suggestive. Mr. Pollard and I did not smooth our observations, as has been done in some other cases, because it was desirable to show how much variability is due to local conditions, how much to observational error. Much of what Prof. Brown would attribute to want of homogeneity in our material I know to be due to the changes in external contour, and this is one of the urgent problems of dam construction. Our experimental work shows that very large changes are made by modification of the contour. Is it not possible to reach experimentally better forms for the flank of a dam than are at present in use?

The cost of experimental work on a suitable scale is prohibitive to the individual experimenter, but it appears to me that something on a really big scale ought to be done by the Institution of Civil Engineers. The problem is a very important one, and I do not believe, notwithstanding what I have read to the contrary, that the keen mathematician feels anything but intense awe in the presence of the creative engineer, who has succeeded, notwithstanding the weak-kneed theory provided by men of science, in producing such monumental works as the Assuan and Vyrnwy dams by the force of mechanical appreciation alone. KARL PEARSON.

The Nature of γ and X-Rays.

IN a letter to NATURE of October 31, 1907 (vol. lxxvi., p. 661), a copy of which arrived here recently, Mr. Barkla criticises a paper of mine which was published in the *Philosophical Magazine* for October. In that paper I tried to show how closely the properties of γ and X-rays were in agreement with the hypothesis that they consisted mainly at least of neutral pairs, and I pointed out that even the peculiar polarisation effects which Mr. Barkla had shown to exist might be explained, not unreasonably, as a consequence of the rotatory motion which such pairs would probably possess. I suggested that a pair might be more likely to become entangled with and deflected by an atom revolving in the same plane as itself.

Mr. Barkla describes an experiment in which he has measured the amount of scattering of X-rays in different directions, and compares his results with those which he expects to obtain as the result of calculations based on each of the two hypotheses in turn, that of the neutral pair and that of the ether pulse. He states the results to be against the former theory.

But for the result from his calculations in the case of the neutral pair, he makes the assumption that the probable direction of motion of a neutral pair on emergence from an atom with which it has been entangled is independent of its original direction of motion.

There is no justification for this assumption. It does not even appear to be probable. Consequently, the experiment has no value as a critical test. Yet I fully agree with Mr. Barkla that the dependence of the amount of secondary radiation upon the angle which its line of motion makes with that of the primary ray is a very proper subject of study; it might be expected to furnish much-wanted information.

For this reason I also have been investigating one aspect of this question. With the assistance of Mr. J. P. V Madsen, of this University, I have been comparing the secondary radiations issuing from the two sides of a plate through which γ rays are passing. On the ether pulse theory there should be complete symmetry, provided that the rays have not been appreciably absorbed on their way through. Secondary radiation, whether material or not, originated in an atom by a passing pulse, is just as likely to go forwards as backwards. This is, indeed, always assumed by writers on the ether pulse theory, *e.g.* by Mr. Barkla himself in the letter already referred to.

On the other hand, if the γ rays are material, it is quite possible, though not necessary, that the secondary radiations on the two sides of the plate should be different.

As a matter of fact, there is the most remarkable want of symmetry, and this is fatal to the ether pulse theory of the γ rays. Moreover, all our experiments so far show that, on the whole, the kathode radiations from a given stratum of matter traversed by γ rays possess momentum in the original direction of motion of the rays, and this shows that the rays are material.

The experiments are very simple, and are not wholly new. The secondary kathode radiations due to a stream of rays impinging on a plate have been studied by many observers, who all concur in the statement that they increase with the atomic weight of the material of the radiating plate. For example, the figures for Pb, Al, and C are about as 100 to 30 to 15. In fact, they follow almost the same law as in the case of β particles. The reason for this will appear presently. The secondary kathode radiations that appear where γ rays *emerge* from a plate have been less studied, but Eve has shown that they consist largely of kathode particles, and Dawes (*Phys. Rev.*, xx., p. 182) that they do not appear to follow the same law as what may be called the "incidence" radiations. Further, Wigger has made the very important observation ("Jahrbuch der Radioaktivität," Bd. ii., pp. 428–430) that in certain circumstances the γ rays issuing from a plate of Al which they have traversed make *more* secondary rays than when they issue from Pb.

All these facts may be conveniently studied together. Let an ionisation chamber be made of cylindrical form with plane ends, and let a pencil of γ rays be directed along the axis. Let the rays first pass through a cm. or so of lead. Let them then pass through a pair of plates which can be inverted; a convenient pair may be made of a lead plate 1 mm. and a carbon plate 1 cm. thick; these are to form one end of the chamber. It will then be found that there is more current through the chamber when the C plate is next to it, and the "emergence" secondary rays are produced in the carbon, than when the Pb plate is next the chamber. But if the plate closing the other end of the chamber be at one time Pb, at another time C, the reverse effect occurs. At this plate "incidence" secondary rays are produced by the same pencil of γ rays, and these are greater when the plate which is struck is Pb than when it is C. The differences are of the order of 10 per cent., 20 per cent., up to 60 per cent., depending on the circumstances of the experiment. The materials and the form of chamber may be largely varied, but the same want of symmetry is always there.

It appears that the "emergence" radiations from the plate through which the γ rays have entered are more important than the "incidence" radiations from the other plate. The latter serves mainly as a reflector or scatterer of the rays from the former, and this is the reason why there is less current when it is formed of a material of smaller atomic weight, following the usual rule for β rays. This effect is a general one, and it serves to explain why all observers have found less kathode radiation due to γ rays from Al or C than from Pb, when actually the rays produce more from the former than the latter when they have been first sifted by a cm. or so of lead. A stream of γ rays contains β particles derived from the γ rays by the influence of the last substance traversed. It is the scattered portion of these which constitutes the main portion of the secondary radiations due to γ rays, and the reason why the incidence radiations run parallel to those of β rays is obvious. Nevertheless, there are small variations due to secondary rays formed in the material of the plate itself, the quantity of which is influenced by the nature, not only of this material, but of the screens through which the rays have previously passed. This is because the γ rays are heterogeneous, as first shown by Kleeman. It is when the rays have passed through some thickness of lead that they are acted on with greater effect by Al or C than by lead. The quantity of kathode radiation set free in the radiator itself depends on the quality of the rays as well as on the radiating material. The particles at first move mainly, perhaps entirely, in the original direction of motion of the γ rays, but are subsequently scattered, and contribute to some extent to the "incidence" secondary radiation. But the principal portion of the incidence radiation is due to β particles which were in the stream of γ rays before it struck the plate.

It would make this letter too long if I were to discuss these results with any fulness, or to show their relations to results already obtained. I hope to publish a fuller account in a short time. Meanwhile, I will point out that the experimental proof of the material nature of the γ rays carries with it, almost surely, a corresponding proof as regards the X-rays. The points of similarity are too numerous for it to be otherwise. Only, as I have said in the paper already quoted, there should, of course, be ether pulses in the X-ray stream, and the γ stream also for that matter, and it may possibly be these which have been the subject of experiment by Marx, and which show Mr. Barkla's polarisation effects. But I think it is certain that at least the γ rays are material, and those X-rays which give rise to a secondary kathode radiation of a speed due to a few thousand volts. W. H. BRAGG.

The University of Adelaide, South Australia,
December 12, 1907.

Drifted Ice-crystals.

THE accompanying photographs, showing the incipient freezing of the sea during severe frost on January 4 at

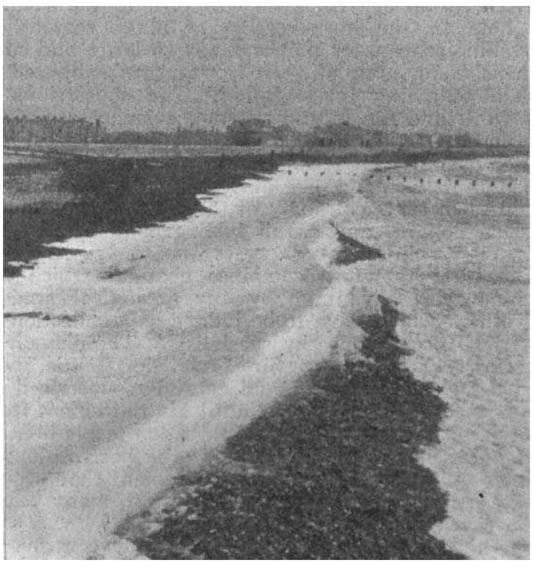

FIG. 1 – Bank of drifted Ice-crystals.

Littlehampton, may be of interest to readers of NATURE. They were taken about high water, at 11 a.m. A high

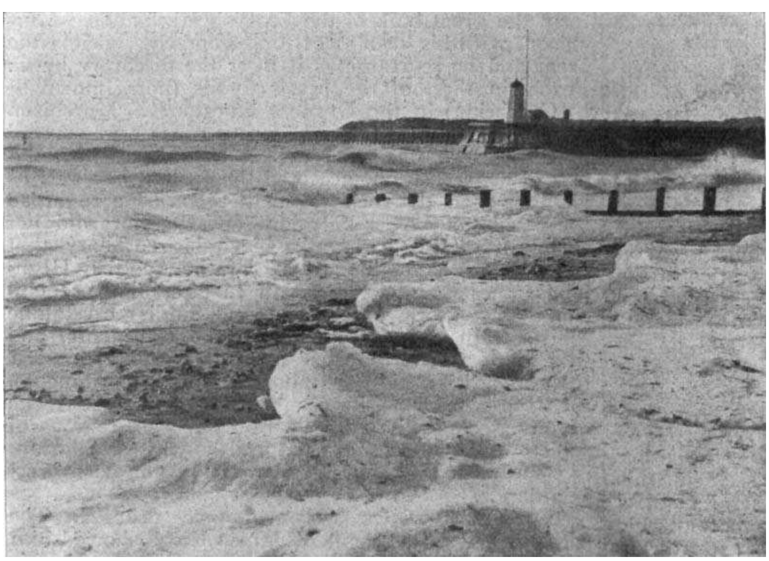

FIG. 2.—Layer of drifted Ice-crystals.

N.E. wind and the flowing tide had drifted ice-crystals formed on the surface of the sea into the slack water in the angle between the east side of the small pier and the shore, until they were collected in a viscous layer covering the whole angle. The layer seemed to be more than an inch thick.

The photographs show the border of ice-crystals thrown up on the beach, with a vertical front towards the sea about 18 inches high.

The effect disappeared rapidly when the tide began to fall. The timbers of the pier were thickly coated with ice at high-water mark, and as far above as the splashing reached, but remained perfectly clear below this line.

WALTER LEAF.

The Interpretation of Mendelian Phenomena.

DR. G. ARCHDALL REID has recently suggested (1) that Mendelian phenomena occur only under artificial conditions, and (2) that they are to be explained in terms, not of segregation, but of "latency" and "patency." As regards the latter contention, it appears to me that it would be justified if, in the case of experiments conducted under stringent conditions, dominant characteristics were, even occasionally, to appear in recessive generations or *vice versâ*; but if this is not the case it seems an abuse of language to describe a thing as "latent" which never gives any manifestation of its existence. Further, Dr. Reid's theory does not explain—as the Mendelian theory does—why these characteristics not only appear and disappear, but play this game of hide-and-seek in accordance with strict numerical rules.

As to the other point, that Mendelian phenomena are confined to cultivated varieties, it is extremely difficult to prove or disprove, because to ascertain the phenomena you must experiment, and to experiment is to place under artificial conditions. But the well-ascertained facts of conjugation and cell-mitosis, which Mr. R. H. Lock regards as affording considerable support to the doctrine of gametic purity, are certainly not confined to cultivated varieties. That all inheritance may be particulate was long ago suspected by Galton, who speaks of skin colour as possibly "a fine mosaic too minute for its elements to be distinguished in a general view" ("Natural Inheritance," p. 12).

May I suggest one line of inquiry that may possibly prove fruitful in competent hands? Is there any connection between the variability of a plant or animal and the number or size of its chromosomes? Man, for example, has a large number of chromosomes, and is extremely variable. The correspondence would no doubt be far from exact if we suppose with Mr. Lock that the biological units are not the chromosomes themselves, but the chromomeres or some even minuter subdivisions. But it might be assumed, at any rate as a first approximation, that the ultimate units were roughly proportional to the number and size of the chromosomes, and in that case species possessing many and large chromosomes would be likely to have a larger stock of the raw material of variation than their fellows. H. H. O'FARRELL.

East India United Service Club,
St. James's Square, S.W.,
December 30, 1907.

Musical Sands.

IN reply to Prof. Poynting's letter (NATURE, January 16), may I say that the article which appeared in NATURE (August 6, 1901) was only intended to supplement my paper of 1888, by recording the results of further investigations up to date, and to show that I claimed, both by analytical and synthetical methods, to have proved the theory previously dealt with in detail?

In that paper (1888) I rejected the conception that the notes emitted from musical sands were due to the vibra-