leaves are stripped, the cambium suffers starvation to a greater or less extent, depending on the intensity of its competition with other tissues, &c. ; of course a starved cambium will form less wood, and, it may be added, the timber will be poorer.

Again, even if the leaves are not stripped quickly from the tree, but the effect of some external agent is to shorten their period of activity ; or to occupy space, on or in them, and so diminish the amount of leaf-surface exposed to the light and air ; or to block up their stomata, the points of egress and ingress for gases and water ; or to steal the contents of the cells—contents which should normally be passed on for the growth, &c., of other parts of the tree—in all or any of these ways injury to the timber may accrue from the action of the agent in question. Now there are numbers of parasitic fungi which do all these things, and when they obtain a hold on pure plantations or forests, they may do immense injury before their presence is detected by anyone not familiar with their appearance and life-histories.

The great difficulty to the practical forester who attempts to deal with these "leaf diseases" is at least twofold ; for not only are the leaves so numerous and so out of reach that he can scarcely entertain the idea of doing anything directly to them, but (and this is by no means so clearly apprehended as it should be) they stay on the tree but a short time as a rule, and when they fall are a continual source of re-infection, because the spores of the fungi are developed on them. It is a curious fact that those fungi which are known to affect the leaves of forest-trees nearly all belong to two highly-developed groups—the Uredineæ and the Ascomycetes—and the remarkable biological adaptations which these parasites exhibit for attacking or entering the leaves, passing through periods of danger, and so on, are almost as various as they are numerous. Some of them, such as the *Erysipheæ* or mildews on beeches, oaks, birches, ashes, &c., only form small external patches on the leaves, and do little if any harm where the leaf-crown is large and active ; others, such as many of the very numerous *Sphæriaceæ* and their allies, which form small dark-coloured flecks and spots on leaves, may also be looked upon as taking only a slight tax from the leaves. Even in these cases, however, when the diseases become epidemic in certain wet seasons, considerable damage may accrue, because two chief causes (and many minor ones) are co-operating to favour the fungus in the struggle for existence: in the first place, a continuously wet summer means loss of sunlight and diminished transpiration, &c., to the leaves, and so they form smaller quantities of food materials ; and secondly, the damp in the atmosphere and leaves favours the fungi, and so they destroy and occupy larger areas of leaf surface.

It should be mentioned here, by the way, that all leaves of all trees are apt to have fungi on them in a wet summer, but many of these are only spreading their mycelia in all directions over the epidermis, in preparation, as it were, for the fall of the leaf: they are saprophytes which feed on the dead fallen leaves, but cannot enter into them while yet alive. In some cases, however, this preparation for the fall is strikingly suggestive of adaptation towards becoming parasites. I will quote one instance only in illustration of this. On the leaves of certain trees in Ceylon, there was always to be found in the rainy season the much-branched mycelium of a minute *Sphæria:* this formed enormous numbers of branches, which, on the older leaves, were found to stop short over the stomata, and to form eventually a four-celled spore-like body just blocking up each stoma on which it rested. So long as the leaf remained living on the tree, nothing further occurred ; but wherever a part of the leaf died, or when the leaf fell moribund on the ground, these spore-like bodies at once began to send hyphæ into the dying tissue, and thus obtained an early place in the struggle for existence among the saprophytes which finished the destruction of the cells and tissues of the leaf.

There is another group of fungi, the *Capnodieæ*, which form sooty black patches on the leaves, and which are very apt to increase to a dangerous extent on leaves in damp shady situations: these have no connection with the well-known black patches of *Rhytisma* from which the leaves of our maples are rarely free. This last fungus is a true parasite, its mycelium penetrates into the leaf tissues, and forms large black patches, in and near which the cells of the leaf either live for the benefit of the fungus alone, or entirely succumb to its ravages: after the leaf has fallen, the fungus forms its spores. Nevertheless, although we have gone a step further in destructiveness, foresters deny that much harm is done to the trees—no doubt because the foliage of the maples is so very abundant. Willows, pines, and firs suffer from allied forms of fungi.

But it is among the group of the *Uredineæ* or rusts that we find the most extraordinary cases of parasitism, and since some of these exhibit the most highly developed and complex adaptations known to us, I propose to select one of them as the type of these so-called "leaf diseases." This form is *Coleosporium Senecionis* (*Peridermium Pini*), rendered classical by the researches of several excellent botanists.

It is true, *Coleosporium Senecionis* is not in some respects the most dangerous of these fungi—or, rather, it has not hitherto been found to be so—but in view of the acknowledged fact that foresters have not as yet been able to devise practical measures against the ravages of these numerous rust-fungi, and since we are as yet very ignorant of the details of the biology of most of them, it seems advisable to choose for illustration a form which shows in a distinct manner the complexities of the subject, so that those interested may see in what directions biologists may look for new results. That the story of this fungus is both complicated and of great biological interest will be sufficiently evident from the mere recital of what we know concerning it.

H. MARSHALL WARD.

(*To be continued.*)

MICHELL'S PROBLEM.

FOR the last two hundred years the attention of logicians and mathematicians has been directed to the inverse principles of the theory of probability, in which we reason from known events to possible causes. Two different methods of calculation are in use, which give approximately the same results. According to the celebrated theorem of James Bernoulli, "If a sufficiently large number of trials is made, the ratio of the favourable to the unfavourable events will not differ from the ratio of their respective probabilities beyond a certain limit in excess or defect, and the probability of keeping within these limits, however small, can be made as near certainty as we please by taking a sufficiently large number of trials." The inverse use of this theorem is much more important and much more liable to objection and difficulties than the direct use. In the words of De Morgan, "When an event has happened, and may have happened in two or three different ways, that way which is most likely to bring about the event, is most likely to have been the cause."

The second principle, due to Bayes, is thus given by De Morgan, "Knowing the probability of a compound event, and that of one of its components, we find the probability of the other by dividing the first by the second."

These principles have been accepted by the great majority of thinkers, and freely used by Laplace, Poisson, Herschel, and De Morgan. Stanley Jevons ("Principles of Science") gives a luminous account of the value of the theory, and accepts Michell's views: "If Michell be in error, it is in the methods of calculation, not in the general validity of his reasoning and conclusions."

On the other hand, Leibnitz, Kant, Forbes, Boole, and Mill ("Logic," xvii., xviii., xxv.), while allowing some value to the theory, doubt if it can be rigorously applied to obtain definite numerical results.

The interest and importance of the subject, and the length of time which has elapsed since any detailed discussion of it has been undertaken, furnish an excuse for the following suggestions, which are made in the hope that they may elicit more valuable arguments and opinions.

More than a century ago, Michell (Phil. Trans., 1767, p. 243) attempted to find the probability that there is some cause for the fact that the stars are not uniformly distributed over the heavens, but frequently form binary combinations or larger groups. Michell's results are quoted with approval by Laplace ("Théorie des Prob," p. 63), and by Herschel ("Astronomy," p. 607), though the latter mentions that Michell's data are too small, and immediately afterwards quotes Struve's solution of the same problem, which seems to be inconsistent with Michell's. I select Michell's problem for discussion, since it has been accepted by high authority and vigorously attacked, and for the sake of simplicity in the calculations shall confine my remarks to binary combinations.

Michell's statements are not very clear, and his arithmetical methods are cumbrous, but his argument may be condensed as follows: "What, it is probable, would have been the least apparent distance of any two or more stars anywhere in the whole heavens, upon the supposition that they had been scattered by mere chance?" Imagine any star situated on the surface of a sphere ($S = 4\pi r^2$) of radius r, and surrounded by a circle of radius a ($= r \sin \theta$, where θ is the angle subtended by a at the centre of the sphere), the area of this small circle is $s = \pi a^2 = \pi r^2 \sin^2\theta$. The probability that another star, "scattered by mere chance," should fall within this small circle is $\frac{s}{S}$, and that it should not fall within it $\frac{S-s}{S}$. But there is the same chance for any one star as for any other to fall within the circle, hence we must multiply this fraction into itself as many times as the whole number of stars (n) of equal brightness to those in question. "And farther, because the same event is equally likely to happen to any one star as to any other, and therefore any one of the whole number of stars (n) might as well have been taken for the given star as any other, we must repeat the last found chance n times, and consequently $\left(1 - \frac{s}{S}\right)^{n^2}$ will represent the probability that nowhere in the whole heavens any two stars among those in question would be within the given distance (a) from one another, and the complement of this quantity to unity will represent the probability of the contrary."

In the case of the two stars, β Capricorni, Michell takes $n = 230$, $\theta = 3' \, 20''$. Hence

$$p = \frac{s}{S} = \frac{(\sin 3' \, 20'')^2}{4} = 1/4254519,$$

which Michell takes as 1/4254603; and

$$Q = \left(1 - 1/4254603\right)^{n^2} = 1 - \frac{52900}{4254603} = 1 - 1/80\cdot4;$$

or, according to Michell, the probability is 80/81 that no two stars equal in size to β Capricorni shall fall so near to one another as they do.

Prof. J. D. Forbes (*Phil. Mag.*, December 1850) objects to the entire principle upon which Michell's work is based, and has pointed out some errors in detail. Todhunter ("Theory of Prob.," p. 334) and Boole ("Laws of Thought," p. 365) countenance these objections; but before discussing them it will be well to mention other attempts to solve the same problem.

Struve ("Cat. Nov.," p. 37) has used an entirely different method. The possible number of binary combinations of n stars is $\frac{n(n-1)}{1 \,.\, 2}$; and the chance that such a pair should fall on a small circle of area s is s/S, where S is the surface of the portion of the sphere in which n has been counted. Hence the chance that any pair of stars should fall within the circle is $n(n-1)s/2S$.

Taking S as the surface from $-15°$ of declination to the North Pole, $n = 10229$, and $\theta = 4''$, Struve finds $p = 0\cdot007814$.

Herschel ("Ast.," p. 607), either in error or by a recalculation from different data, quotes Struve as finding that the probability is 1/9570 against two stars of the 7th magnitude coming within 4″ by accident.

Applying Struve's formula to Michell's data for β Capricorni, we have

$$1 - \frac{230 \times 229}{2} \times \frac{1}{4254603} = 1 - 1/161\cdot5,$$

or 161/162, as the probability that no two such stars fall within the given area.

Forbes, with the aid of a mathematical friend, offers the following solution:—Suppose the n stars are represented by dice, each with $v(>n)$ sides, where v represents the number of small circles in the spherical surface, or S/s. The chance of two stars falling into one circle is the same as that two dice show the same face.

The total number of arrangements without duplication is—

$$v \,.\, \overline{v-1} \,.\, \overline{v-2} \,.\,.\,.\,.\, \overline{v-n+1},$$

and the total number of falls is v^n; hence the probability of a fall without duplication is—

$$v \,.\, \overline{v-1} \,.\, \overline{v-2} \,.\,.\,.\,.\, \overline{v-n+1}/v^n;$$

and the chance that two or more dice show the same face is—

$$1 - \underline{|v} / \underline{|v-n} \,.\, v^n.$$

In the case of β Capricorni $v = 4254603$, and $n = 230$. Evaluating by Stirling's theorem, Forbes gives $p = 0\cdot00617 = 1/160$ nearly, which does not differ much from $n^2/2v$.

A recalculation has given me $p = 1/162$. The result then agrees with that of Struve and differs from that of Michell.

The following suggestions are due in substance chiefly to Boole and Forbes, but their language has been freely altered, and misapprehension of their meaning may therefore be feared.

In all such cases an hypothesis ("the random distribution of stars") is assumed, and the probability of an observed consequence ("the appearance of a double star") calculated. The small probability of this result of the assumed hypothesis is held to imply that the probability of the hypothesis is equally small, and therefore the probability of the contrary hypothesis is very large.

According to Boole, "the general problem, in whatever form it may be presented, admits only of an *indefinite* solution," since in every solution it is tacitly assumed that the *a priori* probability of the hypothesis has a definite value, generally 0 or 1, and also a definite probability is assigned to the occurrence of the event observed if the assumed hypothesis were false.

In Michell's problem it is assumed that the stars are either scattered at random or obey a general law: no notice is taken of the possible case that a general law holds for stars within a certain distance from our system, beyond which an entirely different law may obtain. Again, the subjection of each system to a separate intelligence is tacitly ignored.

The probability of an event is the value of the expectation of its occurrence existing in the mind of the thinker: "We must again warn the reader that probabilities are in his mind, not in the urn from which he draws" (De Morgan, "Enc. Met.," 414); but in the solution of these problems this subjective value is converted with startling ease into a much more objective and concrete expression. As Forbes puts it, "The doubt existing whether an event still future, which may happen in many different ways, shall occur in one particular way is not equivalent to an inherent improbability of its happening, or having happened, in that way"

We do not assume that a friend is speaking untruly when he tells us that, out of 10001 seats, the number of his ticket is 453, yet the antecedent probability is 1/10000 against the truth of his statement. The chances are greatly against ten stars out of 230 appearing as binary combinations; but, according to one view of the meaning of "random distribution," that arrangement is no more unlikely than any other, and we should be no more surprised to hear that one rather than another is the actual one. Forbes objects that "to assume that 'every star is as likely to be in one position as another,' is not the expression of the idea of random or lawless distribution." The expression seems to me to be true, but its interpretation into mathematical symbols has been far too closely restricted both by Michell and Forbes.

"Michell assumes that, with random distribution, the chance of finding a star in a space is proportional to the space, or that a perfectly uniform distribution would be that alone which would afford no evidence of causation."

Suppose the whole surface of the sphere cut up into minute equilateral triangles, and a star placed at each collection of angular points. Each star is the middle point of a regular hexagon, and at a distance, a, from six other stars. If we imagine the six stars to be fixed, and the central star shot out from the centre of the sphere so as to fall within the hexagon, that it may not fall within a distance, r, of any other star it must fall in a regular hexagon, the side of which is $(a - r)$ situated symmetrically within the larger hexagon. The probability of the star falling within this smaller hexagon is expressed by $\frac{(a-r)^2}{a^2}$, which becomes less and less the more nearly r equals a; that is, the more nearly the distribution is truly uniform. When $r = a$, the expression becomes 0, or the probability of exactly uniform distribution is *nil*, and apparently uniform distribution is due solely to the imperfections of our instruments. Michell, however, seems to assume this probability to be 1, or certainty. Struve's method is open to the grave objection that he assumes that the total possible number of binary combinations really occur. Applying his formula to calculate a value for n which makes the chance a certainty, we find that, if 2917 stars are scattered over the sphere, it is a certainty that each will be within 3′ 20″ of another! Of the three methods, that of Forbes seems to be the least open to objection.

Besides these fundamental difficulties in principle, there are several very doubtful points in the calculation which may be worthy of a brief notice.

Michell considered the whole surface of the sphere, though in his time the examination of the southern hemisphere was hardly complete enough to furnish the requisite data The stars do not lie on the surface of a sphere, but scattered through infinite space, so that two stars, the angular distance between which is apparently small, may in reality be very far apart. Suppose that the nearer star lies on the surface of our imaginary sphere, the probability that the direction of the other star is within 15° of the surface is only about one-fourth. Hence the number of apparently double stars must be reduced to a considerable but unknown extent.

Forbes throws considerable doubt on the correctness of raising a second time to the power n. Struve's multiplication by n's seems to prove very curious conclusions. Mr. Venn's reasons for dissenting from Michell's solution will be found well worthy of perusal ("Logic of Chance," p. 260).

SYDNEY LUPTON.

VEGETABLE RENNET.

THE idea that the protoplasm or living substance of both animals and plants is essentially similar, if not quite identical, has long been accepted by both physiologists and botanists. This similarity is most easily seen in the very lowest members of both kingdoms; in fact, for a very long time doubt existed in the case of many organisms—*e.g.* Volvox—as to which kingdom they should properly be included in. Even now it is hardly possible to formulate a definition of "plant" or "animal" which shall put all into their proper positions. When we go higher up the scale in both the animal and the vegetable world, this difficulty of course disappears, on account of the differences of organization and development. It is not difficult even here to trace a remarkable similarity of properties in the living substance, which leads to the conception that not only is protoplasm practically the same in animal and vegetable, but that its activities in the two cases that is, the metabolic processes which accompany, and are in a way the expression of, its life—are fundamentally the same. In both kingdoms we have as the sign of its life the continual building up of the living substance at the expense of the materials brought to it as food, and the constant breaking down of its substance with the consequent appearance of different organic bodies, which are strictly comparable in the two cases. The vegetable protoplasm produces starch, the animal glycogen—both carbohydrate bodies of similar composition and behaviour. In both organisms we meet with sugars of precisely similar character. The proteid bodies long known to exist in animals, and classed into albumins, globulins, albumoses, peptones, &c., have been found to be represented in vegetables by members of the same groups, differing but in minor points from themselves. We have fats of complex nature in the animal represented by oils of equal complexity in the vegetable, their fundamental composition being identical; even the curious body lecithin, so long known as a constituent of nervous tissue in the animal, having been procured from the simple yeast plant.

Further, the changes which give rise to these bodies, or which bring about various transformations of them, have been in very many cases demonstrated to be due to similar agencies at work in both the animal and vegetable organism In many cases, no doubt, they are produced by the actual splitting up of the protoplasm itself; but apart from this we have their formation in large quantities by the agency of bodies which are known as unorganized ferments, and which are secreted by the protoplasm for the purpose of such formation. Perhaps no line of research in vegetable physiology in recent years has been so productive of good results as the investigations that have been made into the occurrence of such bodies, and the comparison of them with those that are met with in the animal organism. Diastase in vegetables, and the ferments of saliva and of pancreatic juice in animals, possess the same power of converting starch into sugar. The peptic and tryptic ferments of the stomach and pancreas respectively have been shown to have representatives in the vegetable kingdom, and these not only in such cases as the carnivorous plants, but to be actually made use of in such truly vegetable metabolism as the processes involved in the germination of the seed. The conversion of albumins and other indiffusible proteids into a further stage than that of diffusible peptone—