ler; 'On the circuits of plane curves,' by C. A. Scott; 'Note on the real inflexions of plane curves,' by C. A. Scott; 'La théorie des plaques élastiques planes,' by J. Hadamard; 'Covariants of systems of linear differential equations and applications to the theory of ruled surfaces,' by E. J. Wilczynski; 'On the rank, order and class of algebraic minimum curves,' by A. S. Gale; 'On superosculating quadric surfaces,' by H. Maschke; 'Algebraic transformations of a complex variable realized by linkages,' by A. Emch; 'On the determination of the distance between two points in space of m dimensions,' by H. F. Blichfeldt; 'A definition of abstract groups,' by E. H. Moore; notes and errata: volumes 1, 2, 3.

THE opening (October) number of volume 9 of the *Bulletin* of the American Mathematical Society contains: 'Some instructive examples in the calculus of variations,' by Oskar Bolza; 'On the sufficient conditions in the calculus of variations,' by E. R. Hedrick; 'Some recent books on mechanics,' by E. B. Wilson; 'On a new edition of Stolz's Allgemeine Arithmetik, with an account of Peano's definition of number,' by E. V. Huntington; 'Lazarus Fuchs,' by E. J. Wilczynski; 'Notes'; 'New Publications.' The November *Bulletin* contains: 'The Ninth Summer Meeting of the American Mathematical Society,' by Edward Kasner; 'The Meeting of Section A of the American Association for the Advancement of Science, Pittsburgh, Pa., June 28 to July 3, 1902,' by E. S. Crawley; 'Second report on recent progress in the theory of groups of finite order,' by G. A. Miller; 'Shorter Notices'; 'Notes'; 'New Publications.'

THE September number of the *Botanical Gazette* contains the following papers: Dr. E. B. Copeland begins an historical and critical discussion of 'The Rise of the Transpiration Stream,' based upon an extended series of experiments that he carried on at the Hull Botanical Laboratory. The paper will be noticed more fully upon its completion. Harley P. Chamder publishes a revision of *Nemophila*, a genus which has occasioned considerable difference of opinion among Californian botanists. The author defines eighteen species and varieties, giving full discussion of critical points, synonymy, and citation of collections. Mr. W. C. Worsdell gives an account of his views concerning 'The Evolution of the Vascular Tissue of Plants,' beginning with the solid stele, which he thinks was derived from some bryophytic ancestry, and which is displayed among the most primitive ferns, and also in the juvenile stages of all ferns. The various stages in the evolution of the vascular tissue from this condition the author describes and illustrates. Professor Conway MacMillan suggests a classification of seeds in accordance with modern ideas of their structure and function. He gives general, structural, and genetic classifications. D. G. Fairchild describes *Mimosa pudica* as a weed in Ceylon, and reproduces a photograph of a large patch of it between Peradeniya and Colombo.

SHORTER ARTICLES.

ON THE STRUCTURE OF THE NUCLEUS.

1. HITHERTO the only irrefragable evidence showing that condensation is promoted by ionization, or in other words that negative ions are somewhat more active as condensation nuclei than positive ions, is the brilliant experiment devised by C. T. R. Wilson.* Nuclei are here produced by the X-rays in communicating condensation chambers, on the two sides of a vertical earthed metal plate, which receives electrical current normally on one side, through the ionized air, saturated with water vapor, and transmits the current in the same way and through the same medium on the other side. Necessarily there was an excess of negative ions on one side of the plate and an excess of positive ions on the other side. It was found, on producing condensation by exhaustion simultaneously on both sides under like conditions, that the fogs subsided on the positive side many times as rapidly as they did on the negative side, or that the negative ions are in correspondingly greater number. The effect is increasingly marked for smaller supersaturations.

2. On extending my work with shaken nuclei to solutions of non-conductors in non-conductors, such as naphthalene and of paraf-

* *Phil. Trans. Lond.*, Vol. 193, pp. 289–308, 1899.

fine in benzol, etc., I obtained results leading to the same interpretation as those already summarized for aqueous saline solutions in my last article. The nucleus is to be regarded as an exceedingly small droplet of concentrated solution, which persists, inasmuch as the decreased vapor pressure due to solution, at a certain specific radius, is exactly counterbalanced by the increased vapor pressure due to convexity. Thus, as my direct experiments have long ago shown, the nucleus depends for its size, cæt. par. on the medium in which it is produced or is generated; or in other words, on the medium into which any emanation is introduced or is generated. For if the nuclei are solutions, then the critical density and the diameter at which evaporation ceases for a given nucleus will depend on the quantity and kind of solute entrapped and on the vapor pressure equation in the broadest sense (involving temperature, surface tension, densities, etc.) of the given medium.

If this is true, then it seems doubtful to my mind whether the experiments of C. T. R. Wilson on the specific condensation effect of ionization can further be regarded as crucial.

3. If one introduces nuclei or makes nuclei by aid of the X-rays, in what is virtually the acid and the alkaline side of a battery, even if the ionized moist air is the electrolyte, one is surely conveying nuclei into, or making nuclei out of, different media. The stuff out of which solutes are to be fashioned may be available in different degrees on the two sides. Whatever chemical effect is produced on one side by the rays, need not at all be the same as on the other side, any more than the effect of shaking a very dilute solution need be the same as the effect of shaking a stronger solution, where the results have been shown to be enormously different as to the number, the velocity and persistence of nuclei produced. Hence from the accumulating evidence which I have brought forward, I am led to infer that the two species of nuclei in Wilson's experiment are, for mere chemical reasons, liable to be of different degrees of permanence, sizes and numbers, quite apart from the electric circumstances involved. One cannot, therefore, affirm that the difference (respectively positive and negative) of ionization is the immediate and sole cause of the difference of precipitation rates specified, or briefly that negative ions precipitate more effectively than positive ions, because both a difference of ionization and a chemical difference is involved; and the right to assert that ionization and not the chemical difference is the *vera causa* may be called in question, when in every other case the phenomena may be explained in terms of the latter.

I refer, of course, to immediate causes. Remotely, affinities and cohesions have the well-known electrical relations; but with remote causes I am not here concerned.

4. Finally, if a marked difference in efficiency as condensation nuclei of positive and negative ions is granted, then any ionized emanation, *neutral as a whole,* like that from phosphorus, should produce two groups of nuclei. On condensation there should be two groups of coronal particles, interpenetrating and subsiding through each other in the way I have frequently witnessed in other experiments. No such effect has been observed. Phosphorus nuclei are rather remarkable for their identity, and the regular coronas observed even after twenty-five or fifty exhaustions. If there is any variation of size of nucleus, it is graded as seen in the haziness of planes of demarcation after long lapses of time.

5. While these conclusions as to the origin of the different nuclei involve a theoretical difference from Wilson's deductions, they are not at variance with his practical conclusions; for if, through any radiation agency two different emanations are generated (with opposite charges or not), they would in a saturated medium correspond to two different nuclei, and the number of each kind and their diffusion rates in general, would also be different. If they should, at the same time, be opposed in ionization, a separation of charges will result. Indeed if two or more groups of ionized nuclei be generated in any manner whatever, they are liable to have different number and speed constants and lead to a separation of charges, be it only by diffusion.

But the case is much more definite, as the following paragraphs may indicate.

6. In this place I may again call attention to the fact that if retarded evaporation were effective in giving the nucleus permanence, if the observed dissipation of the nuclei of solutions were in any way dependent on evaporation, and not on the motion of nuclei, then those nuclei which are produced by shaking solutions of hygroscopic solutes like $CaCl_2$, H_2SO_4, etc., which can not wholly evaporate their water, should be more stable than other saline nuclei. The results show emphatically that this is not the case. Nuclei generated from hygroscopic bodies and their rate of evanescence is not exceptional and is no greater in the first case nor less in the second than that of saline bodies in general. Hence if the nucleus is necessarily a solution in case of the former solutes ($CaCl_2$, etc.), it is reasonable to suppose it always to be. I have found that the pressure decrement $\delta p < 2$ cm. of mercury (the limit is lower, but my apparatus in its present form does not allow me to go below this) is more than sufficient to precipitate the nuclei produced by shaking. For such small decrements the equation .29 $\delta p/p = \delta\theta/(273+\theta)$, where θ is the temperature in degrees Centigrade, may be assumed. Now the decrement of vapor pressure, $\delta\pi$, corresponding to $\delta\theta$, is at 20° about $\delta\pi = .11\,\delta\theta$; whence $\delta\pi = .25$ cm. for the observed *excessive* δp. In other words, the vapor depression of a few millimeters is certainly much more than is required to stop the evaporation of the nucleus; so that if this depression is to be due to the solute, the solution need not even be very concentrated. For the case of H_2SO_4 at 20°, the nucleus would hold more than seventy-five per cent. of water in a saturated atmosphere, and at lower temperatures much greater dilution would suffice.

7. Inasmuch, therefore, as the nucleus, from my point of view, occurs under conditions of potential growth from a few molecules of dry solute to a relatively weak solution, as the air becomes more and more saturated, this growth and diminution must be a common occurrence in nature. The persistent atmospheric nuclei will be more dilute from the surface of the earth upward. The question then arises whether such growth or change of concentration is accompanied by electric charge quite apart from what is usually known as ionization (demonstrable presence of non-saturated chemical valencies); in other words, whether any change of size of these excessively fine particles is *reciprocally* accompanied by surface electrification. To be more specific: In an investigation published in 1892, Lenard showed that in presence of air pure water is electropositive, a circumstance which he attributes to a mere Volta contact effect. It needs but a trace of saline solute to reverse the potential. Solutions in presence of air are electronegative, and more so as a rule, as the concentration increases up to a definite value (6.5 per cent. in case of NaCl) for which the negative charge is a maximum. After this as concentration increases the potential gradually tends toward zero (attained for a solution stronger than about twenty-five per cent., in case of NaCl). Removal of nuclei by condensation and subsidence is then virtually a removal of negative electricity, provided the positive air charge is not simultaneously removed. Here then the possibility of a mechanism, in virtue of which growth or increasing dilution is associated with increased negative charge for the nucleus, is actually at hand; but the difficulty at present rests with the removal of the air charge.

8. Briefly then, the point which I wish to make is that the occurrence of charge is incident and not causal to the existence of the nucleus. What conditions its persistence and condensational activity is purely thermodynamic. What conditions the efficiency of electric transfer is a secondary property, open to investigation though as yet but little understood, and which even in the *same nucleus* is present in very variable amount or may even be quite absent. The phenomenon in its electric aspects depends, therefore, fundamentally on the critical density at which evaporation ceases.

In the above paragraphs I have endeavored to indicate how the current lines of argument bearing more or less remotely on atmospheric electricity at present stand; to point out that

none of them have as yet, to my thinking, been traced to an issue; and to show the direction in which I hope myself to contribute.

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CURRENT NOTES ON PHYSIOGRAPHY.

RIVERS OF SOUTHERN INDIANA.

CERTAIN recent essays that might be gathered under the general title, 'Studies of River Development,' are of interest beyond that which concerns the locality that they treat, inasmuch as they illustrate the degree to which one of the most important divisions of physiographic theory finds practical application.

The 'Drainage of Southern Indiana,' largely outside of the glaciated area, is explained by Newsom (*Jour. Geol.*, Vol. X., 1902, pp. 166–180, map) as 'such as would be logically developed in a country of such combination of hard and soft southwestward dipping strata' as are here found; that is, there are two north-south cuestas formed by the Niagara limestone and the Knobstone standstone, with respect to which the streams are rather systematically arranged, in what seem to be consequent, subsequent and obsequent courses. The author implies an improbably close agreement between the original extent and the present outcrops of certain formations in suggesting that a certain stream, which follows a longitudinal course on weak strata, was deflected into such a course by the sandstones of the next west-lying cuesta when the 'region was first elevated.' The explanation offered for the behavior of one of the master consequents (East White river) in gathering a number of branches from the back (western) slope of the low Niagara cuesta and leading them westward through a notch in the next following Knobstone cuesta, would have been strengthened if it had been presented as exemplifying a type-pattern of drainage well known elsewhere. Indeed, inasmuch as this essay is addressed to professional readers, the essential features of the streams might have been more tersely presented in several instances, had they been named in accordance with a consistent terminology and thus shown to belong to well-recognized classes, rather than described in paraphrases as if they had no relatives elsewhere. The close approach of the Ohio to one of the headwaters of East White river seems to indicate a great and relatively recent increase in the volume of the Ohio, such as has been inferred from other evidence elsewhere.

RIVERS OF SOUTH WALES.

THE most notable characteristic of Strahan's 'Origin of the River-system of South Wales' (*Quart. Journ. Geol. Soc.*, LVIII., 1902, 207–225, map) is the neglect of the capture of headwaters of initial consequent streams by the growth of associated subsequent streams along belts of weak strata. It is shown on good evidence that many streams in the Paleozoic area of South Wales pay no attention to the strong east-west folding or to the pronounced north-northwest faulting of the region; and it is reasonably inferred that they were superposed on the previously much denuded Paleozoic area through a cover of Chalk; but in certain localities where the streams follow a northeast-southwest system of disturbances, a late date is given to the disturbances and the streams are made locally consequent upon them. It is recognized that since superposition there has been great denudation, whereby strong relief has been developed appropriate to the resistance of the rocks; but no accompanying adjustment of streams to structures (except in an altogether minor case) is considered, although it is rather clear that a number of captures must have taken place, as in the growth of the Usk headwaters on the Old Red sandstones north of a resistant Carboniferous escarpment and in the associated beheading of several streams south of the Usk. The theory of the adjustment of streams to structures is altogether too well demonstrated to be set aside as 'transgressing the limits of legitimate speculation.' Yet in accordance with the tacit postulate that all rivers are of consequent origin, Strahan reverts to Ramsay's theory of an anticline to form the divide between the Thames and the Severn. Much of the evidence against this obsolescent solution of the Thames-Severn