

## ON THE "CENTER OF ORIGIN"

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Abstract.--The "Center of Origin" is currently held as a fundamental concept of zoogeography and phytogeography. It is readily apparent, however, that its application to concrete cases has been giving for long years, and still is giving, rise to innumerable discrepancies of opinion and outright controversies. It proves accordingly imperative to submit it to a searching analysis from different viewpoints.

OPINIONS ON THE "CENTER OF ORIGIN" AND RESULTS FROM  
ITS APPLICATION TO PARTICULAR CASES

Considering that historical geology never bothered about producing different worlds for the evolution of plants and animals, respectively, and that the ones are, at any rate, inseparable from the others on the face of the earth, it is obvious that the basic laws of dispersal are the same, in substance, for the two realms of nature. This has been amply demonstrated in a long series of works by the present author (Croizat, 1952, 1958, 1960, 1962, 1968a, 1968b; etc.), and needs not repeating in this short article.

John Briquet, a well known Swiss taxonomist of the past generation

specialising particularly in Labiatae (Briquet, 1901) held that the "dogma" of "monotopic origin" (that is to say, of the single center of origin) had exerted baneful influence on phytogeographic research. It forced, in his opinion (Briquet, 1901:66), the investigator to appeal to a whole string of assumptions in order to determine the original as well as the derivative presumable center of origin of a group. The viewpoint of Briquet has been pointedly rejected by Favarger & K pfer in an article (1969), which invited a rejoinder on the part of the present author (Croizat, 1971a), showing that not only <sup>are</sup> the ideas of Briquet inherently sound, but all too many are the authors who discuss the fundamental concepts of biology, including naturally the "center of origin", without paying attention to a minimum of proper semantics. Confusion in thought and practice does thus extend and deepen, for nobody actually understands himself and others.

Stanley A. Cain, a well known American botanical ecologist, wrote about thirty years ago (Cain, 1945) an article purposefully to deal with the criteria for the indication of "Center of Origin" in phytogeographical studies (see for full quotation and extensive consideration (Croizat, 1962; 595 ff.)). Cain showed that at least 13 different criteria have been advanced to individuate the "center of origin", none of which stands investigation, and concluded in the following vein: "There seems to be only one conclusion possible, and it carries implications far beyond the scope of the present discussion of criteria of center of origin. The sciences of geobotany (plant geography, plant ecology, plant sociology) and geozoology carry a

heavy burden of hypothesis and assumption which has resulted from an over-employment of deductive reasoning. What is most needed in these fields is a complete return to inductive reasoning (Raup, 1942) with assumptions reduced to a minimum and hypotheses based upon demonstrable facts and proposed only when necessary (Hulten, 1937). In many instances the assumptions arising from deductive reasoning have so thoroughly permeated the science of geography [sic] and have so long been a part of its warp and woof that students of the field can only with difficulty distinguish fact from fiction" (*italics the Author's*). This opinion by Cain became known to the present author long after he had reached the very same conclusion, and because of this worked out a new approach to the analysis of problems in dispersal, which Cain had neglected doing.

It would be useless to add to the records by Briquet and Cain other quotations and citations along the same, or similar lines. The reputation of these two sources is firm enough to assure the reader that what they affirmed is not a simple will-o'-the-wisp or the fruit of lack of experience.

It is as a matter of fact easy to check the viewpoints of Briquet, Cain (and the present Author, of course) by appealing to the concrete results, vouchsafed in the analysis of particular problems involving the "center of origin", on the part of zoologists who diametrically

opposed those viewpoints.

In his widely known "History of the North American Bird Fauna" (1946b), Mayr analyzes and assorts by "origins": ( "Pantropical, Panboreal, Old World, North American...Elements") 103 different main groups of birds. Of this total, he disposed without further of 29 important families (28% of the total), including Picidae, as "Unanalysed" on account, among other (Mayr, 1946b:14) that "Most of the families of shore birds also are so widespread as to make it impossible to trace their origin...Among the strictly terrestrial birds, there are eight families [7 are actually listed: Accipitridae, Pandionidae, Falconidae, Caprimulgidae, Apodidae, Picidae, Hirundinidae] that are so widespread or so evenly distributed as to make analysis difficult at the present time...The evidence indicates that all of these families originated at such an early date (Eocene or Cretaceous) that subsequent shifts in distribution have obliterated most of the clues". In spite of this, the ornithologist being quoted believes that Caprimulgidae and Picidae probably originated in the New World, Hirundinidae probably in the Old World. However, to complete the record, we also read the following (Mayr, 1946b:15): "The woodpeckers (Picidae) are represented about equally well in the Americas and the Oriental regions. They are rather poorly developed in Eurasia and Africa and are absent from the Australian region and from Madagascar. This pattern of distribution suggests a New World (but very early) origin for the family, although the fact that their nearest relatives, the wrynecks (Jyngidae), are exclusively Old World

would seem to indicate the opposite" (italics the Author's).

There are authors in excellent standing (Peters (1948), for example) who treat Jyngidae as a mere subfamily Jynginae of Picidae, but this does not remove the difficulty of the Wrynecks being exclusively of the Old World. However understood, Picidae are a cardinal family in the biogeographic sense because they hold the distinction (Peters, 1948:88ff., 97) of having one of their genera, Picimnus, represented by about 25 species in South America (de Schauensee, 1964:187), with one more species, P. innominatus, in southeastern Asia and western Malasia, absolutely disconnected from the rest of its congeners. The present Author has assuredly not neglected to investigate the case in his different works.

Resuming: (a) Mayr evidently relies on the "center of origin" in his analyses. (b) Unfortunately, this reliance forces him to give up on the spot 28% of the major groups of American avifauna as "unanalyzable", which - strictly from the standpoint of method in analysis is not favourable to the method in question; (c) In the case of Picidae, the decision reached by the ornithologist quoted is clearly desultory as basically ambiguous between an "American" and an "Old World" putative center of origin.

The opening statements in Darlington's chapter devoted to the zoogeography of birds (Darlington, 1957:236) ring as follows: "In some ways, birds are the best-known animals. Almost all existing species of them are probably known, some 8600 full species (Mayr, 1946a, Mayr and Amadon, 1951) plus thousands of geographical subspecies, and the distributions of many of the species are known in detail. Of all vertebrates, birds are the ones I know best myself. I have watched them almost all my life and have collected them in a small way in northern South America and Australia. I have had the benefit of many conversations about them with the late James L. Peters and with Ludlow Griscom and James C. Greenway, Jr., of the museum staff. And Dr. Josselyn Van Tyne and Professor Ernst Mayr have read stages of the manuscript of this chapter and made useful criticisms of it; Professor Mayr has allowed me to use his carded references on bird geography. I have therefore had unusual opportunities. Nevertheless, I still find the distribution of birds very hard to understand. The present pattern is clear enough, though complex. But the processes that have produced the pattern - the evolution and dispersal of birds - are very difficult to trace and understand" (italics the Author's).

This text came to the hands of the present Author, when he had already finished the manuscript of Panbiogeography (1958) in which birds are amply used in analysis precisely because of the reasons so clearly brought out by the entomologist and zoogeographer above quoted. It is an altogether remarkable text, for it amounts to the affirmation that: the ampler and better the data bearing on the

classification and distribution of an organic group, the greater the risk of the zoogeographer not being able to understand them.

According to the long personal experience of the Author in these matters, Darlington's introduction to the zoogeography of birds - as quoted - could only demonstrate a complete lack of method in analysis (and, correlatively, in synthesis). A properly devised method of enquiry will make relatively short work somehow, anyhow, of data statistically in optimum shape, and relatively not overwhelmingly abundant. In sum, the Author was moved to suspect on the spot the zoogeographic thinking of Mayr and Darlington had failed to evolve precisely the method needed; the method, by the way, auspicated but not formulated by Cain. The evidence thus before the Author from different quarters did dovetail to a single conclusion.

Darlington (1957:272-3) analyses the zoogeography of Columba along the following lines: "The one genus of pigeons common to the Old and New Worlds is Columba (from which domestic pigeons are derived). This genus is an example of ambiguity of numbers clues. It is nearly cosmopolitan. There are about 32 species of it native in the Old World and about 20 in the New, and the Old World species are more diversified, which suggest an Old World origin. But all 20 New world species occur in South and Central America and the West Indies. One of the Central American species extends into western North America north to southwestern Canada, but the genus is otherwise absent from the main part of North America, above southern Florida. There are about 14 species in temperate Eurasia and associated islands; 11 in the main

part of Africa and closely associated islands, but none in Madagascar; 5 in the tropical Oriental region etc.; and 2 in the Australian Region, but only one of them reaches Australia proper, and only the eastern part of the continent, and none reaches New Zealand. Thus detailed, the numbers suggest a tropical American origin of Columba, dispersal to the Old World through the north (not by the existing western North American species but perhaps by an earlier one), and spread through the Old World from the north. The absence of the genus in Madagascar and the more remote part of Australia is consistent with this history. Alternatively the genus may have originated in temperate Eurasia and radiated from there and then radiated secondarily in tropical America. Or (and I think this is most likely) it may have had a still more complex history". (Italics the Author's).

Such text as this needs no comment: its maker flounders amid clues that are - to him - ambiguous, grasping for a "center of origin" of Columba that eludes him throughout. This text perfectly agrees with the opinion of Fraipont & Leclerq (1932) (quoted by Favarger & Küpfer (1969:323)) to the effect that the quest of the "center of origin" leads on to a : "Effarante paléogéographie où les mers et les continents, les plantes et les animaux dansent sur une terre épileptique une ronde sans repos". Indeed, Columba, as above, flitters without rest from the Old and New World (naturally across Bering Landbridge), but nobody really understands the doing of this bird through space in time: whatever one may imagine of its zoogeography,



the "real history" behind it may still be more and more complex than the wildest flights of imagination.

Most likely on account of their neglect of the work of the present Author (Croizat (1958; 1:273-601, etc.)), Mayr & Phelps are induced in their quest for the "centers of origin" of the birds that managed to "colonize" by chance hops the cerros and mesas of the hoary sandstones of southeastern Venezuela (Mayr & Phelps, 1967) <sup>to find these "centers"</sup> in the Andes, in the Coastal Cordillera of Venezuela, and in the "Brazilian Shield" south of the Amazon. Unfortunately for this choice, the cerros and mesas in question (Duido-Roraiman System or Pantepui) are about coeval with said Shield, and much more ancient than either the Andes [or] the Coastal Cordillera! The "centers of origin" used by the ornithologists cited are accordingly evidently ill chosen because of obviously incongruous chronology and neglect of elementary tectonic rudiments.

It seems useless to continue quoting and citing when it is already amply established that the quest for the "center of origin" leads nowhere, better to say, into a morass of vain and conflicting opinions. The reason why so is transparent: The lone pigeon to reach southwestern Canada, as pointed out by Darlington, is Columba fasciata. This bird is very widely dispersed (from Trinidad to British Columbia and Argentina), and used to be "broken up" into two

distinct species: C. albilinea in South and Central America to Panama/Costarica; C. fasciata s.s. for the rest of the range). It belongs according to Johnston (1962) to the "species-group" Palumbus of subg. Columba, which "species-group" is composed in its turn of 2 "sub-groups" palumbus and fasciata respectively. The former is made up of species (palumbus, trocax, bollii, junoniae, unicincta) endemic to western Eurasia/Northwestern Africa, Equatorial Africa (unicincta), and, particularly, the Atlantic Islands (Madeira-Canaries, Azores) (trocax, bollii, junoniae, palumbus maderensis/azorica). The latter consists of but wholly american species, fasciata (as above), caribaea (Jamaica), araucana, (Chile and adjacent southwestern Argentina).

The determination of the "center of origin" of this group of pigeons leads forthwith to a bitter strife among zoogeographers who have no repugnance, in view of recent geophysical advances, to accept "transatlantic dispersal" as real, and those who, on the contrary, have clung for long years by now to the Matthewian thesis that dispersal of the kind is impossible except by great "chance" (like, for example, Simpson, Darlington, Mayr, etc.). The zoogeographers of the former persuasion will stand by Johnston (and the present Author (see Croizat, 1968)); the latter by Goodwin (1959): as a matter of course, none of them, however, being in the condition of proving his viewpoint unless by a careful critical analysis of the entire dispersal of Columba, effected this time no longer chasing the "center of origin" of the group of C. fasciata all the way from "Bering Landbridge" to Jamaica, Chile, Patagonia, Madeira, the Congo, Italy

etc. etc. in the loose style of zoogeography of which the reader of this article has had samples. What will have to be done (Fig. 1) according to the style of analysis of the present Author is to critically compare the scores of these pigeons all of the way from the Himalayas to Pantepui, Jamaica, Argentina/Chile and Canada, refraining from loose affirmations, theoretical preconceptions and the like. That will no longer do.

As to the quest for the "center of origin" intended as a means to establish what is "primitive", "derivative" etc. of a group according to Matthew's Rule, Hennig's opinions etc., I refer to certain Cetoniid Coleoptera to which Wiebes (1968) has invited attention without being in the position of explaining their zoogeography (see by contrast, Croizat (1971<sup>a</sup>):394 Fig. 2/B, 397 ff.). The case stands as follows (Fig. 2): Goliathus russus, endemic to the Congo (Zaire) Basin differs more from the adjacent G. fornasini (Kenya, Tanzania (Usambara), Mozambique etc.) and G. aureosparsus (Nigeria)/ G. higginsii (Ivory Coast) than do differ among themselves G. fornasini, G. aureosparsus/G. higginsii. Warns Weibes (1968:30) that: "There are more examples of this phenomenon in other groups of African Cetoniidae" which he declined to explain for the present. The present Author has indeed found quite a number of cases of the kind in plants and animals which figure in his works as wing dispersal.

Some zoogeographers will insist that "primitive" is the "species" in the center of the galaxy as - more or less necessarily - "older". Others will take the opposite view for the cases are indeed not wanting in which the forms "in the wings of the dispersal" suggest in their phenotypical characters "primitiveness". The present Author, not being fond of sterile argument, can only point out that - by an opportune choice of examples - anyone can "prove" what he wishes for the very simple reason that the "center of origin" being an imaginary concept, all its corollaries are just as imaginary. Time is being lost, even in science, debating figments.

#### DARWIN'S OWN "CENTER OF ORIGIN"

The question does rise, toward the end of this summary review, who originated the "center of origin", and what for? The answer is that, much in general, the "center of origin" is a very old concept, so old in fact that Spanish clerics were worrying about the "center of origin" of the Indians met in the New World, shortly after the discovery. The one, however, who formally introduced it to "Geographic Distribution" is none else but Charles Darwin, in The Origin of Species (1859), Chapter XII and XIII. The naturalist who has critically perused these two chapters readily understands everything of the zoogeography of the kind current today in the writings of William Diller Matthew (1915) and all his successors down to this day.

The texts basically material to our subject are as follows: (1) (The Origin of Species, Chapter XII "Geographical Distribution", "Single Centers of Supposed Creation, (p.352)) - "We are thus brought to the question which has been largely discussed by naturalists, namely, whether species have been created at one or more points of the earth's surface. Undoubtedly there are very many cases of extreme difficulty, in understanding how the same species could possibly have migrated from some one point to the several distant and isolated points, where now found. Nevertheless the simplicity of the view that each species was first produced within a single region captivates the mind. He who rejects it, rejects the vera causa of ordinary generation with subsequent migration, and calls in the agency of a miracle"; (2) (Darwin, 1859: Chapter XII, "Means of Dispersal" 357-8) - "Whenever it is fully admitted, as I believe it will some day be, that each species has proceeded from a single birthplace, and when in the course of time we know something definite about the means of distribution, we shall be enabled to speculate with security on the former extension of the land. But I do not believe that it will ever be proved that within the recent period continents which are now quite separate, have been continuously, or almost continuously, united with each other, and with the many existing oceanic islands".

So far for Darwin, and it is immediately apparent that: (i) Faithful to his fundamental theoretical bias, and to the very title of his opus magnum, Darwin is uniquely concerned with the origin of species (ii) He has absolutely no argument to offer for his belief

that the species originates in a single center of creation from which it proceeds by active migration to points far and near in space and time. This belief, Darwin affirms, "captivates the mind", and he who rejects it is guilty of invoking a miracle against the true cause (vera causa) of "ordinary generation with subsequent migration"; (iii) So overwhelming is the importance of means of distribution that the day we shall know them, and we shall believe in the single "center of origin" for the "species", we will be in condition of speculating "with security" on paleogeographic questions; (iv) It does not agree with Darwin's own belief that continental outlines could have been so modified "within the recent period" as to make the continent and the "oceanic islands" in any way contiguous, or almost contiguous.

As the texts quoted make transparent, the zoogeography of Darwinian coin relies on: (a) Center of Origin; (b) Active Migration; (c) Means of Distribution; (d) Essential Permanency of Continental Outlines. To this W. D. Matthew added (1915) the postulate that "migration" did proceed from Holarctis to the rest of the earth. These are the foundations of the zoogeography of Simpson, Mayr, Darlington, Hershkovitz, Patterson etc. to this day, 114 years following the publication of the Origin of Species.

In the careful, well supported estimate of the present Author (Croizat (1962:592-706)), Darwin, who himself admitted (1859:809):

"Facts compelled me [Darwin] to conclude that my brain was never formed for much thinking", <sup>of</sup> The Origin of Species is by far inferior to Darwin of the Voyage of the Beagle (Darwin, 1839). In the former, Darwin attempts to generalize and to theorise, a kind of effort for which, as he admitted, he did not qualify on account of his brain not having been formed for much thinking. This will be obvious to anyone who pays attention to the "reasons" Darwin invokes to support his idea of "center of origin". In the Voyage of the Beagle, Darwin observes and factually reports, for which he was eminently fitted. The present Author may not hope to repeat here what is detailed in long pages of his 1962 opus, but points out: (1) Darwin was aware of "replacement" among different birds as between central Chile and the southern islands of Chiloé and Chonos (Darwin 1839: Chapter XIII (entry January 1st, 1835)), and expatiated on it in the chapter devoted to the "Galapagos Archipelago". Notable is the statement that, in these islands: "Seeing this gradation and diversity of structure in one small, intimately related group of birds (Darwin Finches: Geospizinae), one might really fancy that from an original paucity of birds in this archipelago, one species had been taken and modified for different ends", which would of course not imply relevant "migrations" and correlative "means of distribution". Quite as remarkable is the perception that the different islands of the Galapagos archipelago are to a considerable extent inhabited by a different set of beings. Adds Darwin: "I never dreamed that islands, about fifty or sixty miles apart, and most of them in sight of each other, formed of precisely the same rock, placed under a quite similar climate, rising to a nearly equal height, would have been differently tenanted"; (2) In

regard of Mimus (actually Nesomimus) endemic to Galapagos, Darwin observed that, M. parvulus (Albermarle Island) and M. melanotis (James and Chatham) are "closely allied species" that some ornithologists would consider as "only well-marked races or varieties", which has much to say against the "species" deserving a particularly exalted place in nature. Indeed in relation to Geospizinae, Darwin actually uses the term: "representative species" (which is virtually a flat anticipation of the concept of "superspecies"); (3) Concludes Darwin: "I have said that the Galapagos Archipelago might be called a satellite attached to America, but it should rather be called a group of satellites, physically similar, organically distinct, yet intimately related to each other, and all related in a marked though much lesser degree, to the great American continent". This is an excellent summation which has the Galapagos cast in the position of a geologic fragment of America, since separated from the New World continental landmass and having evolved its own life in isolation long enough to have it differ but not long enough to have it lose trace of its "american" ties. Whatever Darwin might have speculated about "casual means" that "colonised" the archipelago does not detract from the very strong probability that, indeed, this "piece" at sea of continental America did "carry with it" from "America" the progenitors of its actual bios. Had it not been so, the actual fauna and flora could not prove to be basically "american" for the atoll of Clipperton nearby (Chace 1962) is still well provided with "pacific" and "polynesian" elements.



Concerning the eminently "oceanic" island of Ascension, in mid-Atlantic, Darwin (1839) comments (Chapter XXI: "Mauritius to England") that in a fragment of larva there collected were found remnants of freshwater infusoria and no less than 25 different kinds of plant, mostly of the graminaceous description. Naturally, Darwin argues that: "We may feel sure that, at some former epoch, the climate and production of Ascension were very different from what they now are. Where on the face of the earth can we find a spot, on which close investigation will not discover signs of that endless cycle of change, to which the earth has been, is, and will be subjected?" This fragment of the Voyage of the Beagle ill agrees indeed with the theory of the "permanency of continental outlines", the absolute division of islands into "oceanic" and "continental", the potency of knowable "means of distribution", and the rest of the "Geographic Distribution/Zoogeography" of our times. However, the fragment in question perfectly well agrees with the very latest from Ascension (Chace & Manning, 1972), that is to say, the discovery of two very notable shrimps endemic to Ascension, one of which belongs to the genus Typhlatya, known elsewhere from the Caribbean islands of Barbuda, Mona, Cuba; from Mexico (Yucatan) and - almost incredible by the modern map - Galapagos. The explanation of the bond: Ascension - Carribeans tendered by Wilson (in litt., quoted in Chace and Manning, 1972:6) implements from the strictly geophysical side of the tale the conclusions of the present Author's down to the manner in which "new" islands and mountains (Croizat, 1962:247 ff., 258 Fig. 50) managed to retain very old fauna and flora.

At his return to England following the period on the Beagle, Darwin was in possession of some of the main principles and motives of modern systematics and biogeography. He definitely knew, for example, that, in spite of its rating in our times as an "oceanic island", still Ascension, in mid-Atlantic, was nothing such on account of a very different biological and geological past. He had a definite understanding of polytopism/vicariism, up to the point that he could visualise the Archipelago of Galapagos - as such, a nearly unique center of evolution - as a galaxy of separated but interrelated centers of evolution of lesser rank, having evolved parallel in time and space, which means to say that Darwin had gained a proper understanding of the march of form-making in general as well as in detail, that is in reference to a specific point of the earth's surface and to particular animals. Darwin also was certain that an obvious nexus did bind the history of the earth with that of its inhabitants, plants and animals, past and present. If he harboured questionable notions about - for instance - the potency of "barriers to dispersal", as when believing that the Andes, for example, had existed as a: "Great barrier since the present races of animals had appeared" ( The Voyage of the Beagle, Chapter XV, entry March 23rd::, still Darwin easily could by working out his own notes and observations alter his mind constructively when not completely.

Unfortunately - and this rates as a tragedy for biology at large - upon his return to England, Charles Darwin embarked on the dubious task of building up a whole theory of "natural selection", "survival

of the fittest", "geographic distribution" centering around the "origin of species". It would have been beyond comparison better, if less potent in regard of popular appeal, for Darwin to face the general problem of the origin of biological differentiation in time through space on the basis of the material - indeed unique at the time-in his hands.

The gift that most decisively sets aside the authentic genius from less capable types of mind is the unerring capacity of choosing, as if by instinct, the right side of the question or task, and at this point Darwin surely did fail, for in opposing, whether he intended it or not, the Darwin of the Origin of Species to the Darwin of the Voyage of the Beagle, Darwin made the wrong choice by far.

As a matter of fact - looking at the matter from the outside - it is this dual nature, this opposition between Darwin the keen observer and Darwin the indifferent thinker that has given origin to a bewildering literature (see for a discussion Croizat, 1962) in the pages of which Simpson extols Darwin as the genius of the ages, and Himmelfarb - by no means a second rate biographer - views him (1959) as little short of a mediocrity. Thompson (1958:810) goes so far as flatly to impute to the thinking of Darwin " a decline in scientific integrity."

The charge would seem preposterous were it not so that it can be amply substantiated, referring for example to Cain's well documented opinion that zoogeography and phytogeography (and related sciences) have become so deeply shot through by pointless theorizing that one no longer knows how to separate fact from fiction in their literature. Definitely in favour of Thompson's and Cain's viewpoint also is the fact that, for example, quite recently a Soviet botanist with an ample following in the United States, Armen Takhtajian (1959:120), has given course (see Croizat (1973:102)) to a theory of "adaptation", pretending that the nectaries of the angiospermous flowers have been actually brought into being by a form of evolution intending to replace, for the benefit of insects, "costly pollen" with "cheap sugary exudates"! The devout darwinist in question probably never suffered from hayfever.

In sum, to judge accurately of "darwinism" under historical and present conditions has become a chore that exceedingly few naturalists are qualified to shoulder. On this account, the immense majority of naturalists is content with going along with the belief of the day, accepting for example the "center of origin" as a badge of orthodoxy, and a virtually fool-proof, primary concept of "zoogeography" and "phytogeography", without the slightest understanding of the origin of this monumental, most pernicious figment. It is the failure to dissect it to its core that has thrown out of gear contemporaneous "zoogeography" and "phytogeography", begetting a whole string of pseudo-concepts such as "chance hops", "great capacities for

dispersal", "translation by natural rafts over watergaps", "mysterious means of distribution" etc. This is assuredly not the private opinion of the present Author, for Mayr, for example, admits it when stating (1946b:36): "The close relationship between the Old and New World members of the Pantropical element , whose ranges are now widely discontinuous, proves that such a faunal exchange must have taken place, and this places the zoogeographer in a real quandary. The customary solution for the problem is to ignore it" (italics the present Author's). The statement by Mayr is demonstrably so true that no "zoogeographer" has thus far tried to account for the dispersal, for example, of Picumnus between tropical America and the tropical Far East/western Malasia; of Ciccaba on both sides of the Atlantic (see Croizat, 1971b). The reader needs not be told that avoiding fundamental problems out of veneration for the principles of the "Geographic distribution" of Darwin and Matthew (a factual reissue of Darwin's) is a form of mediaeval involution absolutely repugnant to the spirit of modern investigation.

In chapter XII, on "Geographical Distribution" of Origin of the Species Darwin writes as follows: "It is obvious that the several species of the same genus, though inhabiting the most distant quarters of the world, must originally have proceeded from the same source, as they are derived from the same progenitor". Obvious indeed it is, and from it logically derives a concept of vicariant form-making in time over space, meaning that an original prototype with generic characters  $a+b+c+d+e+f\dots(n+1)$  has dissolved into a galaxy of correlated species/

superspecies, each one distinct on account of representing - in specific ranks - different combinations of the original generic characters, as for instance: a+b+c, d+e+f, a+e+d, etc. The dissociation of the "genus" into "species" has naturally been effected around particular centers of form making, by which reason the species vicariate - in principle - in space within the genus.

What is true of the genus/species relationship is, naturally, also true as regards the species and the subspecies/variety (in botany). It will be noticed that inasmuch as the breaking down of the "species" into "subspecies" is normally more recent in geological time than the dissociation of the "genus" into "species" and "superspecies" geographic replacement is as a rule much more precise below the rank of species than that of genus. A "genus" dating from the second half of the Cretaceous, for example, has certainly undergone a greater amount of disturbance, wear-and-tear than "species" that formed only in the Oligocene, for instance, hence the territorial limits of "species" within the "genus" are, as a rule, less well definite than those of the "subspecies" within the bosom of the "species". This naturally does not mean that the "species" have been wandering at will: to prove the contrary it is sufficient to refer to the enormous crops of species that in certain vegetal and animal genera and groups crop up at certain points of the earth, as for example, 600 species of Erica in South Africa, about as many of Rhododendron in the Far East and Malasia, over 500 species of Eriocaulaceae in Minas Gerais of Brasil, about 1000 species of avian "Mesomyodes" in continental

America (but no more than about 15 in the whole of the Antillean Region - this disparity as between the continent and antillean islands is not as "fantastic" or "due to sheer chance" as it might seem: Hummingbirds figure with some 320 species in the avifana of the New World (Colombia alone numbers 136, Venezuela apparently a few more) but in the West Indies only 17 are present). Statistics of the kind do prove against the theory of "chance colonisation" in no uncertain terms, favouring on the contrary the conclusion that the "species" is eminently sedentary within main centers of mass established at no later date than the end of the Jurassic/beginning of the Cretaceous. The works of the present Author are replete with analyses of vegetal and animal life in dispersal that leave no doubt as to the proper interpretation in substance when not in all details.

Let us now suppose that instead of understanding the text by Darwin above quoted in the sense that it refers to species orderly formed within the bosom of the parent-genus, we understand it, as by other texts by Darwin, to refer to species having each originated in a "single center of creation" and spread therefrom in free "migrations" through "means of distribution" often of the "mysterious" kind. In this case, taking as our subject for instance an avian genus of some 10/20 different species and subspecies we ought to imagine every species/subspecies originating at its own time and convenience in any one spot of the map, then starting an untrammelled round of free "emigrations". Evidently absurd as a natural process, this approach to

"center of origin", "migration" and "means of distribution" is also diametrically opposed to the very concept of vicariism/polytopism on which is based, ever since the days of Kleinschmidt (see Croizat, 1962:177 ff.), the praxis and philosophy of modern systematics.

The opposition between modern systematics/taxonomy and "geographic distribution" in the manner of Darwin (of *The Origin of the Species*, 1859), Matthew and his disciples is so stark, so impossible of conciliation, that no one accustomed to logical thinking can explain how a practicing mammalogist, ornithologist, entomologist, botanist etc. can - by the farthest stretch of imagination - believe in "center of origin", "migration" and "means of distribution" in the Darwinian and Matthewian manner, and stand at the same by the concept of vicariism/polytopism. This concept calls for an orderly conception of evolution over time in space, <sup>and is</sup> therefore radically opposed to the notion of "chance dispersal" and all its corollaries. It is highly fortunate for biology at large that, having geophysics shown definitely by now how brittle are the foundations of the "Geographic Distribution"/Zoogeography" of Darwin/Matthew and all their disciples, these pseudosciences have begun to crumble away, freeing biology of the nightmare of said "center", said "migrations", said "means".

The way out is precisely the one outlined by Cain, and rigorously followed by the present Author ever after 1952: to do away with theory



and "authority" based on theory; to formulate proper methods of factual analysis of the "numbers" that figure in the scores of organic life; to reject finally and absolutely every affirmation lacking proper analytical and synthetic basis; to learn that ideas and belief have a history, and that it is necessary that the young student be informed of it in order that he should no longer wander in a world of make believe and pretense while feeling sure of acting in a "respectable" and "orthodox" manner. The lone "orthodoxy" and "respectability" in science is that which brings in concrete results, and no one well informed of the status of "zoogeography" and "phytogeography" can have any illusion about their gross inefficiency.

#### CONCLUSIONS

The basic conclusions of the present article may be resumed as follows:

- (1) The three cornerstones of the darwinian "Geographic Distribution": Center of Origin, Migration, Means of Distribution are bound with an essentially theoretical conception of animal and vegetal dispersal over space through time, therefore actually unworkable.
- (2) The persistent attempt to having them "work" at all costs during over a century by now, has favoured the coming into being of a string of derivative notions, the value of which in research concerning evolution over space through time is even less than the original darwinian figments. Indeed, a concentration of spurious methods and principles having firmly taken root, we are today at the point where, according to the considered opinion of Stanley A. Cain (and of the

present Author, not to mention others), it has become impossible to discriminate facts from fiction. Not to challenge this state of affairs is to do disservice to biological advance in regard of problems involving correlation among form-making (evolution in general), space and time.

(3) A number of pertinent citations and quotations are given to the above effect.

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## FOOTNOTES

1. An objective analysis of Goodwin's notes makes it clear that he assuredly does not find decisively against Johnston, rather the contrary (see Croizat in ms.).
  
2. As any naturalist may know, the Andes are, on the contrary, a relatively recent Tertiary feature on the face of the New World. When they began to soar, replacing older heights, genera and "species" of passeriform birds, for example (Howard 1950) were already so "modern" that they can be assigned to still extant families and genera. The Andes grew accordingly under the roots and feet of the immediate progenitors of the species and subspecies still on the spot (or since become extinct in it, on account of their inability to stand orogeny). See for example Croizat (1971a; 383 Fig. 1) for immediate orientation.
  
3. These relationships have been forged both "transpacifically" and "transatlantically", and certainly not by chance, as the recent advance of geophysics is beginning at last to prove against the tenets of Darwin, Matthew, and his school. See the opera omnia of the present Author, 1952-1973 (part of the relevant bibliography is contained in Nelson (1973), which see.).

## CAPTIONS TO FIGURES

Fig. 1 The dispersal of Columba (Aves: Columbidae), species-group palumbus (after Johnston 1962). This species-group includes two sub-groups: palumbus, and fasciata. The distribution of the former within continuous line in Eurasia and Africa. Lettering as follows: A - "Atlantic Sector" (Canary Islands, Madera, Azores), in which are concentrated C. palumbus maderensis, C. p. azorica, C. trocaz, C. (trocaz) bollii, C. junoniae; B - range of C. uncinata (a classic "West African" taxon); C - range of C. palumbus outside the Atlantic Sector. The distribution of the complex C. fasciata/C. albilinea (the former to the north of the double-bar, Panama/Costa Rica, the latter to the southeast) is indicated by full triangles connected by a line (track). This complex, C. caribaea (Jamaica: circle 1), and C. araucana (Chile and Argentina; circle 2) form the sub-group fasciata.

Fig. 2. The island of Ascension (in mid-Atlantic) as A. The stations of Typhlatya (Decapoda: Natantia, Atyidae) marked in full circlets connected by a line (track). The stations of the terrestrial crab, Gecarcinus lagostoma (Chace & Hobbs 1969: 197-8) indicated by full inverted triangles: 1 (Trinidad), 2 (Fernando de Noronha), A (Ascension), 3 (islands in the gulf of Guinea), 4 (Cameroon), connected by a track in dotted outline. This type of distribution should be compared with: Croizat (1968b: 84 Fig. 13, 85 Fig 14C, D, E, 99 Fig. 15, 102 Fig. 16A, 151 Fig. 22A; etc.) INSET - The American stations



of Typhlatya: 1 - Barbuda (T. monae): 2 - Mona (between  
Puerto Rico and Hispaniola: T. monae): 3 - Cuba (Oriente,  
Pinar del Rio: T. garciai): 4 - Yucatan (south east Mexico:  
T. pearsei): Galapagos (T. galapagensis).