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VICARIANCE/VICARIISM, PANBIOGEOGRAPHY, "VICARIANCE BIOGEOGRAPHY," ETC.: A CLARIFICATION

Several times in the last two years I have been asked through letters or by word of mouth to write something that would clarify the conflicting concepts of current biogeography. I have for long resisted these pleas, feeling that so overpowering are the conditions of disorder and strife in the science dealing with space, time and form in relation to organic evolution that the best thing to do is to give time to time as the ultimate solver of present difficulties. I have done my work, and I feel assured that it is not worse than many, many others.

My resolution to leave the field to others crumbled, however, when I chanced to read the review that a British paleontologist, Brian R. Rosen (1982:11–12) recently contributed of: Nelson and Rosen, *Vicariance Biogeography: A Critique*, 1981, covering the papers presented in a Symposium held at the American Museum of Natural History on May 2–4, 1979. This Symposium is in fact the embodiment of: "The Systematics Discussion Group of the American Museum of Natural History," and the paper that I contributed to it is obviously misplaced in the context of that gathering. I never had part in the discussions of that group, and, as it will next be seen, I flatly disagree with most of its conclusions. The editors of the proceedings of the Symposium in question, well aware of this, relegated my paper to the very end of the gathering, when it could no longer be discussed. Rather curiously, as the reader is duly to see as soon as the subject calls for it, the authorized review of the Symposium written by Virginia R. Ferris (1980: 67–76) utters—unwillingly, it seems—from both corners of the Mouth. The first part of her review speaks in unison with the "Museum group," but the second, beginning with a pointed reference to Croizat (p. 73), laments that this paper was brought to the attention of the gathering so late that there was no longer opportunity to discuss it. Ferris found this lamentable, for she believed that, opportunely discussed, what I wrote would materially influence the course of the gathering as a whole.

That a candid reviewer like Brian Rosen (not to be confused with Donn E. Rosen of New York) would feel bewildered facing the printed results of the 1979 Symposium is easily understandable. As a matter of fact, he writes: "I think that in Britain at least, many palaeontologists have not yet found out what vicariance biogeography is, let alone learnt anything from it, but I also think that this symposium volume indirectly explains both failings . . . . With contributions by Croizat, Nelson, D. E. Rosen . . . we could reasonably have expected this from a collective horse's mouth. It was not to be. The title gives the clue and so what we have instead is a labyrinth Hansard of infighting over the facts, methodology and philosophy of historical biogeography."

What Brian Rosen ignores and with him the totality, I am certain, of the biologists now living is that there was no "collective horse's mouth" uttering in the premises, but two of them indeed uttering at crosspurposes: Nelson's (and his colleagues at the American Museum of Natural History of New York) and Croizat's, respectively.

I will, directly challenged this time by Brian Rosen from the British Museum under my name, deal separately in coming pages with my own (pan)biogeography, vicariance/vicariism, and alien "vicariance biogeography," in order that the reader may form, once for ever, *a solid understanding of these matters*, ready to detect for himself the countless falsehoods, misconstructions, etc., that now foul the history of their origin, growth, etc.

I will, however, for the moment go straight to the roots of my subject, thus clearing the field at one stroke of the basic confusion that befogs it.

Rosen (1981:1) has this much to say in principle of Croizat's biogeography: "PANBIOGEOGRAPHY, the term first used by Leon Croizat (1958) to designate a world view of biotic interrelationships, is not just a germinal concept out of which modern vicariance biogeography has emerged. For Croizat and for at least some of today's vicariance biogeographers, it is also a touchstone for a methodologically and conceptually new evolutionary biology."

What Rosen thus writes has for its background an exhaustive paper (Rosen, 1975:431–464), in which this distinguished ichthyologist tested his own conclusions as to Caribbean marine dispersal against the method and conclusions of Croizat in the Panbiogeography (1958). Summing up, Rosen wrote (p. 459): "Such coincidence of observations and theories [as between Croizat and Rosen] is no more than Croizat had foreseen and suggests the fundamental soundness of his insights and of the investigational method that he devised." Further (p. 461): "I [Rosen] have heard some of my colleagues remark that because they have identified in Croizat's writings some particular errors of fact or interpretations of fact, that the whole body of Croizat's work and ideas can be discarded. I personally consider such summary dismissal of a man's work extremely unwise and more inimical to scientific progress than any number of errors that Croizat as a compiler may have committed." It is worth adding that Colin Patterson—an author whose ideas are not always crystal-clear (he has me down, for example, 1981:446, as a developer of "vicariance biogeography," which is obviously false)—believes, indeed, that Rosen's theory (1975, not 1976 as he cites it) establishes a concordance between biology and geology (fruit of the panbiogeographic method as acknowledged by Rosen, 1975) which is "striking and satisfying." Additionally (p. 458): "To sum up, Rosen's 1976 [1975] paper has great interest as a precise exposition of Croizat's vicariance method." Again Colin Patterson misrepresents my life-work and method, which has been throughout *panbiogeographic*, not at all "*vicariant*," as I will carefully explain in a coming page.

Rosen, then, correctly appreciates the Panbiogeography as a landmark of scientific biogeography, indeed "the touchstone for a methodologically and conceptually new evolutionary biology." As a member in excellent standing of the American Museum of New York, Rosen cannot divest himself of the livery befitting a "vicariance biogeographer" (he is a mild one, indeed), but this does not warp his discerning powers. At any rate, we owe Rosen the tantalizing information that Croizat's panbiogeography has served as the "germinal concept" out of which "modern vicariance biogeography" eventually managed to "emerge."

Rosen is coeditor together with Gareth Nelson of *Vicariance Biogeography: A Critique*, 1981, and it should be normally anticipated that as members of the same team they see eye to eye in matters of essentials. It would hardly seem so, however, to judge from what Nelson says on p. 524 of the same book, thus precisely: "The revival of continental drift during the 1960's was the doom of Wallace's synthesis (dispersalist biogeography). That dynamic concept of earth history made it clear that the 100-odd-year tradition of dispersalism was based on some fundamental error. A suggestion of a possible cause and remedy was supplied by Brundin (1966) who coupled Hennig's (1950) notions of cladistics with repetitive vicariant distributions of fresh-water midges of the temperate parts of the southern continents. One result was that Croizat's panbiogeography was brought into the light of present relevance where fused with cladistics it matured into vicariance biogeography."

Both Rosen and Nelson, then, identify the panbiogeography of Croizat as the pedestal upon which a later "vicariance biogeography" built its castle. However, for Rosen, panbiogeography rates as a fundamental contribution to the sciences of dispersal and evolution. Not so indeed for Nelson, who cuts it down to a little something that everybody would have ignored if Brundin had not married it with Hennig: which felicitously sired the "vicariance biogeography" of which Nelson is today the prophet.

That the text I have just quoted from Nelson's hand is tendentious is immediately evident to anyone who knows even a little of the history of biogeography. What Nelson identifies as "Wallace's synthesis" is the zoogeography, so called, of Darwin, Matthew, Simpson, Mayr, Darlington etc., usually referred to today as dispersalist biogeography. Against that "Wallace's synthesis" I waged, beginning in 1952 and following up in 1958, 1960, 1962–1964[1], a relentless opposition based not on ground of geological theories (see, for instance, Croizat, 1962–1964: 191, fig. 45), but strictly on the merits of botanical and zoological evidence, at least ten years before geophysical evidence began seriously to affect the credibility of "Wallace's synthesis" imposed at the time by an ironclad monopoly upon the biological sciences by the luminaries of the hour, Simpson, Mayr, Darlington etc. This necessary opposition brought on me (Abele, 1982:79) the charge of belonging to the "insane fringe," of thinking and writing in a thoroughly "unscientific manner," etc.

My work, however, already consisting of thousands of closely documented pages had obviously gained some "relevance" before Brundin, as claimed by Nelson, managed to rescue it from presumably deserved obscurity. Brundin as a matter of the record, mentioned Croizat relevantly only once (1966:61), stating: "The book of Darlington (Zoogeography 1957) illustrates in a conspicuous way all the weaknesses of the Wallacean type of approach, and it is not surprising that it became a highly appreciated target of Croizat (1958) in his *Panbiogeography*. From Darlington's recent *Biogeography* (1965) it is evident that he has learned nothing from the blazing sermon of Croizat. The approach of the latter is not wholly sound, it is true, and his criticism of the Wallacean camp may be considered too violent, but his search for the truth has been frenetic, and it is only fair to stress that much of his message concerns everyone dealing with the history of life very deeply." This is all, and it does not support, I believe, Nelson's claim that I owe the relevancy of my work only, at least mainly, to the personal ef-

[1] This title is dated 1962 on the title-page. It was as a matter of fact in full print by 1963, but unforeseen circumstances delayed its being placed on sale until 1964. I took advantage of the interval to add something to the bulk of the text. The date of 1962 is not due to a casual error.

forts of Brundin. I might remark that the violence of which I stand accused, if any such really exists, finds its logical explanation in my facing an unjustified, stifling monopoly on the part of the "Wallaceans" (Simpson, Mayr, Darlington etc.), and in their absolute refusal to come out in the open to meet criticism with scientific arguments, not with whispering campaigns and evasive tactics.

This is obviously not the place to analyze in detail the many pages which Brundin consecrates to Hennig, substantially repeating what I have already written (1976, 1978 (in Spanish and English)). Hennig's thinking is characteristically teutonic: it appeals to minds readily enthused by highsounding definitions and principles, whatever their value as effective tools of investigation in the end. Briefly to illustrate: (1) (Brundin, 1966:16) "Definition of *phylogenetic relationship*: A species x is more nearly related to an arbitrary species y than to an arbitrary species z if, and only if, it has at least one ancestor in common with species y which is not at the same time the ancestor of species z (Hennig 1957, p. 60)"; (2) (p. 17) "The definition of a *monophyletic group* is a direct consequence of the definition of phylogenetic relationship. Monophyletic is every group of the system fulfilling the demand that any species belonging to it is more closely related to any other species likewise belonging to the group than to any species which does not belong to it (Hennig 1953, p. 9)."

These pseudo-mathematical definitions look marvellous on paper, very simple etc., but their concrete application involves him who believes in them with no end of doubts and difficulties bringing about in the end a perennial state of confusion. On this ground, I see them as unworkable, indeed noxious.

As to Brundin himself (1966:23), we read out of his pen the following: "For the discussion and interpretation of phylogenies it is of fundamental importance to note that the speciation process MUST be looked upon as a splitting of an ancestral species into daughter species, and not as a branching off of daughter species from a persisting ancestral species. Accordingly, when the isolated daughter population acquires reproductive isolation and the position of separate species, then the mother species ceases to exist. WE HAVE TO HOLD TO THIS INTERPRETATION EVEN IF THE ANCESTRAL SPECIES, AS MAY OFTEN BE THE CASE, SHOULD REMAIN PRACTICALLY UNCHANGED" (capitals mine).

I will of course have nothing to do with the definitions, axioms, etc. of Hennig and Brundin, on grounds that do not require comment to make the point in a circle of competent biologists. And as to Nelson, I hold as tendentious and actually misleading the account in the text above quoted from his pen, how, supposedly, the panbiogeography of Croizat was charitably rescued from the limbs of oblivion thanks to Brundin, Hennig, and Nelson, in order to be eventually hybridized with "cladistics" (that is, Hennig's "phylogenetic systematics"), and made thus to flower as a new "science" of "Vicariance Biogeography," the seat of which is in New York, Nelson being presumably its supreme pontiff.

The information I have given in the preceding pages may seem fantastic, such as to justify—in spite of precise evidence in its support—my status as a mentally disarranged, violent, disgruntled author. Why indeed should I complain concerning what Brundin, Nelson etc. have done with my panbiogeography, when I have no complaint against Rosen for having used it to check his own conclusions as to the dispersal of Caribbean fishes?

The difference, as always the case is in human affairs, depends on tone and manner. Rosen took advantage of the Panbiogeography allowing it to stand as originally conceived, and presented it under its own name, which is normal citation. Apparently not so Nelson, who incorporated the Panbiogeography—hopefully indeed—with Hennig's Systematic Phylogeny, renaming the mixture "Vicariance Biogeography." Apart from the properties in the case, Nelson's handling had for its result the confusion into one of very different streams of thought which is never to advantage of scientific understanding, no more so than renaming the same plant or animal twice.

To clear up the question, Nelson (with Platnick as coauthor) fortunately contributes a text indeed perspicuous, as follows (1981:ix): "The views presented in this volume have their source largely in the work of two biologists, the late Willi Hennig, author of a 1966 book called *Phylogenetic Systematics*, and Leon Croizat, author of a 1964 book called *Space, Time, Form: The Biological Synthesis*, and in the writings of a philosopher of science, Sir Karl Popper. Hennig and Croizat have not found their work particularly compatible (Hennig never cited Croizat, and Croizat (1976) has published negative comments of Hennig), and neither one has indicated any interest in Popper's views or cited them as being compatible with his own. Yet both Hennig and Croizat have made substantial, and substantially similar, contributions .... We believe that the contributions of both Hennig and Croizat can be readily (and fruitfully) understood within the context of Popper's view of the nature and growth of scientific knowledge, and that the ideas of all three men are largely compatible. At the same time, were it possible for all three to read this book each might disagree with large parts of it. The reader can judge to what extent we [Nelson and Platnick] have been successful in synthesizing and extending their contributions, and what value the resulting perspective may have."

Had I been given—as normal courtesy would require—a preview of this broadside, I would have pointed out to its first author that he was lightly taking on himself a particularly heavy burden of responsibility in trying to "synthesize and extend" the contributions of authors like Hennig and Croizat (Popper comes in strictly as window-dressing), who, to start with, have found their work incompatible (Nelson has overlooked Croizat (1978), here a particularly significant title). Hennig is a *dispersalist*, who true to the style of his sect systematically overlooks Croizat as inconvenient and unanswerable; Croizat is a *(pan)biogeographer* whose thinking and work (witness the content of the indexes of his major works) is beyond comparison richer than the glittering generalities—hard indeed to apply concretely—constituting the contributions of Hennig. I do, of course, reject as unwarranted Nelson's affirmation that the contributions of Hennig and Croizat are "substantially similar."

Whatever the case, in this case as always, *the proof of the pudding is in the eating*, and there is no difficulty in learning what sort of taste the pudding cooked by Nelson with the assistance of Platnick has left, after over 500 dreary pages, in the mouth of its own makers.

This is what they conclude in the last five lines of the Epilogue to their *opus magnum* (1981:543): "What we have, then, are unanswered questions: about organisms in general, about their interrelationships, about areas of endemism, about their interrelationships .... We hope that this volume may at last focus attention on the questions, and perhaps lead to some answers as well."

So far, so good, and Nelson can be praised for a final outburst of truth, acknowledging that he has wasted, with Platnick as helper, a whole book ending in futility. His candor at this point deserves plaudits, but his judgement and knowledge are evidently at fault. To finally disqualify him as a biogeographer—whatever the class and description—are two figures (1981, fig. 8.42, p. 515; fig. 8.43, p. 516) he borrows straight from Hennig (1966: fig. 3g, p. 135; fig. 40, p. 136) without becoming aware that what Hennig—no biogeographer at all, indeed—palms off as evidence of a fanciful *progression rule* is, on the very face of

Figure 8.42, the figuration of two contrasting clines coming to a head between New Guinea and the Solomons/New Hebrides. I could without difficulty extend the bill of particulars proving that Nelson did bite much more than he could chew, but I have no space for it here.

Naturally, I deeply resent that my lifework, *Panbiogeography*, has been dragged in with Hennigism to the very extent of publicly losing its identity under the improper designation of "Vicariance Biogeography." There can be here no question of opinion, personal vanity and antagonism. The fact plainly is that an enormous amount of confusion now, and for long years, alas, to come, will be imputable to Gareth Nelson for having overambitiously pretended to "fruitfully" synthesize—imagine that—Hennig with Croizat, hoping thus to achieve for himself the highest possible mark in historic and scientific biogeography.

With this, I close considerations that the coming section of this article will properly extend and qualify. Time is already overdue to call in the scores, so straight from the shoulder, there being no other way out of a mess of overwhelming proportions.

WHAT REALLY IS VICARIANCE/VICARIISM?

Beyond argument, most unfortunate is the mention of the term *vicariance* in the following text (1981:524) by Nelson and Platnick: "Croizat's panbiogeography . . . fused with cladistics . . . matured into vicariance biogeography." The uninformed reader of this supposedly authoritative statement will understand virtually as a matter of course that *vicariance* is the particular invention of the father, of course, of "vicariance biogeography."

This is not so at all. When in Galapagos about a century and a half ago, Darwin saw and reported most distinctly distributions of vicariant type in the various islands of that archipelago. Not only that, but Darwin coined in connection with vicariance the term *representative species* that we still use today in its original sense. I cannot deal here with the literature that establishes the facts in the case, but the reader will find it in Croizat (1981:509 ff.). As a landmark in the history of scientific thinking this literature is well worth knowing.

It is impossible for me, on account of strictures of printing space, to deal in general with *vicariance/vicariism*. I will, however, bring here before the reader some examples of vicariant distribution that will clarify his understanding!

I refer for the purpose, strictly as a matter of present convenience, to the records of the classification of certain birds, Piciformes (Woodpeckers, *sensu latissimo*). In the by now aged classification of James L. Peters (1948)—no classification was, is and ever shall be perfect, with or without cladism, Hennigism, Popper and "philoso-mathematicobiologists" to boot—these birds fall in the following families: Galbulidae, Bucconidae, Capitonidae, Indicatoridae, Ramphastidae, and Picidae. By right of geography, Galbulidae, Bucconidae, and Ramphastidae are tropical American; Indicatoridae tropical African and Asiatic; Capitonidae tropical African and tropical American; and Picidae worldwide to the notable exception of Madagascar, Malaysia east of Celebes, New Guinea, Australia, New Zealand and Polynesia. This is an exception, common to Piciformes in general, that no competent biogeographer will ever agree to overlook.

This mass of birds—evidently distributed *vicariously* in various ends of the geographic world without much regard of its present outlines of land and water—totals 38 genera, 212 species, 621 (nonnominal) races, at home from the hottest tropics to the cold north. Facing this much of life, the devout "zoogeographer," that is to say, the follower of Darwin, Wallace, Matthew, Simpson, Mayr, Darlington etc. is being offered the assistance of a *theory* resting its case on three essential concepts, as follows: (a) The "species" originates in a *center of origin* of its own; (b) It eventually *emigrates* actively out of this center; (c) For the purpose, it uses the *means of dispersal* that are particularly its own, wind, storm, animals etc.

I have written thousands of pages in English, French, and Spanish (see, for example, Croizat, 1952, 1958, 1961, 1964, 1968a, 1968b, 1976, etc.) to dispose of this theory, currently understood today as *dispersalism/dispersalistic biogeography* etc. Those of the readers of this article who do not care to read "Croizat's stuff," ought to be at least so generous as to finger through its indexes before going to print half-baked.

It should indeed be difficult to feel much sympathy for an author who goes to print on questions involving *vicariance* as between Croizat and Nelson, while overlooking the 100 references to *vicariance/vicariism* carefully listed, p. 875, of the index of Croizat (1964). What Croizat thought of the subject, long before Nelson came to handle it for his own purposes, brooks no doubt, and must not be confused and mixed up to crosspurpose with other concepts. Should it ever be so that Croizat *vicariance* requires being placed into synonymy under the "vicariance" of some other author, the deal must be arranged without distortion of the record. Perfectly understood by Rosen (1975), this fundamental principle has not been respected by others.

To document the incredible state of confusion in which "vicariance" and "vicariance biogeography" happen to be lost today, the declarations of Brian Rosen of the British Museum, quoted in the introduction of the present article, should be sufficient. In the United States, etc., the situation is not a bit clearer, however. George Gaylord Simpson who for over a quarter century has imposed together with Ernst Mayr et al. an unflinching monopoly of dispersalism, has recently published two sizeable works, both dated 1980 (Simpson, 1980a, 1980b). In one (1980a) the "so-called vicariance biogeography" is dismissed (p. 191) as "absurd and not worthy of the attention it has received in some restricted circles." In the other (1980b:252–253), Simpson writes that there is conflict of opinion in regard of the "general principles of biogeography." One of them, called "vicariance" according to Simpson, simply means that "related species evolved in geographically separated centers"; in this understanding vicariance is virtually a synonym of allopatry. This view is opposed by another one, which Simpson leaves unnamed but which is clearly dispersalism, the doctrine to which Simpson clung for long decades, and Croizat relentlessly opposed for some thirty years.

This opposition Simpson has never forgiven, and to have Croizat properly castigated, he made for himself a taboo never to so much as to write down my name. He feelingly writes of "some few enthusiasts" having maintainted that "absurdity" (meaning vicariance) as even "to descend to personal vituperation"! In the end, Simpson, forgetting his year-long, firm faith in dispersalism, concludes (p. 253) that: "A reasonable biogeographer is neither a vicarist nor a dispersalist but an eclecticist." I do agree, but with the understanding that a biogeographer must be a vicarist in principle and a dispersalist in detail, case by case according to the merits of each case. I was of this opinion in 1964 (see fig. 44, p. 188; etc.), and am still of it today (Croizat, 1981:509).

Summing up: when mentioning *vicariance* Simpson refers to what is said of it by Croizat (1958, 1964, particularly). He finds it absurd (Simpson, 1980a) as opposed to his dear dispersalism, but cannot avoid granting it a measure of recognition (Simpson, 1980b), as *acceptable.* In the end, Simpson is so badly mixed up on *vicariance* that he innocently speaks from both corners of his mouth. Innocently indeed, for he acts out of confusion even more than by malice against those (understand: Croizat) who have "vituperated" him!

An author in excellent standing, Joel Cracraft—American like Simpson—finds it easy to spike into perdition (Cracraft, 1981:461) Simpson's (1980a) argument.

He, too, speaks of *vicariance*, but it is readily clear that Cracraft's *vicariance* is not the same as Simpson's.

What *Vicariance Biogeography* Cracraft has in mind is made clear by the following synonymy (Cracraft, 1981:460, 161): Croizat et al., 1974; Rosen, 1978; Platnick and Nelson, 1978; Nelson and Platnick, 1981; and the added comment that: "Vicariance Biogeography . . . is itself strongly dependent on cladistic analysis."

This synonymy and comment emanating from so well placed a biologist as Joel Cracraft are challenging, indeed perturbing. None absolutely of my own works depends, strongly or otherwise, on "cladistic analysis." I have been throughout an uncompromising foe of Hennig and all his doings in fields outside specialized taxonomy (Croizat, 1976, 1978). Cracraft is surely informed that my bibliography is the largest of any by biogeographers dead and alive, and why should he short-circuit everything of it by a title of less than thirty pages in which I stand associated with others, some ten years after having published strictly on my own over 8000 pages? Why does not Cracraft refer to Rosen (1975) a work in which Rosen clearly acknowledges the debt he owes to Panbiogeography? Summing up: What is really the status of "Croizat et al., 1974"?

I can answer this question—it is a *vital* one if we ever intend to clear up in our minds the incredible confusion that has grown around "vicariance"—only at the price of sharing with the readers some personal data, first, and some quotations next.

I wrote some time in 1972 alone the contribution later published in 1974 as it is my custom: I do not like to share responsibilities, so I always do or die by myself; and sent it for transmission to my correspondent, Dr. Gareth Nelson, because I did not know at the time in Coro the address of the editor of *Systematic Zoology*. Nelson promptly answered he would, but may I not consent that he, and Rosen (if I well recall, only as an afterthought), join in the publication as *junior author(s)*? I agreed as a matter of courtesy but seeing eventually Croizat et al., 1974 (Croizat, Nelson, and Rosen, 1974) in print I wondered whether the stuff would not be better attributed to Nelson et al. My original ms. had been so retouched etc. that I eventually got censured by Brundin (1981:128) quite rightly on account of the article "Croizat et al., 1974" containing ideas contradictory of Croizat's contributions anterior to 1974.

Much as I regretted this, I felt that "Croizat et al., 1974" would cut very little ice, if any, but I soon found out that I was roundly mistaken. As a matter of the record, this unwelcome paper has achieved—to mine and common detriment—very wide diffusion, and is referred to by a majority of biologists today as a key-piece of my bibliography. It is relatively short, it does not tire, it is easily accessible, and as to the rest, well . . . I happen to be out of luck.

Croizat et al., 1974, is the lone title displaying my name in the bibliography of the review of the Symposium, 1979, held at the American Museum of Natural History of New York. Insofar as I am directly concerned, this review (Ferris, 1980:67) falls—rather challengingly—into two parts. In the second (p. 73), I stand mentioned to advantage to the very extent that: "Though Croizat's name and ideas were mentioned frequently during the symposium, the presentation of his paper came so late in the proceedings that its effect on the gathering was minimal . . . . If the paper had been read earlier, I [Ferris] suspect many of the discussions would have taken a different turn." So far, so good, but I do not well see how this opinion on the part of the relator, Dr. V. R. Ferris, agrees with what she—or someone else—says a little farther in her article (p. 75), to the effect that: "The selection of speakers and discussants was masterly . . . . All sides were heard and carefully considered." Croizat surely was not: his paper is the only one among the twelve recorded for the symposium that was not discussed at all. Somebody it was, that masterly arranged that in order to have the gathering run true to a certain preconceived rut.

Ferris' article stands under the remarkable title: "A Science in Search of a Paradigm? Review of the Symposium, 'Vicariance Biogeography: A Critique.'" Had I known (and see also Tryon Rolla (1981) and Jarvik (1981)) that this was the purpose of the Symposium, I would not contribute to it at all. I see as totally unwarranted (p. 69) that Croizat's Panbiogeography is equated, whatever be the title and reason, to a: "Method of vicariance biogeography dealing only with sister groups on separated land masses." Additionally (p. 67): "The biogeographic method is to 'interpret the geographic distribution of sister groups parsimoniously.'" I do not agree at all with these misinterpretations of my work and ideas.

Even less do I agree (p. 67) that phylogenetic analysis, achieved by means of Hennigian methods, is to precede biogeographic inference. This notion belongs to times and minds when dispersalism ruled the roost, and no concrete analysis of dispersal was possible in the light of its flimsy theories. With biogeography in the saddle, concrete analysis of a biogeographic equation is answerable to a precise method of enquiry: it ascertains the facts in play, and as such comes first before hazy form of theoretical "phylogeny." Those who are enamoured of "cladism," "Hennigism" etc. may theorize to their heart's content, but let everybody respect panbiogeography under its proper name as the royal gate to establishing the *base of fact* on which next to dream.

The showiest bloom—is it really her own?—in Ferris' review lifts its head (p. 69 fn.), as follows: "A problem with the definition of 'vicariance' persisted among those not acquainted with the literature of vicariance biogeography. The word does not appear in anyone's desk dictionary, and the reader is referred to the definition given in Croizat, Nelson, and Rosen, 1974."

This entry is untrue to the historical record and obviously tendentious. Anyone really bent upon knowing what Croizat has preached on vicariance/vicariism over the years needs not refer to the skimpy pages of "Croizat et al., 1974" (Croizat, Nelson, and Rosen, 1974), when Croizat alone, years before 1974, has displayed in the Indexes of his basic works (see particularly 1958, 1961, 1964) long scores of references and cross-references to vicariance/vicariism carefully discussed *in textu*. Croizat et al., 1974—to repeat—has been questioned by Brundin (1981:128) quite correctly as displaying ideas alien to Croizat before 1974. The ideas that are thus questioned (Brundin is a reliable judge in the matter) belong not to Croizat, but to the et al. end of the authorship, almost certainly to Dr. Gareth Nelson.

This same entry is tendentious in addition, because: (a) It leads an innocent reader to take for granted that, indeed, Croizat is the main founder together with Nelson of "Vicariance Biogeography," which is totally false; (b) It induces the same innocent reader to take for granted that Croizat et al., 1974 is indeed a keypiece of Croizat's bibliography, which is absurd; (c) It subtly works to confuse Panbiogeography with "Vicariance Biogeography," which has had catastrophic results on biological thinking by fomenting needless doubts, unjustified questions etc. This adverse condition is quite clearly documented by Brian Rosen in the introductory section of the present article.

CONCLUSIONS

The present article is intended to show that:

(1) Under the ill-fitting name "Vicariance Biogeography" stand today confused two very different streams of thought and praxis, that is, the *Panbiogeography* of Leon Croizat, and the *"Vi-*

cariance Biogeography" by Gareth Nelson as its principal author and promoter.

(2) The *Panbiogeography* (1952–1982) of Croizat is a *method*, the *Vicariance Biogeography* of Nelson a *theory*. The former basically consists of a manner of (statistical) wholly factual investigation of living and fossil records of the geographic distribution of plants and animals, directed to establish the coordinates of *time* and *space* present in organic *evolution* [mainly approached in its aspect of form-making (taxogeny, phylogeny, historical development in general)]. The panbiogeographic method stands beyond question as based on the straight search, analysis and synthesis of factual material. It might naturally, entail errors of judgement on the part of those who make use of its byproducts in the light of personal lack of information, theoretical preconceptions of their own etc., but—*as a method*, to repeat—it is the only really scientific way of thinking and doing. The *track* is essentially a graph drawn to render visible and comparable the results of biogeographic investigation, so not at all a philosophical concept inviting controversy as to its "nature," "validity," philosophical "capacity" etc. It is neither more or less than a symbol like, for instance, the symbol which in mathematics expresses the square root, the fraction etc. This symbol, this graph—to repeat—has for its sole purpose to render visible and comparable the data secured from panbiogeographic investigation.

(3) The *Vicariance Biogeography* of Gareth Nelson et al. (texts vouching with finality for its origin both formally and conceptually are: Nelson and Rosen, 1981:524; Nelson and Platnick, 1981:ix, 543) is a *theory* gotten together by hopefully fusing Croizat's panbiogeography with Hennig's systematic phylogeny.

(4) The results of the *panbiogeographic method* are recorded as positive by reviewers zoological and botanical in excellent standing. In addition to the opinion of Rosen, already mentioned in the text of the present article, I might here quote the judgement of Edwin J. H. Corner, professor of botany of Cambridge University, U.K. This opinion—originally published in the New Phytologist, 1959—is quoted by Gareth Nelson (in Nelson and Rosen, 1981:533–534) without disapproval, and reads: "This [Panbiogeography, 1958] is the amplification of the principles put forward by Croizat in his *Manual of Phytogeography* (1952), their betterment and their application to zoology. I will neither praise nor condemn it, but state that it is the most important contribution in plant and animal distribution that has appeared. It is not a class-book, but an emporium, and right glad we must be that one mind has planned it. Here are plants, animals, rocks, men, books, theories, jewels of genius, and much tripe, displayed with astonishing salesmanship. Here is learning as of Gibbon, garrulity as of Montaigne, homelines as of Bunyan, conceit as of Shaw, pervaded with Darwinian love of nature and common sense. I [Corner] use these unscientific words because Croizat deals with the immense biology which transcends physics and chemistry, which we shall lose if coming generations are not inspired. Panbiogeography is the story of 'Flesh and rocks' and, for the first time, in this bottomless pit of antiquity, I have been shown whereon to stand." I venture to think that my British readers particularly will be impressed by the very warm tone of what Corner thus wrote, and will also appreciate that, as a dyed in-the-wool son of England, Corner may not necessarily love the tripe from beyond the Channel, quite acceptable also to the frog-eating gentry of outlandish regions.

(5) The results achieved by Gareth Nelson's *vicariance biogeography* are, of course, extolled by certain reviewers, whom I will not bring to book here, for no one has better judged of the results of mixing up Croizat with Hennig than Nelson and Platnick themselves, when concluding (p. 543) at the end of over 500 pages replete with cladograms etc. as follows: "What we have, then, are unan-

swered questions: about organisms in general, about their interrelationships, about areas of endemism, about *their* interrelationships, about human populations, and about *their* interrelationships. We hope that this volume [Nelson and Platnick, 1981] may at least focus attention on the questions, and perhaps lead to some answers as well"!

The bleak finality of this conclusion exempts me from the task of fishing here and there in Nelson and Platnick's *opus magnum* evidence by no means scarce that the two authors have bitten more than they could chew. If need will arise, I shall come to that in a coming article.

(6) The oft mentioned and cited "Croizat et al., 1974" contribution fits rather ill with the thousands of pages of my own bibliography. Technically, alas, it hangs heavily on my shoulders; factually, it should be charged to Nelson. Toward D. E. Rosen, I feel deep friendship and understanding.

(7) I never had anything to do with the rigging up, promotion, etc. of "Vicariance Biogeography." Authors who declare, as for example Colin Patterson (1981:446), that Croizat took part in developing vicariance biogeography disseminate a falsehood that goes to increase the already colossal confusion besotting today every aspect of a science of biogeography.

(8) My parting advice to the readers is that before risking to go to print halfbaked, they generously consent at least to finger through the Indexes, Conclusions and Introductions of my main works.

(9) The accusation that panbiogeography is too rigid, too unfeeling to make necessary room for a minimum of "mobilism"/migrationism is quite false. Over a quarter of a century ago I defined dispersal as: translation is space + formmaking, and never altered afterwards my viewpoint, as proved by Croizat (1981: 508–509). The point is that in panbiogeography *immobilism*, as the maker of vicariance comes conceptually first before *mobilism*; the interplay of the two to be assayed in detail case by case. See on *mobilism/immobilism*, to start with (Croizat, 1964:862, 865; sixty direct references).

(10) Vicariism/Vicariance was discovered neither by Nelson nor by Croizat, but observed in act by Darwin—who indeed coined the term "representative species" in connection with its effects on biogeography, classification etc.—when visiting the Galapagos (Croizat, 1964:609–632) a century and a half ago. Some half a century after Darwin, Kleinschmidt (op. cit., p. 179) used it to reform taxonomy with the introduction of the concept of "polytopic species." Nelson and Platnick (1981:46–47) entertain of *vicariance* vs. *dispersalism* an understanding that is broadly acceptable, but it is difficult to understand how their mixture of the ideas of Croizat with those of Hennig came to be identified as "Vicariance Biogeography," when the references they give to vicariance in their Index barely reach some ten, while Croizat displays in his main works well over a hundred. Something is here hard indeed to explain.

(11) Little inclined by nature avidly to mix up "philosophy," "mathematics," quest for the "perfect" and the "absolute," etc., with objective investigation, I cannot enthuse over "cladism," whatever it might be supposed to be by one or the other of its adepts, propagandists etc. Nelson and Platnick, surely two authors who understand the subject (1981:512) stand on record as follows: "The point of concern . . . is that dispersal relationships can be rendered as cladograms, which at some level might be informative, or not, in any given case. 'Informative,' of course does not mean 'true,' but only that dispersal relationships may *sometimes* have a cladistic aspect" (emphasis mine). This plainly means—and I agree—that the "cladogram" is no panacea, revealer of hidden, esoteric truth, just one of the many devices to express biological, biogeographic etc. relationships. To illustrate the point; Nelson and Platnick (1981, p. 468 ff.) face different cases of dis-

persal in which Hawaii stands bound by certain plants (*Keyseria, Argemone, Sophora* sect. *Edwardsiana, Xylosma*) with islands and lands situated at different points of the compass. The authors in question try to face "cladistically" the issue, and bring it to final reason, appealing of course to "cladograms" (pp. 473–474, figs. 8.4, 8.5, 8.6, and 8.7). Croizat has *panbiogeographically* examined the status of Hawaii in dispersal (Croizat, 1964:4–15, fig. 1; 542, fig. 73; 1961; 1976: 1339, fig. 123; etc.), with results that Croizat is inclined to believe in no way inferior to those of the most exalted, "philosophical" etc. "cladism."

(12) In a review of Nelson and Rosen, *Vicariance Biogeography* (1981), Doctor Lawrence G. Abele (1982:79–82) includes several statements worthy of attention. For instance: much to the point Abele remarks: "Most of the remaining authors [taking part in the 1979 Symposium] use the volume as a forum to present their own ideas of biogeography and deal with current methods of vicariance biogeography only indirectly. Perhaps this straying from the point could have been avoided if the symposium had started with a clear statement of vicariance method and definitions of terminology (especially dispersal, mobilism, and immobilism)." So far this reviewer, who—unfortunately for him and American science at large—would be very amply informed about what he most wished to know if he had generously consented to take in hand Croizat's *Space, Time, Form: The Biological Synthesis*, 1964, to read pp. 4–15, and fingered its Indexes. No doubt, the 1979 New York affair was not properly presented: while most of the readers of its results believed that it would be concerned with clarifying general questions, Gareth Nelson and his friends had other things in view; that is, to promote a "new" brand of "vicariance biogeography" of their own making. This slant plainly away from normal anticipations has backfired and filled the study of a science of dispersal with an incredible confusion of purposes, ideas and results.

(13) Of Croizat personally, Abele (1982) says: "Croizat has long been an enigmatic and controversial figure in North America, having been referred to as a 'member of the lunatic fringe' by G. G. Simpson and as having a 'totally unscientific style and methodology' by E. Mayr."

Of course, all of this is slightly exaggerated, and as such hardly worthy of attention. However, a few lines may be contributed once at last revealing the highlights of my *curriculum vitae*, as follows: Like every other mortal, I had a date of birth (July 16th, 1894; I am in my 89th year at this writing), and a place of birth (Turin (Torino) in NW Italy). My father and mother were French, had immigrated to Italy around 1860 for reason of business, and belonged to the well-to-do *bourgeoisie* destroyed as a class during the World Wars, 1914–1918, 1938–1945. From my earliest days, I was passionately curious about plants and animals, and never doubted I would study the natural sciences. The events of 1914 decided otherwise: I was sucked for some six years into military service, and survived it only by many miracles. All I could do in 1920 was to earn the title of *Doctor Jurisprudentiae* (University of Torino), taking advantage of special terms in favor of veterans. As I prepared to reenter the university in 1922 to graduate as Ph.D., Mussolini began actively to rise, and, to escape the worst, I was forced to leave Italy and Europe. In 1923, I settled in the United States and had a hard time until I secured a job with the Arnold Arboretum of Harvard University mapping the grounds. When the job ended, I remained in the employ of Harvard as technical assistant at $30 weekly salary. As such, the situation I had did not look brilliant, but it had for me at least certain priceless advantages such as: (1) Access to a superb library; (2) Free use of a magnificent herbarium and live plants; (3) Plenty of free time of my own; (4) A comfortable apartment on the grounds of the Arboretum; (5) A small greenhouse to shelter my private collection of xerophytes. In possession of these hardly credible advantages, master of Latin and all the languages of science (English, French, German, Russian, Spanish, Italian etc.), I formed the plan not to give myself up to some specialized form of research, taxonomic, anatomic, etc., but to investigate for myself overall the sinews and marrows of the thinking of botany from the days of Caesalpin (or, about 1600). In a morning spent "at the stacks," I could consort with the best (and the worst) of minds busy during the centuries with all kinds of botanical subjects in Berlin, Paris, Boston, London, Moscow etc., and drink their juices to my profit. Ten years, 1938–1947, day in day out, of this kind of "project," taught me what others never had the opportunity of learning, so not because Croizat was necessarily smarter, but because he had ways and means to use denied to the rest. This is the reason why I am "enigmatic," "controversial," "lunatic," "unscientific" etc.; and it is a reason which I have no reason to conceal, and to lament. I left the States for Venezuela only a few months short of tenure when I was dismissed from Harvard following the defenestration of Dr. E. D. Merrill, who had hired me. In Venezuela from February 1947, I found time and means to publish my work with exception of Croizat (1952), already written when still in Harvard. Naturally, my work is, if not technically "Harvardian," based on what I learned during 1938–1947 in Cambridge, Mass., embodied in about 400 booklets of original notes, sketches, references etc. which left the States with me. My feeling is that Harvard University might have gained, had I remained in its membership, and been allowed to reveal my ideas in peace through the press. The reason why I still live in Venezuela in spite of never having broken contacts with the English-speaking world is transparent. I have in Venezuela a wife and house, and past my 88th birthday I am still employed with full pay in a technical capacity of potentially international scope.

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PHYLOGENETICS, AREAS, GEOLOGY AND THE BIOGEOGRAPHY OF CROIZAT: A RADICAL VIEW

Radical (3): Going to the root or origin; touching upon what is essential and fundamental; thorough.
Oxford English Dictionary.

As part of an ongoing study of the *panbiogeography* of New Zealand, based on the approach of Croizat (1952, 1958, 1964), a number of points that seem to be of general interest to all historical biogeographers have emerged. These points are presented below in the form of a critique directed against recent *vicariance* phytozoogeography studies, in order to emphasize the fundamental importance of the work of Croizat to historical biogeographic studies.

CROIZAT'S THEORETICAL AND PRACTICAL APPRECIATION OF PHYLOGENETICS

Apparently taking their cue from Cracraft (1975a) and Ball (1976), a number of authors (e.g., McDowall, 1978; Patterson, 1981a, b) have claimed that "phylogenetic relationships" play no role in the biogeography of Croizat. In the cases of Cracraft (1975) and McDowall (1978) this claim was shown to be false, and based