DISCUSSION.

Professor F. G. Donnan, F.R.S. (communicated): Mr. Ellis has given the members of the Faraday Society an excellent, though necessarily brief, survey of the work which he has been carrying out under my direction for the last two years on the subject of pure oil emulsions. We see that no distinction can be made between "suspensoids" and "emulsoids," if based merely on the liquid or solid state of the disperse medium, as has been attempted by some authors. For the oil emulsions investigated by Mr. Ellis behave as typical suspensoids; so that indeed such pure "mechanical" oil emulsions form a very striking model of a "suspension colloid."

It is also clear that Bredig's "Lippmann Effect" theory of the stability of suspensoids is not borne out by the results obtained by Mr. Ellis. This is shown by a comparison of the contact-potential, stability, and interface-tension measurements.

In conclusion I may mention that further work on emulsions is at present being carried out in various directions in the Muspratt Laboratory.

Mr. Emil Hatschek thought Mr. Ellis had correctly described the emulsions dealt with in his Paper as "models" of suspension colloids, inasmuch as that title was generally applied to reproductions differing considerably in size from the objects represented. He understood that the size of the initial oil globules was about 2μ , and such a system was coarse compared even with many ordinary precipitates, and still more with real suspensoids, which began about the limit of microscopic visibility, say about 0.4μ . As Mr. Ellis had chiefly considered properties more or less independent of the size of the particles, such as contact-potential, the point was not vital, but it could not be disregarded in considering the stability. This seemed to him a very ill-defined quality, especially in a system which was unstable from the outset: an emulsion with particles of that size would clear in a short time, simply under the influence of gravity. He also had great doubts as to the accuracy of the nephelometer, as it appeared to him unlikely that light from all particles, except at enormous dilutions, could reach the eye. He considered the only conclusive method, no doubt much more laborious, microscopic measurements and counts, which were fairly easy with particles of this size.

As regards the reason suggested for the absence of collision between the particles while still charged, viz., the double layer, he always found great difficulties in dealing with this conception; while he understood that energy was required to shear the double layer, he could not understand why it should prevent collision if the particles with the layer were looked upon as condensers. The point brought out by the Paper, viz., that the maximum charge and stability occurred in neutral or slightly alkaline solution, appeared to him deserving of attention.

With reference to the action of ferric hydroxide as a positive colloid, he found some difficulty in accepting the suggested theory, which made the sign and magnitude of the radius of curvature the determining factor. In the capillary coagulation of ferric hydrate, the latter was undoubtedly deposited not only in the meniscus, but on the whole surface of the glass. He had also found in experiments described in 1910 that ferric hydrate in the gel condition, i.e., already precipitated on large surfaces like those of wood-wool, took oil globules out of condense water, and he was inclined to believe in a purely mechanical action.

He had always advocated the study of oil-water emulsions, as they were

the only systems which could be obtained free from electrolytes, and he hoped Mr. Ellis might extend his investigations to such emulsions, prepared with due precautions, which might perhaps help to throw light on the question—still obscure—of the origin of the charge on suspensoid particles.

Professor A. W. Porter, F.R.S. : I wish first to congratulate the author of the Paper on having raised so many interesting points. I do not feel altogether in agreement with the criticisms made by Mr. Hatschek. For example, I do not share his lack of enthusiasm for the electric double layer. No doubt the mechanism of this layer is not immediately obvious. A perfectly uniformly charged double layer would, of course, be a self-contained system irresponsive to external electric fields. The fact that suspensoids drift in an electric field demonstrates, however, that the charges cannot be uniformly distributed. In such a case there may be an action between neighbouring particles, and this will be a repulsion, as is supposed in the Paper.

Mr. Hatschek has some doubt about the validity of Whetham's conclusions in regard to the numbers obtained for the precipitating power of mono-, di-, and tri-valent ions respectively. I do not think that taking the suspended particle as fixed will have any serious effect on the values calculated theoretically. A similar problem turns up in the kinetic theory of gases, and in that case no serious change is required when the movement of a particle is recognized.

With regard to the meaning of the results obtained with the nephelometer, Mr. Hatschek's criticisms seems to be more just. Even for large and uniformly sized particles the interpretation of the results can only be simple when the particles are so sparsely distributed that the light reflected from any one can reach the eye without having been intercepted by intervening particles. I should like to learn, however, from the author, more details with regard to the size and density of distribution of the particles in his experiments, in order that one may judge as to how far this criticism is valid.

Dr. E. Feilmann remarked that the fact that slightly alkaline media favoured the production of stable suspensions, of colloidal character and otherwise, had long been known, and was supported by much experimental work by Wottemoser and others. He himself had observed the fact repeatedly in the case of colloidal suspensions of tungsten and similar substances, and had been inclined to attribute it to the removal of surface layers of oxide from the metallic particles, but the same action took place in cases where no oxide could form. Further, in some cases slightly acid solutions helped the stability of the suspension, as in the case of colloidal zirconium.

Dr. G. Senter said the theory that the particles in a colloidal solution were kept from coming into actual contact by the presence of an electrical double layer unless the kinetic energy of collision was sufficiently great to cause the rupture of the layer was very interesting, but required more detailed statement. The suggestion that a particle with such a double layer could be regarded as a closed system without any electrical effect towards the outside appeared improbable for several reasons.

He agreed with Professor Donnan that Mr. Ellis's results appeared to do away with any real distinction between suspension and emulsion colloids, and rendered necessary a reconsideration of the classification of colloidal systems. This view was supported by the recent results with colloidal sulphur obtained by Ódén.*

* *Zeitsch. physikal. Chem.*, 1912, **80**, 709.

Mr. H. M. Ridge suggested that the condition of electric oil emulsions was very important in view of the ore flotation processes now in use, in which enormous masses of material were worked up into oil emulsions. He thought it was only to be expected in such cases that considerable differences of electric potentials would be produced between different parts of the mixture. In the treatment of ore by emulsion methods there was frequently about 1 per cent. of sulphuric acid present.

Mr. Ridsdale Ellis (*communicated reply*): Considering Mr. Hatschek's criticisms with regard to the use of the term "stability," this term was used in a purely relative sense, since, as Mr. Hatschek pointed out, the globules were of such size that the emulsion largely separated out in the course of a few weeks. The fact that the emulsions were never permanently stable was not of importance, as the position of the minimum rate of settling was the essential factor to determine. The emulsions employed were the finest obtainable by shaking the pure oil with pure water, and the use of an emulsifying agent was not feasible, as it would tend to alter the contact potential.

A series of tests on the accuracy of the nephelometer with emulsions of known strengths were made, and it was found that if, as Professor Porter suggested, the concentration of the oil was kept sufficiently low, an accuracy of 10 per cent was obtainable. The more usual concentrations were only one part of oil in 20,000 of water. The effects of gravity made microscopic counts and measurements a matter of considerable difficulty.

The theory of the electrical double layer was a difficult one to crystallize into an exact concrete form. In the first place he believed, and he thought Professor Donnan shared his view, that there were two layers of ions or of charged particles around each globule, an inner negatively charged and an outer positively. Such a globule might not repel another similarly surrounded globule, but would move towards the anode when placed in an electric field owing to a slip or displacement between the two layers (Theory of Helmholtz). The ions forming the outer layer on the side nearest the cathode might move into the body of the solution and their place be taken by ions entering the outer layer on the anode side and flowing over the surface of the globule in the direction of the cathode.

Similarly, with regard to the double layer preventing coalescence in spite of the fact that it exerted no appreciable repulsion as a whole on the other particles, this was probably due to the fact that work has to be done in the disruption of the electrical double layer.

With regard to the action of ferric hydroxide as a positive colloid, Mr. Hatschek's "objection" that the ferric hydroxide was deposited on the surface of the glass as well as on the meniscus was one of the assumptions underlying the theory given. The experiments on the filtering action of gels have no point in this connection, as the whole question was the primary cause of the gel formation and not the action of a gel after it has been formed. Further, the experiments made with aluminium silicate show that the phenomenon is most certainly not a purely mechanical one.

The question of oil flotation processes is a very difficult one, and the present results do not touch them sufficiently to enable anything definite to be said about them.