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THE EFFECTS OF BACKGROUND ON THE COLOR BEHAVIOR OF
ANOLIS CAROLINENSIS (CUV.).

by

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Experimental Psychology.

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It is a peculiar fact, so far as the literature of the color changes in *Anolis Carolinensis* is concerned, that no one has as yet investigated the effects of background on these changes,---- it is a further peculiarity, tho I must speak with some reserve on this matter, that popular opinion as to the adaptability of *Chameleón Vulgaris* to its surroundings, has been persistently contradicted by scientific investigators; and finally, it would have been nothing short of miraculous if in animals possessing the faculty of changing the color of their skins and having eyes as well, natural selection had not made for correlation between the visual functions and the cutaneous functions, in short, for adaptability.

I do not propose to enter into a long discussion of the literature of the color changes in *Chameleón*, first because this has been admirably worked out (1); and then too, because I am only interested in this animal in an indirect way. It suffices to say, that in the quasi-scientific literature of Greece and Rome, popular opinion as to adaptability found support from such men as Aristotle (2), Plinius(3), Ovid (4), Seneca (5), whereas it was contradicted by the results of scientific investigations undertaken thru-out the whole of the nineteenth century. After 1850, when Bruecke's (6) work had already appeared, only one or two voices protested against the doctrine of non-adaptability, notably that of Campana (7), whose work I venture to predict, will find more adherents in the future than it has in the past from quite other points of view, but who entered into no experimental investigations, merely stating that he had often seen *Chameleón* green in the daytime when on green bushes and small trees, but that this background effect, if such it were, soon pass-

ed away, the animals resuming their normal daylight skin-color, as theory demanded they should.

In our country three well known works have appeared on Anolis, notably those of Carlton (8) and of Parker and Starrett (9). The former investigator concluded from his experiments that;

- 1) the brown state for animals in confinement is taken on in daylight,

- 2) the brown state is ordinarily maintained by a tonus established by the sympathetic nerves, and dependent upon stimulation of the end-organs in the skin of Anolis by light,

- 3) the melanophores are not directly stimulated by light,

- 4) the green state is taken on in the dark,

and others with which the present paper is not concerned, and which are irrelevant to anything it has to add.

The latter concludes that,

- 1) At 10 degrees Centigrade, Anolis becomes brown and stays brown without respect to illumination, (115 C.M.)

- 2) At 40 degrees Centigrade, Anolis becomes green and remains so without respect to illumination,

- 3) At intermediate temperatures, 20 to 35 degrees, light and darkness are the controlling factors, but the effect of heat is still evident over this range, in that it may affect the rate of color change, or the rate at which they take place.

Lockwood's paper I only know indirectly (10), but from what I gathered from a review, he presented nothing new. So far as I am aware then, this is the status of the whole matter of color change at the present time.

My experience with *Anolis*, obtained from a dealer, extends over a period of about eight months. I kept the animals confined in a large aquarium, drained, floored with sand, and furnished with limbs of trees. In the fall months, these were leaf-clad, in the winter months, the leaves dried and browned. Now, on the basis of daily and nightly observations, covering a period of about three months, the following soon became evident and prominent;

1) At night, moonless nights, I have often seen individuals that were brown; not many, but always one or two. Rarely indeed were all the animals green as theory demands they should be in the absence of stimulation by light,---- this when there were no green leaves in the aquarium;

2) During the winter months, when there were no green leaves in the aquarium, there were always a few individuals that were green at day time, contrary to the theory which demands that they be brown in the light;

3) When leaves were present on the limbs and twigs, the animals would preferably rest upon them, and of all the animals that happened to be resting on them, three-fourths were always green whereas such animals that were resting on the bared limbs or on sticks of wood were predominately brown, contrary to the theory which demands that all animals be brown in day-light;

4) Here very lately, after the animals had been kept confined in a large screened cage, instead of in the aquarium as heretofore, I have noticed, that, the the animals were quite predominately brown

under ordinary circumstances, whenever food in the form of live flies was introduced into the cage, all animals very quickly turned green.

These superficial observations lead me to think that the background was a factor which contributed to bring about these manifest deviations from the normal as established by previous workers; ~~the~~ indeed these investigators said nothing that would cause one to believe that such deviations did exist and did occur, altho they must have observed them also. Further these observations lead me to believe, that even granting the traditional factors of light, heat and even of background, there must be others, most probably internal factors, and quite likely especially important ones, for why otherwise do I see green individuals in daylight on a dark background, and brown animals at night, while all other individuals in the same general ⁱⁿ environments as these conform to theory. It is insufficient, yes, quite unscientific, to explain these apparently abnormal behaviors by referring them to so-called psychical factors and the like. For whatever be the apparent effects of illumination, of moisture, of heat, and as I will endeavor to show, of background, in making for this or that color change, no one will imagine that these color changes are an end, and not a means to an end. This end may be an internal metabolic end, one connected perhaps with respiration, or with secretion, or indeed excretion, or what not; and such a point of view, one that can not be avoided, certainly makes for still greater complication, on which account we ought not therefore be surprised if in our experimental manipulations, we get apparent abnormalities of behavior. For if we prove, as this paper intends, that background is an effective factor in determining skin-color, I do not desire to lay any undue emphasis upon this fact. Biologically considered, if it ^{is} adaptation, it is quite likely only accidentally so,----- it suffices to show this influence of background, however it may be in-

terpreted. In other words, I do not seek to establish any causal nexus between the existence of different backgrounds and the existence of color changes, nor is it meant to say, that the animals possess this mechanism in order that they may voluntarily adapt themselves to this and that background, in nature notoriously green and brown. Such anthropomorphisms I leave to speculative philosophy. I only insist, that it would have been quite incomprehensible, if in the scheme of things, animals, that for quite other reasons possessed this mechanism, should not also possess the additional mechanism that would bring about reflexly adaptive color changes. That *Anolis* does possess this additional mechanism, is the purpose of this paper to show.

The particular technique used will be sufficiently described at the proper time and place, so that I will not have to enter into the matter at this time. A word or two ought be said about the criteria as to the effectiveness of this and that stimulus or factor, for it is evident from what has already been said, that if more than one condition is effective to bring about a given color change, it is quite necessary to know that in this and that case the effective stimulus has been one thing rather than another, especially is this so, since there is evidence from mere superficial observation, that there are involved in these color changes factors that are quite unknown, and which for that reason cannot be directly manipulated for experimental purposes.

In the first place, and with the exception of experiments made on spinal animals, all observations and conclusions are based on a great number of experiments, for any particular kind of experiment, on several hundred trials. So that the results are practically statistical. Then too, failure to react was never regarded negatively until after

a great number of trials it became evident that it so must be regarded. Whereever it became necessary to negative the influence of another factor, a control animal accompanied the test animal; finally a test was never considered conclusive if in the same day or portion thereof, the same results were not repeatedly obtained under given conditions; in other words, an observation is not meant to mean merely a single individual observation of a single color change, but a series of such changes obtained from the same animal under identical conditions within relative short periods of time, so to speak, within a sitting.

To determine the effects of background, three kinds of experiments were performed. The first was rather crude and simple, but singularly to the point and definite. I placed several animals on a fern quite accidentally for lack of a better place, and noticed that such animals were invariably green thruout the day. Later, I modified the experiment in that I hung green curtains and brown curtains of ordinary brown and green crepe paper to several similar and similarly situated windows of a large room, and hung to these windows thermometers. These always registered about the same temperature, ranging from about nineteen to twenty-three or twenty-four degrees Centigrade on different days with a variation of about one-half of a degree for the same day. Samples of this green and brown paper are given in the appendix, chart 1., from which it is at once evident that the green in question is much brighter than the brown, a fact which will be taken into consideration later in this chapter. These animals were all quite tame, and were simply picked up by their tails and carried from one curtain to another. In order to determine whether this "carrying act" was a factor, animals were frequently carried from one brown curtain to another similar brown curtain at another window (of which there were ten in the experimental room), or from one green curtain to another green curtain. In practically every case did such animals retain their green or brown color, showing that the color changes were not at all influenced by the manipulation. The four windows in question face the same direction, none were in any way shaded to the exclusion of others. If moisture content entered as a factor, it was a constant factor. The windows were all double glass storm windows, and drafts did not occur. The results of this group of experiments may be briefly and definitely

summarized by saying, that so far as these two particular colors were concerned, the green paper curtains practically invariably determined green skin coloration while the brown paper curtains likewise determined brown coloration.

The second series of experiments gave us quite as definite the more comprehensive results. Seven similar, indeed identical cardboard boxes, measuring about six by ten inches and an inch and a half deep were lined with different colored paper, the sides as well as the bottoms, and so lined, that creasing or improper folding or fastening permitted no part of the box to appear darker or lighter by reason of shadows cast by parts of the same box, or by the paper lining. These colors are given by sample in the appendix chart 2, and for purpose of reference I will call them white, green, yellow, red, blue, brown and black. Into each of these boxes two animals were placed, and all were then covered with a large pane of glass, so supported that a space of about a quarter of an inch intervened between the top edges of the boxes and the bottom plain of the glass thus affording proper as well as equal ventilation and aeration. Long strips of heavy cardboard were then so folded and fastened that they prevented the admission of light thru the space between the top of the table upon which the boxes were resting and the pane of glass on all four sides as well as thru the upper and outer borders of the glass itself, but never preventing its admission into a box or portion of a box from above. The table was located directly under a large skylight, and all experiments were carried on at room temperatures, artificially heated, and under quiet conditions.

The pane of glass was now covered with heavy cardboard, so excluding all light from the space under the glass, and it remained until it was thot that a l animals had turned green. Experiments were never made until all animals were seen to have turned green

on uncovering, and where certain animals persisted in a green or brown state, they were replaced by others that fulfilled the required conditions. It ought^{to} be said, that in all the experiments hereafter to be reported, and in others that will be reported later, I always encountered certain individuals, not always the same ones that tended to persist in a given green or brown state for varying lengths of time, however the conditions as to heat, and light were varied. Such animals needless to say were quite valueless. Then there were others that fulfilled the given conditions at the beginning of the experiment, but which later persisted in their status quo. Such animals too had to be replaced, and the results of experimentation with these were thrown out of consideration and ignored.

When therefore these conditions had been obtained, the large card-board covering was quickly removed, the animals observed for a half hour, and the cover again replaced to be again removed when it was that that all the animals were green, and so on thru-out the day until a number of consecutive tests had been obtained.

According to latter-day theory, on uncovering, all animals ought within relative short periods of time turn brown, since it was supposed, brown was their day-light color. I asked myself this question in connection with this experiment: on what colors do these animals not turn brown? or, is the change to brown in any way retarded by the fact that an animal was resting on this rather than that color?

In this connection, I ought^{to} make an objection to a remark of Carlton (8), especially since this remark, if valid, would negative all my experiments with back-ground. In working with the effects of illumination, Carlton noticed, that under what seemed to him perfectly uniform conditions, some animals would require at certain times a very much longer period of time to undergo a given color

change than at other times, and noticed further, that different individuals acted differently in this respect also, which variability he attributed to individual differences. He regarded three hours, for instance, as the required time for light stimuli to bring about color changes in certain animals, while in quite other individuals four minutes sufficed to make this same change. To attribute this great difference to individual variations is manifestly naive, for certainly the following analogy must be acceptable and show the absurdity of the point in question. If I stimulate a suspended decerebrate frog with a little dilute acid and get the notorious reflex, I am at liberty to say, provided I have repeated the experiment often enough, that the stimulation by the acid is the determining antecedent condition for the reflex. If the reflex however does not occur at once but three hours later, and if in addition, this happens comparatively seldom, I am not at liberty to assume that stimulation by the acid was the determining condition. I simply say, provided I am certain that the strength of the acid was sufficiently high, and that the sensitivity of the skin has not been lowered, that this reflex has been inhibited by another and unknown factor at the time of stimulation; and in the case of the color change in the skin of this individual animal, I add, that two and one half hours after exposure to light, or two and three-quarter hours afterwards, depending on thermal and other conditions, this inhibitive factor ceased to be effective, and gave way to light as the effective factor. This should be manifest to any one who has had any experience with physiological manipulations.

The results of these experiments may be grouped into the following classes;

- 1) In the great majority of these experiments, the animals on

black and brown back-grounds turned brown at once, those in the black box generally preceding those in the brown one by some fifteen to thirty seconds.

2) The animals on green, yellow and white, more particularly those on yellow and white, remained green thruout the whole time of observation.

3) The animals on the red and blue backgrounds, especially on the red, turned brown, requiring on the average from a half to one minute more than did those on the brown, and the shade of brown was never as dark as it was on the latter. The assumption of this color was quite generally followed by a quick return to the green, in fact by an oscillation between the brown and green state, until finally the animals became and remained brown.

4) Frequently one could see green animals on the brown and black backgrounds, and brown animals on the white green and yellow.

The third series of experiments were not as conclusive as were the first two series, for reasons that will presently be obvious. Four boxes of the same kind as previously described were covered in the same way as before with red, blue, yellow, and green paper; generally the whole manipulation of this experiment was similar to the above, except that hourly or half-hourly records were taken of the skin colors of the animals in the several boxes, this time one large and one small animal being placed in the same box in order that they might be told apart. For the following no very definite results were to be expected, nor were they obtained;

1) The illumination conditions did not remain constant, since only natural sources of light were used; hence the oncoming of dusk, and all other general variations of the illumination must have been operative.

2) These long intervals between observations gave greater opper-

tunity for unknown determinants to become operative and thus obscure the general effects of back-grounds.

3) Noises in the adjoining laboratories and corridors entered as factors.

But so much may be said with certainty,--- that the tendencies to remain green on green and yellow, and brown on blue and red are sufficiently pronounced beneath these obscuring factors to be almost self-evident, but only not as definite and convincing as were the observations in the first two experiments. I append such records, typical of the various records obtained, in the appendix, charts 3 and 4.

IV.

The credit for the little piece of apparatus used in the following experiments, in so far as it was not modified by me belongs to Carlton, who first used it and reported his results with it (8). It consisted of a small box with a circular aperture in ^{one} of its lateral faces, into which an animal was placed, now with its body wholly within the box and its head protruding thru the aperture, then with its head enclosed in the box, and its body protruding. A black neck-band was placed and fastened around the neck, to make the inside of the box, when the lid and the animal were in place, as light-proof as possible. His control animals, which were always placed entirely within the box, showed that this condition had been obtained to a sufficiently high degree. This small box with its little occupant was then placed within a larger one which could be so opened and closed that the inner small box could be either fully exposed to the light or shielded from it. The apparatus was thus designed to expose either the head end or the body end of the animal, never the whole animal. Carlton's results agree perfectly with mine, in so far as illumination is concerned, hence it will not be necessary to state them here. I reviewed them for two reasons, namely, because Parker and Starret reported inconstant results in similar experiments (9) and thus throw doubt on a condition that seems to be peculiar to *Anolis*; and because in my modification, I extended this principle to three segments of the body, and was thru this able to show that the influence of back-ground was a visual factor, one which both Carlton and Parker and Starret did not take into account in their work with *Anolis*.

My small box was a cigar-box of the ordinary deep type, fitted up with a double floor about one and a half inches from the lid surface. This floor however did not extend thruout the whole length of the box, but stopped short about a half inch from the two end faces. Into the lateral faces, about at their middle, several small holes were bored between the floors, the whole arrangement merely contributing to the proper ventilation of that part of the box above the new floor. About one fourth of the way down in the upper space a transverse partition was fitted into two slots in the lateral faces. This partition was provided with two semi-oval indentations, so made, that when the partitions ~~were~~ fitted in to the slots, and lowered, they would fit over the necks of two animals resting on the top floor of the box. A similar partition fitted over the backs of the animals, just in front of their hind limbs. Finally, longitudinal partitions isolated the animals from each other. All these partitions rested upon strips of black velvet, similar neck-bands were placed around the necks of the animals to insure as nearly as possible absolute light-proof conditions in such of the three compartments where this condition was a desideratum. So far as the tops of these compartments were concerned, this condition was simply accomplished by covering such compartment or compartments with a double layer of black velvet, so made that it would fit neatly over the edges of the box and of the partitions, and to insure that no light escaped thru or between the velvet and these edges, suitable weights were placed over the velvet.

The two longitudinal halves of the box, each with its three compartments, which we shall hereafter know as the 'head', 'body', and 'tail' sections were absolutely identical, so that so far as the two pilloried animals were concerned, any factor, normal or

abnormal, could certainly be presumed to act on both equally. Any difference of behavior, consistently manifested, could therefore be ascribed to the varying conditions of the experiment.

It may be objected that this arrangement tended to bring in other abnormal conditions by virtue of the constrained positions of the imprisoned animals. Indeed it did so, tho it was to the decided advantage of the method rather than to its disadvantage, in that it tended to put the animals after a very short residence into a sort of hypnoidal state, in which, whatever there was of reflex type in these color changes, could certainly manifest itself more definitely than in the free state, where the animals would be constantly subjected to divers accidental environmental conditions of such slight character, that they would not very lend themselves to regulation. If the animals struggled in the beginning to free themselves from their fetters, within a short time they became perfectly quiet; moderate noises were unnoticed by them, at least in not producing movements; the acts of covering and uncovering the various compartments elicited no excitability, tho the animals were apparently awake, having their eyes open. What is indeed more striking, the removal of all the partitions and their absolute liberation, attended by slight noises and jars, left the animals perfectly quiescent in their original positions, it generally requiring a vigorous jab in the sides to arouse them.

With this arrangement then, any third or any two thirds, or the whole animal could be exposed to the illumination while the rest of the animal was in comparative darkness. The experiment was generally carried out in two ways; 1) all thirds of the animals were covered until they had become green, and then any third or any two thirds exposed, or 2) the animals were simply left with any third or two thirds covered and observed from time to time.

To determine whether the mere act of covering or uncovering might act as a probable stimulus to color changes, one of the animals was from time to time called to act as control, in fact both animals were uncovered (any third) and the control at once covered again, or if it was a question of covering rather than uncovering, one animal was covered with black velvet as usual, and the other with a piece of glass. In either case the control animal ought not to change, while the test animal would undergo a change. Experiment so found it to be. The control never changed, the test animal always did. To determine the degree of darkness of the several compartments when covered, control animals were simultaneously placed into the unexposed sections with the imprisoned animals in their usual position, and their behavior noted. Experiment indicated that these controls never responded in the usual manner to stimulation by light.

The observations extended over a month, during which trials and retrials were made, and with but few exceptions, they indicate definitely, that as far as delimitation into thirds is concerned, the exposure of any third or of any two thirds to illumination, determines, so far as this depends upon illumination, the color of the whole animal, and not only of the exposed parts. Further if the change from green into brown is interrupted at any stage, my observations show, that the coloration takes place uniformly over the whole animal, and does not reach its completion any sooner in the exposed region than in the unexposed ones. Moreover, the time rate of change, so far as I was able to determine from time measurements seem to indicate practical equivalency for all thirds.

These results have considerable theoretical importance if they be admitted. For, if exposure of the tail section determines brown coloration over the whole body, and further if the process of coloration goes on uniformly instead of radiating from the exposed parts gradually to the unexposed parts, it is presumptive evidence that these colorations and decolorations are physiologically spinal reflexes whose ascending and descending fibres pass either thru the cord or thru the sympathetic chain. On no other basis seems this behavior explicable.

A second series of experiments and observations were made with with the box, in that the head sections of one side was covered with green or white paper, while that of the other side was covered with brown or black paper. All three sections of both sides were then covered in the usual manner, until it was thot that all animals had turned green. Then both head sections were quickly uncovered with the results, that the animals on the brown or black paper very quickly turned brown, (generally in about a minute and a half) while his mate on the green or white paper retained its green color. If the animals were interchanged, the same occured, that is, the brown or the black back-grounds determined brown coloration, the green and white determined green coloration. These observations extended over a period of fifteen days, and the results with few exceptions were always as stated. It goes without saying, that the coloration or decoloration was as before in toto.

A third series of experiments was undertaken, in that now the body and tail sections of both sided were faced with the colored papers in the above manner, instead of the head sections. The same manipulation was resorted to as above, only the posterior two thirds

were uncovered, with the definite and invariable result that both animals immediately turned brown in toto. That is, the difference of back-ground has no effect unless it affects the eyes.

V.

In previous chapters, it was said, that, on the basis of the experiments with the small box, this behavior was incomprehensible unless it was assumed, 1) that the reactions to the illumination were cutaneous spinal reflexes, 2) reaction to background, visual reactions, and finally, 3) that the ascending and descending fibres of both the cutaneous type and the visual type must either lie in the cord, or in the sympathetic system, or partly in both.

I now operated with animals whose spines, or spinal cords had been transected in the region about a half inch below the base of the skull, generally a little posteriorly to the cervical cord. The following observations are on the basis of a months experimentation upon an aggregate of six animals. That such animals lived and thrived need not of course be stated; I have been able to keep individuals in that state for two weeks at a time, and they could perhaps have been kept indefinitely with the proper precautions.

A second series of experiments with these same animals were made, except that in addition to the spinal transection, they were pithed from the point of transection posteriorly to a point just behind the lower limbs, so leaving the anterior part of the cord as well as the extreme posterior part intact with the intermediate part destroyed. I used great precaution in the matter of the destruction of the cord, selecting for that purpose a darning needle that would just fit into the cavity of the spine, and repeated the operation two or three times in order to insure as complete destruction as possible. Post mortem sections too showed that this purpose had been uniformly accomplished. Moreover,--- within the limits of the destroyed parts of the cord, one could obtain none of the usual reflexes that one could ordinarily obtain, which was presumptive evidence that such connect-

ions did no longer exist.

With both kinds of animals then, I operated in the usual manner, as already described to determine, 1) the influence of the illumination upon the color changes under these new conditions, and 2) the influence of the background. The observations may be stated and generalized as follows;

1) Transected animals respond normally to illumination.

2) Spinal animals respond normally and uniformly, in toto, to illumination.

3) Transected animals on a green background or any light background respond by remaining green, or turning so if previously brown. This response is quite definite, tho it must be said that frequent alternation from brown to green and back again to brown on proper change of background takes place progressively more slowly with time indicating a probable fatigue effect. In later changes too, this adaptive change is only partial in that on light backgrounds, the animals take on a lighter or brighter brown, while on dark backgrounds, they become darker, often blackish.

4) Spinal animals of the kind described more often failed to respond at all than in any of the other cases described; indeed, it was quite trying at times to fail to get responses from certain animals. But two of the six animals operated upon, made from two to four reciprocal changes, three times in the case of the first animal and twice in the case of the second, more generally however by but partial changes, in two cases by complete changes. So while the evidence is not at all conclusive, it indicated a high degree of probability. Indeed, the fact of not changing in response to background in these animals is not to be taken to mean that they can not do so, as might be gleaned from the large number of failures. But the fact that two animals did

add
make such reciprocal changes, seems to indicate that notwithstanding the transection and the destruction of the cord, these animals can nevertheless respond to the background by visual reflexes, which being accepted, lends support to the views of Carlton on the basis of his adrenaline injections as to the sympathetic nature of the color reactions, and to the assertions of Bert (12) that the cervical cord is probably the locus of the outlet of the pre-ganglionic fibres, which mediate the connections between the central nervous system and the sympathetic. The facts above pointed out make such assumption a necessity.

5) In all of these animals, and particularly in the pithed animals, all the color reactions were enormously prolonged, the change to brown being very gradual and requiring for its consumation^m as much as fifty times the ordinary time. In these cases, it was at all times very evident that the change was going on all the time, and that it was not of the nature of a delayed reaction. Indeed, if we grant that they can take place at all, we must under these new conditions expect that the times required for ^{completion of} the changes be long.

VI.

In as much as it has already been conclusively shown that the reactions to background were visual in character, the following observations were really superfluous, but they were undertaken more for the purpose of ⁿcorroboration, than to bring new evidence.

In all, four animals were blinded; they were etherized and the eyes grasped with forceps, pulled out and separated from the optic nerve. In two other animals the eyes were destroyed with red hot darning needles, which were plunged into the eye-cup. After several days of rest, and recovery, they were subjected to all the experimental conditions already described for normal animals, including the small box experiments, and the following conclusions substantiate the results obtained with normal animals;

- 1) Animals respond normally to illumination, and to its absence.
- 2) On any background, irrespective of color or brightness, animals respond definitely and practically invariably by turning brown, that is to say, since these backgrounds were illuminated, they responded to the illumination, but never to the background.

If these color reactions are nerve functions, I argued that they have a certain definite reaction time, or latent period. The determination of this time was attended with a great deal of difficulty. Carlton already had remarked and observed the great variability of these rates of color reactions in different animals, which he attributed to individual characteristics, or peculiarities, but this assumption is untenable unless one understands what is meant by the term 'individual characteristics'. If it is meant, that certain animals actually require four minutes and others three hours to make the same kind of change, under the further assumption that the illumination was an effective stimulus ab initio, in other words, if it is meant that in some animals the chromatophores take three minutes to reach the periphery with their appendages, and that others require three hours to do the same, then the term 'individual variation' or 'individual characteristics' is invalid and meaningless. But if we mean by it, that in some cases light and darkness are not always at once the effective stimuli for given color changes, but that they are at times antagonized by other and sometimes uncontrollable factors, then such a phrase has scientific validity.

If now these individual differences do exist, understanding this term as just described, even under the most constant conditions, if these color changes are nervous functions, and if finally it is desired to determine the reaction time, or the latent period of these color changes, it can only be obtained by weeding out so to speak all observations that on the face of them are suspicious. For instance, in looking for a reaction time for illumination, I do not admit that three minutes or five minutes, or an hour, or three hours indicate reaction times or latent periods that are in any sense

of the word subject to the arithmetic of averages; indeed, provided one chooses the animals with care, all other conditions being equal and favorable, it will soon become manifest, that the normal animal has a very definite reaction time, whose character is a good indicator of the kind of nervous activity involved in these color reactions.

Out of a total of eighteen animals, I was able to find but two that fulfilled completely the following conditions of the experiment. Other animals either did not always respond at once to changes in the illumination, or they did so less than ten times, generally they responded once or twice and then remained either brown or green and sometimes partly brown and partly green. The conditions were as follows;

- 1) Each animal gave me ten consecutive reactions to changes in the illumination within two days or less.

- 2) They responded at once to these changes.

The technique resorted to was as follows; an ordinary bacteriological constant temperature chamber was set up in a dark room. Into this a floor was set up about an inch and one half from the top glass cover and the latter was raised about a fourth of an inch from the lateral edges of the chamber to provide for ventilation. Over this glass cover at a height of one meter a 100 Watt lamp was hung, and directly under this light within the confines of the chamber, a space was partitioned off from the rest of the chamber in order that the confined animals might always be directly under the lamp, and in general under the same illumination conditions thruout the whole of the experiment. The partitions as well as the floor were covered with dark gray paper, this background being chosen because it would not antagonize the assumption of the brown state.

Around the whole apparatus a heavy curtain of black velvet was hung

so that the experimenter remained invisible to the animal under observation and that these might be seen by him, two small openings were made in the curtain. A thermometer with its bulb in the small area and itself within the chamber completed the arrangements.

The lamp was further provided with a large cardboard reflector, and was operated with a switch. In both of the experiments, the temperature was 25 degrees Centigrade, and while the lamp, when it was switched on, did cause a rise of never more than one half of a degree, this was unavoidable without resorting to more complicating apparatus, which, considering the small amount of the rise, its practical constancy, and the short interval during which it was operative, would have made conditions needlessly accurate.

The observations were made in the following manner. The lamp was switched on when it was thought that the animal under observation had turned green by reason of the absence of illumination and the stopwatch, which was habitually carried in a pocket and never regarded throughout the course of the experiment, started simultaneously with the switching on of the lamp. It was stopped on the very first appearance of brown coloration, the area of observation being the dorsal and lateral surfaces of the animal. In practice, after a latent period of short duration, this area suddenly blazed forth in a brown color, and it seemed to me, that of all the parts of the skin, those covering the dorsal ridge had the shortest latent period, which became progressively longer toward the abdomen, so that it seemed as if the brown coloration started here first and then proceeded in very very quick intervals laterally and inferiorly. To the uncritical eye, though, within the limits stated, the whole lateral and dorsal surfaces seemed to assume the brown color simultaneously, and very suddenly. I did not attempt to record times for complete changes to brown, nor for the reverse c

changes to green, because the subjective elements as to when a complete change had been reached were too great and too variable to make for any great degree of scientific accuracy.

As already stated, I found but two animals that met the conditions of the experiment. The others failed, because they either did not ~~on~~ change at all, or they changed at varying rates, some at the probably correct time, others at intervals of fifteen, or thirty, or sixty, or more minutes. Others again changed in the proper manner in the beginning and then permanently stayed green or brown. Others turned brown and then back again to green, within a twinkling of the eye, so to speak. And finally others remained brown, any change in illumination to the contrary notwithstanding.

The two animals that were finally selected were obtained, quite accidentally in the course of experimentation. Their reaction times are as follows;

Animal #1.					Animal # 2.				
Feb. 15	trial 1			42 sec.	Feb. 24, trial 1				36 sec.
" "	" 2			40 "	" " " 2				39 "
" "	" 3			41 "	" " " 3				43 "
" "	" 4			40 "	" " " 4				40 "
" 16	" 5			38 "	" " " 5				41 "
" "	" 6			38 "	" " " 6				38 "
" "	" 7			42 "	" " " 7				39 "
" "	" 8			41 "	" 25 " 8				38 "
" "	" 9			37 "	" " " 9				41 "
" "	" 10			41 "	" " " 10				40 "

Average				40 "					39.5 "

For these two animals, then it would appear that the latent period was about forty seconds. The figures indicate two things, first, that the latent periods are very definite and uniform in the same animal, and second, that they are about the same in different animals, a fact that is of course not at all remarkable, and which was to be expected. Moreover, compared with ordinary reflexes, and even such more complex ones as for instance that of the scratch reflex in the dog, the latent period of these color reactions ^{is} ~~are~~ enormously prolonged; and this extremely long latent period is one of the characteristics of all nerve activities that have to do with the sympathetic system, and with unstriated muscle.

The following conclusions are obtainable from what has preceded;

- 1) *Anolis Carolinensis* reacts to illumination by turning brown, to its absence, by turning green.
- 2) *Anolis* reacts to certain unknown factors by turning brown, and to others by turning green.
- 3) Animals turn brown in toto, in response to partial illumination, this transformation is uniform over the whole animal, it has a definite reaction time, or latent period, which facts seem to imply mediation by the nervous system, thru sensory end-organs in the skin.
- 4) Normal animals respond to background, apparently to brightnesses, white backgrounds, and backgrounds of medium wave length inhibiting the brown formation, while long and short wave lengths reinforce the reflex which makes for brown coloration.
- 5) This effect of background is a visual factor, is not mediated thru the skin, and where the head of the animal alone is exposed to the background, the brown or green color which it determines depending upon its character, is effective over the whole animal, no matter if the posterior parts of the animal are exposed to the illumination or not, or if they are exposed to backgrounds other than that to which the head part is exposed, or the same backgrounds. Blind animals do not react to background.
- 6) Animals with transected cords react normally and in toto to the illumination and to background.
- 7) Animals whose posterior cords have been destroyed respond to illumination, and in two cases, did also respond to background.
- 8) Bert's (10) and Carlton's views regarding the participation of the sympathetic nervous system in these color reactions seem to be substantiated on purely behavioral grounds.

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Chart 2.

CHART III.-IV.

COLOR OF BACKGROUND.	TIME OF DAY.					COLOR OF ANIMAL.
	11.30	1.30	2.30	3.00	7.00	
GREEN	G.	G.	G.	G.	G.	"
BROWN	B.	B.	B.	B.	G.	"
RED	B.	B.	B.	B.	G.	"
YELLOW.	G.	G.	G.	G.	G.	"

NOTE: Of a total of 28 records, fifteen are of the above type.

	1.00	2.30	4.30	7.00	
GREEN	G.	G.	G.	G.	"
"	G.	G.	G.	G.	"
BLUE	B.	B.	B.	G.	"
"	B.	B.	B.	G.	"
RED	G.*	B.	B.	G.	"
"	B.	G.*	G.*	G.	"
YELLOW	G.	G.	B.G.*	G.	"
"	B.G.*	B.G.*	B.*	G.	"

NOTE: A majority of the remaining records are of the above nature.

	2.00	2.00	3.00	3.30	4.15	5.30	7.00.	
GREEN	G.	G.B.*	G.B.	G.B?	G.	G.	G.	"
"	G.	G.	G.	G.	G.	B.*	G.	"
BLUE	G*	B.	G.B*	GB*	G*	G.	G.	"
"	B.	B.	B.	B.	B.	B.	G.	"
RED	G*	BG*	B.	B.	B.	GB*	G.	"
"	G*	B.	B.	B.	G.B*	GB*	G.	"
YELLOW	B*	B*	B*	B*	B*	B.*	G.	"
"	G.	G.	G.	G.	B*	G.	G.	"

NOTE: The remaining records are of this nature.

Legend: B stands for brown; G stands for Green; GB, for green-brown.
 * indicated abnormality as concerning background expectation.

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