first president of the Society; and the second by Mr. Macgowan, kept at Hawkhill, near Edinburgh, from 1770 to 1776. In the first, the mean temperature of the ten years is 44°; in the second, 45°—not a very genial retrospect. Things must have been somewhat discouraging for the farmers in 1782, for a paper is noticed in the second volume of the *Transactions*, by Dr. Roebuck, of Sheffield, who was the manager of the Carron Iron Works, recommending farmers not to cut their corn green in October, although there was ice three-quarters of an inch thick at Borrowstounness, because corn would fill at a temperature of 43°. Things looked brighter from 1794 to 1799, for which years we have results furnished by Playfair. For the first three years—1794, 1795, and 1796—the mean temperature was 48°; and that although 1795 was one of the most severe winters on record, the thermometer having stood frequently several degrees below zero, and a continuous frost having lasted for 53 days. The mean temperature in 1794, however, was 50°. The account of the great frost of 1795, which is given in the *Transactions*, is well worth referring to. In the next three years the mean temperature was 48°, that of 1798 being 49°·28. Of this year (1798) Playfair says that the climate of this part of the island hardly admits of a finer season. No tables were furnished to the Society, in continuation of those of Prof. Playfair, until 1830, when fortunately Dr. Barnes of Carlisle communicated to the Society a series of meteorologica tables kept at Carlisle for the first twenty-four years of the century. The results seem mainly to concur with those of Prof. Playfair—the mean temperature for the twenty-four years being 47°·4547, being 3° higher than the average of the ten years from 1773 to 1783 at Branxholm, and 2° higher than the mean temperature of the seven years from 1770 to 1776 at Hawkhill. The highest temperature I have noted in these returns is that of May 1807, when the thermometer stood at 85° at Carlisle, and the next, that on the 5th of August, 1770, when the thermometer at Hawkhill was at 81°. The two years of the century in which the mean temperature was the highest were 1811 and 1822, in both of which years it was 49°.

"Of the purely scientific part of the Royal Society's work for the first fifteen years of its labours, while Hutton and Black and Playfair and Stewart were in full vigour, it is not too much to say it was brilliant—full of interest, full of power, and full of enthusiasm. The first great founders of course gradually waned, and all such associations are necessarily subjected to alterations of the tide, but as the tale goes on the mathematical papers begin to bear the names of John Leslie and William Wallace. We encounter Walter Scott in 1800, in 1808 the name of David Brewster, and in 1811 that of Sir Thomas Macdougall Brisbane, whose names adorned and whose labours were in the future the prop and stay of the Society. Of Scott I need not speak; but of the services rendered by Brewster it is impossible to express myself too strongly. He, too, like Playfair, had a mind of rare versatility. He could observe, as well as draw from his own resources. He could reason as well as describe. He could build a framework of sound deduction from the most unpromising hypothesis, and work out with unflagging spirit the thread of demonstration, however slender. He was the most prolific contributor of his day; nor do I think that any one but himself in these times could have kept the fire lighted by Hutton and Playfair burning so brilliantly. For it is not to be disguised that in the heat of the Continental struggle an air of languor creeps over the proceedings. The joyous enthusiasm of 1783 refuses to be invoked, and is elicited in vain. Nor is it wonderful. When the Gauls were so nearly at our gates, the safety of our own commonwealth was comparatively our only care. But when 1815 had arrived, and men's minds, set free from the long anxiety, had again tranquillity to cultivate the arts of peace, the energy of the rebound was great, and the history of British science has been one continued triumph ever since. By the exertions of Brewster and Brisbane, and many other associates, our Society again began to flourish, both leading and following the course of discovery as the stream flowed on. Both of these men continued to be the pride and ornament of the Society long after the expiration of the half-century which I have assigned to myself as my limit, for Thomas Brisbane succeeded Sir Walter Scott as president in 1832, and survived until 1860. Long before that a new generation had surrounded the veteran philosophers, and their destiny has been to recount and carry forward discoveries of which even Brewster and Brisbane hardly dreamt.

"Enough for the present of this retrospect, and the slender tribute I have attempted to pay to the memory and labours of a masculine and powerful generation. That we have built on their discoveries and learnt even by their errors is quite true; for the history of the second half of the century exhibits science far in advance of 1783, and even of 1833. In 1783 geology was in its infancy; palæontology was all but unknown. Cuvier was only then commencing his pursuits in comparative anatomy, which were to end in reproducing the forms of extinct life. The Glacial epoch had not then been elucidated by the research and genius of Forbes and Agassiz, and the dynamic theory of heat was still unproclaimed. The wonders of the photographic art were unknown even in 1833, for Talbot and Daguerre did not come on the scene for several years afterwards. In 1833 the apostle and disciples of evolution had not broken ground on that vast field of inquiry. Spectrum analysis and the marvellous results which it has already furnished and those which it promises have in our day only heralded the advent of a new science. But however far in advance of the founders of the Royal Society the current philosopher may be, there was a robustness and characteristic individuality about the great men of that generation which we may not hope to see replaced. We may assume—indeed, we hope—that the close of the next century will find the progress of knowledge as far advanced beyond its present limits as we think that the science of to-day is beyond the point reached a century ago. We may be assured that before that time arrives many surmises, still in the region of hypothesis, will have become certainties, and that many supposed certainties will have turned out fallacies. Many errors will have been corrected, many dogmas discredited, many theories confirmed or refuted, at the bar of ascertained fact, as those of 1783 have been. Yet even then will our successors, I trust, as we do now, stand reverently before the memory of our founders. Happy is the institution which can show such a muster-roll, and happy the country which can boast such sons. I take leave of my theme with the fervent hope and firm conviction that in the century which we now inaugurate the Royal Society will continue with success the noble task to which by its charter it is devoted, of investigating the hidden treasures of nature and appropriating them to the benefit and happiness of mankind."

INSTINCT

1. *Is there a Science of Comparative Psychology?*

"IN the family of the sciences Comparative Psychology may claim nearest kinship with Comparative Anatomy; for just as the latter aims at a scientific comparison of the bodily structures of organisms, so the former aims at a similar comparison of their mental structures." These words form the opening sentence of Mr. G. J. Romanes' Introduction to his recently published volume on "Mental Evolution in Animals," and in a footnote he is careful to remind us that the phrase "mental structures" is used in a metaphorical sense. Let us consider how far a comparison of the mental structures of animals, even in a metaphorical sense, is possible.

Our knowledge of mind is either direct or inferential: direct on the part of each individual so far as his own individual mind is concerned; inferential so far as the minds of others are concerned. For it is a law of our being that mind cannot come into direct contact with mind. This fact—that the mental processes of our neighbours can never come within the sphere of our objective knowledge—has long been recognised (see *ex. gra.* Berkeley, "Princ. Hum. Know.," §§ 27 and 145; Kant as quoted in F. Pollock's "Spinoza," p. 177); and the late Prof. Clifford (see "Lectures and Essays," vol. ii. p. 72) coined the exceedingly convenient term *ejective* as descriptive of that class of phenomena which belong neither to the subjective nor to the objective category. My neighbour's mind is not and never can be an object; it is an eject, an image of my own mind thrown out from myself. Into every human being that I meet I breathe this subtle breath; and that man becomes for me a living soul.

Our knowledge of mind is therefore partly subjective, partly ejective. Now it is perfectly obvious that, were I an isolated unit, shut off from all communication with my fellows, no science of psychology would be possible for me. I might by the analysis of my own mental processes arrive at certain conclusions with regard to my own states of consciousness; I might reach some sort of knowledge of the working of my own mind. But this would not be a science of mind. A science of mind only becomes possible when I am able to compare my own conclusions with those which my neighbours have reached in a similar manner. By means of language human beings can communicate to each

other the results which each has obtained; and each human being is able *to submit these results to the test of subjective verification*. For human beings therefore a *science* of psychology is possible just in so far as the results obtained indirectly are capable of direct *verification*.

One of the most remarkable results of modern scientific investigation is the establishment of a more or less definite parallelism between the phenomena of ejective psychology (thus capable of subjective verification) and certain objective phenomena of physiology—a parallelism of psychosis and neurosis. But these phenomena of physiology are not restricted to the human subject; and we therefore have grounds for believing that running parallel to the neuroses of animals there are certain psychoses. And it would seem at first sight possible that corresponding to a science of comparative neurosis we might have a science of comparative psychosis. We must remember, however, that it is only on the lower mental levels, so to speak, that we know anything approaching to definiteness with regard to the parallelism of neuroses and psychosis. All, therefore, that, as scientific investigators, we seem to have any grounds for inferring is that accompanying the neuroses of animals there are in all probability some kind of psychoses. We may speculate as to the character of these psychoses—and in the case of the higher mammalia our speculations are probably by no means worthless—but we cannot construct a comparative *science* of these psychoses because the results we obtain ejectively are incapable of direct verification. As a speculation modern constructive psychology has its value—like other speculations it may give direction to our scientific investigations—but let us not forget that the invaluable process of verification is, from the nature of the case, impossible.

To sum up. All our knowledge of minds other than our own is ejective; but in the case of human psychology the results reached ejectively may be verified subjectively. Animal minds are also ejective; they are more or less distorted images of our own minds. But such is the extraordinary complexity of the human mind—a complexity largely due to the use of language—that we may well suppose that any conception we can form of animal consciousness is exceedingly far from being a true conception. The results of comparative psychology—the science which has for its object the comparative study of these distorted images of our own mental processes—are incapable of verification. These are the facts which have to be taken into consideration when we seek an answer to the question "Is there a science of comparative psychology?" Notwithstanding that it has won for itself a more or less recognised place among the sciences, I venture to submit that our answer to this question should be an emphatic negative.

It must be noted, however, that I here mean by psychology the science which deals with subject and eject. If we include under psychology the science which deals with the "perpetual adjustments of special inner actions to special outer actions which accompanies increasing evolution of the nervous system," or that to which Mr. Herbert Spencer gives the name *objective* psychology ("Prin. Psychol.," vol. i. p. 142), our answer will of course be different. Objective psychology, or the comparative physiology of the nervous system *plus* a comparative study of the corresponding adjustive actions, has every right to be termed a science because the results obtained admit of verification. And it is a science in which Ferrier, Hitzig, Romanes, and others have done good work.

2. *The Place of Consciousness*

There would seem to be four hypotheses with regard to the place of consciousness in the animal world.

1. That according to which consciousness is a motive power (Free Will).
2. That according to which consciousness is altogether absent (Automatism).
3. That according to which consciousness is a product (Conscious Automatism).
4. That according to which consciousness is a guide (Determinism).

1. *Free Will.*—By free will I here mean the power of initiating actions by the mere volition of the self-conscious *Ego*. The exercise of free will involves an interference *ab extrâ* with the normal working of the nervous system.

This is not the place for a discussion of free will and determinism. That battle must be fought out within the domain of human psychology. From its bearing on the question of animal consciousness, however, I may be permitted to say a few words on the subject.

The answer which the ordinary believer in free will gives to the determinist is contained in three words—*I can choose*—and he thinks that there is an end of the matter. But the real point at issue lies deeper down, and is involved in the question—*What am I?* Let us hear the answer which the determinist gives to this question. I am, he replies, the sum of my states of consciousness at any moment. Apart from the stream of my mental states I, as a self-conscious individual, have no existence. This stream of conscious states or psychoses I believe to be the subjective aspect of a stream of nervous states or neuroses. And this stream is rigidly subject to law. But if these states of mind—under which head must be included states of definite consciousness, states of sub-consciousness, and states of submerged consciousness—if these states of mind, I say, constitute *me*, then, since these states of mind determine those which follow, these following states, and the actions which accompany them, are determined by me. But at the same time they are part of an orderly sequence subject to law. The moment I identify myself with my states of mind I begin to see clearly that free will in the common-sense acceptation of the term—that is, a sense of individual choice—is perfectly compatible with the doctrine of determinism—that my mind is completely subject to law. The sense of choice I undoubtedly possess is due to the temporary equilibrium of motives, and the eventual prevalence of one set of motives over another set of motives. The freedom which every man is conscious of possessing is freedom to act in accordance with his own character.

"Freedom," says Kant, "is such a property of the will as enables living agents to originate events independently of foreign determining causes." This at first sight seems utterly opposed to determinism. And yet it contains a central core of truth which every determinist will accept. No determinist can deny that every human being carries about with him a special something, peculiar to himself, which is a most important factor—constituted as we are, the most important factor—in determining his choice in any act of volition. This special something we call, ejectively, his character, and, objectively, his organisation. Men are not like inorganic clouds at the mercy of external forces, but contain the springs of action in themselves. The brain is not merely a mass of inert matter; but a mass of matter cunningly organised, in which is locked up a vast store of potential energy. The organism is, moreover, a *variable* piece of mechanism. Hence at different times it reacts differently under the influence of the same stimulus. And this difference of reaction helps to fix the idea that the will is absolutely free. On a certain occasion we acted in a certain way. We see on reflection that our action was not the best. On a similar occasion afterwards we act differently. And we then imagine that we could have acted differently in the first instance. But it is clear that the two cases are not alike. Reflection has altered one of the determinants of action, the character. The character having changed, the action is different. Such a definition as Kant's—the essential truth of which I take to be that a man's actions are the outcome of his character—is as valuable to the determinist as to any one else. At the same time "it is inconceivable," as Chaldai Creskas said long ago (*circa* 1410), "that two men, being themselves of like temper and character, and having before them like objects of choice in like circumstances, should choose differently" (quoted from F. Pollock's "Spinoza," p. 96).

Determinism simply comes to this—that both on the objective side and on the subjective side our actions are determined by law. On the one hand a perfect knowledge of the organism *plus* a perfect knowledge of any stimulus and the surrounding conditions would enable us to say how the organism would act under that stimulus. On the other hand a perfect knowledge of the character *plus* a perfect knowledge of any motive and the circumstances of the case would enable us to say what feelings would result (the actions being the objective side of the feelings). If by free will it is meant that our actions are the outcome of the play of a motive-stimulus on our character-organisation, then free will and determinism are at one.

But this is not what is meant by those who maintain the doctrine of free will. What is meant by them is this—that presiding alike over our thoughts and actions, initiating, guiding, and inhibiting, there is a certain "masterful entity," the self-conscious *Ego*. This *Ego*, though in no wise connected with our bodily organisation, has nevertheless the power of interfering with the action of that organisation. And it is absolutely free, utterly unfettered

by law. This doctrine I reject: not because I am in a position to disprove it, but because I see no reason for accepting it. And rejecting this doctrine in the sphere of the human mind, I feel bound to reject it in the sphere of animal intelligence. But I am not blind to the fact that many of my neighbours do not reject it in the sphere of the human mind. To them two courses are open: either to extend it into the sphere of animal intelligence, or not so to extend it. If they do so extend it, they thereby render the study of animal intelligence incapable of scientific treatment, even from the objective standpoint, by the introduction of a factor not subject to law. If they do not so extend it, they must accept one of the three views next to be considered.

2. *Automatism.*—Very little space need be devoted to a doctrine that very few believe. Those who accept the doctrine of the parallelism (or identity) of neurosis and psychosis and add to this a belief in evolution are logically bound to accept the corollary that the neuroses of animals are accompanied by some kind of psychoses which more or less dimly foreshadow our own psychoses. Those, however, who reject the hypothesis of evolution, or at least deny its application to the mind of man, and who believe in the doctrine of free will as restricted to the human being, will, not improbably, accept the doctrine of automatism in animals. In any case it is a theory upon which the study of organic processes, reflex, instinctive, and intelligent (or selective), admits of scientific treatment. It is indeed "objective psychology" *plus* the dogmatic assertion that consciousness is absent.

3. *Conscious Automatism.*—"Materialism," says Mr. Romanes, "is logically bound to argue in this way: We cannot conceive of a conscious idea, or mental change, as in any way affecting the course of a cerebral reflex, or material change; while, on the other hand, our knowledge of the conservation of energy teaches us as an axiom that the cerebral changes must determine each other in their sequence as in a continuous series. Nowhere can we suppose the physical process to be interrupted or diverted by the psychical process; and therefore we must conclude that thought and volition really play no part in determining action. Thoughts and feelings are but indices which show in the mirror of the mind certain changes that are proceeding in the matter of the brain, and are as inefficient in influencing those changes as the shadow of a cloud is powerless to direct the movements of that of which it is the shadow. . . . This is opposed to common sense, because we all feel it is practically impossible to believe that the world would now have been exactly what it is even if consciousness, thought, and volition had never appeared upon the scene—that railway trains would have been running filled with mindless passengers, or that telephones would have been invented by brains that could not think to speak to ears that could not hear" (*Nineteenth Century*, December, 1882, p. 879). How far the materialist—the logical results of whose doctrine are apt to be forced on him from all sides—is ready to accept this particular logical result I leave it for him to say. It is at any rate a possible view, and, like that of unconscious automatism, is one upon which a scientific treatment of organic processes is admissible.

4 *Determinism* —This view has already been incidentally given under the heading of the directly opposed doctrine of free will. It is the doctrine of the parallelism (or identity) of neuroses and psychoses, which, both in their subjective and objective aspects, are rigidly law-bound. Determinism may be treated either from the philosophical or from the scientific standpoint. From the point of view of the man of science we may say that consciousness is a *guide* to action and has been a guide in evolution; that during the process of evolution there gradually emerged something distantly related to what we know in ourselves as consciousness, which at a very early stage of evolution became, so to speak, polarised into pleasurable and painful; that those actions which were associated with pleasurable feelings were more frequently performed than those associated with painful feelings; that those organisms in which there was an association between right action and pleasurable feelings would stand a better chance of survival than those in which the association was between wrong actions and pleasurable feelings; and that finally those organisms in which conscious adjustments of all orders were more perfectly developed would be the winners in life's race. Some such deductions as these would seem to be admissible on the hypothesis of evolution. With such questions as How have psychoses become associated with neuroses? or Why have psychoses been associated with neuroses? or How can psychosis exercise a guiding influence on neurosis?—with such questions as these the man of science, as such, has nothing to do. These are questions for the philosopher, and this is, therefore, not the place to discuss them. Suffice it to say that we must either accept some such view as that advocated by Clifford in his masterly essay "On the Nature of Things in Themselves" ("Lectures and Essays," vol. ii. p. 71) or be content to confess our ignorance.

Upon this view of the place of consciousness in the animal kingdom, the study of organic processes, reflex, instinctive, and intelligent (or selective), admits of scientific treatment. A science of "objective psychology" is possible for us; and a science of ejective psychology is also possible, but not for us.

3. *The Lapse of Consciousness*

One of the most surely established inductions of psychology is this: that the more frequently an action is performed the more perfectly automatic does it become—the more does it tend to pass into stereotyped reflex action. Actions which are at first performed with that definite consciousness implied in the term close attention can, after frequent repetition, be performed almost, if not altogether, without even indefinite consciousness. It would seem that after the definite establishment of the nerve connections necessary for the performance of certain actions or sets of actions the guiding influence of consciousness might be withdrawn.

This principle is too well known to require illustration here. I shall therefore content myself with drawing attention to one or two of its corollaries.

1. Since the same action or set of actions may be performed with full consciousness—a consciousness of the end in view, and of the means necessary to that end—with indefinite consciousness, or with a vanishing amount of consciousness, it is impossible for me to say what amount of consciousness, if any, an action performed by my neighbour involves. Again and again we see our neighbours perform most complicated actions—such as winding up their watches—with so little consciousness as to leave no trace upon the memory. Abernethy quotes a case of a lawyer writing out an important opinion in his sleep. Still more impossible is it for me to say what amount of consciousness, if any, an action performed by one of my dumb companions involves. Decapitated frogs—in which we have some grounds for believing that consciousness is absent—perform a number of seemingly purposive actions.

2. Since those actions which are frequently and persistently performed by the individual have a tendency to pass into the automatic and unconscious stage, it would seem highly probable that those actions which have been performed not only by the individual but by a long line of ancestors whose organisation he inherits are, or very soon become, completely, or in a very high degree, automatic and unconscious. Who can say what amount of consciousness, if any, is involved in the actions of newly-born piglings or newly-hatched chicks?

3. It would therefore seem difficult or impossible to disprove the hypothesis that all truly instinctive actions—in so far as they are not modified (as they so often are modified) by a little dose of reason—are automatic and unconscious. I do not mean to maintain that hypothesis. But I say that, having regard to the known phenomenon of the lapse of consciousness, I do not see how that hypothesis could be disproved.

4. *The Psychological Definition of Instinct*

"Instinct," says Mr. Romanes in his recently published "Mental Evolution in Animals" (p. 159), repeating the definition given in "Animal Intelligence" (p. 17), "Instinct is reflex action into which there is imported the element of consciousness. The term is therefore a generic one, comprising all those faculties of mind which are concerned in conscious and adaptive action, antecedent to individual experience, without necessary knowledge of the relation between means employed and ends attained, but similarly performed under similar and frequently recurring circumstances by all individuals of the same species."

To such a psychological definition of instinct there seem to me to be two grave objections. First, there is the general objection, indicated in the first section, arising out of the ejective nature of our knowledge of animal consciousness. Secondly, there is the special objection raised under the head of "The Lapse of Consciousness." These objections have not escaped Mr. Romanes' notice, but I think he underestimates them. "No doubt," he says ("Ment. Evol.," p. 160), "it is often difficult, or even

impossible, to decide whether or not a given action implies the presence of the mind-element—*i.e.* conscious as distinguished from unconscious adaptation; but this is altogether a separate matter, and has nothing to do with the question of defining instinct in a manner which shall be formally exclusive, on the one hand of reflex action, and on the other of reason." But I venture to think that the difficulties of application are from the very nature of the case insuperable, and that the definition is therefore, whatever its logical value, practically of little service.

Again, on p. 17 of his recent volume, Mr. Romanes tells us that "the only test [of the conscious choice element] we have is to ask whether the adjustments displayed are invariably the same under the same circumstances of stimulation. The only distinction between adjustive movements due to reflex action, and adjustive movements accompanied by mental perception, consists in the former depending on inherited mechanisms within the nervous system being so constructed as to effect *particular* adjustive movements in response to *particular* stimulations, while the latter are independent of any such inherited adjustment of special mechanisms to the exigencies of special circumstances." And a little further on (p. 18) he says, "It is enough to point to the variable and incalculable character of mental adjustments as distinguished from the constant and foreseeable character of reflex adjustments." All which may be very true. But it seems to cut away the ground from under his definition of instinct. For surely what he says here of reflex actions is also true of instinctive actions. Surely instinctive actions "depend on inherited mechanisms within the nervous system being so constructed as to effect *particular* adjustive movements in response to *particular* stimulations." Surely we may also point to the "constant and foreseeable character of *instinctive* adjustments."

But though an instinctive action may involve no consciousness in the individual, it may have involved consciousness, during the process of its evolution, in the ancestors of the individual. In this way, perhaps, we may admit consciousness into our definition of instinct. But if we hark back to ancestors in one case, we may fairly do so in another. And since the secondary instincts of the individual involved intelligence in his ancestors, we must import not only consciousness but intelligence into our definition of instinct. If we admit lapsed consciousness, why not admit lapsed intelligence? Our definition will then become: Instinct is reflex action into which is imported (ancestrally) the elements of consciousness and intelligence. In which case instinct and reason run together.

It seems to me, therefore, that the psychological definition of instinct lacks that definiteness of application which is not merely desirable but essential. If I might be permitted to paraphrase Mr. Romanes I would say, "I am persuaded that if we are to have any approach to definiteness in the terms which we employ —not to say to clearness in our ideas concerning the things of which we speak—it is" *not* "desirable to restrict the word instinct to mental as distinguished from non-mental activity." And this just because it is so "difficult, or even impossible, to decide whether or not" instinctive actions "imply the presence of the mind-element—*i.e.* conscious as distinguished from unconscious adaptation."

5.—*A Physiological Definition of Instinct*

"Instinctive actions are actions which, owing to their frequent repetition, become so habitual in the course of generations that all the individuals of the same species automatically perform the same actions under the stimulus supplied by the same appropriate circumstances." This physiological definition of instinct, which is incidentally given by Mr. Romanes ("Animal Intelligence," pp. 16–17), is, if I mistake not, of more practical and scientific value than the psychological definition which immediately follows, and which introduces "the element of consciousness" and "faculties of mind."

Were it impossible to define instinct in such a manner as to be formally exclusive, on the one hand, of reflex action, and, on the other, of intelligent (or selective) action, without having recourse to the associated phenomena of consciousness, then it might be advisable to introduce consciousness into our definitions for the sake of giving them a logical status. And Mr. Herbert Spencer seems to see this difficulty when he defines *or describes* instinct as compound reflex action. But, though reflex action shades into instinctive action, and instinctive action (as seen in the phenomena described by Mr. Romanes, under the heading "The Plasticity of Instinct") into intelligent action, still some such definitions as the following would seem sufficiently to answer to the demand for formal exclusiveness:—

1. *Reflex Actions* are actions taking place in, or performed by, an individual in virtue of his possession of a general type of nervous organisation.

2. *Instinctive Actions* are actions performed by the individual in virtue of his possession of a special type of nervous organisation, that is, a type of organisation common to his species.

3. *Intelligent (or Selective) Actions* are actions performed by an individual in virtue of his possession of an individual nervous organisation, that is, an organisation special to himself.

If we call the foundation type of nervous organisation (in the mammalia, for example) *a*, the special modification of that type (in all dogs, for example) *b*, and the individual modification developed in some individual (say Dr. Huggins's "Kepler") *c*; then reflex actions are the outcome of *a*, instinctive actions the outcome of $a+b$, and selective or intelligent actions the outcome of $a+b+c$.

That there are difficulties in the application of these definitions to special cases I readily admit, but I venture to submit that they are by no means of so grave a nature as those involved in the psychological definitions advocated by Mr. Romanes.

I need not say here that such definitions do not by any means imply the absence of consciousness, since I have devoted a special section to *The Place of Consciousness* with the special object of showing that the doctrine of determinism, which I accept, maintains the parallelism or identity of psychosis and neurosis.

6.—*The Origin and Development of Instincts*

This article has already exceeded the length to which it was intended to run. On this head, therefore, I must be brief. The problem of the origin and development of instincts comes to this—How has it come about that certain nervous structures, and the actions which are their external and obvious manifestations, are developed in all the members of a certain species? It is clear that such a development of certain structures and their corresponding actions in all the individuals of a particular species must answer to a widely felt need. The actions answer to circumstances of frequent occurrence in the life-history of the species, just as intelligent actions "answer to circumstances of comparatively rare occurrence in the life-history of the species" ("An. In.," p. 17). The question is—How far is the equilibration direct, *i.e.* by adaptation, and how far is it indirect, *i.e.* by natural selection? To discuss this question would require a separate article. I content myself with giving two quotations, the former from Mr. Darwin, the latter from Mr. Spencer. "I believe that most instincts are the accumulated result, through natural selection, of slight and profitable modifications of other instincts, which modifications I look at as due to the same causes which produce variations in corporeal structures. . . . . But in the case of the many instincts which, as I believe, have not at all originated in hereditary habit, I do not doubt that they have been strengthened and perfected by habit; just in the same manner as we may select corporeal structures conducing to fleetness of pace, but likewise improve this quality by training in each generation" (quoted "Ment. Ev. in Ans.," p. 264). So far Mr. Darwin. Mr. Spencer says: "The equilibration of organisms that are comparatively passive is necessarily effected indirectly by the action of incident forces on the species as a whole. But along with the gradual evolution of organisms having some activity, there grows up a kind of equilibration that is relatively direct. In proportion as the activity increases, direct equilibration plays a more important part. Until, when the nervo-muscular apparatus becomes greatly developed, and the power of varying the actions to fit the varying requirements becomes considerable, the share taken by direct equilibration rises into co-ordinate importance" ("Princ. Biol.," vol. p. 468). It seems to me that we have here substantial agreement as to the part played by indirect equilibration in laying the foundation, and the part played by direct equilibration in perfecting the superstructure. (I venture to think that Mr. Romanes somewhat mistakes Mr. Spencer's position with regard to the "very subordinate importance of natural selection as an evolving source of instinct," and with regard to the question of "lapsed intelligence.")

7. *Conclusion*

One or two words in conclusion by way of summary.

1. While fully admitting the great interest that attaches to the study of the inferred mental faculties of the higher brutes, I believe that, from the ejective nature of the animal mind and the

necessary absence of verification, no science of comparative psychology, except such as is restricted to "objective psychology," is possible.

2. Of the four views of the place of consciousness in the animal world only one—that of *free will*—renders the study of the actions of animals incapable of scientific treatment. Of the other three I believe *determinism* to be the most satisfactory. According to this view both neuroses and psychoses are subject to law. But from our necessarily ejective knowledge of psychoses, we are forced to confine our attention (from the scientific point of view) to the objective phenomena of neurosis, especially as manifested in conduct. Of the psychoses we can know nothing with certainty; of the neuroses we may learn a little; of conduct we may learn much.

3. From the principle of the lapse of consciousness certain corollaries may be drawn—(*a*) that it is difficult or impossible to say what amount of consciousness, if any, an action performed by my neighbour involves; (*b*) that it would seem probable that the lapse of consciousness in the individual is paralleled by a lapse of consciousness in the species; and (*c*) that the hypothesis that instinctive actions are unconscious is incapable of disproof.

4. On the general grounds given in 1, and on the special grounds given in 3, I see grave difficulties in accepting the psychological theory of instinct—that instinct is reflex action into which is imported the element of consciousness.

5. In accordance with the principle thus advocated a physiological definition of instinct must be sought. Some such definition as this may be proposed: *Instinctive actions* are actions performed by the individual in virtue of his possession of a special type of nervous organisation, that is, a type of organisation common to his species.

6. The question of the origin and development of instincts thus becomes a question as to how this special type of structure has been evolved. It takes its place as part of the general question of the evolution of structures—the actions being the external manifestations of internal structures. To the question as to the relative importance of direct and indirect equilibration I could give no definite answer within the limits of this article, and therefore gave quotations from Darwin and Herbert Spencer.

C. LLOYD MORGAN

A NEW OBSERVATORY FOR PARIS

THE last number of the *Comptes Rendus* of the Paris Academy of Sciences contains a memoir by Admiral Mouchez, urging the necessity of removing to a separate establishment beyond the city the chief departments of the Paris Observatory. When the building was originally erected by Perrault about a mile to the south of the Luxembourg, the city scarcely reached beyond that point. But since then it has spread in every direction, completely surrounding the Observatory with lofty edifices, and charging the atmosphere with all sorts of gases, smoke, and other impurities. These altered conditions are all the more injurious that, thanks to the progress of astronomical studies, the power and accuracy of the instruments have to be continually increased, while a clear and still atmosphere is more than ever needed for taking observations. The vicinity of the Catacombs and of busy streets has also rendered the ground less firm than formerly.

In 1854, and again in 1868, these adverse conditions were brought before the Government, and discussed in the Academy. After a careful study of the situation, the Commission appointed by the Academy to inquire into the matter unanimously reported in 1869 in favour of a branch establishment outside of Paris; but this suggestion, although fully approved of by the Academy, was for various reasons allowed to fall into abeyance.

Since then the evils complained of have been aggravated, in spite of all the improvements introduced for the purpose of modifying them. Hence it becomes more than ever indispensable to carry out the project forthwith, if the Observatory wishes to maintain its efficiency and keep pace with similar establishments abroad. The most serious obstacles to its legitimate development are the disturbed and clouded state of the atmosphere in the centre of a large city, the constant vibrations of the ground, and the impossibility of accommodating the astronomers in the building, as is done in all foreign observatories. Hence arises an insurmountable obstacle to the proper organisation of the night service, while extreme difficulty is felt in improving the existing plant and obtaining other much needed instruments, for which no suitable position can be found.

Merely to erect the long-contemplated tower and cupola of the great telescope there would be required a Government grant of from 20,000*l.* to 24,000*l.*, besides at least an equal sum to prevent the erection of lofty houses in front of the new grounds and to purchase the instruments still needed. But even were such grants obtained, the Observatory would continue to labour under the serious inconveniences above described. Without, however, imposing such a burden on the State, the difficulty might be met, and the old historical edifice of Louis XIV. preserved, by erecting in one of the public domains a new and magnificent observatory furnished with all the improvements and appliances of modern science. In order to effect this, it would suffice to alienate about 22,000 square metres of gardens and open spaces surrounding the present Observatory, and serving only to isolate it from the neighbouring houses. Sold at the moderate estimate of from 4*l.* to 6*l.* per metre, a sum of nearly 120,000*l.* might be raised, which would be more than sufficient for the purpose.

After sacrificing enough land for the construction of two new streets in continuation of the Avenue du Luxembourg, and isolating the Observatory on all sides, it would still retain the northern court and a garden on the south 70 to 80 metres long by 50 broad. The building would thus also retain the exact appearance that it presented when originally constructed by Perrault. Here might be preserved the Archives, the Bureau des Calculs, the Museum, and three or four instruments still capable of rendering some service if placed at the disposition of the Faculty of Sciences for the instruction of students.

All the plans of some such project as is here proposed have already been prepared with the greatest care by the able architect, M. Deharme. They include accommodation for thirty astronomers and assistants with their families, all the instrumental and service rooms, the halls, and an underground gallery, a structure 300 metres high for the study of the atmosphere, gas works, a covered gallery connecting all the instruments with the apartments of the astronomers; lastly, the great cupola for the 16m. telescope, at a total cost of 98,350*l.* Including the price of the new instruments, fittings, and inclosing wall, this sum would be raised to 108,000*l.*, which might be obtained by the proposed sale of lands.

The Council has unanimously adopted this project, demanding that it be referred to the Academy and to the Bureau of Longitudes, which bodies had already pronounced favourably on some such scheme in 1854 and 1868. Thus no serious objections seem to stand in the way of a project by which alone the present adverse conditions may be removed, and France endowed with the most complete and finest observatory of modern times.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Sherardian Botanical Chair at Oxford has at length been filled up by the election of Mr. Bayley Balfour, Professor of Botany at Glasgow. Mr. Balfour has had a distinguished career. Passing his student life at Edinburgh, he finally graduated as a Doctor of Medicine, receiving the University Gold Medal for his thesis, having previously carried off first class honours as Doctor of Science in Botany. Two years were spent in acquiring a practical knowledge of the methods of morphological and physiological research in the botanical laboratories of France and Germany under Profs. De Bary and Sachs. We next find him assisting his father, the Regius Professor of Medicine and Botany in the University of Edinburgh, in conducting his classes alike in the lecture-room, in the laboratory, in the herbarium, and in practical field work. For four years he was assistant to the Regius Professor of Natural History in the University of Edinburgh, and for six years he lectured on botany to the students of the Royal Veterinary College, until finally he was appointed Crown Professor of Botany in the University of Glasgow. Of good scientific work done there is an ample record. A valuable paper published in the *Philosophical Transactions* gives the result of his labours at Rodriguez, where he was sent by the Royal Society in 1874 as botanist and zoologist to the Transit of Venus Expedition. In 1880 we find him making a scientific exploration of the Island of Socotra, the results of which have been published in various periodicals, the final report on the botany of the island being now in course of publication by the Royal Society of Edinburgh. Prof. Balfour's wide experience in field, laboratory, and herbarium, will make him a valuable addition to the Natural