

tip in spite of all attempts to right it with the combined rudder and wing-warping control. They eventually discovered that the only remedy was to put the nose of the machine down, and quite rightly attributed the trouble to loss of speed causing the controls to become inoperative.

If I may be forgiven criticising so sound an authority as Prof. Melvill Jones, I think he is wrong in lumping all the early experimenters with the Wrights in what I have always called in my own mind the 'acrobatic' as opposed to the 'stability' school. Surely the early French pioneers—such as the Voisins and Santos Dumont—quite definitely tried to produce a stable aeroplane, seeking to attain this end by such devices as 'curtains' between the wings and exaggerated dihedral angles. That their methods proved unsuccessful does not alter the fact that that was their aim. The greater success aerodynamically of the Wright machine gradually brought the French school to the view that stability must be sacrificed to control, but it has always seemed to me that originally they started from the other of the two extremes Prof. Melvill Jones so graphically describes. It was to me absorbingly interesting in the early days to watch the two 'schools' gradually converging. The French machines discarded their 'stability' devices, while the Wrights in time gave up the front elevator and fitted a wheeled undercarriage, until the Wright biplane of 1910 was in appearance scarcely distinguishable from, for example, the Farman of the same period.

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May 21.

IN referring to the Wrights' use of a powerful rudder as a means of dealing with the stall, I had mainly in mind its use to control the lateral rolling of the aeroplane in the manner indicated in Figs. 6 and 7 of my article. I am much interested to hear from Lieut.-Col. Lockwood Marsh that the Wrights, as I had surmised, deliberately fitted it with this object in view.

My statement that on the whole the men who tried to fly were themselves interested in control, whilst the theorist and model constructor were more interested in stability, was intended to be read in a broad sense only, and in that sense I still think it is correct, bearing in mind such men as Lilienthal, Cody, and a host of others both before and after the Wrights. I had not, however, realised the extreme interest in stability of the French pioneers, and am grateful to Col. Lockwood Marsh for pointing it out.

B. MELVILL JONES.

Base Exchange and the Formation of Petroleum.

I HAVE read with great interest Dr. McKenzie Taylor's letter entitled "Base Exchange and the Formation of Coal" which appeared in NATURE of May 19, and am particularly interested in his description of the experiments conducted by him concerning the bacterial decomposition of fats under a roof containing hydrolysing sodium clay. Fats, as a body, are lighter than water, and I am wondering whether Dr. McKenzie Taylor has conducted any experiments, or has any evidence, to show that in Nature solid fats could be a sedimentary deposit in water and accumulate as such together with sand. Some eighteen years ago I demonstrated that oils can be deposited in considerable quantity as an aqueous sediment together with mud or clay, but my experiments indicated that oils could not be similarly deposited by,

or together with, sand. For a full description of the phenomenon and my experiments, and also how in Nature the oil would afterwards be squeezed out of the clay into a sand bed, reference may be made to my book, "The Geology of Oil, Oil-Shale, and Coal."

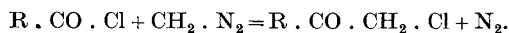
I gather from Dr. McKenzie Taylor's description that in his experiment the bacterial decomposition under alkaline anaerobic conditions of the fat which was distributed through a sand layer at the bottom of the beaker took place in fresh rather than in saline water. If so, this may possibly account for the yield of methane rather than of higher members of the paraffin series. Connate oilfield waters which occur, together with oil, sealed up in oil-sands, have a distinct resemblance to ordinary sea water. They differ from ordinary sea water in increased salinity and deficiency in sulphates and magnesia. Two years ago I deduced that natural petroleum oil owes its origin to the bacterial decomposition under anaerobic conditions, in some cases of vegetable oils, and in others of protoplasm, by sulphate-reducing bacteria in sea water, and that the differences between connate oilfield waters and normal sea water were just the differences which would be produced by the action of such bacteria. The actual discovery of sulphate-reducing bacteria in the connate oilfield waters of the Illinois, Sunset-Midway, and Coalinga oilfields of America, by Dr. Edson S. Bastin, affords a certain amount of support for my deductions. If, parallel with the experiments which he is conducting, Dr. McKenzie Taylor would conduct a second set of experiments to determine the effect of the bacterial decomposition in sea water under anaerobic conditions by *Microspira aestuarii* (van Delden) and its associates, of protoplasm on one hand, and of vegetable oils on the other, incorporated as sediments in clay, a comparison of the results obtained would be extremely valuable.

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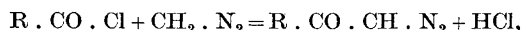
The Nierenstein Reaction.

DURING the past thirteen years, and in conjunction with several different collaborators, I have studied the reaction which takes place when diazomethane is added to various acyl chlorides, and I have shown that the change which takes place is always of the type represented by the equation,



The details of the method are described in my first paper (*Jour. Chem. Soc.*, **107**, 1491; 1915) on the subject, and it suffices to state here that the reaction was conducted by passing the diazomethane into the acyl chloride, the latter reactant being always in excess during the reaction. This is commonly known as the Nierenstein reaction.

Quite recently Arndt (*Ber.*, **61**, 1122; 1928) has confirmed my experimental results, and has also shown that if the method of conducting the process be reversed, and the acyl chloride added to the ethereal solution of diazomethane, so that the latter remains in excess, a change takes place which is represented by the equation,



the main product being of the type, $R \cdot CO \cdot CH \cdot N_2$, instead of the type, $R \cdot CO \cdot CH_2 \cdot Cl$.

In a paper published in the *Journal of the Chemical Society* a few days ago (p. 1310), Bradley and Robinson state that when diazomethane is treated with benzoyl