attempt has been made to find out if injurious effect is produced by sterilizing this soil, unless we are to understand that pots Nos. 4 and 5 in tables Nos. 1, 2, 3, 4, 5, 6, 7, 8, 11 and 12 are inoculated with a mixture of culture and unsterilized soil. If this premise is correct it is evident that neither culture nor soil inoculation was able to produce nodules in the sterilized soil. If, on the other hand, one is to understand that pots Nos. 4 and 5 in tables Nos. 1, 2, 5, 7 and 11 are inoculated with culture mixed with sterilized soil then we must admit that no true parallel exists between the two series of experiments, and that it is impossible to determine what the effect of the use of pure cultures has been. There is also a contradiction between the headings and subheadings of some of the tables, making it impossible to determine whether that particular series was inoculated or uninoculated.

For the above reasons I would take exception to the summary of results reported by Dr. Stevens and Mr. Temple, and return the Scotch verdict of not proven to their strictures upon pure cultures and the pure culture method of inoculation. The note following the summary referring to Farmers' Bulletin No. 315, "Progress in Legume Inoculation," issued January 11, 1908, quotes the figures reported in that publication in a way that is very misleading. It is obviously impossible to determine whether or not a culture produced nodules if the entire crop is withered by drought or carried away by floods or if other uncontrollable factors entirely apart from the question of inoculation have destroyed the crop. It is, therefore, unfair to compare the 2,037 doubtful results with the 1,770 successes. As stated in Farmers' Bulletin 315, "the successes credited to the culture have been so recorded only when a clear gain was shown to be due to inoculation. A less strict interpretation of the doubtful reports would place many of them in the column of successes, and undoubtedly many classed as failures to secure inoculation would prove upon adequate investigation to have been failures from causes other than deficient nodule formation." If one must express the result in percentages it would be necessary to consider only the failures and successes, making the percentage of successes 78, instead of less than 50.

In closing, I wish to emphasize the necessity in experimental work of paying more attention to the soil conditions which may affect nodule formation. Some reasons for this Mr. Robinson and I have clearly indicated in Bureau of Plant Industry Bulletin No. 100, Part VIII., "Conditions Affecting Legume Inoculation."

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A STUDY OF THE REMARKABLE ILLUMINATION OF THE SKY ON MARCH 27, 1908

ON the night of Friday, the twenty-seventh of March, 1908, between the hours of 7:45 and 8:30, there was an unusual illumination of the heavens. The display was noted by many observers at Sandy Hook, N. J., and at Montclair, N. J. Some of the New York papers stated that the phenomenon was also visible at Hartford, Conn. Beyond a casual and unscientific reference to the matter in the daily press at the time, I have not been able to find any further reports or study of the phenomenon.

The 27th of March was a remarkably clear and warm day, the temperature mounting well above 70 degrees. The evening was also clear, but decidedly cooler. There was no moon, but Venus shone unusually bright in the western sky. This last fact is mentioned particularly, because the best authorities state that the light of a brilliant evening star is sufficient to preclude any marked illumination like that observed. Every one whom I have interviewed informs me that he had never before witnessed any such display. With the exception of one eye-witness at Millburn, N. J., all of my information has been obtained from observers at Sandy Hook, N. J. I was so unfortunate as to witness the last part of the spectacle, only. Details beyond my own knowledge are furnished from accounts given me by army officers stationed at Sandy Hook and members of their respective households.

The illumination was first noted at about 7:45 P.M. It consisted of a bright nebulous band rising north of west from about twenty degrees above the horizon. The light extended across the sky to near the north of east horizon, diminishing in brightness from west to east, the bands in the east and west being connected by three separate bands. At about 8:15, the illumination faded, except the western solid band, which persisted for about ten minutes. Before it disappeared, however, a series of short narrow shafts, nearly parallel to one another, appeared about fifty degrees above the horizon in a direction slightly west of north. The eastern-western illumination was steady, while the northern shafts were "trembly," somewhat suggesting the aurora borealis. It should be remembered, however, that there were no lights of whatever nature in the north, except these detached shafts.

It would seem plausible on first thought to attribute this display to the zodiacal light, or the aurora borealis, or to a combination of the two. The season of the year and the location of the steady glow appear to indicate the zodiacal light. This is rarely seen in our latitude, except near the equinoctial periods; when the inclination of the ecliptic to the horizon is at a maximum—and then only in localities where outdoor illumination is not general, and the air is unusually clear. In the spring the light is first seen as a pale illumination in the west, suggesting an unusual prolongation of twilight. In the autumn, the phenomenon, often called the "false dawn," is visible before daybreak. The zodiacal light is of frequent occurrence in low latitudes, where the illumination sometimes extends across the meridian, forming a secondary display in the east. At times a detached luminous patch is observed in the sky, about 180 degrees from the sun's position. This is called the "gegenschein," or "counter-glow." I can recall no authentic reports of the appearance in our latitude of a secondary light or the counter-glow.

Returning to the exhibition of last March, the zodiacal light hypothesis fails to account for the detached shafts high above the horizon to the west of north. Some writers appear to make a distinction between auroral displays ("fictitious" auroras, as it were), and the characteristic aurora borealis. Reports of the simultaneous displays of the zodiacal light and auroral phenomena are matters of authentic record. In the case under discussion, there is a chance that two independent phenomena were occurring at the same time, but the chance was infinitesimally small. Moreover, one of the most pronounced sensations of the beholder was that he was witnessing *one* phenomenon, with *one* cause.

As is generally known, neither the aurora borealis nor the zodiacal light has been quite satisfactorily explained. The latter has been variously attributed to extensions of the sun's corona, to the reflection of the sun's light from masses of meteoric matter revolving around the sun in planes near the ecliptic, or around the earth itself. Chaplain G. Jones, of the U. S. Navy, who, in 1855, made a particular study of the zodiacal light while on duty in Asiatic waters, could not explain the disposition of the light as he observed it on any hypothesis other than the last mentioned. Reports have also been published of the appearance of a similar band about the moon.

The main difficulty in the way of the study of the zodiacal light is found in the fact that, owing to the nature of the light, the telescope can not be brought into service. Again, a brilliant display is a rarity, except in equatorial latitudes, where observatories are very scarce. If the light were due to the sun's corona, its spectrum should be identical with that of the solar corona, and if due to reflected sunlight alone, the polariscope should show that the light is polarized. Observations with both kinds of instruments show conflicting—or rather mixed results.

The following hypothesis is submitted as a possible explanation of the phenomenon of last March, and is believed to be in line with the latest theory as to the constitution of matter.

Whatever the sun's corona may be, it is not a heat phenomenon pure and simple. If it is

composed of matter at all, it must be in that sub-atomic condition characteristic of the manifestation of electricity. The corona from its very appearance suggests a streaming out from the sun of attenuated matter, or of force. That a repellant force actually emanates from the sun is shown by the solar action upon the tails of comets, always turning them from itself. It seems to have been fairly well established that all substances are radio-active, differing only in degree in the possession of this property. It is but a step further to conclude that all celestial bodies are sending out emanations of matter in the most attenuated state, and that these effects, in the case of the sun, become visible as the solar corona. Following this trend of thought, we may safely assume that the earth and moon each has its own corona. The aurora borealis then may be an exhibition of our corona shining by its own light, the angle at which the sun strikes the corona being such as to preclude the reflection of sunlight to the observer's eye. The zodiacal light might be explained as being due *mainly* to sunlight reflected from our own coronal matter. As in this case we should not be viewing the earth corona by its own light, the flickering effect of the northern light would not be prominent.

The hypothesis here offered seems to account for the puzzling mixed spectra of the so-called zodiacal light. It further explains the existence of the shafts high in the north and the undecided character of the light, on the evening of March 27. Both the zodiacal and auroral theory utterly fail to account for these. Wandering into the domain of conjecture, it is interesting to speculate whether the solar, terrestrial and lunar coronas are identical in nature. If they are not, it would seem to indicate that radio-activity was a function of the heat of the radiating body, and we might expect the spectra to group themselves in the order named as regards simplicity. If the spectra should prove to be the same, we might fairly conclude that coronal material is the final form of disintegrating matter, as a nebula is the first form.

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QUOTATIONS

THE CAVENDISH LABORATORY

LORD RAYLEIGH, as chancellor of the University of Cambridge, performed his first official act by opening the new wing of the Cavendish Laboratory, which Lord Rayleigh, as a Nobel prize-man, presented to the university. The ceremony was all the more interesting because, as Professor J. J. Thomson observed, it occurred upon the anniversary of the opening of the original Cavendish Laboratory, which the university owed to the generosity of the seventh Duke of Devonshire, who was chancellor in 1874. During the thirty-four years that have elapsed since the founding of the laboratory, Lord Rayleigh has been closely connected with it, and the physical research which it was designed to promote. His interest in it, indeed, began, as he remarked yesterday, before it existed. He had then become acutely aware of the scientific destitution of the university, and of the difficulty of acquiring systematic scientific training. Much good work had been done in physical research, but it had to be carried out by earnest students either in their own houses or in some college where the equipment was more meager than students of the present day can easily realize. Lord Rayleigh's activity in seeking a remedy for that state of things was much greater than might be inferred from his characteristically modest remark that he had some share in urging Clerk-Maxwell to accept the appointment of professor of experimental physics. That brilliant man's tenure of the post was not a long one, and on his lamented death in 1879 Lord Rayleigh succeeded him as Cavendish professor. During the five years of his professorship Lord Rayleigh carried out some fundamental researches with results which more recent investigations have only corroborated. Since that time the post has been held and adorned by Professor J. J. Thomson; but Lord Rayleigh's interest in the laboratory and its work has been continuous and keen. The extension which he has given to its accommodation was very urgently needed on account of the steady growth in the