the conditions of life." Was not this a prophet? Yea, I say unto you and more than a prophet! Of course if the conditions of life are unfavourable, the incipient variations cannot become species. But surely it is obvious that in *variation* is the real *origin* of species. Variations must occur before the selection of some of them in preference to others. To consider the theory of natural selection as a theory of the origin of species, is therefore clearly an error. In his "Origin of Species" Darwin certainly expounded variation, and I might have ventured to think that as the book deals more largely with the subsequent selection of a few varieties to survive as species at the expense of many extinguished, a more exact title for it would have been "The Evolution of Species." But what says the great master? See page 71:—"Owing to this struggle for life, any *variation*, however slight . . . will tend to the *preservation* of *that individual*, and will generally be inherited by its offspring. . . . I have called this principle by which each slight *variation*, if useful, is *preserved*, by the term natural selection, in order to mark its relation to man's power of selection." And who will not recognize the wisdom of his selection of the term? It has been before observed that the "*Ascent* of Man" would seem a more accurate title than the "*Descent* of Man." But I have no doubt that his reasons for preferring the latter were equally cogent.

But Mr. Romanes proceeds:—"This proof is drawn from three distinct heads of evidence. (1) The inutility to species of a large proportional number of their specific characters. (2) The general fact of sterility between allied species, which admittedly cannot be explained by natural selection, and therefore has hitherto never been explained. (3) The swamping influence of even useful variations of free intercrossing with the parent form." I have advanced, I think, ample reasons why No. 3 may be regarded as imaginary, and which therefore reduce the value of No. 2 to a minimum. No. 1 depends entirely upon the definition of "*utility*." Has this word any real significance outside human interests and considerations? The idea of utility, if extended to Nature's operations, may, it seems to me, apply to the interests of any other variation than the one whose specific characters are in question, which may therefore be, without compunction or regret, sacrificed to the most fit, as we know that innumerable species have been extinguished in the interest of those that supplanted them. But utility to Nature may be the extinction of one variation and the preservation of another. As Mr. Romanes's whole paper is built upon what I have already quoted from it, I need scarcely follow it any further. With your permission, however, I have another remark to make.

Mr. Romanes seems to me to have been much exercised by the consideration of the intercrossing with parent forms, and, not knowing of the simple solution given above, to have cleverly invented his physiological selection to escape from the dilemma. Of course Nature is not clever, but simple in its operations. I was always much impressed with what appeared to me a greater difficulty, which might be thought to have a clearer title to be called "physiological selection." I allude to a general tendency in the (human at least) sexes to prefer a mate with opposite characteristics, with the apparent result of insuring mediocrity in the progeny. Thus, as a general rule, the tall prefer the short; the dark, the fair; the wise, the silly; &c., and *vice versâ*. Variation is, on the other hand, apparently insured to a large extent by the differences between parents, but still it would seem that the tendency should, *cæteris paribus*, be inevitably towards a mean in the progeny. The general migration, however, as above indicated, of young males and females, gives plainly ample opportunity for the preservation of viable variations, besides others which experience and care will doubtless discover.

Melbourne, April 11. H. K. RUSDEN.

Weight, Mass, and Force.

APPLICATIONS of the data previously given, in the extract from the American journal, to the dynamical principles of varied motion are easily provided for Mr. Hayward. Take the following: "Determine the weight of the greatest train the Strong locomotive can take up a 96-feet grade from rest at one station to stop at the next station a mile off in four minutes, taking the brake power as a resistance of 400 lbs. to a ton."

The main points at issue, however, are whether the language of the engineer, and in fact the usage of our own and other languages, is scientifically correct or incorrect in its use of the words *weight* and *weighing*; and whether the mathematician is to be allowed to restrict the word weight to the subsidiary sense of force of attraction by the earth.

It is of great importance that this question of dynamical terminology should be thoroughly thrashed out now, before Mr. Hayward's Committee on Dynamics, of the Association for the Improvement of Geometrical Teaching, prepare their final report on the subject. A. G. GREENHILL.

Woolwich, July 11.

The Sky-coloured Clouds.

ON the evenings of June 14, 18, and 19 there was a feeble reappearance in Sark of the sky-coloured clouds, as I may call them in default of a better name, which were so brilliant in the twilights of the last two summers. Though the display this month has been comparatively faint, it has been unmistakably of the same character. I have seen nothing of these clouds since the 19th in travelling in the Channel Islands and through France.

Geneva, June 29. T. W. BACKHOUSE.

P.S.—*Chamounix, July* 13.—I have seen one more display—a brilliant one seen from this neighbourhood on the 6th inst.—T. W. B.

The Migrations of Pre-Glacial Man.

THE question raised by "Glaciator" has been treated by me in a paper entitled "The Faunas of the Ffynnon Beuno Caves and of the Norfolk Forest Bed" in the *Geological Magazine* for March 1887. I there stated that, "Although man probably reached this country from the east, it seems to me equally clear that he must also have arrived here with the reindeer from some northern source during the advance of glacial conditions." Though the Norfolk Forest Bed fauna contains abundant remains of deer and of other animals suitable as food for man, it is curious that so far no implements or other traces of man have been found there. The Forest Bed contains in the main the fauna of an eastern area, as the river on the banks of which the animals roamed flowed from the south-east. If pre-glacial man arrived in this country from the east or south, we should therefore expect to find evidences of this in the Forest Bed. On the other hand, wherever the remains of northern animals, such as the reindeer, mammoth, and rhinoceros, occur in any abundance, there we almost invariably find traces of man. Now that we know that man arrived in this country before the climax of the Ice age, as proved by the explorations carried on for several years at the Ffynnon Beuno Caves (amply confirmed also by this year's researches), it seems but natural to infer that man arrived in this country with the northern animals as they were compelled to migrate southwards by the gradually advancing glacial conditions, and that he kept mainly with the reindeer near the edge of the advancing ice. HENRY HICKS.

ABSTRACT OF THE RESULTS OF THE INVESTIGATION OF THE CHARLESTON EARTHQUAKE.[1]

I.

THE amount of information now in possession of the United States Geological Survey, relating to the Charleston earthquake, is probably larger than any of similar nature ever before collected relating to any one earthquake. The number of localities reported exceeds 1600. The sources of information are as follow: (1) we are deeply indebted to the U.S. Signal Service for furnishing us the reports of their observers; and (2) equally so to the Lighthouse Board, which has obtained and forwarded to us the reports of keepers of all lighthouses from Massachusetts to Louisiana, and upon the great lakes; (3) to the Western Union Telegraph Company, which instructed its Division superintendents to collate and transmit many valuable reports; (4) to the associated Press, which has given us access to the full despatches (with transcripts thereof) which were sent over the wires

[1] Paper read before the National Academy of Sciences at Washington, on April 19, 1887, by C. E. Dutton, U.S.A., and Everett Hayden, U.S.N., U.S. Geological Survey.

centering at Washington during the week following the earthquake; (5) to geologists and weather bureaus of several States, who have kindly exerted themselves in this matter and collected much important information; (6) to a considerable number of scientific gentlemen who have distributed for us our circular letters of inquiry in special districts,—notably, Profs. W. M. Davis, C. G. Rockwood, J.P. Lesley, T. C. Mendenhall, and Messrs. W. R. Barnes, of Kentucky, and Earle Sloan, of South Carolina; (7) to a large number of postmasters in the Eastern, Central, and Southern States; and, finally, to hundreds of miscellaneous correspondents throughout the country.

In collecting this information, a printed list of questions was prepared. This practice has been resorted to in Europe and in Japan with considerable success, and the questions which have been devised for distribution in those countries have been prepared with great skill by some of the ablest investigators of earthquakes. Prof. C. G. Rockwood, of Princeton, has also been in the habit of distributing formal questions of this character in this country whenever apprised by the newspapers of a notable shock. Availing ourselves of his advice and assistance, questions prepared by him were printed and widely distributed. They were much fewer and more simple than those employed in Europe, because European investigators depend almost wholly upon the educated classes to answer them, while in this country the uneducated but intelligent and practical classes of the people must be the main reliance. These questions were designed to elicit information: (1) as to whether the earthquake was felt, (2) the time of its occurrence, (3) how long it continued, (4) whether accompanied by sounds, (5) the number of shocks, (6) general characteristics which would serve as a measure of its intensity and indicate the kind and direction of motion.

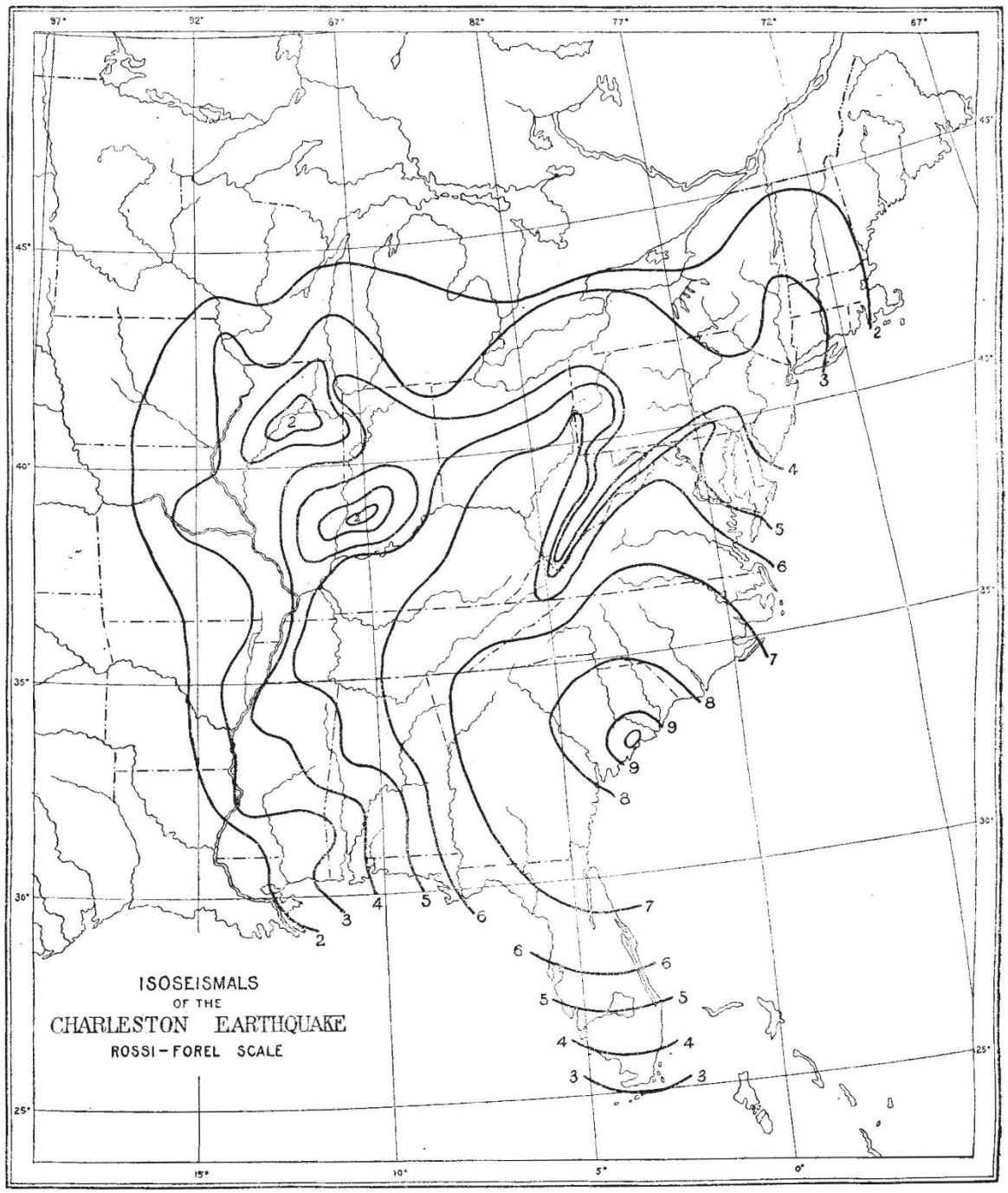
It is to be observed that the only information to be hoped for which can have even a roughly approximate accuracy is the time of transit of the shock. The degree of approximation in the time data actually obtained will be adverted to later. Special effort was made to obtain information as to the relative intensity of the shocks in all parts of the country. At the very outset a serious difficulty presents itself. In the estimates of intensities there is no absolute measure. What is really desired is some reliable indication which shall serve as a measure of the amount of energy in any given portion of the wave of disturbance as it passes each locality. The means of reaching even a provisional judgment are very indirect, and qualified by a considerable amount of uncertainty. To estimate the force of a shock, we have no better means than by examining its effects upon buildings, upon the soil, upon all kinds of loose objects, and upon the fears, actions, and sensations of people who feel it. In view of the precise methods which modern science brings to bear upon other lines of physical research, all this seems crude and barbarous to the last degree. But we have no other resource. Even if it were possible to obtain strictly comparative results from such facts, and decide with confidence the relative measure of intensity which should be assigned to each locality, we should have gained measures only of a series of local surface intensities and not of the real energy of the deeply-seated wave which is the proximate cause of the surface phenomena. Notwithstanding the indirect bearing of the facts upon the real quantities we seek to ascertain, and their apparently confused and distantly related character, they give better results than might have been supposed. When taken in large groups, they give some broad indications of a highly suggestive character, and though affected with great inequalities which for the time being seem to be anomalous, these anomalies are as instructive as the main facts themselves.

We have given the preliminary plotting of the intensities in the map before you. The first point to which we shall invite attention is the magnitude of the area affected by the shocks. It was sensibly felt in Boston, which is the most distant point on the Atlantic coast from which affirmative reports have been received. From Maine the answers are all negative. Most of those from New Hampshire are negative, but two or three positive ones show clearly that it was felt in sensitive spots. In Vermont, affirmative reports come from St. Johnsbury and Burlington on Lake Champlain. No positive reports come from the province of Quebec. In New York State it was felt in the vicinity of Lake George, and at Lake Placid and Blue Mountain Lake in the Adirondacks. In Ontario, it was quite noticeable in several localities, though the great majority of reports from that place are negative. In Michigan, it was noted in several places, and at Manistee Lighthouse, on Lake Michigan, the trembling was strongly marked. In Winconsin, though most of the reports are negative, it was felt quite strongly at Milwaukee, and was also noticed at Green Bay, and at La Crosse on the Mississippi, 967 miles from Charleston, the remotest point in the United States which gives a positive answer. In Central Iowa and Central Missouri, it was unmistakably felt. In Arkansas, the eastern portion of the State, from sixty to seventy-five miles west of the Mississippi, gives numerous favourable reports. In Louisiana, the reports are mostly negative, but numerous persons in New Orleans felt the shocks and recognized their nature. In Florida, it was universally felt, and in the northern part of the State was severe and alarming. From the Everglade region, of course, no reports have been received, as it is uninhabited; but in some of the Florida Keys it was felt in notable force. From Cuba a few reports have come, and the most distant point in that island which was shaken was Sagua la Grande, where the vibration was very decided. Lastly, a report comes from Bermuda, 1000 miles distant from Charleston, which leaves little doubt that the tremors were sensible there.

The area within which the motion was sufficient to attract the attention of the unexpectant observer would be somewhat more than circumscribed by a circle of 1000 miles' radius, and the area of markedly sensible shaking would, including the oceanic area, be somewhere between two and a half and three million square miles. In this estimate, however, only well-defined seismic movement of notable force is considered. There are reasons for believing that by proper instrumental observation the movement could have been detected over a much greater area. In the first place it is to be noted that the peripheral portions of the observed area lie in districts which are rather thinly populated, sometimes, also, in districts which from the nature of the ground do not disclose forcibly the passing shock. Furthermore, the passing wave in the outer portions of the area was almost everywhere of an undulatory character and of great wave-length, and while still retaining a large amount of energy, did not often dissipate itself into those smaller and shorter tremors which are very much more likely to attract attention, though really possessing very much less energy. Six hundred miles from the origin the long swaying motion was felt, and was often sufficient to produce sea-sickness, yet was unaccompanied by sound or by the tremulous motion due to short waves.

It will be observed upon the map that there are several large tracts which show a comparatively feeble intensity, while completely surrounding them is the general area of greater intensity. The most conspicuous of these areas of silence is the Appalachian region. The facts here are extremely interesting and suggestive. It has been generally supposed that a mountain-range serves as a barrier to the propagation of earthquakes—not from any known relation of cause and effect, but merely as the result of observation. In Japan it is universal testimony that the central range of the island marks the dividing line between earthquake and no earthquake. The shocks so frequent there are seldom or never felt beyond the mountains. A

similar conclusion has been drawn from South American earthquakes, and also from those which have visited Southern Italy. As soon as the data in the earlier stages of the inquiry began to indicate insulated areas of minimum action, they were completely investigated, and every effort has been made to secure full data from them. The result has been to show satisfactorily that such was the case. The Appalachian belt south of Middle Pennsylvania disclosed a few spots where the shaking was considerable, but in the main it was but slightly affected



until we reach the extreme southern portion of this range, where the shocks begin to be somewhat vigorous, even in the mountains. West and north-west of the range, however, the force of the undulations resumes even more than its normal vigour. In Eastern Kentucky and South-Eastern Ohio, the force of the shocks was very considerable, causing general alarm. Chimneys and bricks were shaken down, and the oscillation of the houses was strongly felt. In South-Eastern Ohio, nearly every theatre, lodge, and prayer-meeting, was broken up in confusion. It does not

appear that the Appalachians offered any sensible barrier to the progress of the deeper waves, but it does appear that they affected in a conspicuous degree the manner in which the energy of the waves was dissipated at the surface. Another minimum area was found in Southern Indiana and Illinois, and also in Southern Alabama and Mississippi. There is a curious circumstance connected with the minimum area in Indiana and Illinois. On February 6 last, an earthquake of notable force occurred in just this locality. Circulars were sent out at once, and on plotting the isoseismals they showed a singular coincidence in almost exactly filling the vacancy or defects of intensity of the Charleston earthquake. At present there is nothing to indicate whether this coincidence is accidental or whether there is some hidden relation.

Where the waves passed into the newer delta region of the lower Mississippi, the surface intensity of the shocks rapidly declined. This is indicated in the map by the compression of the isoseismals in those localities. We incline to the opinion that this sudden diminution of the intensity is due to the dissipation of the energy of the waves in a very great thickness of feebly elastic, imperfectly consolidated, superficial deposits. It is a matter of common observation in all great earthquakes that the passage of the principal shocks from rigid and firm rocks into gravels, sands, and clays is, under certain circumstances, attended with a local increase in the amplitudes of the oscillations and in the apparent local intensity and destructiveness, and the reason for it is intelligible. But where such looser materials are of very great thickness and great horizontal extent the reverse should be expected. For when a wave passes from a solid and highly elastic medium into a less solid and imperfectly elastic one, the amplitude may be suddenly increased at the instant of entering; but so rapid is the extinction, that, if the new medium be very extensive, the impulse is soon dissipated.

Many reports throughout the Central States indicate localities of silence which are not expressed upon the map. The reason for omitting them is that it has been impracticable to secure a sufficient density of observation (*i.e.* a sufficient number of reports per unit area) to enable us to mark out and define these smaller areas with very great precision. To do this for the whole country would require some tens of thousands of observations and the expenditure of tens of thousands of dollars to systematize and discuss the data. A map shaded to show the varying intensity by varying the depth of the shading would have a mottled appearance, in which the mottling would be most pronounced in the areas of a little below the mean intensity, say between the isoseismals 3 and 5. This fact is of great importance in the interpretation of the isoseismals, for the omission to consider it results in giving to the middle isoseismals too high a value. In any isoseismal zone, what we should like to ascertain is the mean intensity of the whole area included within that zone. As a matter of fact, the data we possess consist more largely of maximum than of minimum or average intensities, and therefore tend to considerably augment the mean derived intensity above the true mean. This will become apparent by an inspection of the map where the zones of 5, 6, and 7 intensity are disproportionately broad, while those of 3 and 4 are disproportionately narrow. We have not attempted to allow for this source of error, though fully aware of it, because we had no means of determining what allowance to make. We have drawn the lines wholly upon the face of the returns, and the investigators who may attempt to utilize our results must grapple with the corrections as best they may.

Throughout the States of North Carolina, South Carolina, Georgia, and North-Eastern Florida, and in general anywhere within about 250 miles of the centre, the energy of the shocks was very great. At Columbia, Augusta, Raleigh, Atlanta, and Savannah, the consternation of all the people was universal. The negroes and many of the poor whites were for a week or two not exactly demoralized, but intensely moralized, giving themselves to religious exercises of a highly emotional character, the stronger and deeper natures among them being impressed with a feeling of awe, the weaker natures with a feeling of terror. And this was general throughout the large region just specified. In all of the large towns within 200 miles of Charleston more or less damage was suffered by houses and other structures. Walls were cracked to such an extent as to necessitate important repairs; dams were broken, chimneys were overthrown, plastering shaken from ceilings, lamps overturned, water thrown out of tanks, cars set in motion on side tracks, animals filled with terror, fowls shaken from their roosts, loose objects thrown from mantels, chairs and beds moved horizontally upon the floor, pictures banged against the walls, trees visibly swayed and their leaves agitated and rustled as if by a wind. These occurrences were general, and were more strongly marked until they became terrifying and disastrous as the centre of the disturbance was approached. At Augusta, 110 miles distant from the epicentrum, the damage to buildings was considerable; and at the arsenal in that place the commanding officer's residence was so badly cracked and shattered as to necessitate practical reconstruction. In Columbia, 100 miles distant, the shock was very injurious to buildings and appalling to the people, but no substantial structures were actually shaken down. In Atlanta, 250 miles distant, there was no worse injury than falling chimneys and some slight cracks in the walls, but the houses were instantly abandoned in great alarm and confusion by their occupants, and many preferred passing the night in the streets to re-entering their dwellings. At Asheville, N.C., 230 miles distant, and at Raleigh, 215 miles distant, the shocks were quite as vigorous as at Atlanta.

Coming nearer the seismic centre we find the intensity increasing on all sides as we approach it. The region immediately about the epicentrum in a great earthquake always discloses phenomena strikingly different from those at a distance from it, and the differences are not merely in degree but also in kind. The phenomena characteristic of the epicentral area cease with something like abruptness as we radiate away from the epicentrum. The central phenomena are those produced by shocks in which the principal component of the motion of the earth is vertical. Proceeding outwards, these predominating vertical motions pass, by a very rapid transition, into movements of which the horizontal component is the greater, and in which the undulatory motion becomes pronounced. The epicentrum, and the zone immediately surrounding it, is the portion of the disturbed tract which merits the closest attention, for it is here that we may find the greatest amount of information concerning the origin and nature of the earthquake. To appreciate this we will venture to offer some theoretical considerations.

Allusion has already been made to the indefinite character of the data used for estimating the intensity of the shock. There is no unit of intensity which is at present available. In selecting certain effects of an earthquake to characterize varying degrees of intensity, the most that can be hoped for is a means for discriminating whether the relative energy of a shock is greater or less in one locality than in another. But how much greater and how much less—in conformity with what law—is a problem which remains to be solved. An earthquake impulse, however, is a form of energy transmitted as an elastic wave through the deeply-seated rocks, and its propagation and varying intensity are subject to the laws of wave-motion There must be, therefore, some typical law governing the rate at which such a wave diminishes the intensity of its effects as it moves onward. To anticipate the objection that this typical law would apply only to a

medium which is perfectly elastic, homogeneous, and isotropic, while the rocks are far from being so, we reply that we have investigated the objection, and are satisfied that while it has some validity, the effect of these inequalities is not great enough to seriously impair the applicability of the law, nor to vitiate greatly the results to be deduced from it. The analysis we offer is a novel one. We attach considerable importance to it, and the consequences which flow from it are somewhat remarkable.

(*To be continued.*)

EXPERIMENTS ON THE SENSE OF SMELL IN DOGS.[1]

I ONCE tried an experiment with a terrier of my own, which shows, better than anything that I have ever read, the almost supernatural capabilities of smell in dogs. On a Bank holiday, when the Broad Walk in Regent's Park was swarming with people of all kinds, walking in all directions, I took my terrier (which I knew had a splendid nose, and could track me for miles) along the walk, and, when his attention was diverted by a strange dog, I suddenly made a number of zigzags across the Broad Walk, then stood on a seat, and watched the terrier. Finding I had not continued in the direction I was going when he left me, he went to the place where he had last seen me, and there, picking up my scent, tracked my footsteps over all the zigzags I had made, until he found me. Now, in order to do this, he had to distinguish my trail from at least a hundred others quite as fresh, and many thousands of others not so fresh, crossing it at all angles.[2]

The object of the experiments about to be described was that of ascertaining whether a dog, when thus distinguishing his master's trail, is guided by some distinctive smell attaching to his master's shoes, to any distinctive smell of his master's feet, or to both these differences combined.

I have a setter-bitch, over which I have shot for eight years. Having a very good nose, she can track me over immense distances, and her devotion to me being very exclusive, she constituted an admirable subject for my experiments.

These consisted in allowing the bitch to be taken out of the kennel by someone to whom she was indifferent, who then led her to a pre-arranged spot from which the tracking was to begin. Of course this spot was always to leeward of the kennel, and the person who was to be tracked always walked so as to keep more or less to leeward of the starting-point. The district—park-lands surrounding a house—was an open one, presenting, however, numerous trees, shrubberies, walls, &c., behind which I could hide at a distance from the starting-point, and so observe the animal during the whole course of each experiment. Sundry other precautions, which I need not wait to mention, were taken in order to insure that the bitch should have to depend on her sense of smell alone, and the following are the experiments which were tried :—

(1) I walked the grass-lands for about a mile in my ordinary shooting-boots. The instant she came to the starting-point, the bitch broke away at her full speed, and, faithfully following my track, overtook me in a few minutes.

(2) I set a man who was a stranger about the place to walk the park. Although repeatedly put upon his trail by my servant, the bitch showed no disposition to follow it.

(3) I had the bitch taken into the gun-room, where she saw me ready to start for shooting. I then left the gun-room and went to another part of the house, while my gamekeeper left the house by the back door, walked a certain distance to leeward in the direction of some partridge-ground, and then concealed himself. The bitch, who was now howling to follow me, was led to the back door by another servant. Quickly finding the trail of the gamekeeper, she tracked it for a few yards; but, finding that I had not been with him, she left his trail, and hunted about in all directions for mine, which, of course, was nowhere to be found.

(4) I collected all the men about the place, and directed them to walk close behind one another in Indian file, each man taking care to place his feet in the footprints of his predecessor. In this procession, numbering twelve in all, I took the lead, while the gamekeeper brought up the rear. When we had walked two hundred yards, I turned to the right, followed by five of the men; and at the point where I had turned to the right, the seventh man turned to the left, followed by all the remainder. The two parties thus formed, after having walked in opposite directions for a considerable distance, concealed themselves, and the bitch was put upon the common track of the whole party before the point of divergence. Following this common track with rapidity, she at first overshot the point of divergence; but, quickly recovering it, without any hesitation chose the track which turned to the right. Yet in this case my footprints in the common track were overlaid by eleven others, and in the track to the right by five others. Moreover, as it was the gamekeeper who brought up the rear, and as in the absence of my trail she would always follow his, the fact of his scent being, so to speak, uppermost in the series, was shown in no way to disconcert the animal when following another familiar scent lowermost in the series.

(5) I requested the stranger before mentioned to wear my shooting-boots, and in them to walk the park to leeward of the kennel. When the bitch was led to this trail, she followed it with the eagerness wherewith she always followed mine.

(6) I wore this stranger's boots, and walked the park as he had done. On being taken to this trail, the bitch could not be induced to follow it.

(7) The stranger walked the park in bare feet; the bitch would not follow the trail.

(8) I walked the park in bare feet: the bitch followed my trail; but in quite a different manner from that which she displayed when following the trail of my shooting-boots. She was so much less eager, and therefore so much less rapid, that her manner was suggestive of great uncertainty whether or not she was on my track.

(9) I walked the park in new shooting-boots, which had never been worn by anyone. The bitch wholly refused to take this trail.

(10) I walked the park in my old shooting-boots, but having one layer of brown paper glued to their soles and sides. The bitch was led along my track, but paid no attention to it till she came to a place where, as I had previously observed, a small portion of the brown paper first became worn away at one of my heels. Here she immediately recognized my trail, and speedily followed it up, although the surface of shoe-leather which touched the ground was not more than a few square millimetres.

(11) I walked in my stocking-soles, trying first with new cotton socks. The bitch lazily followed the trail a short distance and then gave it up. I next tried woollen socks which I had worn all day, but the result was the same, and therefore quite different from that yielded by my shooting-boots, while more resembling that which was yielded by my bare feet.

(12) I began to walk in my ordinary shooting-boots, and when I had gone fifty yards, I kicked them off and carried them with me, while I continued to walk another three hundred yards in my stocking-soles; then I took off

[1] Paper read by Mr. George J. Romanes, before the Linnean Society, on December 16, 1886. Reprinted from the Linnean Society's Journal—Zoology, vol. xx.

[2] "Mental Evolution in Animals," pp. 92-93; where also see for additional remarks of a general kind on the sense of smell in different animals.