*Indian Journal of History of Science*, **18** (*2*): 137—153 (1983)

# WHY DID GREGORY BATESON OVERLOOK SOME BASIC LAMARCKIAN TENETS ?

HARRY GERSHENOWITZ
Department of Life Sciences
Glassboro State College
Glassboro, New Jersey 08028
U.S.A.

*(Received 29 September 1980)*

This paper deals with the reasons why Gregory Bateson (1904-1980), noted anthropologist, ethnologist, psychologist and trained biologist, overlooked basic Lamarckian tenets in two of his major books, *Steps To An Ecology Of Mind* (1972) and *Mind And Nature* (1979). Gregory Bateson attended Cambridge University during the 1920's when the orthodox Darwinians and neo-Mendelians dominated and controlled most of the major professorships in the large British and American universities. The followers of Lamarck were fewer in number in these countries and less influential in the literature. During his student years, Bateson was influenced by those who espoused the theory of natural selection and Mendelian laws. Bateson was the son of the famous geneticist, William Bateson (1861-1926), who spent a number of his professional years attempting to discredit the works of the Austrian scientist, Paul Kammerer (1880-1926). Kammerer was one of the few Lamarckian biologists who still enjoyed a high level of confidence and popularity among his colleagues. Gregory was identified with his father's theories, which rejected Lamarckism and held that inheritance comes about through the successful reproduction of discontinuous variations, something currently labeled mutations. The father's views were strongly influential in shaping the views of the son in relation to evolutionary theory.

The long-term imbroglio that William Batseson entered into with Paul Kammerer dominated Gregory's attitude toward Lamarckian theories of use and disuse as well as the inheritance of acquired characteristics. There appears to be a peculiar anomaly in Gregory Bateson's work; outwardly, it seems that he carefully read the cited books of Jean Lamarck (1744-1829) and Arthur Koestler (1905-). However, he repeats commonplace errors made about Lamarck's four laws, errors one would not expect from a professional biologist. A detailed examination of the literature during the past forty years indicates that a small but persistent minority of respectable scientists throughout the world have been negating the "lucky gene" theory as one of the major causes or the major cause of evolution. Their literature is readily available in libraries of large universities.

The over-identification with his Edwardian father, a successful scientist, and the desire to be accepted as son and peer pressured Gregory Bateson to be somewhat loose with Lamarckian facts, data which have been supported in the recent past by highly respected scientists such as Frederic Wood Jones, Stanley D. Porteus, Ernest Everett Just, Enrique Beltran and Herbert Graham Cannon.

Gregory Bateson (1904-1980), anthropologist, ethnologist, psychologist, proved himself to be a prominent student of evolutionary theory through two of his major works, *Steps To An Ecology Of Mind*[1] (1972) and *Mind And Nature*[2] (1979). In them, he attacked Lamarckian biological theories, but at the same time apparently made the mistake of overlooking some of its fundamental tenets. Bateson followed a course towards the Lamarckians somewhat akin to the Roman General Fabius (Second Punic War, third century B.C.), wearing out his opposition without engaging in a definitive battle. This paper will examine the reasons why Bateson followed this course.

Gregory Bateson earned his B.A. in Natural Science in 1925 and M.A. in Anthropology in 1930 from Cambridge University. In the pre-World War II period, he practiced anthropological fieldwork in Bali and New Guinea. In 1946-47, he served as Visiting Professor at the New School for Social Research in New York City, and in the following year, as Visiting Professor at Harvard University. In the period 1948-1971, Bateson conducted research in the fields of psychology and animal behaviour at various institutions. From 1972 until his death, he was Visiting Professor at the University of California at Santa Cruz, California.

One of the obvious reasons why Bateson held anti-Lamarckian views was that he subscribed to a standard bias among most Western biologists. The *odium theologicum* between Darwinian and Lamarckian interpreters of evolutionary theory became more pronounced after the Darwinian Centennial meetings. Darwin's differences with Lamarckism were like the proverbial Samian Letter of Pythagoras (sixth century B.C.); the more Darwin diverged from Lamarck's view, the greater grew the irreconcilable gap. Darwin's letter to his co-discoverer of the Theory of Natural Selection, Alfred Russel Wallace (1823-1913), states Wallace's and his common bond against Lamarckism. Darwin recorded:

> Down, Bromley, Kent, S.E. April 14, 1869.
>
> My dear Wallace, —I have been wonderfully interested by your article, and I should think Lyell will be much gratified by it. I declare if I had been editor and had the power of directing you I should have selected for discussion the very points which you have chosen. I have often said to younger geologists (for I began in the year 1830) that they did not know what a revolution Lyell had effected; nevertheless, your extracts from Cuvier have quite astonished me.
>
> Though not able really to judge, I am inclined to put more confidence in Croll than you seem to do; but I have been much struck by many of your remarks on degradation.
>
> Thomson's views of the recent age of the world have been for some time one of my sorest troubles, and so I have been glad to read what you say. Your exposition of Natural Selection seems to me inimitably good; there never lived a better expounder than you.
>
> I was also much pleased at your discussing the difference between our views and Lamarck's. One sometimes sees the odious experssion, "Justice to myself compels me to say, etc.," but you are the only man I ever heard of who persistently does himself an injustice and never demands justice. Indeed, you ought

in the review to have alluded to your paper in the Linnean Journal, and I feel sure all our friends will agree in this, but you cannot "Burke" yourself, however much you may try, as may be seen in half the articles which appear.

I was asked but the other day by a German professor for your paper, which I sent him. Altogether, I look at your article as appearing in the Quarterly as an immense triumph for our cause. I presume that your remarks on Man are those to which you alluded in your note.

If you had not told me I should have thought that they had been added by someone else. As you expected, I differ grievously from you, and I am very sorry for it.

I can see no necessity for calling in an additional and proximate cause in regard to Man. But the subject is too long for a letter.

I have been particularly glad to read your discussion, because I am now writing and thinking much about Man.

I hope that your Malay book sells well. I was extermely pleased with the article in the Q. J. of Science, inasumch as it is thoroughly appreciative of your work. Alas! you will probably agree with what the writer says about the uses of the bamboo.

I hear that there is also a good article in the Saturday Review, but have heard nothing more about it. —Believe me, my dear Wallace, yours ever sincerely,

CH. DARWIN[3]

Wallace, in his enthusiasm to get the Darwinian nod of approval, wrote about the inheritance of modification due to use and disuse, "But no direct evidence of this has ever been found, while there is a good deal of evidence showing that it does not occur."[4] In an earlier work, *Darwinism*, he propounded, "The arguments of Lamarck did not, however, satisfy naturalists. . . ."[5] Wallace concluded his thoughts by declaring that "...Lamarck's views gained few converts; and although some of his arguments have been upheld in recent years, the fatal objections to his general principle as a means of explaining the evolution of organic forms has never been overcome."[6]

In the previously cited letter to Wallace, Darwin exerts his authority in a paternal fashion and maintains his suzerainty over any change to be made in the development of evolutionary theory. Darwin, the Mandarin of scientific thought of the nineteenth century, had sent his Letter of Uriah to Wallace (if only poor Wallace knew at the time that Darwin only sups with Darwin and none but Darwin can draw Darwin's bow).

History tells us of the long friendship between the Darwin and Bateson families, and this factor cannot be ignored in Gregory Bateson's acceptance of the Darwinian standpoint. In 1925, at the age of twenty-one, Bateson received his chance to travel and study life on the Galapagos Islands, but, unlike Darwin, he did not come up with any puzzling questions or leading ideas or evidence to shock the academic world with new hypotheses.[7]

Another reason for Bateson's treatment of Lamarck *de haut en bas* was that he was influenced by his father, William Bateson (1861-1926). The older Bateson was a Professor of Biology at Cambridge, 1908-09, and Director of the John Innes Horticultural Institution, Merton Park, Surrey, England, 1910-1926. In 1894, William was elected a Fellow of the Royal Society and was the first scientist to introduce the term. "Genetics" in his study of heredity and variation. The reintroduction of Mendel's work brought a fresh tool to Bateson in helping him to understand basic genetic action in the plant and animal kingdoms. His search led him to explain evolutionary changes in terms of genetic variations. He believed that unusual new characteristics account for variations.[8]

In the years following the Great War, William Bateson found it difficult to reconcile his theories of variation with those of his contemporaries who stressed the newly-discovered chromosome theory. He felt uncomfortable with the theory of the microscopists that the chromosome is the master of the cell nucleus and the factor which acted as the steward of heredity.[9] He advocated, not the slow influence of natural selection on continuous variation for evolutionary change, but of the great changes that caused permanent mutation.

William Bateson was one of the first to understand the natural process of development of the separation of allelomorphic characters and their connection to heredity.[10] He strongly believed in the origin of new varieties by the inheritance of successful discontinuous variations. These clearly discernible variations are more easily distinguihsed from members of the former species and presents fewer difficulties to study.[11] In 1894, in his well-grounded book, *Materials for the Study of Variation*,[12] he came to the conclusion that the Darwinian belief of continuous variation was not the major cause of evolutionary change.

At times, William Bateson taunted August Weismann (1834-1914) the German anti-Lamarckian biologist for believing in small continuous variations as the basis of evolution. One area where they did agree was in attacking Lamarck. Here Weismann had mutilated the tails of several generations of white mice to demonstrate that future generations would not inherit this characteristic. In 1905, Bateson reviewed (The Speaker, 24 June 1905) Weismann's two volume study, *The Evolutionary Theory* (1904). In an obstreperous and excessive manner for the suppression of Lamarckism he wrote:

> To Professor Weismann the gratitude of naturalists is ever due for two excellent services. He it was who first taught us to distinguish the "soma", or body, from the germ, thus ridding evolutionary science of the distracting belief that the experience of the organism is transmitted to its offspring. Formerly "use and disuse" were good enough answers to any troublesome conundrum of adaptation. Weismann's demand for evidence that in a single case such effects were transmitted brought this vague reasoning to an end. The inheritance of acquired characters was then seen to be an assumption needing independent proof, and, when proof was called for, there was no reply that a critical mind could accept as valid,[13]

Bateson in his paper, "Heredity And Variation In Modern Lights,"[14] written while he was at the University of Cambridge, judged that Weismann's challenge was also directed to those Darwinians who no longer believed in inheritance of acquired characteristics nor in the mutation theory to explain changes in the characteristics of populations of organisms. In the early 1900's some of Darwin's "Old Guard" were still alive and did not fully appreciate the new discipline of Genetics.

Bateson then took issue with the biometricians who followed the approach of Francis Galton (1822-1911); these biometricians applied various statistical tools for establishing the causes of individual differences. The proponents of biometrics established a journal, *Biometrika*, which declined publication of Bateson's promulgations because they were hostile to them. The lack of sportsmanship did not deter Bateson from delivering his views.

Hugo Iltis (1882-1952), Professor of Biology at Mary Washington College (1939-1952), a specialist on the life and work of the early geneticist, Gregor Mendel (1822-1884), in his scholarly biography, *Life Of Mendel*, informs us that "Galton's laws have only a general statistical validity, and as regards their applicability to individual cases they have been severely criticised by Bateson. . . ."[15] The school of biometricians seemed to decline after the death of its leader W. F. R. Weldon (1861-1906), and, in 1908, Edward Bagnall Poulton (1856-1943), Hope Professor of Zoology in the University of Oxford, stressed that "There is no dispute between Darwinians and Mutationists as to the germinal origin of variation and hereditary individual difference of every kind and degree. Darwinians hold that evolution has proceeded by small steps: Mutationists hold that it has advanced by large ones. That I believe to be the sole essential difference. . . ."[16]

The culminating point of William Bateson's professional career was his unabashed attack on the Austrian scientist, Paul Kammerer (1880-1926). Bateson, a Lamarckian, had travelled to Central Asia to find evidence, found none to confirm his premises, came home disheartened and disillusioned with the theory of the inheritance of acquired characteristics. His chance of becoming accepted as a peer to Charles Darwin was lost, and this turned him against his former belief. Dr. Paul Kammerer was an ideal target to vent his emotions upon, for Kammerer was at the height of his career, highly respected by the scientific community, youthful and strong, a rising star in the scientific fellowship.

In the Soviet Union he was being hailed as the new forerunner of socialistic science. Bateson, on the other hand, was no longer accepted by either the Lamarckians or the Darwinians, and in the field that he helped found he was considered antiquated by the new school of neo-Mendelians.

Kammerer, who did many Lamarckian experiments, conducted several studies on Alytes obstetricans (midwife toad) to induce recurrence in male descendants of

nuptial pads for reproducing in the water as has been formerly possessed by its ancestor many generations before. Kammerer did not consider this particular series of investigations as being exemplary for demonstrating the Lamarckian principle of the inheritance of acquired characteristics. Bateson made it the most important work of his life to expose the purported claim made by Kammerer that he succeeded in reverting specimens of a current generation to its remote ancestral type.

In 1923, in *Nature*, William Bateson published his paper, "Dr Kammerer's Alytes," in which he provided critical notes: "Those who have followed the discussion of Dr. Kammerer's claims will be aware that special interest has centred on the question whether he could produce for examination males of Alytes showing the modification alleged to occur in consequence of his treatment."[17] He continued: "Up to 1919 nothing but vague diagrams....had been offered us to show what these new organs looked like, and no detailed description had appeared."[18] Reginald Crundall Punnett (1875-1967), friend and scientific collaborator (1904-1910), of William Bateson refers investigators of this disputation to see *Nature*, CXVIII, 1926, pp. 200, 264, 578, 661.[19]

Conrad Hal Waddington (1905-1975), F. R. S., Professor of Animal Genetics, University of Edinburgh, believed that Bateson's actions toward Kammerer were significant in ruining Kammerer's scientific career. Bateson, a modern Cincinnatus, ex magister, became the acknowledged authority on Kammerer's experiments. Waddington remarked sorrowfully that "By the 1920s, when Kammerer visited London and exhibited his evidence, William Bateson, the main champion of genetics and the inventor of the word, declared it anathema, and almost openly expressed the opinion that Kammerer was a charlatan."[20] In his seminal work, *The Nature Of Life*, he concluded that"....at least behaviour (biologists tend to be very timid about mentioning the mind), is a factor of importance in evolution. Lamarck's insistence on the 'Will' is not wholly unjustified."[21]

In 1926, shortly after the death of Bateson, Dr. G. K. Noble (1894-1940), of the American Museum of Natural History, examined the sole specimen of Alytes in Vienna and agreed with Bateson's diagnosis. He then reported that the nuptial pads were in reality a slight elevation due to an injection of India ink. This account was published in the highly recognized scientific journal, *Nature*. Two months later, for various reasons, Kammerer *felo-de-se*.

Why did Gregory Bateson overlook some basic Lamarckian tenets? David Lipset (1951- ), Bateson's biographer, acquaints us with the fact that William "....expressed fatherhood through science and taught his children as he did his students. There was no distinction drawn between science and family."[22] This type of field preparation reflected itself in Gregory's early anthropological practice. Margaret Mead (1901-1978), the noted anthropologist who worked with Gregory in New Guinea, observed that "He had a naturalist's training in attending to ongoing reality, instead of forcing nature in the laboratory to give limited answers to limited questions,"[23]

In the chapter, "The Role of Somatic Change in Evolution," in *Steps To An Ecology Of Mind*, Gregory defended Weismannism by asserting, "August Weismann's argument. . . . still stands. There is no reason to believe that either somatic change or changes in environment can, in principle, call (by physiological communication) for appropriate genotypic change. Indeed, the little that we know about communication within the multicellular individual indicates that such communication from soma to gene script is likely to be rare and unlikely to be adaptive in effect."[24]

Notwithstanding, as early as 1928, Frederic Wood Jones (1879-1954), Professor of Physical Anthropology at the University of Hawaii and his colleague, Stanley D. Porteus (1883- ), Professor of Clinic Psychology at the same university, explained in their theory of the germ plasm that it was possible for sudden brief outbursts of new forms to occur. In the same description they found flaws in the Weismannian mutilation experiments. In a clear, tersely worded style, they wrote:

> The weight if scientific opinion leans towards the view that advances in development are due to mutations or variations, that is to say, a variability of structure in the germ cells which if advantageous to the animal, is fixed and transmitted in the descendants. But we must remember that the improved structure is the end result and that it must be built up in the individual by the ordinary life processes of metabolism, the material being gained in the ordinary way from the animal's environment. The advantageous mutation takes the form then of a potentiality for adaptation to environment. The adaptation itself is not present but is merely represented in the germ plasm. Now we have seen that through what we have called biological memory the animal's reactions have become set or stereotyped. The only way in which a new adaptation can be made, or in other words behaviour can change, is by a more plastic condition of the germ plasm so that to its already present capacity for building up a certain structure with its associated behaviour patterns, some new potentiality is added. When this plasticity, this adaptability is so increased as to make sufficiently marked differences in structure and behaviour then a new species is recognized. But if it is degree of adaptability or plasticity that is subject to mutation or occasional variation why should not some germ plasm be so plastic as to be affected by the experience of the individual in whose body it is housed and which forms for the time being its environment? If this were possible then it would be easier to account for the building up of the immensely complicated behaviour patterns of the higher animals by small cumulative gains attributable to the life experience of the individual.
>
> The answer that the geneticist gives to the query regarding this possible transmission of acquired characteristics is that it is not possible because in his experience it does not occur. But before we accept this dictum we must stop and realize that this increased plasticity of the germ plasm, this potentiality for new adaptations in the behaviour of the structurally complete or mature descendant, must surely occur very rarely, or else we should be confronted with a bewildering riot of new species. It seems as though at times in the history of a phylum there has been a period of variable plasticity resulting in a flare-up of new forms, but this may occur more readily when the species is young and not biologically set in its ways.

> It may be necessary to remind the geneticist that he has had but a very brief time and a very few generations of animals with which to carry on his experiments whilst Nature has had many, many aeons of years and illimitable organisms with which to work. Thirty generations of mice whose tails have been cut off are surely inconclusive evidence that the plasticity of germ plasm cannot be affectred by individual experience. At the same time it must be admitted that the theory of inheritance by use has at present meagre support among biologists.[25]

The most amazing phase of Bateson's academic development was his treatment of Lamarck's biological laws. In 1923, the Cambridge Natural History Society and the University's Biological Tea Club of which Gregory was a member invited Paul Kammerer to the University in order to demonstrate his specimen of Alytes obstetricans. Kammerer claimed this specimen possessed the atavistic trait of nuptial pads. As a gifted undergraduate student, Gregory must have read and/or heard that this demontsration was to establish a case for reversion to an ancestral form. Yet, in *Mind And Nature*, he presented a hypothetical debate between a Lamarckian and a modern biologist concerning the possibility of the inheritance of acquired characteristics. The supposed controversy goes as follows:

> BIOLOGIST: What exactly is claimed by Lamarckian theory? What do you mean by "the inheritance of acquired characteristics"?
> LAMARCKIAN: That a change in the body induced by environment will be passed on to the offspring.
> BIOLOGIST: Wait a minute, a "change" is to be passed on? What exactly is to be passed from parent to offspring? A "change" is some sort of abstraction, I suppose.
> LAMARCKIAN: An effect of environment, for example, the nuptial pads of the male midwife toad.*
> BIOLOGIST: I still don't understand. You surely do not mean that the environment made the nuptial pads.
> LAMARCKIAN: No, of course not. The toad made them.
> BIOLOGIST: Ah, so the toad knew in some sense or had the "potentiality" for growing nuptial pads?
> LAMARCKIAN: Something like that, yes. The toad could make nuptial pads when forced to breed in water.
> BIOLOGIST: Ah, he could adapt himself. Is that right? If he bred on land, in the way normal to his species of toad, he made no nuptial pads. If in water, then he made pads just like all the other sorts of toad. He had an option.
> LAMARCKIAN: But some of the descendants of the toad who made pads in water made pads even on land. That's what I mean by the inheritance of acquired characters.
>
> *Most species of toads mate in water, and during the mating period, the male clasps the female with his arms from a position on ber back. Perhaps "because" she is slippery, he has roughtened black pads on the dorsal sides cf his hands in this season. In contrast, the midwife toad mates on land and has no such nuptial pads. In the years before World War I, Paul Kammerer, an Austrian scientist, claimed to have demonstrated the famous inheritance of acquired characters by forcing midwife toads to mate in water. Under these circumstances, the male developed nuptial pads. It was claimed that descendants of the male developed such pads, even on land.[26]

> BIOLOGIST: Ah, yes, I see. What was passed on was the loss of an option. The descendants could no longer breed normally on land. That's fascinating.
> LAMARCKIAN: You are willfully failing to understand.
> BIOLOGIST: Perhaps. But I still do not understand what is supposedly "passed on" or "inherited". The claimed empirical fact is that the descendants differed from the parent in lacking an option which the parent had. But this is not the passing on of a resemblance, which the word inheritance would suggest. It is the passing on of a difference. But the "difference" was not there to be passed on. The parent toad, as I understand it, still had his options in good shape.

Compounding Gregory Bateson's error concerning Kammerer's experiments is Professor Gerald James Stine (1935- ), Department of Natural Sciences, University of North Florida, who in his recent book, *Biosocial Genetics* (1977) states:

> In 1918, Paul Kammerer, an Austrain experimental biologist, conducted experiments on the development of the nuptial pad of the midwife toad (Alytes obstetricans) that gave support to the Lamarckian argument that hereditary traits could be acquired from the environment. Even though his work was declared a fraud by Bateson, who himself once believed in Lamarckism, and Nobel in 1926, Kammerer was invited by the Soviet Academy to move his laboratory equipment and continue his Lamarckian experiments in the Soviet Union. It would appear that the Russian political structure even then would influence or dictate the manner and method of genetics that would be pursued in the Soviet Union (Koestler, 1972).[27]

These two sources (Bateson, Stine) of ambiguity make student clarification on Lamarckian principles nearly impossible.

Arthur Koestler (1905- ), historian of science, in his book, *The Case Of The Midwife Toad* (1971), recorded the manner in which Kammerer viewed these specific experiments. Koestler points out that "....Kammerer commented: 'As the atavism objection can always be raised, it is not very clear to me why just this experiment (with Alytes) is so often looked upon as an *experimentum crucis. In my opinion it is by no means a conclusive proof of the inheritance of acquired characters*' (my italics)."[28]

Kammerer had clearly informed European scientific circles that it was not his decisive experiment which proves the inheritance of acquired characteristics but his detailed studies with Ciona intestinalis (Phylum, Chordata, Subphylum B, Tunicata, Class 2, Ascidiacea, Order 1 Enterogona) was for him more exemplifying modifications of structure or function caused by environmental factors.

This writer again returns to the question of why Gregory Bateson overlooked this elementary subject matter when dealing with the principles of Lamarckism. Forty-seven years after Bateson met Kammerer, he replied to an inquiry from Koestler regarding the relationship between the Batesons and the Lamarckian scientist. It is possible that Gregory Bateson replied through "screen memories". He presented his comments about Kammerer's dislike for William Bateson. These early encounters with Kammerer were deposited in Gregory's remembrance during a period when he

may not have fully comprehended the meaning of these meetings. These residual retentions pertaining to Kammerer and Lamarckism are expressed in Gregory Bateson's recent books. In any case, Gregory Bateson seemed to have had a Janus complex. On the one hand, like the Greek goddess Mnemosyne, he remembered Kammerer positively for his personality. On the other hand, he appeared to have drunk from the River Lethe and forgot Kammerer's Lamarckian tenets.

The famous Professor of Zoology and Comparative Anatomy at the University of Vienna, Carl Friedrich Wilhelm Claus (1835-1899), distinguished himself among contemporaries through his authoritative work, *Lamarck As Founder Of The Theory Of Descent* (1888). How was it possible that Gregory Bateson overlooked this prominent work written by a scientist considered by his peers to be one of the most informed scholars on Lamarckian thought? It was Claus who introduced Lamarckian theory to the young medical student, Sigmund Freud (1856-1939).[29] The founder of the psychoanalytic movement, Freud utilized Lamarckian themes as a foundation for his subsequent investigation of human behaviour.

Claus, returning to Lamarck's biological application of the use and disuse theory, wrote: "Lamarck had acquainted himself with an astonishingly large number of animal and plant species, and it had not escaped him that the individual members, by changing their environment, under the influence of changed feeding requirements with regard to their body proportions, underwent many changes in their physical appearance and organization."[30] (Translated by H. Gershenowitz).

Astonishing, fifty-seven years later in 1945, Enrique Beltran (1903- ), Professor De Zoologia En La Universidad De Mexico, the internationally recognized Hispanic Philosopher and Zoologist, in his book, *Lamarck-Interprete De La Naturaleza*, conveyed Lamarck's thoughts to the Spanish speaking world that it was the "new needs" that caused the use and disuse of organs. Beltran's study was very similar to those of Claus. Beltran puts forth for consideration:

> En efecto, para Lamarck, como ya lo hemos visto, y haciendo caso omiso de lo qué suponia én los vegetales, los animales no cambiaban por la influencia directa del medio, sino solo porque éste provocaba en ellos nuevas "necesidades", la tendencia a satisfacer las cuaes era la cautes directa para el desenvolvimiento o atrofia de los órganos, por el uso o desuso de los mismos.....
>
> Lamarck, como ya hemos visto, postulabia en sus leyes fundamentales una continua influencia de los cambios del medio sobre los animales, provocando en ellos nuevas necesidades, que, al tratar de satisfacerse, determinaban alteraciones en las funciones de los órganos, originando el desarrollo de los mismos cuando habia aumento en tales funciones o su atrofia cuando habia disminucion. De esta manera iban transformándose los individuos, y al transmitir tales caracteres a sus descendientes, por medio de la generacion, daban los materiales para la lenta transformación de las especies.[31]

It appears that some of those who attacked biological Lamarckism at Cambridge University may have been less than keen observers. In *Steps To An Ecology Of Mind,*

Bateson replays the banal subject of the pregiraffe and giraffe development in order to disprove the theory of use and disuse. He stated in this book, "A hypothetical pregiraffe, which had the luck to carry a mutant gene 'long neck', would have to adjust to this change by complex modifications of the heart and circulatory system....If the hypothetical pregiraffes carrying the mutant gene 'long neck' could also get the gene 'big heart', their hearts would be enlarged without the necessity of using the homeostatic pathways of the body to achieve and maintain this enlargement."[32]

The reader will find in Lamarck's *Zoological Philosophy* that Lamarck actually presented different causes for the growth of early prototypes of the modern giraffe during remote times. Lamarck summarized the matter:

> It is interesting to observe the result of habit in the peculiar shape and size of the giraffe (Camelo-pardalis): this animal, the largest of the mammals, is known to live in the interior of Africa in places where the soil is nearly always arid and barren, so that it is obliged to browse on the leaves of trees and to make constant efforts to reach them. From this habit long maintained in all its race, it has resulted that the animal's fore-legs have become longer than its hind legs, and that its neck is lengthened to such a degree that the giraffe, without standing up on its hind legs, attains a height of six metres (nearly 20 feet).[33]

As early as 1940, Ernest Everett Just (1883-1941), Professor of Zoology at Howard University, questioned the worth of the "lucky gene" theory. In his invaluable paper, "Unsolved Problems Of General Biology," Just was very critical of accepting the gene theory as the determiner for all forms of life. He stated the following reasons: "....consider the concept of the gene. Many a sufferer of typhoid fever or of another infectious bacterial disease knows, to his discomfort, that bacteria hand on their heritage. Most bacteria possess no chromosomes. The gene theory does not hold for such organisms. Indeed, the theory may or may not hold for organisms containing nuclei and chromosomes....The gene theory, however much it pretends to be mechanistic, is only crudely vitalistic...."[34]

It makes the writer wonder if Gregory Bateson believed and advocated the return to vitalism. Historians of science have long known that Lamarck was one of the first of modern biologists to maintain the separation of evolution of life's processes from vitalistic forces.

One of the greatest analysts of the Lamarckian-Darwinian dispute was Herbert Graham Cannon (1897-1963), Professor of Zoology and Director of Zoological Laboratory, Manchester University (1931-1963). He became a Fellow of the Royal Society at the age of thirty-eight. In his book, *Lamarck And Modern Genetics*, Cannon searches for some form of sound reasoning as to why well-trained scientists believe in the Weismannian myth of the separation and noninfluence of the somatoplasm from and on the germ plasm. Cannon believed that "Clearly every organ in the body must have its own blood supply, so that every organ is continuous with every other part of the body."[35] In the case of Weismann's maiming a number of generations of mice in order

to substantiate Darwinism as a psychological-philosophical practical system, caused Cannon to decry these heraldic results as contra-factual fantasy. Again to quote Cannon directly: "However, Weismann's ideas produced a profound effect. More particularly they lead to an absurd spate of experimentation —experiments to prove (or more often disprove) the possibility of the inheritance of acquired characters. They were commenced by Weismann himself, who cut the tails off mice as soon as they were born for generation after generation and showed that the experiments did not produce a Manx mouse. Of all the idiotic experiments —as Giard, a staunch supporter of Lamarckian tradition, is reputed to have said —one might just as well study the inheritance of a wooden leg!"[36]

In an earlier paper, "What Lamarck Really Said," Cannon states Lamarck's Second Law:

> The production of a new organ in an animal body results from a new need (besoin) which continues to make itself felt, and from a new movement that this need brings about and maintains.[37]

New needs are derived from the biological drive to produce an existing state of anatomical-physiological equilibrium within the species and cannot be induced by crippling some necessary part of the body.

In his masterpeice, *The Evolution Of Living Things*, Cannon takes hold of and solves the derivation of the erroneous ideas dealing with the evolution of the giraffe. Unfortunately, Gregory Basteson repeated these errors in his research. According to Cannon:

> I will finish . . by discussing the famous case of the giraffe's neck, first because it illuminates the differences between Darwin's and Lamarck's points of view but also because Lamarck's views have been, without exception as far as I can find out, entirely misrepresented. I have already discussed the inadequacy of Darwin's views where he stated that 'those individuals which had someone part or several parts of their bodies rather more elongated than usual would generally have survived'. But now compare this with what is attributed to Lamarck and there is little to choose between the two ideas for their stupidity. It was Wallace who made the first misstatement. He said that, according to Lamarck, the giraffe acquired its long neck by desiring (sic) to browse on the higher trees, and from that day to this it has been the evolution of the ncek of the giraffe which has been held up to ridicule. What Lamarck actually said was that the animals continually strove or exerted themselves to reach higher leaves on the trees and it is reasonable to interpret this to mean that the animals developed the habit of continually reaching upwards by pushing themselves up on their front legs and as a result these became elongated. It cannot be denied that the continued extra pressure put on the ends of the long bones of the front legs might stimulate those bones to elongate. But now apply the same argument to the neck. It has been said by all authorities that, according to Lamarck, the animal strove to elongate its neck and so it elongated. But the fallacy is that neither a giraffe nor any other vertebrate can strive to stretch its neck. A neck can be stretched out but it cannot be stretched! The act of stretching out a neck might in fact by pressure on the intervertebral elements

> slightly diminish the total length of the neck, but it cannot increase it. It is morphologically impossible. Let any of my readers try to will themselves to elongate their necks and they will see how absurd the idea is. It cannot be done, simply because there is no muscular system by which such a process could be carried out and hence no corresponding nervous mechanism. It is the absurdity of the idea that has been one of the chief sources of ridicule for those who disbelieved in Lamarck. What he actually said was that the front legs elongated in the way I have described and the neck elongated accordingly. There was a need of a long neck to correspond to the long legs as I have already explained, in order to allow the animal to drink and incidentally to browse on grass which is its main food. There being that need then according to Lamarck's second law, that need was satisfied.[38]

Finally, Cannon emphatically declares in his paper, "Is The Problem Of Evolution Solved?," the resolution of the one hundred year debate with the following words: "There is no more mystery in the suggestion that the body cells should affect the germ cells, which is the basis of Lamarckism, than that the germ cells should control the development of the soma, which is the basis of Mendelism."[39] Cannon accepts that there is enough evidence for trivial patterns to be inherited. He challenges the neo-Mendelians to prove experimentally that genes preceded and determined the origin of major organs in the vertebrates such as the heart. He continues to challenge the neo-Mendelians to pinpoint the time in history when genetic change(s) developed large hearts in giraffes in order to accomodate the need for blood pumping necessary to develop long necks. Can the neo-Mendelians create a time tunnel and bring into being a period when protovertebrates were developing into vertebrates? Therefore, Cannon advocated that current scientists should recognize and accept that trivial genetic differences are caused by the process of natural selection by way of Mendelian laws. However, it still stands that the anatomical and physiological structures and functions of large organs had to originate by responding to the "inner needs" of the evolving species.

As a learned man, Gregory Bateson acknowledged some of the above opinions and scientific convictions and did concede that it is possible for some substance (ribonucleic acid) in the somatoplasm of the biceps to engram its new characteristics into the germ plasm. These specific characteristics will then be eligible to be passed on to future generations.[40]

Many scientists who derive new hypotheses are often personally identified with the theme that provides them with a basis for further investigation. In the field of psychology, Gregory Bateson has been recognized as the originator of the "double bind" hypothesis which attempts to explain one of the causes of the pathological conditions known as schizophrenia. This hypothesis accounts for a type of mental disorder which is characterized by indifference, withdrawal, hallucinations, and delusions of persecution and omnipotence suffered by the individual receiving conflicting communications of love and rejection from his/her parent(s). For example, Gregory Bateson held in high esteem his father's scholarly status in the fields of

genetics and evolutionary theory. Bateson was an initiator of a hypothesis strongly advocating the avoidance of conflict between love and hate. In his professional life, he shunned any conflict with his father's long-standing professional reputation by not contradicting his father's undeniably mistaken interpretations of Lamarckian theory. William Bateson demonstrated a single-mindedness and did accept that some acquired characteristics can be inherited. Thus, Gregory Bateson maintained the love nad respect of traditional historians of science by being a loyal follower of neo-Darwinism-neo-Mendelism as well as Batesonism, thereby eschewing rejection from his father's admirers. Perhaps, Gregory Bateson thought of the philosophical wisdom of Pythagoras who said, "*Non ex quovis ligno Mercurius fit.*"[41] (You cannot make a Mercury of every log.) This statement implies that "Not every mind will answer equally well to be trained into a scholar."[42] Gregory Bateson was determined not to be labeled by William Bateson's followers as the mercurial son.

## Conclusion

This is a study of Gregory Bateson as an example of a disposition toward Jean Lamarck, the biologist and evolutionist, as in some way representative of attitudes which the mainstream of current western biologists have felt toward Lamarck and his theories. In the case of Bateson's attitude in his student days, he displayed some openness and saw the Lamarckian view in a balanced way. However, he soon became influenced by the general trend of biological thinking that was diametrically anti-Lamarckian. This seems very strange to this writer because even if Lamarck did err in some of his postulates, one must consider the characteristics and distinguishing circumstances of the historical period. Surely Lamarck's thoughts concerning gradual changes in evolving life were more accurate and advanced than the existing teleological explanation. In an in-depth study of the works of Gregory Bateson, the traditional anti-Lamarckian course which was crystallized by Charles Darwin shows itself in various offered examples: Darwin's analytical studies displayed simultaneous conflicting feelings in utilizing and accepting the Lamarckian theory of the inheritance of acquired characteristics, although he put to use this theory in his major writings. Perhaps, Darwin's personality required that he become the new father of modern biology and in order to do so had to destroy the ideas of the older father figure of evolutionary thought. It almost appears that it was an oedipal conflict between the new generation of Darwinian biologists and more firmly established Lamarckian scholars. This pattern of intellectual parricide in the field of the history of science seems to have continued into twentieth century biology and includes the actions of the Batesons.

Additionally, there are some factors that appear to be peculiar to the Bateson family itself which tended to continue the pattern, Gregory, the youngest son; William, the father, and a successful biologist who was identified as a strong Lamarckian, then became anti-Lamarckian but not a Darwinian, and eventually differed with the new genetic school of the neo-Mendelians. William had three sons, the eldest, John was killed in battle during World War I, and the second son, Martin, committed suicide.

The fledgling son Gregory was left to carry on the family heritage. What a powerful influence his Edwardian father had on his remaining youthful heir! The circumstances of the death of his two older brothers created some reaction formation which may have moved Gregory to identify more strongly with his father's evolutionary theories. The weight of William Bateson's experience with Paul Kammerer's experiments also indicated forms of "screen memories" of Gregory's meeting with the Austrian scientist. Both Batesons interpreted Kammerer's experiments on the Midwife Toad as having Lamarckian amifications. The accusation made by William Bateson concerning a forged specimen drew Gregory further away from being professionally objective to the Lamarckian standpoint.

Notwithstanding, Gregory Bateson called Lamarck a great biologist, but his writings only showed respect for Lamarck's systematized knowledge in the development of early phases of modern psychology. Bateson in one apercu wrote: ".... I prefer to follow Lamarck, who, in setting up postulates for a science of comparative psychology, laid down the rule that no mental function shall be ascribed to an organism for which the complexity of the nervous system of the organism is insufficient."[43] Nonetheless, he overlooked what Lamarck really said in the bioloigcal sciences.

Gregory Bateson, in one significant instance, misconstrued Lamarckian description concerning the development of the anatomical parts of the pregiraffe. Bateson, who was, obviously, quite literate, interpreted Lamarck's views on the evolution of the giraffe as the major change occurring in the neck structure, whereas a careful reading of Lamarck's *Zoological Philosophy* indicates that the crucial change takes place in the extension of the animal's forelegs. This hackneyed example was used by Bateson, but, had he read Lamarck's works carefully and thoroughly, he would have found numerous other examples illustrating use and disuse. One may conclude that Gregory Bateson *chose* to disregard an enormous number of descriptions explaining Lamarck's four laws. Nevertheless, Gregory Bateson did admit, to some extent, that it was possible that RNA (ribonucleic acid) material found in the soma might have some detailed effect on the germ plasm.

Gregory Bateson, as a scientist and writer on the history of science, should have known that there were and are challenges to those scientists who only give recognition to the theory that evolutionary change and species variation occurs in a singular fashion, as dictated by the neo-Darwinians-neo-Mandelians. In the past forty years, some of the leading biologists specializing in different areas —Ernest Everett Just, Frederic Wood Jones, Stanley D. Porteus, Enrique Beltran and Herbert Graham Cannon —have questioned the "immortal power" of chromosomes and genes to act as the sole factors in determining species' development. Certainly, a trustworthy scientist such as the late Gregory Bateson should not have failed to completely examine the literature of his contemporaries before attempting to record his views on a hundred-year historical disagreement. This writer has offered an explanation concerning the reasons why Gregory Bateson was so frivolous and resorted to trivial faultfinding in the works of one of the greatest biologists in modern history.

## References


[1]Bateson, Gregory, *Steps to An Ecology of Mind*, Ballantine Books, New York, 1972.

[2]———*Mind and Nature*, Bantom Books, New York, 1979.

[3]Marchant, James, *Alfred Russel Wallace Letters and Reminiscences*, Harper & Brothers Publishers, New York, 1916 pp. 198-199.

[4]Wallace, Alfred Russel, "Evolution," *The Progress of the Century*, Alfred Russel Wallace *et al.* Harper & Brothers Publishers, New York, 1901, pp. 21-22.

[5]———*Darwinism*, Macmillan and Company, London, 1889, p. 3.

[6]——— *The Progress of the Century, op cit.*, p. 22.

[7]Lipset, David, *Gregory Bateson the Legacy of a Scientist*, Prentice-Hall Inc. Englewood Cliffs, New Jersey, 1980, pp. 110-114.

[8]Bateson, William, "Materials for the Study of Variation, "Scientific Papers of *William Bateson.* R. C. Punnett (ed.), *The Cambridge University Press*, Cambridge, 1928, Vol. I, pp. 219-224, C. D. Darlington, "Mendel and the Determinatnts, "L. C. Dunn (ed.) *Genetics in the 20th Century* (The Macmillan Company New York, 1951), pp. 315-319. C. D. Darlington, *Genetics and Man* (*New York:* Macmillan Company, 1964), pp. 97-101. L. C. Dunn, *A Short History of Genetics*, Mc Graw-Hill Book Company, New York, 1965, pp. 62—72. Robert C. Olby, *Origins of Mendelism*, Schocken Books, Inc., New York, 1966, pp. 133-134. H. F. Roberts, *Plant Hybridization before Mendel*, Hafner Publishing Company, New York, 1965, (Facsimile of 1929 edition), pp. 359-366.

[9]Lipset, *op. cit.*, p. 75.

[10]Haldane, J. B. S., *The Causes Of Evolution*, Cornell University Press, Ithaca, New York, 1966, (First Published, Longmans, Green & Co. Limited, 1932), p.15.

[11]Sturtevant, A. H. *A History Of Genetics*, Harper & Row, Publishers, New York, 1965, p. 21.

[12]Bateson, William, *Materials for the Study of Variation Treated with Especial Regard to Discontinuity in the Origin of Species*, The Macmillan Company, London, 1894.

[13]Bateson, William, "Evolution for Amateures, "*William Bateson, F.R.S. Naturalist—His Essays & Addresses*, The Cambridge University Press, Cambridge, 1928, pp. 449-455.

[14]———, "Heredity And Variation In Modern Lights, "Haeckel, Thomson, Weismann and Others, *Evolution In Modern Thought*, Boni and Liveright Publishing Corporation, The Modern Library, 1917, pp. 87-110.

[15]Iltis, Hugo, *Life Of Mendel*, (Trans.) Eden and Cedar Paul, Hafner Publishing Company, New York, 1966, first published in 1924 by Julius Springer, Berlin, p. 295.

[16]Poulton, Edward, Bangall, *Essays. On Evolution 1889-1907*, University of Oxford, Oxford, 1908, p. XXXVIII.

[17]Bateson, William, "Dr. Kammerer's Alytes, "*Scientific Papers Of William Bateson*, R. C. Punnett (ed.) The Cambridge University Press, Cambridge, 1928, Vol. II p. 378.

[18]Ibid.,

[19]Ibid., p. 381.

[20]Waddington, C. H. *The Evolution Of An Evolutionist*, Cornell University Press, Ithaca, 1975, p. 178.

[21]——— *The Nature Of Life*, Atheneum Publishers, New York, 1962, p. 91.

[22]Lipset, David, *Gregory Bateson The Legacy of a Scientist, op. cit.*, p. 44.

[23]Mead, Margaret, *Blackberry Winter My Earlier Years*, Simon & Schuster, Inc. New York: 1972, p. 227.

[24]Bateson, Gregory, *Steps To An Ecology of Mind, op. cit.*, p. 346.

[25]Jones, Frederic Wood, and Porteus, Stanley D. *The Matrix of the Mind*, University of Hawaii Press Association, Honolul, 1928, pp. 328-329.

[26]Bateson, Gregory, *Mind and Nature, op. cit.*, pp. 167-168.

[27]Stine, Gerald James, *Bisocial Genetics*, Macmillan Publishing Co., Inc., New York, 1977, p. 488.

[28]Koestler, Arthur, *The Case of the Midwife Toad*, Random House, Inc., New York, 1971, p. 44.

[29]Ritvo, Lucille B., Darwin As The Source Of Freud's Neo-Lamarckianism, *Journal of the American Psychoanalytic Association*, **13** (3), 1965, p. 499.

———, "Carl Claus As Freud's Professor Of The New Darwinian Biology, " *International Journal of Psychoanalysis*, **53** (277) 1972, p. 281.

[30]Claus, Carl, *Lamarck As Founder Of the Theory of Descent*, Alfred Holder, Vienna, 1888, p. 11.

[31]Beltran, Enrique, *Lamarck-Interprete De La Naturaleza*, Mexico City, 1945, pp. 62, 72.

[32]Bateson, Gregory, *Steps To An Ecology Of Mind*, *op. cit.*, pp. 348, 353.

[33]Lamarck, J. B. *Zoological Philosophy*, Trans. Hugh Elliot, Hafner Publishing Company, New York: 1963, (Originally Published in 1915 by Macmillan & Co. Ltd.) p. 122.

[34]Just, Ernest Everett, "Unsolved Problems Of General Biology," *Physiological Zoology*, Vol. XIII, No. 2, April, 1940, p. 141.

[35]Cannon, H. Graham, *Lamarck And Modern Genetics*, Manchester University Press, Manchester, England 1959, p. 39.

[36]Ibid., p. 40.

[37]Cannon, H. Graham, "What Lamarck Really Said," *Proceedings Of The Linnean Society Of London*, Vol. 168, Parts 1 and 2, May 21, 1957, p. 74.

[38]———, *The Evolution Of Living Things*, Manchester University Press, Manchester, England, 1958, pp. 137-139.

[39]———, "Is The Problem Of Evolution Solved?" *School Science Review*, Vol. 126, 1954, p. 233.

[40]Bateson, Gregory, *Mind And Nature*, *op. cit.*, p. 167.

[41]Brewer, E. Cobham, *Dictionary Of Phrase And Fable*, Henry Altemus, Philadelphia, 1898, p. 831.

[42]*Ibid.*

[43]Bateson, Gregory, *Mind And Nature*, *op. cit.*, p. 104.