when a source of light was reflected from a metal surface covered with regular scratches or corrugations of a given form, but beyond this he does not remember having seen any other bookwork or examples on the same subject. A general investigation of the theory of brilliant points is now given by Mr. W. H. Roever in the *Annals of Mathematics* for April, pp. 113–128.

When a ray of light emanating from a source which we will call P_1 is reflected at any surface, and an eye is placed at another point, P_2, a point of the surface from which the reflected ray travels directly towards the eye appears luminous and is called by Mr. Roever a *brilliant point*. A mathematical investigation also involves the consideration of points from which the reflected ray travels directly away from the eye, and although such points obviously do not correspond to any visible phenomena, it is necessary to consider them under the name of *virtual brilliant points*. If the reflecting surface is a thin wire, a point P_0 will be a brilliant point if the lines P_0P_1 and P_0P_2 make equal angles with the tangent line to the wire at P_0. We thus get the notion of a brilliant point on a *curve*. Taking next a

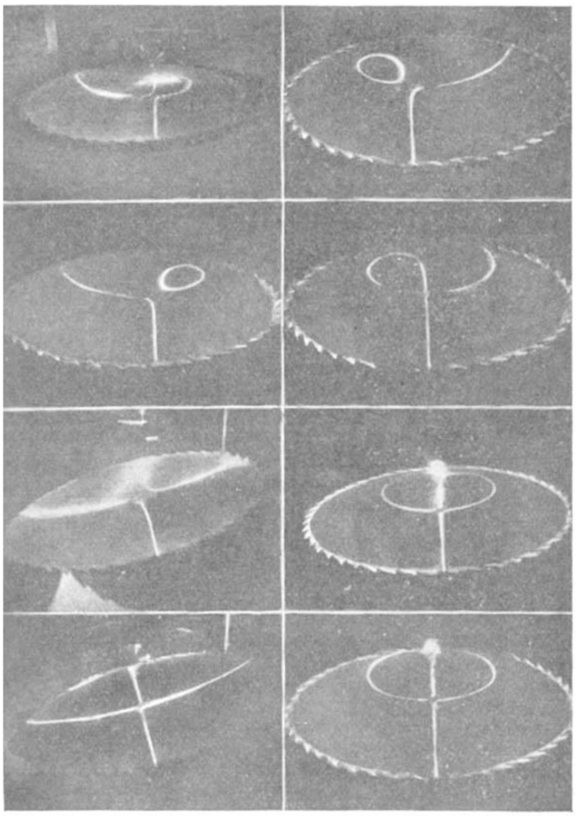

FIG. 1.—Bright lines on a circular saw.

finely but regularly scratched or corrugated surface, the locus of the brilliant points with respect to the curves defined by the scratches or corrugations is the brilliant curve of the system. For a doubly infinite series of curves, the equations of which contain two independent parameters, the mathematical theory leads to the consideration of a brilliant surface as the locus of the brilliant points, although it is not easy to see how this generalisation could be made the subject of experimental verification.

The author, after giving a general investigation, considers the particular cases of the brilliant curve for a circular saw or disc of steel in which the scratches form concentric circles, and also for a rotating carriage wheel in which the curve is generated by the brilliant points of the spokes, *i.e.* of a family of radiating lines in one plane. In both cases the curves are of the fourth degree. The accompanying diagrams are reproductions of photographs of some of the curves obtained with the circular saw. An obvious further example of loci of brilliant points is afforded when moonlight is reflected from waves or ripples on the sea or a lake.

VARIATION—GERMINAL AND ENVIRONMENTAL.[1]

"THE most critical and momentous period in the life-history of any plant or animal," says Prof. Cossar Ewart, "is during the conjugation of the male and female germ-cells." The variation which flows from this blending of the reproductive elements he speaks of as "germinal." That which occurs in the germ-cells up to the moment of conjugation, together with the variations during development and growth, he designates as "environmental."

It may perhaps be questioned in passing whether the distinction is one that can always be observed in practice, and also whether Prof. Ewart's terms are the best that could have been adopted. However, they serve sufficiently well for the purpose of the paper before us.

Some "congenital" characters, he proceeds, may be "acquired"; for example, dwarfing due to embryonic malnutrition. The double uterus of a wild rabbit contained eight young in one division and four in the other, the weight of the two divisions with their contents being nearly equal. In such cases the offspring that has been starved before birth, should it survive, may eventually reach the normal size and produce normal descendants. Antenatal injury, as in constriction by the cord, may lead to "congenital" abnormalities which are neither "inherited" nor transmitted.

Individual plasticity in response to environmental conditions is an obvious and undoubted fact. But can variations so induced be transmitted to descendants? This is still a burning question, in regard to which Prof. Ewart has as yet met with no evidence to support an affirmative answer. On the other hand, the results of his experiments have afforded much reason for the positive belief that the handing on in any form of acquired traits is extremely improbable.

But although there is no evidence of the transmission of somatic characters acquired in virtue of individual plasticity, it is still possible that the general vigour of the somatic tissues may be reflected in the germ plasm, and also that the condition as to ripeness of the generative products may influence the nature of the combinations formed during conjugation. A young Jacobin-barb pigeon mated with an old turbit produced first two young ones which were devoid of all the distinctive points of both parents; but afterwards, on several successive occasions, hatched out offspring which presented points of resemblance with the dam. Prof. Ewart declares that he can only account for this by saying that as the female parent increased in age and vigour her germ-cells increased in prepotency. When she went out of condition, the single offspring then produced more closely resembled the sire.

Some experiments with rabbits led to unexpected results. Several white does were mated with wild males, and several wild does with a white buck. In every case the offspring resembled wild rabbits in form and colour. But the mating of the half-wild offspring with each other, uniform as they were, led to an "epidemic of variation," not only in colour, but also in size, disposition and other qualities. When the half-wild rabbits were crossed with white bucks or does, there were also always several colours represented in the cross-bred litters. An intimate relation was discoverable in all these and their offspring of the next generation between the colour, the "wildness," the time at which maturity was reached, and the rate of growth. Though the half-wild progeny were all wonderfully like wild rabbits, it was evident that in them the stability of the wild rabbit had been broken down.

Further experiments with rabbits—one of which, narrated at length by Prof. Ewart, is of remarkable interest—tend to show that, as in the case of pigeons, the relative maturity of the male and female elements has a definite influence on the character of the offspring. The general results may be summarised as follows:—When insemination precedes ovulation, the young resemble the buck; when it follows, they resemble the doe; when it coincides, some take after the buck, some after the doe, while others may differ from both parents and resemble some of the less remote ancestors. It was incidentally shown that in the rabbit, spermatozoa may retain their potency several days after they reach the fallopian tube. Prof. Ewart notes the

[1] "Variation: Germinal and Environmental." By J. C. Ewart, M.D. F.R.S., Regius Professor of Natural History in the University of Edinburgh. *Scientific Transactions* of Royal Dublin Society, 1901 p. 353–378. (Williams and Norgate).

correspondence of the foregoing results with those obtained by Vernon in echinoderms.

The difference between different members of the same family must be in part attributed to the potential difference of the cells from which they are respectively developed. Whether the reducing division of the germinal cells is qualitative as well as quantitative is an open question, but there is reason to think that the life-history of these cells previous to conjugation may give opportunity for environmental variation like that of the Protozoa.

Turning now to the subject of "germinal variation," the author points out that the existence of such environmental variation in the germ-cells, apart from reducing division, together with the physiological differences dependent on diverse conditions of vigour and maturity, may be expected in most cases to preclude the new individual from assuming an exactly intermediate position between its parents. When the male and female germ-cells unite, a series of contests takes place between groups of vital units, the issue being decided by their respective qualities, individuality or character.

When different varieties or species are intercrossed, the effects may differ not only in degree but also in kind from those of ordinary cross-fertilisation. The following are some of the results that have been obtained experimentally from such intercrossing :—

(1) The offspring may be almost exactly intermediate between the parents.

(2) The offspring may resemble one of the parents and not the other. This is often the case when wild animals are crossed with tame varieties of the same species. (It must, however, be remembered that the resemblance may be only superficial, as was clearly the case in the experiments with half-wild rabbits cited above.)

(3) Some of the offspring may resemble one parent, some the other. (This seems especially likely to occur if one or both of the parents is a sport. Standfuss's results with insects are in accord with this.)

(4) The offspring may combine, almost unimpaired, the more striking characters of both breeds. This has been seen in both pigeons and rabbits.

(5) New or unexpected characters may appear in the progeny. Three out of four of a litter of cross-bred rabbits developed a habit of "spinning."

(6) When half-breeds are crossed, the offspring tend to be extremely variable. Evidence of this is plentiful both in animals and plants.

(7) Sometimes the offspring, instead of resembling the parents, resemble former ancestors. Prof. Ewart mated a cross between an "archangel" and an "owl" pigeon with a white fantail. The issue was a bird with a striking resemblance to the ancestral "blue-rock." (Analogous results have been several times obtained in the case of insects.)

Prof. Ewart's paper is interesting and suggestive to a high degree. It would be hard to overestimate the value of the experiments which he is conducting with so much care and judgment in his well-selected menagerie at Penycuik.

F. A. D.

RUST-FUNGUS.

PROF. MARSHALL WARD'S investigations into the relations between host and parasite in the case of the Brome-grasses and their rust-fungus are bringing to light some interesting facts which have important bearings on the long-vexed questions of wheat-rust and the rust problem generally, which, as is now well known, have passed into an acute stage of late, principally owing to Eriksson's enunciation of his belief that the fungus can be transmitted in an invisible form *viâ* the seed.

In addition to testing this mycoplasm hypothesis of Eriksson's, the researches undertaken by Prof. Marshall Ward are also directed to put to the proof the questions of degrees of specialised parasitism raised during the last decade by the researches of Plowright, Kleebahn, Eriksson, Magnus, Fischer, and others, and more especially, to see if any deeper insight can be obtained into the causes of epidemics and the relative predisposition or immunity of certain plants to attack.

In a paper read to the Cambridge Philosophical Society on January 20, 1902, Prof. Ward gave a summary of his results with more than eighteen hundred infection experiments, made on twenty-two species and varieties of Bromus with the Uredospores of *Puccinia dispersa* (Erikss.), the brown-rust of these grasses. These results show clearly that, other conditions being the same, the infection of a given species of Bromus—say *B. mollis*—by the Uredospores of the Puccinia depends on the origin of the spores, that is to say, on the circumstances of nutrition and breeding generally to which they have been hitherto accustomed. For instance, if the spores are reared on *B. mollis*, they infect another plant of *B. mollis* readily; but if they are reared on *B. sterilis*, they refuse to infect *B. mollis*, though they will readily infect another plant of *B. sterilis*.

But, in addition to the infective capacity of the spores conditioned by their past history, there is the question of the predisposition or immunity of the host. For instance, it is easy to infect *Bromus mollis* with spores from *B. mollis*, but far less easy to infect *B. racemosus* with such spores, and practically impossible to successfully infect *B. sterilis*. Part of Prof. Marshall Ward's work goes to prove that the immunity of given species of Bromus is not due to anatomical peculiarities, such as the number and size of the stomata, hairs, the volume of chlorophyll tissue and so forth, but to some substances or conditions in the living cells which escape microscopic investigation. In other words, the inquiry is being pushed into the domain of enzyme reactions, anti-toxins and so forth.

In a forthcoming paper it will also be shown that the external conditions of germination of the spores, and of infection by way of the stomata, require far more attention than they have yet received.

In a paper read to the Royal Society on February 20 last, another aspect of the investigation was opened up, namely, the possibility of obtaining pure cultures of these Uredines, a method which applies to other parasitic fungi as well.

In order to obtain more decisive answers to such questions as —Are any of the results obtained on plants in the open, or merely covered with bell-jars and so forth, due to spores accidentally introduced, or to mycelium, &c., already in the plant? a number of infections were made on seedlings germinated and grown antiseptically in tubes as follows :—

Clean picked seeds were placed singly, by means of forceps, on filter paper at the bottom of Petri-dishes properly sterilised by heat. When these had germinated, and observation showed that the whole series was free of moulds or other signs of contamination, the seedlings were removed by means of sterile forceps and transplanted singly into sterilised tubes of various kinds as described below, and the further growth allowed to proceed in the light under conditions varied as will be seen.

Prof. Ward had already shown that seedlings will continue to grow in such tubes, but, as we have seen, in the cases previously described he had no guarantee that the seedlings introduced into the culture-tubes did not already carry on their leaves wind-borne or otherwise transmitted spores.

In the case of these seedlings germinated from clean "seed" in sterile dishes and tubes, it is obvious that the only chance of infection depends on spores attached to the "seed" or on mycelium in the seed.

Experiments with seed gathered even from badly rusted plants and germinated as above have never given rusted seedlings, although other experiments have shown that the germ-tubes of attached spores can infect seedlings when the plumule is only 3–5 mm. high. Nor has Prof. Ward ever been able to discover any trace of mycelium in the seeds.

But if the "seed" of the Bromus is sterilised before germination—as can be done by steeping in various antiseptics, or by heating to 60–70° C.—it is found that pure cultures of the Brome may be obtained in the tubes, and it is then only necessary to infect such a clean seedling with the spores of the parasite to obtain a pure culture of the latter.

Preliminary experiments soon showed that the Brome seedlings thus raised from seeds treated antiseptically, and protected from the first by glass, may be grown for weeks and even for a couple of months in such tubes under proper precautions, and Prof. Ward set himself the task of ascertaining how such cultures would behave in infection experiments.

In the following experiment upright tubes of the kind known to chemists as "drying towers" were prepared as in the diagram (Fig. 1), so that by means of an aspirator attached to the tubing at G, a continuous current of damp air could be slowly drawn through the whole series, aërating the roots of the seedlings F, which burrowed into the cotton-wool B, day and night. The tubes were charged each with one seedling,