ments only, made by use of a Leyden jar, that I believe to be decisive, and to prove irrefutably that Swan's spectrum cannot be ascribed solely to hydrocarbon. As to the phenomenon shown by sparks, it is not quite so convincing; for, accepting Wüllner's* ideas, one could object that the spark affecting only a set of molecules of infinitely small transverse section could have carried forth from the electrodes an exceedingly small quantity of hydrogen, which would quite escape observation when expanded throughout the whole vacuum tube, but would be sufficient to induce the hydrocarbon spectrum in the line of the spark. This explanation, however, I do not think to be at all probable. The tube used in these experiments was of common size; the ends of the electrodes, which were of aluminium, had a distance of about 15 centims. from one another. Some results, differing from those just mentioned, were obtained in a wide spectrum tube without capillary tube, the ends of its electrodes being distant about 3 centims. from one another. The spark then no longer exhibited Swan's spectrum, even when the carbonic acid was not dried with the utmost care, as in the above-mentioned researches; but there were seen the enlarged maxima of Watts' second spectrum, which I have shown by some researches, of which a short account has been published in the "Berlin. Monatsb.," 1880, p. 791, to be generally due to the continuous form of discharge, according to the nomenclature of Messrs. Thalén and Ångström. If the density of the gas was still increased, there flashed a very brilliant line spectrum, never before observed by me, in a tube of common size. These facts, at all events, show that there exists some relation between the different orders and forms of spectra and the conditions of discharges (probably, according to my opinion, the quantities of electricity sent through the unit of space in the unit of time, till now unknown to us) that will doubtless become clear as soon as the causes governing spark discharges shall be better recognised—a subject on which Sir W. Thomson† says that it is difficult even to conjecture an explanation.

V. "The Wave of Translation and the Work it does as the Carrier Wave of Sound." By John Scott Russell, F.R.S. Received June 13, 1881.

Synopsis.

Short history of the wave of translation discovered by me in 1832–3. Investigation of its shape, nature, speed, and difference from all other waves in its character as a solitary carrier wave.

* Wüllner, "Lehrbuch der Experimental Physik," vol. 2, p. 250, &c.

† Papers on "Electrostatics," &c., p. 247.

The application of this solitary wave in air to the carrying of sound.

Errors of the phrases in general use for the explanation of the conveyance of sound.

My theory of the mode in which sound is propagated as distinguished from the mode of its creation.

Consideration of the aerial wave of sound, its analogy to the solitary wave in water.

The *depth* of the air ocean and its correspondence to that of the water ocean.

The speed of the solitary or carrier wave in each element. The speed is due to the depth. What happens in the interior of the solitary wave in water? The same motions take place in the same nature of aerial wave.

Elucidation by the comparison of three equivalent oceans.

Similarity and dissimilarity between the oscillating waves and the carrier or solitary wave.

Musical sounds in their melodic and harmonic relations to this wave. Numerical nomenclature of sounds.

How the message of the sound wave is delivered to the brain.

VI. "On the Effect of Electrical Stimulation of the Frog's Heart, and its Modification by Cold, Heat, and the Action of Drugs." By T. LAUDER BRUNTON, M.D., F.R.S., and THEODORE CASH, M.D. Received May 16, 1881.

(Abstract.)

From the results of the recorded experiments conducted on the frog's heart in its normal position and still exercising its circulatory function, we have found—

I. That electrical stimulation by a single induced shock has either an obvious effect on the contraction and rhythm of the organ, or no such effect is apparent.

II. That the effect is modified by—

(a.) *The time of the cardiac cycle in which stimulation falls.*—Thus Marey has already shown that a so-called refractory period is demonstrable under certain conditions.* Well-marked variations in latency when the stimulation is potent to induce a systolic contraction are to be recognised.

(b.) *The strength of the stimulation applied.*—Refractory periods possible under minimal stimulation can no longer be demonstrated

* The conditions of this refractory period, or "period of diminished excitability," have been very fully investigated by Dr. Burdon Sanderson and Mr. Page. "Proc. Roy. Soc.," vol. 30, p. 373.