

origination these manifested were *seen in the things themselves*,¹ and after the hypothesis of their physical origin had been arrived at, were "to be seen there still." Yet we should have deemed the brothers Darwin very unworthy representatives of their family if, after having arrived at the physical hypothesis, they had continued to argue in favour of a teleological enthusiasm for mud pies, on the ground that "the inference was not one from an intelligent originator to design in the (in-)organic world, but from marks . . . in the latter which indicated design to an intelligent originator." In other words, a change in the hypothesis concerning the *origination* of the mudballs entirely changed the logical cogency of the teleological inference.

Now I have purposely chosen this illustration because it is so simple a character, and therefore serves in a clear manner to show how greatly a teleological inference may be modified by a change of hypothesis concerning the mode of origin of a structure, even though the structure remains the same; if there had been no evidence of a purely physical mode of origin in this case, it might truly have been said of the teleological interpretation, "the inference to most minds was convincing; at least it was legitimate." Of course in organic nature the apparent marks of design "in the things themselves" are much more numerous, varied, and complex than any that we meet with in inorganic nature; but no matter how numerous, varied, and complex such marks of design may be, if we see good reason to conclude that they have *all been produced by physical causes*, they are no more available as *evidences* of special design than are the mudballs—although both they and the mudballs, being alike formed under an orderly system of causation, may be due to a general design pervading the cosmos. And here I understand that Prof. Gray is in agreement with me, for he says that when I assign the whole results to known [or unknown] physical causes and discard the factor of intelligence, I am bound to render their adequacy at least conceivable. This appears to show that Prof. Gray is at one with me in holding that physical causes as such do not constitute other or better evidence of design in the organic than in the inorganic world; and it is only because he cannot conceive how such causes are adequate to produce the results observed in the former that he deems these results unique as evidence of "the factor of intelligence." In other words, supposing for the sake of argument that all these results have been due to purely physical causes, and supposing further that all these causes were as perfectly well known as the less complicated physical causes of the inorganic world, then I take it Prof. Gray would agree with me in saying that under such circumstances the former would constitute no other or better evidence of design than the latter.

If so, our only difference resolves itself into a difference in the estimate which we respectively form of the probable adequacy of purely physical causes to produce all the results which are observable in organic nature. To me the probability appears overwhelming that in respect of method "all nature is of a piece," and therefore that the terms "physical" and "natural," when applied to causation, are logically, as well as etymologically, convertible. To Prof. Gray, on the other hand, the probability appears to be that such is not the case, but that, when we meet with the "*direction of action to ends*," we have special evidence of "the factor of intelligence," which therefore makes nature "of at least two pieces," and so makes the term "natural" to mean more than the term "physical."

Supposing that I am right in understanding this as the only difference between us, I may point out that if, while following my ideas of probability, I have erred on the side of rashness in drawing "the downright conclusion" that the facts of organic nature present no other or better evidence of design than the facts of inorganic, Prof. Gray, in following his ideas of probability, can scarcely be able to shut out the suspicion (more especially in view of abundant historical analogies) that, in resorting to "the factor of intelligence" as a hypothesis wherever physical causation is found to be complex or obscure, he may be merely supplementing our present ignorance of such causation by an inference which is at least as rash as my statement.¹ And here I should

¹ I suppose it will be admitted that the validity of an inference depends upon the number, the importance, and the definiteness of the things or ratios known, as compared with the number, importance, and definiteness of the things or ratios unknown, but inferred. If so, we should be logically cautious in drawing inferences from the natural to the supernatural; for although we have the entire sphere of experience from which to draw an inference, we are unable to gauge the probability of the inference when drawn—the unknown ratios being confessedly of unknown number, importance, and degree of indefiniteness; the whole orbit of human knowledge is insufficient to obtain a parallax whereby to institute the required

like to observe, with special reference to the natural or physical causes summed up in the term "natural selection," that although I speak with all the respect which I sincerely feel for so distinguished a naturalist and so able a dialectician, I am not able to follow Prof. Gray in his understanding of this subject. For he says of the theory of natural selection that it is destitute of any pretensions to act as the substitute of the theory of special design, "until it is explained how the physical destruction of a part should have set the rest into varying at all, into varying advantageously, and into varying into the very special ways they have done." But surely it is no part of the theory of natural selection to suppose that the *physical destruction* of unfit organisms is, or has any need to be, the *cause* of advantageous variations arising in other and allied organisms. The theory merely supposes that variations of *all kinds and in all directions* are constantly taking place, and that natural selection seizes upon the more advantageous. Therefore, so far as this theory is concerned, there is no call to explain why promiscuous variation occurs; it is simply a fact that it does occur, though not necessarily *made to occur* by the destruction of other organisms. Neither is there any call to explain why the variations occur in special and advantageous ways, for they are not supposed to occur in special and advantageous ways, but only to appear to do so on account of all other variations being eliminated, while those which happen to occur in the specially advantageous ways are preserved. Again, Prof. Gray says in his postscript that the theory of natural selection supposes successive generations to be slowly changing, "yet always so as to be in compatible relations to the environment." Now it is true that where the changes in the environment are gradual, and the variations of specific type are being slowly accommodated to them, each generation is, on the whole, in compatible relations with its environment. But it is not true that such continuous compatibility in itself points to design; it only points to the plasticity of the varying type, which, if not sufficiently plastic to meet the new demands upon it in this respect, simply becomes extinct.

In conclusion, I agree that "natural science leaves aside the question whether evolution and design in nature are compatible or not," and I agree that, "if science has no call to settle the question, it has none to prejudge it." But I do not agree that I have prejudged this question by saying that in my opinion the theory of evolution, in supplanting the theory of special creation, has necessarily removed the special evidence of design in organic nature, by showing that in respect of causation organic nature and inorganic nature are one. GEORGE J. ROMANES

The High Springs of 1883

THE high springs of the present year, consequent upon the excessive rainfall of the past winter, are an event that ought not to pass unrecorded in the pages of NATURE. I can speak only of phenomena which I have observed upon my native chalk hills of Hampshire, but I doubt not that similar facts have attracted attention elsewhere.

The Candover, a confluent of the Itchen from the north, burst forth this year in a field near Preston Candover, where it has not been known to rise for the last fifty years, and has flooded the road between Preston Candover and Chilton Candover. The Itchen itself rose in the valley above Cheriton beyond its recognised source, and has flooded fields on the road to Kilminster, where no one recollects to have seen water before.

The Hampshire tributaries of the Thames have acted in exactly the same manner. The Whitewater has issued forth in the valley just below Upton Grey, far above its usual origin even in the highest springs, and has flooded the whole road between Bidden and Greywell. Another branch of the same stream has risen in the fields on the left of the main road from Odiham to South Warnborough, where spring water has never been known within the memory of the oldest inhabitant. In like manner the Wey, which, in wet seasons, takes its rise in the meadows adjoining Chawton House, has issued forth this year at a much higher level in the fields below Farringdon.

These facts are the more worthy of notice because it has been generally believed that, in the Hampshire hills at least, owing to more efficient drainage and other causes, the springs were

measurement or proportion between the terms known and the terms unknown. Or, otherwise phrased, we may say—As our knowledge of a part is to our knowledge of a whole, so is our inference from that part to the reality of that whole. Who, therefore, can say, even upon the supposition of Theism, that our inferences or "idea of design" would have any meaning if applied to the "All-Upholder," whose thoughts are not as our thoughts?

getting lower every year, and would never again attain the level that they once had according to the traditions of past generations. It should be added that the springs were at their highest about the commencement of this month, and are now gradually falling.

P. L. SCLATER

Hoddington House, Odiham, March 31

Scorpion Suicide

I AM sorry that my experiments on scorpion suicide have given pain to some of your correspondents. Allow me to explain in a few words the object of my investigation. It is commonly believed in this colony and elsewhere that scorpions commit suicide; Dr. Allen Thomson, in a letter to NATURE, lent the weight of his scientific name to this view; and Dr. G. J. Romanes, in his "Animal Intelligence," treats it as an open question. Now if this habit of committing suicide be an established fact, we have in scorpions a highly persistent type of creature that inherits a habit detrimental alike to the individual and the species. *Scorpion suicide, therefore, if a fact, is one of the strongest individual cases against the Theory of Evolution by Natural Selection that is presented to us in the animal kingdom.* It seemed to me that the only way of settling this question was by the direct appeal to experiment. But is the Theory of Natural Selection of sufficient importance in its bearing upon human life and human progress to justify the infliction of pain upon, say, sixty scorpions? I am one of those who believe that it is. I am one of those who believe that the theory of evolution has enormously influenced human thought and action, and is destined to influence it in a constantly increasing degree. I believe that much of the moral and intellectual progress of our race is indissolubly associated with this theory of evolution. I may be wrong in that opinion, but that is the opinion I hold. And holding that opinion it became to me a duty to do something towards settling a question which seemed to me to be of great importance in its bearing on the evolution theory. And it was my object to do the work, as far as I could, thoroughly and once for all. I believed that if I could show that even under torture scorpions do not commit suicide, the view that they do so when irritated by the bright light of a candle-flare became highly improbable. To establish a negative in the face of positive assertions is, however, difficult, and I considered it necessary to experiment upon a number of individuals. *Hinc illa lachrymæ!* One of my friends, however, protested as follows: "The theory of evolution," he said, "is now so strongly established, that scorpion suicide is *a priori* impossible." But I hold it to be dangerous in the extreme, in the present position of science, to set up the theory of evolution as a doctrine from which to draw deductions, *unchecked by an appeal to nature where such appeal is possible.*

C. LLOYD MORGAN

Rondibosch, March 12

Nesting Habits of the Emu

I AM able fully to confirm Prof. Moseley's statement of the habits of the emu in nesting at Blenheim. Some years ago my father was very successful in rearing these birds at his place at Brockham Lodge, near Dorking. The first egg was usually laid shortly after Christmas; the total number of a brood being from fifteen to twenty, laid usually at intervals of about forty-eight hours. Some time before the full number was laid the cock bird would commence the incubation by carefully drawing them under him. When the hen bird was ready to add to their number she would sit down by his side, produce the egg, and her mate would then carefully draw it under him with his foot. As soon as the number was completed, it became necessary to seclude the hen bird, as she was from this time "vicious" towards her mate and towards her own eggs; and the seclusion continued until the young birds had attained a considerable size, as she showed every disposition to destroy them. The number of eggs laid was often too large for the cock bird to get comfortably under him. Still during several years that my father kept the birds a considerable number of eggs were annually hatched, and the young birds reared to the breeding age. No brood from native birds was, however, obtained. They showed no disposition to change the breeding season from January to July. In captivity the birds strikingly exhibited their singular inquisitive propensities. They were not usually vicious, except during the breeding season, but were very easily frightened.

London, March 31

ALFRED W. BENNETT

The Recent Cold Weather

THE excessively severe and prolonged cold weather of the month of March has hardly a parallel in this century. It appears to have been felt throughout Europe, and has even reached the shores of Africa. Frost, snow, and wintry gales we expect at a season proverbial for its fitful severity, but the scarcely interrupted sweep of the frigid atmospheric waves which have overwhelmed us for three successive weeks is an experience of weather so remarkable that I conceive the record will probably interest some of your readers.

In position, altitude, and in its freedom from the sheltering influence of large towns, this station may be accepted as favourable for giving an accurate account of the weather in the centre of England. Our instruments are on a proper meteorological stand, and are by Negretti and Zambra. I may add that, in its blighting influence on vegetation stimulated into activity by a mild and moist period in February, this weather has proved more destructive to early fruit blossoms, certain shrubs and plants accepted as hardy, than from any weather previously experienced in March in other years; but apart from vegetation, and acting on the upturned fallows and soddened clods of clay, the penetrating winds, frequent frosts and falls of snow have pulverised the land, so that it falls before the plough or harrow like calcined limestone, and in respect to the preparation of land the weather has had a beneficial action.

Record of Weather, March, 1883, at Belvoir Castle, Leicestershire

March.	Min.	Max.	Grass.	Wind.	Rain.	Snow.
4	27	50	27	S. to N.	—	—
5	27	51	20	N.	—	—
6	33	52	29	N.	—	0".2
7	26	40	22	N.	—	0".2
8	24	41	24	N.	—	0".25
9	20	35	14	N.	—	0".12
10	9	37	4	N.	—	0".5
11	20	38	10	N.	—	—
12	25	39	23	N.W.	—	0".2
13	25	39	20	W.	—	0".1
14	29	40	22	W.	—	—
15	27	39	20	N.	—	0".5
16	26	38	19	W.S.W.	—	—
17	28	38	24	S.W.	—	0".9
18	25	40	20	S.	—	0".1
19	28	42	21	N.	—	0".1
20	31	40	31	E.N.E.	—	0".31
21	32	37	31	N.E.	—	—
22	28	35	27	E.	—	—
23	28	35	26	N.E.	—	—
24	18	42	5	W.	—	—
25	26	45	16	N.W.	0".4	—
26	26	41	19	N.W.	0".5	—
27	27	40	18	N.	—	—
28	26	43	16	N.W.	—	—
29	24	41	12	S.	—	—
30	35	48	35	S.	0".3	—
31	30	55	24	S.W.	0".11	—

Belvoir Castle Gardens

WILLIAM INGRAM

Sap-Flow

A REMARKABLE instance of the strong up-rush of sap in trees at this time of the year occurred here during the late severe weather. The boughs of a sycamore overhanging a road were trimmed on the 21st of this month during a very keen frost, and next day icicles of frozen sap, varying in length from a couple of inches to a foot, were hanging from the severed ends. The icicles were semi-opaque in appearance and slightly iridescent, like the sheen on the moonstone, and, when put in a bottle and melted, the product was pure sap.

The sycamore, being one of the earliest trees to develop leaves, had its sap rising, notwithstanding the intense cold and late season; while a beech, which is much later in coming out, and an ash, which is usually latest of all, whose boughs had also been lopped, showed no signs of bleeding, and the cuts remained dry and bare.

The icicles have been melted, reformed, and melted again since the 21st, and still the sap is dropping from the cuts.

Highfield, Gainsborough, March 28

F. M. BURTON