric methods would be quite justified if he had provided us with a satisfactory substitute; but he gives no *systematic* plan for resolving an assigned singularity, and this is the main object of the parametric method as used by earlier writers. T. J. I'A. B.

FRANCIS GALTON.

FEBRUARY 16, 1822—JANUARY 17, 1911.

THE death of Francis Galton marks, not only the removal of another link with the leaders of the great scientific movement of the nineteenth century—represented by Darwin, Kelvin, Huxley, Clerk-Maxwell, and Galton in this country—but something far more real to those who have been in touch with him up to the last, namely, the cessation of a source of inspiration and suggestion which did not flag even to the day of his death. The keynote to Francis Galton's influence over the science of the last fifty years lies in those words: suggestion and inspiration. He belonged to that small group of inquirers, who do not specialise, but by their wide sympathies and general knowledge demonstrate how science is a real unity, based on the application of a common logic and a common method to the observation and treatment of all phenomena. He broke down the barriers, which the specialist is too apt to erect round his particular field, and introduced novel processes and new ideas into many dark corners of our summary of natural phenomena.

The present writer remembers being asked some years ago to provide a list of Francis Galton's chief scientific achievements for use on a public occasion. It did not seem to him that a list of isolated contributions, such as the establishment of anthropometric laboratories, the introduction of the composite photograph, the transfusion experiments to test pangenesis, the meteorological charts and improved nomenclature, the practical realisation of the possibilities of finger-print identification, the demonstration of the hereditary transmission of the mental characters in man, the law of regression, the idea of stirps, or the foundation of the novel science of Eugenics, fully represented the nature of the man. What is the spirit of the contributions—large and small, almost two hundred in number—which Francis Galton made to the science of the last sixty years?[1] The unity of those contributions lay largely in the idea that exact quantitative methods could be applied, nay, rather must be applied, to many branches of science, which had been held beyond the field of either mathematical or physical treatment. In this manner his inspiration and suggestion tended to give physical and mathematical precision to a large number of outlying sciences, to meteorology, to anthropology, to genetics,

[1] His first contribution dates from 1849 and concerns a method of printing telegraphic messages at the receiving station.