

having a sharp pointed Rod fix'd on the Top of their Masts, with a Wire from the Foot of the Rod reaching down, round one of the Shrouds, to the Water, will not be hurt by Lightning.

From the foregoing it is evident that Franklin drew up definite instructions for erecting lightning-rods before the close of the year 1752 and from the contemporaneous letter, describing the electrical kite, it would appear that at this time some edifices in Philadelphia were thus equipped, but whether for their protection or for experiment is uncertain.

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DRIED COTTON CULTURES ONCE MORE.

It would appear from a recent communication in *SCIENCE* that the original dictum of Messrs. Harding and Prucha that the nodule-forming bacteria will not survive when dried upon cotton is now to be modified to apply only to those cultures dried (?) in a Petri dish enclosed in a paper bag and stored in a drawer or under conditions considered to be similar to this. Although an attempt is made to show that such a method closely resembles that used in the original preparation of cotton cultures, the actual difference in drying time will be so evident to any one who cares to try the experiment that it need not be discussed here. Indeed, it would hardly seem necessary to demonstrate the fact that of two pieces of cotton containing the same amount of moisture, one enclosed in a Petri dish, and the other exposed to the action of an abundance of warm air, the one in the air will dry much sooner than the protected piece. One would as readily expect clothes to dry as rapidly enclosed in a box, wrapped in paper and put away in a drawer as when hung upon a line. In fact it was this very question of rapidity of drying which caused the various 'germ-proof' methods of preparing the cotton to be abandoned. It was found that even in a specially constructed box of large size (approximately 8 x 2 x 2 feet) through which air was forced over a warm coil, that the length of time required for drying was entirely too long, and for practical purposes, considering

the way in which the cultures were to be used, it was better policy to turn out a culture which might be more or less contaminated than to destroy the nodule-forming bacteria in an attempt to produce a pure culture. Experiments showed that all attempts, with the facilities available, to produce absolutely pure cultures upon cotton involved an unwarranted sacrifice of efficiency.

It has been supposed that the result obtained by the users of the dried cotton cultures would of itself be a sufficient refutation of the statement that dried cotton cultures were valueless. This seems to be the case since an attempt is now made to explain the reports received by users of the cultures upon 'psychological' grounds.

If the bacteria will not live upon cotton it is of course necessary to account in some way for the thousands of satisfactory reports received from this country and elsewhere, and the psychological explanation would seem to be as good as any. It should be remembered, however, that many of the favorable reports were furnished by those fully as competent to judge of the results obtained as a member of an experiment-station staff—that a considerable number of these experiments were checked by uninoculated plots and that photographs showing the difference between these treated and untreated plots are available in all cases where it was possible to obtain them. Furthermore, the reports covered tests with sterilized soil and sand as well as field experiments.

Since the publication of a few of the favorable reports first received, there have been many others which emphasize even more strongly the benefit derived from cultures when used under the conditions for which they are designed. During the past season, while the statement that the cotton cultures were valueless was very widely published, producing, one would suppose, anything but a favorable psychological effect, most satisfactory reports continued to be received.

Although it is manifestly impossible in this statement to refer directly to any of these reports, it may not be out of place to cite

briefly the results obtained with dried cotton cultures in competition with pure cultures on agar obtained from Hiltner of Munich, by the Board of Agriculture of Great Britain. This test was conducted by thirteen different agricultural colleges and experiment stations during the year 1905. The general plan of the experiment was as follows:

1. Laboratory experiments with sterilized soil and sand.
2. Pot cultures in unsterilized soils from various sources.
3. Experiments in accord with actual agricultural or horticultural practise.

The results of these experiments are published in detail, and to any one examining them it can not but be evident that the contention that the nodule-forming bacteria properly dried upon cotton will not live is entirely erroneous. In spite of the fact that the cotton cultures were in some instances a year old and had undoubtedly been subjected to various unfavorable conditions, there was no advantage whatever in the Hiltner fresh pure culture over the dried cotton culture. The conclusion reached by the board speaks for itself:

The experiments in which positive results were obtained show that in many cases both Moore's and Hiltner's cultures were alive, and capable of infecting the plants for which they were designed and producing an increased yield.

Concerning the statement that no United States experiment station has been able to obtain 'good results' from dried cotton cultures it would seem that in view of the results obtained either by experiment stations, or under government supervision, in Great Britain, Australia, New Zealand, South Africa, China and other foreign countries—to say nothing of the unquestionable ability of those reporting success in this country—that the 'psychological' explanation should be applied to the experiment station reports rather than to those obtained from successful users of dried cotton cultures. Furthermore, an analysis of the reports from those experiment stations which I have had an opportunity to examine does not reveal the worthlessness

of the cultures which it seems so desirable to demonstrate, and finally it is not an impossibility that under the circumstances the experiment station may not be the final court of appeal in such a matter, after all. One would hardly expect 'any well-marked result' from a culture which, after being prepared, was carefully kept upon ice until ready for use—a method actually employed at one of the experiment stations.

The question of the vitality of nodule-forming bacteria in dried cotton cultures is one that can not be settled by an attempt to explain away the actual results which can be and have been obtained from their use. The incompatibility of these results with the idea that the bacteria are killed by drying is admitted, but it is not the results which will have to fall. I shall be glad to send dried cotton cultures to any one who may wish to arrive at an independent conclusion regarding the ability of the nodule-forming bacteria to withstand drying. These cultures may be retained any length of time up to twelve months before testing, so there will be no question about the period during which vitality is retained. The inoculated cotton will have been dried in a room through which a current of air has been forced, not in a Petri dish.

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SPECIAL ARTICLES.

GEOLOGY OF SOUTH BRAZIL.

[THE following note from Dr. I. C. White, chief of the Brazilian Coal Commission, should have appeared in connection with the communication of Professor Osborn on 'Vertebrate Paleontology,' published in the issue of SCIENCE of August 3, but was received too late for insertion in that issue.]

Mesosaurus Brasiliensis occurs in the Iraty black shale, state of Paraná, a formation whose base lies about 100 meters above the Brazilian Coal Measures which hold the typical Glossop-teris flora of the Ecca series of South Africa, and the Karharbari series of India. It is