

founding. To begin with, iron-founding is an art most difficult for the non-professional man to understand, even when going through a foundry, where the various branches of the work are going on before his eyes. How much more difficult it must be for a student to get much real knowledge of the art from a book it is easy to imagine.

As an elementary hand-book this volume will, no doubt, serve its purpose. At the same time, it ought to be clearly understood that the iron-foundry is the only place where iron-founding can be learned thoroughly. A little idea of the art may be obtained by other means, but moulding, of all the engineer's arts, is the one which requires the practical work in an engineer's foundry for its development. The machine tool is largely to blame for the deterioration of our skilled workmen generally, but this has been least felt in the foundry. The moulder must still have his trade between his fingers to be efficient, and no amount of machinery as at present designed will help him to mould, say, a pair of locomotive cylinders in one casting. The book is carefully written, and represents good all-round practice as far as it goes. The illustrations of tools, &c., are clear and accurate.

N. J. L.

LETTERS TO THE EDITOR.

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Ice Blocks on a Moraine.

BLOCKS of ice, so far as I know and so far as I remember to have read, are not usual constituents of a moraine. So it may be well to call attention to an instance which I saw lately when walking over the Gorner Glacier with my friend Mr. J. Eccles, who is even more familiar than I am with glaciers, and to whom the sight was novel. At the base of Monte Rosa, where it begins to rise from the Gorner Glacier, are two buttresses of ice-worn rock; the northern called *Ob dem See*, the southern *Auf der Platte*. Between these a glacier, evidently of no great thickness, descends towards the west, and adjacent to each, rather on the in-side, as it may be called, is a little lake. In the northern of these (called the Gorner See, and the only one some five-and-twenty years ago, if I remember rightly) several blocks of ice are now floating; not far from it are the blocks on the moraine.

To explain how they attained their present position, in some cases more than a hundred yards from the water, and probably quite twenty above it, a little more topographical description is needed. The moraine of which they form a part is not a ridge composed of, or at least masked by stones, but a very gentle swell of ice, over which, especially on the eastern side, blocks are scattered in open order. It extends from one lakelet to the other, and is produced as follows. As said above, the glacier which passes between these rock-buttresses is by no means a thick one, but the southern flank of *Auf der Platte* is swept by a huge ice-stream which descends from the snow-fields of the *Lys-joch*, and is prevented from much lateral expansion by the pressure of a second large glacier which drains the northern face of the *Zwillinge*. This enormous mass of ice tends to pond back the smaller glacier; thus the moraine, mentioned above, is mainly formed by the left lateral moraine of the latter, by a few blocks which come down its mid stream, and possibly by the right lateral moraine of the *Lys Glacier*. The obstructed ice, however, is forced up so as to form a sort of flattened wave, so that if one were coming right down the face of Monte Rosa one would mount 50 or 60 feet from the margin of the Gorner See, or perhaps half as much from the middle part of the Monte Rosa glacier, and then, after a slight descent, would again ascend a gentle slope in order to arrive on the broad united ice-stream which bears the name of the Gorner Glacier.

The blocks of ice are numerous. A few of the largest must contain about 8000 cubic feet—many vary from 2000 to 5000 cubic feet—indeed, in the northern part of the moraine I think the ice exceeds the rock in actual volume. These ice-blocks, in some cases, are mounted on ice-pedestals, just as is rock in a glacier-table; the support rising perhaps a couple of feet above

the level of the glacier. Of course they were "perspiring" freely under a July sun, and do not make a long journey; probably few succeeding in getting a furlong away from their source.

That these blocks of ice began as bergs in the Gorner See is indubitable. They have been elevated to their present position by the struggle between the confluent ice-streams; the smaller of these impinging upon the larger almost at right angles, and being thus forced upwards by the obstacle. The number of the blocks suggests the possibility that the glacier itself may form part of the bed of the Gorner See; for they would be more readily removed from the water, if the actual bed of the lakelet, instead of being at rest, were slowly travelling forward and upward.

The above description illustrates the way in which (as I have seen suggested) blocks of rock in past geological ages may sometimes have been carried up-hill by glaciers. At the same time I may observe that I should myself be reluctant to found upon it any very sweeping generalization.

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The Inheritance of Injuries.

IN the notice of Dr. Weismann's "Ueber die Hypothese einer Vererbung von Verletzungen" (NATURE, July 25, p. 303) there occurs the following commentary:—"It is not so certain that all will admit Weismann's contention that the demolition of the inheritance of injuries furnishes strong presumptive evidence that acquired characters are not inherited. *It might well be urged that there is a great distinction between characters which are obviously not useful (such as injuries) and useful characters.*"

I have italicized the last sentence, desiring to call the attention of those interested in the subject to some points of difference between useful and not useful or disabling variations, as these may be supposed to lend themselves to transmission by inheritance. The appreciation of these points of difference is calculated, I believe, to greatly assist in settling the important question as to the inheritance of acquired characters.

In my work on "Dissolution and Evolution and the Science of Medicine" (Longmans and Co.) an attempt is made to show from various considerations that non-congenital diseases, including injuries, are not inheritable. The chief contention is that diseases and injuries are simply disorganizations of pre-existent functions and structures. They are not, as useful and normal characters are, integrated and organized arrangements of the organism's energies, but bodily disintegrations inseparable from the actions of the environment. Diseases as dissimilar as a common burn and general paralysis of the insane, are shown, in the work I speak of, to be alike in so far that they are disintegrations of the body and causally related to the environment. It is this intrinsic nature of disease and injuries and their dependence on external conditions which goes far, as I believe, to make them uninheritable. Since my work is probably accessible to few of the readers of NATURE, I may perhaps be permitted to quote the following extracts as further argument and illustration.

"True diseases, as we have just seen, cannot be separated from their causes; and causes, being of the environment, are not handed down. But there are additional reasons for the feeble hold which heredity has upon pathological states. When we discriminate between the variations of function and structure that are passed on by parent to offspring and those that are not, we are forced to see that natural selection, working always in confederation with heredity, seizes upon *favourable* variations. Natural selection appropriates organismal acquisitions. But analysis discloses the fact that diseases are losses, not gains; are unfavourable variations, and offer no 'purchase' for the co-operative influence of these two modes of action. . . . But more important than influences of this sort is that influence which springs from the differences of nature and conditions between normal and abnormal traits. Normal structures were evolved in long periods of time, and have been transmitted through generations unnumbered; therefore, the tendency to their perpetuation by inheritance must be immensely predominant over any tendency to the perpetuation by inheritance of the transitory changes of disease. I believe that the 'vestiges' of once useful structures owe their astonishing persistence to the fact that they have become deeply pressed into the organic arrangement by the selection and transmission of such structures for secular periods. This makes intelligible the rarity with which deprivation of a limb or other part leaves any impress upon offspring. Though circumcision has been practised among the Jews for ages, it has not produced congenital preputial imperfection in the race.

Nor do we ever find that amputation of a limb, or loss of the cortex of the kidney from Bright's disease, is followed by corresponding anatomical deficiencies in children. In the African transported to northern latitudes, the dark skin persists through indeterminate generations—provided there is no cross-breeding—but the endemic diseases of his race are not transported with him.

"Hitherto all reasoning upon the heritableness of diseases has proceeded on the tacit assumption that morbid changes are subject to the same laws of vital action as healthy changes. It has been discovered, however, that the two are dissimilar both in nature and in the circumstances of their genesis. The traits we every day recognize as inherited are the results of an infinity of co-ordinate actions. There may be instanced the bony framework of the face, the colour of the iris, the gait, special mental aptitudes. All these, and attributes of the same order, represent a vast integration of forces, groups of organized energies. It is this organization which gives them individuality and makes their hereditary transmission possible. They are, in other words, self-existent, having been independent of the original conditions out of which they grew."

The conclusion, deduced from evolutionary principles, that non-congenital diseases and injuries are not inheritable, might, I think, be supported inductively from the facts of medical observation, and it is most interesting that the results of Dr. Weismann's investigations are confirmatory. But from what has been said it clearly appears, in harmony with the annotation I commenced by quoting, that the non-inheritance of injuries is no evidence of the non-inheritance of acquired useful variations.

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Classified Cataloguing.

WHENEVER a collection has been catalogued anew, and all the numbers are in the museum order of the specimens, the placing of additions at the end, without any sequence but that of acquisition, always seems a melancholy collapse of the order just established. So strongly is this felt that some curators even enter additions with the same numbers as similar specimens, distinguished by letters, as 3247*a*; but, as formerly in the British Museum, this system breaks down when such additions far outnumber the original series, and we reach figures like 3247*fj*. At the same time this is an approach to an entirely different and logical system of cataloguing, which ought to be considered. Another stage of arrangement has been by appropriating so many thousand numbers to each branch, so that the articles of one class may have contiguous numbers.

The complete system of cataloguing which has been thus felt after, and sought for, is what may be called "fractional cataloguing," treating all numbers as decimal fractions and arranging them accordingly. Thus 21'765, 21'77, 21'8, and 22 might appear as successive numbers in a catalogue: the numbers being arranged solely by their successive order of the left-hand figures, regardless of the length of the number. By this system, therefore, any quantity of additions can be brought into their right order without disturbance; fifty new specimens like No. 371, for instance, being numbered 371'01 to 371'50.

The first two or three places of the number will therefore indicate the nature of the specimen in any given catalogue: and this leads at once to the desirability of all collections having a similar numerical basis for their catalogues, so that if all the parrots, for instance, begin with 56 in one collection they should do so in all other museums.

The first step therefore in classified cataloguing would be to agree on a set of 100 or 1000 numbers, to subdivide each branch of science, the distribution of the numbers being partly settled by the average number of specimens, partly by natural divisions. Thus in mineralogy, elements might be 001 to 099; binary compounds 100 to 299; silicates 300 to 799; non-metallic acid salts 800 to 899; metallic acid salts 900 to 999. In all museums, then, silicates, say of lime, magnesia, and alumina, would begin with 61, the different species being marked 610 to 619, and varieties and individual specimens numbered with additional decimals following these bases, e.g. 615'47. The set of numbers in each science would be best fixed by a committee at some International Congress, so as to insure general acceptance, like the scheme of geological colouring.

The disadvantages of this system would be—(1) that the catalogue would have to be kept like that of a library, subject to

additions at any point, and therefore on slips which could be transferred; and (2) that the total number of specimens would not be known except by counting. These are not serious difficulties, and the following advantages seem to entirely outweigh them.

(1) The numbers would indicate to all students the nature of the specimens quoted in any collection. (2) The catalogue would be classified in natural order throughout, so that all similar specimens would be described together. (3) The numbers in the museums would be in order from end to end. (4) Any specimens moved could be rearranged by unskilled assistance, solely by the numbers. (5) Any object in the catalogue or hand-books could be at once found in the museum by its number. (6) A great help would be given to the arrangement of minor museums by having a uniform scheme of cataloguing fixed. (7) The numbers being in constant use would soon form technical symbols for species, a short-hand briefer than chemical symbols even, and applied to all sciences; and also a valuable key to the memory.

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I WAS rather too precipitate when I stated that the figures relied on by Mr. Galton were totally inadequate to support his conclusions; for, as regards the second of them, viz. that the "high honour" man has a head perceptibly larger than the "poll" man, the evidence is fairly strong; but with regard to the other three conclusions, referring to the growth of heads, I must repeat what I have said. In the light of the discussion given below of a large number of observations, I cannot even admit that the tables and curves given in Mr. Galton's paper (see NATURE, vol. xxxviii. p. 15) give even "an approximately true idea of what we should find, if we had the opportunity of discussing a much larger number of observations."

Having heard that all the measurements taken have been indexed for reference, I went to the laboratory, and, by the kind permission of the custodian, copied out the head measurements of fifteen individuals, each of whom had been measured at least five times. In one case, measurements had been taken at seventeen times. The average number was 7'1.

Since the first case quoted in my last letter forms one of these, I had better point out that Mr. Galton's objection to it is unsound. He notes a grouping of the observations, which makes him suspect that "some peculiarity in the shape of the head caused doubt as to the exact line of maximum height." But the observations of height are 5'2, 5'3, 5'4, 5'5, 5'5, and 5'6; and show no grouping. Mr. Galton must have meant that the calculated products were grouped. This is the case, but could not be due to the cause he suspects, for that would cause grouping in the simple heights.

The fifteen series of measurements fully bear out the conclusions which I drew before from two only. The measurements of width vary 0'1, 0'2, or 0'3 inch, those of length vary to the same extent, and in one instance up to 0'4 inch, while the height in most cases varies 0'4 or 0'5 inch. In only two cases does it vary so little as 0'2 inch, while in one case it varies 0'7 inch. The last case (where the figures are 5'4, 5'6, 5'1, 5'8, 5'5) is partly accounted for by the fact that the first three observations were taken by one observer, the other two by a second. (The statement in Mr. Galton's original paper that all the measurements were taken by one observer, must have been due to misinformation.) I have calculated the probable error of each observation of the height of head for each series of observations, using the approximate formula

$$r = \frac{0'8452v}{\sqrt{n(n-1)}}$$

and I find it on the average 0'095 inch. Since the average height is less than 5'5 inches, this error amounts to 1'7 per cent. If the error in length *a*: *d* width were each half of this, the probable error of the product would be about 2 per cent.

To test whether any of the variation found is due to actual growth, and not to accidental error, I have used the following method. Arrange all the measurements of any one individual in the order of the dates on which they were taken, and separate them into two equal groups. Take the mean measurements of the first set, and put opposite them the mean of the dates; then