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Author

Holland, Brenden

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book review

If the conceptual straitjacket fits, chances are, you're already wearing it

Molecular Panbiogeography of the Tropics. Michael J. Heads, 2012, University of California Press. 565 pp. \$75.00, £52.00 (hardback) ISBN: 9780520271968; <http://www.cambridge.org>

This is part of a series of volumes aimed at, in the words of its editors, examining “the role of descriptive taxonomy, its fusion with cyber-infrastructure, its future within biodiversity studies and its importance as an empirical science.” The author’s stated intention is to attempt to demonstrate “patterns of community immobilism leading to allopatric differentiation, as well as other patterns of mobilism, range expansion, and overlap of taxa.” It was with these themes in mind that I delved into Heads’ latest work.

But first, in the spirit of full disclosure, I have encountered the author of this book, as well as several of his fellow panbiogeographers, from the New Zealand as well as the New York schools of the subdiscipline, which together comprise a vocal minority in biogeography. My interactions with the panbiogeographers have come via a number of academic exchanges, each of which has unofficially ended in an understood ‘agree to disagree’, intellectual truce. Receipt of this review request generated feelings of both hesitancy and curiosity. I was hesitant to spend time on this topic because each prior exchange with Heads and his associates has boiled down to the willingness on my part (and that of our colleagues in mainstream phylogeography), to approach biogeographic data from a perspective that is open to both dispersal- and vicariance-based interpretation. By contrast, panbiogeographers strongly reject the significance of dispersal *a priori*, in any form. Based on past interactions and published exchanges with this group (e.g., Holland and Cowie 2006, Nelson 2006), a clear way forward in this dialogue has yet to materialize. My curiosity came from the chance to evaluate this recent offering, being due in part to my admitted lack of appreciation of the usefulness of panbiogeography, and the possibility that my views might change given the “new” panbiogeographic per-

spective promised in the Preface.

Perhaps a brief refresher on the nuts and bolts of the field is warranted, since panbiogeographic reasoning can be perplexing for a number of reasons, not the least of which is that its proponents have created their own terminology. The field was proposed by Léon Croizat in 1958, and furthered by M.J. Heads as well as R.C. Craw and J. Grehan. Panbiogeography was defined by Craw et al. (1999) as a method whereby known species distributions are first mapped and then extended by drawing connecting lines on maps. Lines are termed ‘tracks’ and are drawn such that they ‘connect’ collection localities or disjunct distributions of a particular taxon. Multiple overlapping tracks for unrelated taxa form ‘generalized tracks’. The figures presented in the new book consist of dozens of black-and-white maps with lines and shapes superimposed to indicate distribution patterns.

Croizat was correct and ahead of his time in realizing that speciation can occur after emergence of a barrier within the existing boundaries of a distribution, a phenomenon that ultimately became known as vicariance. But this somehow led him, and the panbiogeographic movement which followed, to vehemently deny, *a priori*, any role of dispersal in natural history. Croizat (1964) stated “I intend to destroy these notions (pertaining to ‘means of dispersal’) because they positively interfere with the advance of knowledge on far-reaching points of evolution over space, in time, by form” (p. vi). This vague notion of evolution in space and time by form is repeated frequently in panbiogeographic publications. According to Cox and Moore (2005) “Even after the theory of plate tectonics had been well documented and widely accepted, Croizat refused to accept it, and never integrated it into his methodology” (p. 31). Heads also expresses a refusal to

acknowledge accepted tectonic models and instead focuses on his ideas of the 'immobilism' of lineages. According to this idea, originally articulated by Croizat, continents are viewed as 'composite terranes' and come with their resident biota intact. Panbiogeographers argue that organisms have *always* occupied the areas where they are found today, and that these areas had been colonized via slow continuous spread *over land*. Regarding the continental drift model, Craw (1985) wrote: "one cannot pull them back into a single supercontinent as Wegener did, and modern plate tectonic proponents do." Croizat (1958) stated that the concept of *Pangea* was "wrong" because it conflicted with "evidence" he had obtained from "his purely biogeographic studies", which I assume refers to the drawing of lines on maps with a pencil and a ruler. The panbiogeographic position on geologic history and plate tectonics has been reviewed at length by Cox (1998).

Heads, like the other panbiogeographers, fervently rejects dispersal followed by speciation as a possible mechanism influencing an organism's distribution under any circumstance. This radical, recurrent theme is seen throughout the book (multiple times per chapter) as Heads forces observed data to fit the preconception that there is no dispersal, aside from what he and other panbiogeographers term 'normal ecological dispersal'. The ideas presented in this book are anything but "new". The underpinnings of panbiogeography were summarized by Craw (1984) as Croizat's "bold and novel attempts to empirically refute" the work of Charles Darwin, Alfred Russel Wallace and Alfred Wegener (p. 8). Implicit in many of the arguments of panbiogeography is the idea that fossils of common ancestors represent minimum (rather than the standard interpretation as maximum) ages of extant lineages, and there were emergent Pacific land masses, some of which connected the far-flung island archipelagos and continents surrounding the Pacific basin. The New Zealand panbiogeographers—including Heads, Craw and Grehan—claim that Croizat's concept of 'horstian dispersal' has been validated by the recognition of 'accreted terranes' embedded in the

Pacific margin of the Americas, and that further support has been given by the hypothesis that some of the terranes might originally have been part of the sunken 'Pacifica' continent. According to this scenario, oceanic islands were formerly mountain peaks (e.g., Heads' so-called "Mid-Pacific Mountains", Figure 7-1, p. 315). This will sound familiar to many readers, since this extraordinary idea was proposed in various iterations in the 18th and 19th centuries. Given that this was an era where advanced technology consisted of wooden sailing ships, sextants and sounding lines, what sounds peculiar today was perfectly reasonable, even brilliant reasoning at the time. The original "mid-Pacific plate" hypothesis preceded by several centuries the advent of modern scientific techniques (K–Ar radiocarbon dating; high-resolution, side-scan, multibeam sonar; GPS tracking) and a detailed understanding of the ages and dynamics of tectonic plates. So the panbiogeographers intentionally not only ignore or misinterpret many important advances in natural science (molecular clock theory, plate tectonics, continental drift, radio-carbon dating, oceanic island formation, erosion and subsidence) but they seem to imply that they would prefer to turn back the hands of time to a period of technological simplicity and scientific ignorance, in order to fulfill their agenda.

The first two chapters of *Molecular Panbiogeography of the Tropics* revisit the father of panbiogeography's semi-metaphysical "evolution in space and time by form" ideas, and present Heads' own take on his effort to go against the grain of "biogeographic interpretation over the last 2,000 years" (p. 2). Chapters 3–5 present scenarios and offer explanations of origins and relationships between New and Old World primates, each unsurprisingly chosen to favor the vicariant agenda.

The next three chapters concern what seems to be one of Heads', and my own, favorite topics: the evolution of life on insular Pacific islands.

It is here (Chapter 7) that Heads' recurrent thesis, that long-distance dispersal is not a factor in evolutionary history, reaches its pinnacle. This chapter deals with what he refers to as "the near-universal consensus" (p. 314) that Hawaiian Island

biota largely derives from long-distance dispersal, followed by adaptation and radiation. The nature of his argument is that we should use caution prior to deducing that species ever colonize oceanic islands and evolve phyletically in place. He suggests that arcs of once-emergent ancient seamounts between, say, the Hawaiian and Marquesan islands played a role, by harboring the lineages we see today (which would surely necessitate dispersal via island hopping?), over tens of millions of years. But again this would require numerous departures from what is understood and accepted by geophysicists. He presents maps showing filled-in outlines of implied land masses in places where there is no hydrographic or geophysical evidence that such landscapes ever existed (Figure 7-1 p. 315 and 7-2 p. 316). Even though these features are described in the figure captions as “2,000”, “4,000” and “5,000 m isobaths”, the figures appear to be a disingenuous and misleading depiction aimed at advancing the vicariant agenda. To me, this seems a high interpretive price to pay in order to avoid founder speciation.

Heads argues further, attempting to cast doubt on established geological principles, that the Pacific hot spot position is not well characterized, that mantle plumes may not exist, that the Hawaiian Islands may have once been joined with other land masses, and “In fact the whole subject of intraplate volcanism is currently being debated among geologists” (p. 317). I wonder which geologists? Each case study ultimately devolves into an endeavor to cast suspicion on conventional reasoning, and hence promote one or more of the recurring themes, discounting: the role of long-distance dispersal, founder speciation, center of origin and the use of fossil ages and/or molecular divergence values to estimate lineage ages. The author invokes vicariant forces in each and every instance. Examples include endemic oceanic island lineages, whether strong-flying insects and birds, marine taxa such as cetaceans or invertebrates and fish with a planktonic larval phase. All of this adds up to a strategy reminiscent of other efforts aimed at undermining confidence in empirical science, namely the creationist/intelligent design

and climate change denial movements. Similarities in approach include attempts to cast doubt, exaggerate controversy and overplay scientific uncertainty where convenient, in support of the agenda. For example “maps of the seafloor are far from complete”, and rates of molecular evolution are deemed universally “elastic” and therefore unreliable. In Heads’ world, even great white sharks are not permitted to have attained their present distribution and genetic structure via dispersal in the Pacific basin, but “instead, the pattern could be the result of vicariance in a widespread ancestor...the westward annual migrations of the populations may have been extended a few centimeters every year in step with the geological movement” (p. 332).

Early in Chapter 8, regarding the islands of Maui and Hawaii, and against all geophysical evidence, Heads states that “there may have been land between the two islands” (p. 357). Later he goes on to explain the extraordinary endemism of the Hawaiian Islands using wild speculation: “For example, if all of North America were flooded except for one central area the size of the Hawaiian Islands, and this was preserved and isolated from other continents, after 70 million years it would probably have a very high level of endemism, assuming there was little or no colonization of the community from outside areas” (p. 388). He then attempts to critique the widely held, useful biogeographic concept of disharmonic distributions of island biota, by stating that were comparisons made among continental species, since they are not identical, they could be termed “disharmonic” as well (p. 388). Conveniently overlooked in Heads’ interpretation here are ideas of biogeographic dispersal barriers, filters, missing lineages, sweepstake routes (e.g., Cox & Moore 2005), and the idea that geographically isolated islands tend to lack entire categories of low-vagility flora and fauna and have disproportionately high biodiversity of dispersing lineages (Cowie & Holland 2008). Heads’ approach renders scientific evaluation of the panbiogeographic ideas presented difficult. This book inadequately addresses the stated goals of the series, especially in terms of examining the role of descriptive tax-

onomy in biodiversity and empirical science. There is nothing empirical about determining an outcome in advance, then forcing all observed data to fit the preconception. I will go one step further and argue that this approach is antithetical to the scientific process.

After spending time with this book I view it as less of a scientific publication than a 10-chapter philosophical argument, continuing the promotional efforts of the panbiogeographic agenda in all of its *a priori* glory. This is an attempt to sway the reader to consent to a peculiar conceptual framework lacking in scientific rigor. The author has expended a commendable amount of effort to review the relevant systematic literature, only to incessantly return to the central views delineated by preceding panbiogeographic efforts. Where this book succeeds is in convincing me that the central incongruities between approaches of conventional biogeographic science and those of panbiogeography are fundamental, and emanate from opposing interpretations of scientific data.

In one interesting passage, Heads states “It seems that the progression rule has become a conceptual straitjacket” (p. 360). On the contrary, the progression rule is used by conventional scientists as a theoretical starting point, a model from which we discover and acknowledge both departures and adherence equally, readily and frequently (Cowie and Holland 2008). Close evaluation of panbiogeography suggests that the colorful term “conceptual straitjacket”, used by Heads himself, is a flawless fit for their own mode of reasoning. Heads claims, and I agree, that “a healthy skepticism is one of the pillars of science” (p. 3). Perhaps where we begin to disagree is on precisely what is a “healthy skepticism”. Panbiogeographic skepticism is in my view agenda-driven, obdurate and misdirected. This relates to my personal tolerance for both how far afield alternative viewpoints *in science* can go, as well as the validity of the justification underpinning iconoclastic positions.

I echo the statement in the review of panbiogeography by Cox (1998) that it is time for panbiogeographers to abandon their defensive position, stop objecting that conventional biologists do not take their work seriously, and instead begin to ad-

dress the criticisms that have been leveled at their work for decades. In the wishful words of Craw (1984) “...geophysical findings...promise to corroborate Croizat’s biogeographic work, in novel and exciting ways”. Alas, this was not to be.

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Brenden S. Holland

Pacific Biosciences Research Center, University of Hawaii, Honolulu, USA; bholland@hawaii.edu

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