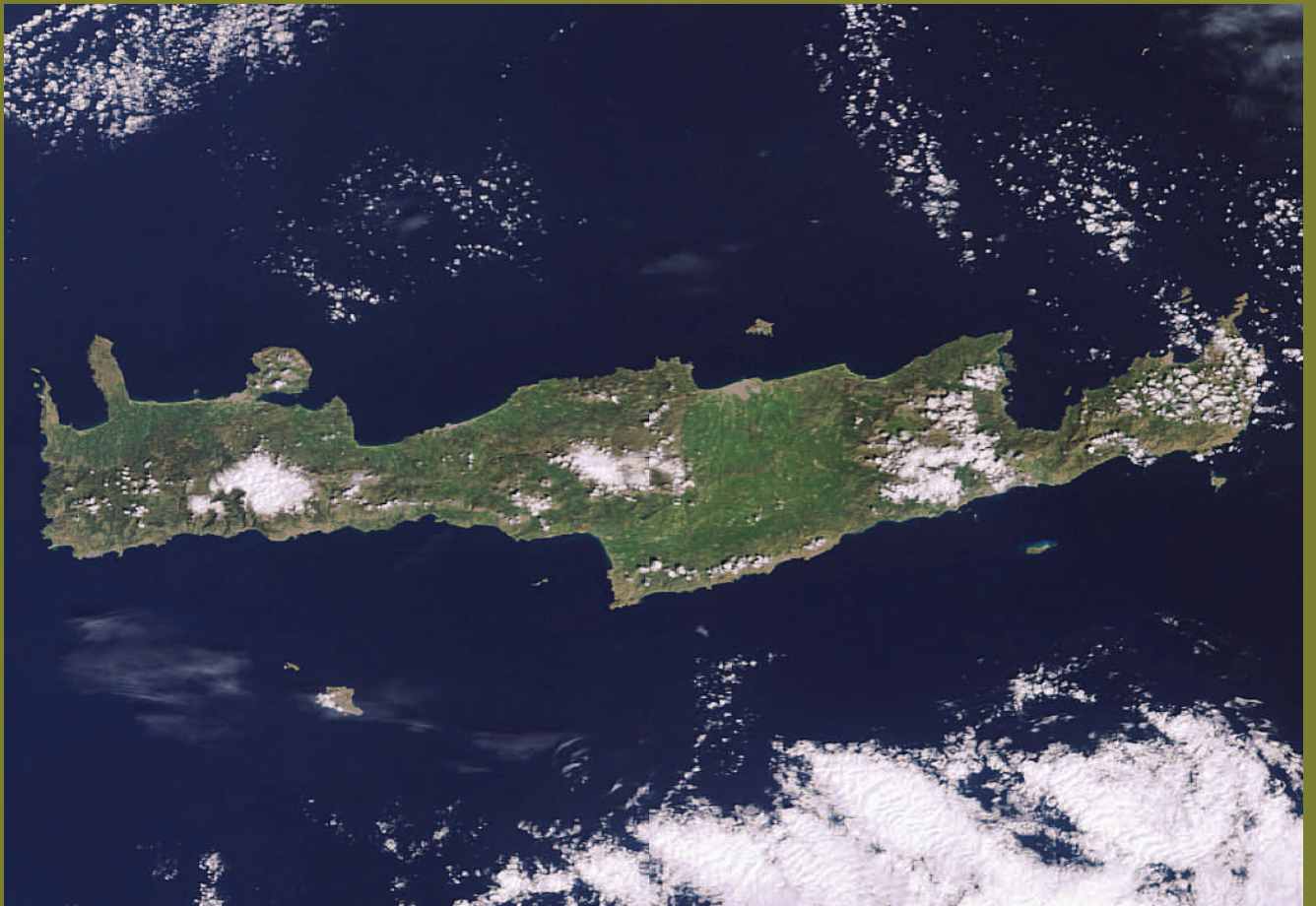




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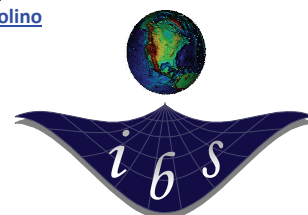
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cover: The Shoreline of Crete, Greece, March 26th 2009. Image and composition from Chelys, <http://www.eosnap.com/>.

commentary

Tree refugia and slow forest development in response to post-LGM warming in North-Eastern European Russia

Though the Postglacial period counts its thousands of years, it was not indefinitely long, and few plants that merely scatter their seed could advance more than a yard in a year [...]. The oak, to gain its present most northerly position in North Britain after being driven out by the cold, probably had to travel fully six hundred miles, and this without external aid would take something like a million years. (Clement Reid 1899, p. 25)

Driven by an urge to understand how the composition of the British flora arose, Clement Reid was puzzled by the discrepancy between the apparent low dispersal capacity of big seeded trees and the high postglacial migration rates deduced from fossil evidence. Possible explanations resolving Reid's paradox, as it was later termed, were fast migration by long-distance dispersal events or glacial survival of small scattered populations at sheltered sites further north than evidenced by the fossil record (Reid 1899, Skellam 1951). More than 100 years have passed since the early writings of Reid, but two questions leading from his thoughts are still debated: *How far north did tree species survive the Last Glacial Maximum (LGM, ca. 21 kya)? How fast did tree species spread and thus current forest structure develop during post-LGM warming?*

A new paleoecological study by Väiliranta et al. (2011, *Journal of Biogeography*) provides, in addition to several other recent studies (e.g., Binney et al. 2009, Heikkilä et al. 2009), insight into these unresolved questions. Väiliranta et al. (2011) summarized macrofossil and stomata records from five sites at the late-glacial–early Holocene transition at the European Russian arctic tree line; an area east of the margin of the Scandinavian ice sheet and west of the Ural Mountains (Fig. 1). Their data evidenced the presence of Norway spruce (*Picea abies*) and tree birch (*Betula pendula/pubescens*) during the early Holocene (11,500–10,000 cal. yr BP) at all five sites. At one site, two radiocarbon dates even suggested the presence of these taxa up to 1000 yrs prior to the beginning of the Holocene. Historical biogeogra-

phy, however, depends on reliable chronologies of fossil records. Ideally, each site should have a well established chronology based on an adequate number of radiocarbon dated fragile and precisely identified terrestrial plant remains that show no signs of redeposition. In this respect, the chronologies of Väiliranta et al. (2011) were not optimal and the late-glacial presence of *P. abies* and tree *Betula* can be questioned, but their early Holocene records were well documented. Hence, the smoking gun, i.e., direct palaeoecological proof of LGM-populations, might be evasive, but scattered glacial and widespread early Holocene fossil records indeed reflect consistent patterns of early expansions in North-Eastern European Russia. Furthermore, independent climate reconstructions are crucial when evaluating the possible LGM survival of boreal tree populations so far north. Glacier-based climate models do indeed indicate relatively modest LGM temperature depressions in the arctic Ural Mountains (Allen et al. 2008). This might have facilitated survival of trees. Besides, the ability to produce krummholz forms (Öberg & Kullman 2011) or other strategies for non-reproductive persistence through millennia (Bhagwat & Willis 2008) may have been decisive. Nevertheless, as concluded by Väiliranta et al. (2011), it certainly seems likely that postglacial development of high-latitude forests occurred through expansion of local tree populations rather than through re-colonisation from distant southern refugia.

Using molecular markers, recent phylogeographic studies for both *P. abies* and *Betula* support glacial survival in north-eastern Europe (Maliouchenko et al. 2007, Tollefsrud et al. 2008). For example, *P. abies* shows distinct gene pools of similar within-population genetic diversity for northern and central-southern Europe (Tollefsrud et al. 2008): a pattern that supports west expansion from such north-eastern refugia and not re-colonisation of northern Europe from the south. For decades, pollen data have suggested early

Holocene north-eastern expansion of *P. abies* and tree *Betula* (e.g., Huntley & Birks 1983, Lang 1994). This notion is now supported and extended by the results of Välranta et al. (2011) together with other recent macrofossil evidence (Fig. 1), models of suitable climate conditions (e.g. Svenning et al. 2008), and insight from phylogeographic studies (Maliouchenko et al. 2007, Tollefsrud et al. 2008).

The three lines of evidence (paleoecological, modelling, and phylogeographical) highlight that the traditional view of Central Europe (including the Russian Plain) as mainly treeless at the LGM needs revision: the ice-free European Russian Plain but also sheltered valleys in the Ural Mountains possibly acted as LGM refugia for boreal tree species serving as sources for colonisation during post-LGM times. Evidence for glacial survival of temperate tree species, however, is still restricted to southern and more rarely, Central Europe north of the Alps (e.g., Bhagwat & Willis 2008, Svenning et al. 2008).

The implications of these results for our understanding of postglacial migration rates remain unclear. While evidence of more northern refugia might help explaining Reid's paradox for some species, the role of long-distance dispersal

during migration and for initiating early Holocene populations still remains undecided. Some recent paleoecological evidence suggests rapid responses of species to Quaternary climate warming (Öberg & Kullman 2011) while others suggest migrational lags (cf. Willis et al. 2010). Interestingly, even with survival of tree species as far north as suggested by Välranta et al. (2011), forest development was indicated with lags of 800 to 3000 years. Slow soil reactions due to relaxation of permafrost severity but also many other factors may have led to such considerable time-lags (Välranta et al. 2011).

In summary, the study of Välranta et al. (2011) consolidates our understanding that north-eastern tree refugia contributed importantly in the rapid re-colonization of northern Europe but also highlights that forest development was rather slow even though these northern nuclei existed. We conclude that in order to understand the spread of boreal tree and other plant species with post-LGM warming, the areas of the Russian plain and Ural need special attention.

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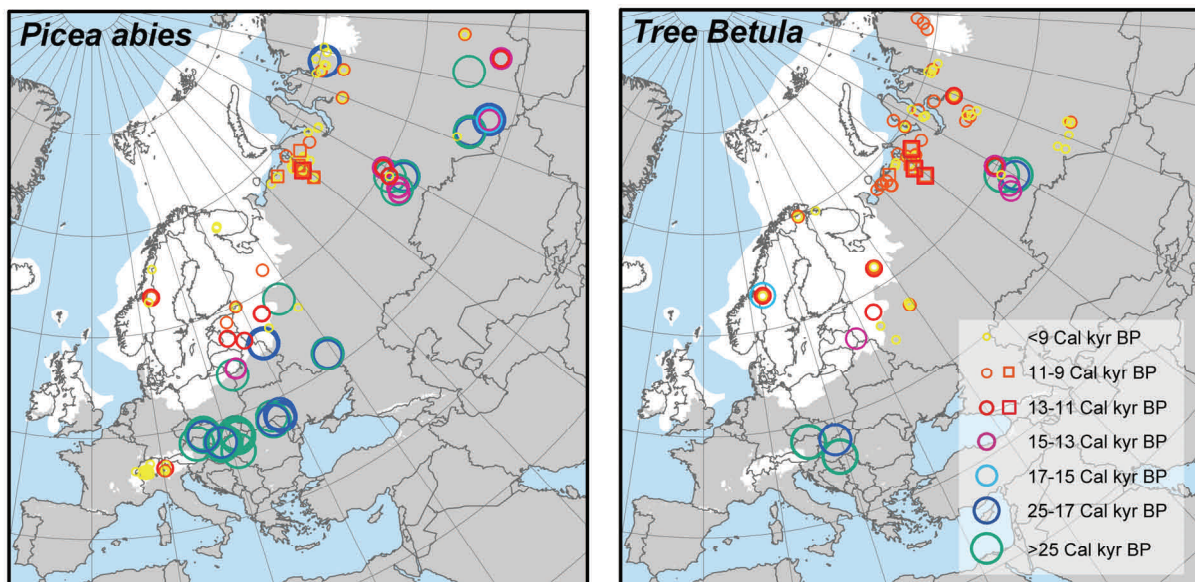


Figure 1. Distribution of macrofossil and stomata records for *Picea* and tree *Betula* in Eurasia. The macrofossil records from Välranta et al. (2011) are indicated with squares. Additional data were compiled from the Northern Eurasian Macrofossil Database (Binney et al. 2009), the European Pollen Database (<http://europeanpollendatabase.net/data/>), Heikkilä et al. (2009), Koff (unpublished), and Willis & Van Andel (2004). The LGM ice sheet extent follows Ehlers & Gibbard (2004). Data were compiled with focus on late-glacial–early Holocene records in Eastern Europe and Eurasia but cannot be claimed to be complete.

Minna Väliranta for kindly providing the Northern Eurasian Macrofossil Database and the geographic coordinates for Lake Kharinei as well as Tiiu Koff for an unpublished late-glacial macrofossil record of *Picea* from Latvia.

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Edited by Camilla Fløjgaard

commentary / book review

Island Biogeography: Paradigm Lost?

The Theory of Island Biogeography Revisited, ed. by Jonathan B. Losos and Robert E. Ricklefs
Princeton University Press, 2009, 476 pp. ISBN 978-0-691-13652-3

<http://press.princeton.edu/>

It is axiomatic that we can answer only those questions that we ask, and paradigm shifts in any field of study often consist of dramatic changes in perspective that facilitate the emergence of a large number of new questions. This simple definition certainly describes the impact “The Theory of Island Biogeography” (MacArthur and Wilson, 1967) had on ecology and biogeography. In a matter of a few years, a largely descriptive, non-quantitative, historically-oriented approach was largely supplanted by one that was quantitative and hypothesis-testing, and heavily influenced by a single, visually simple, conceptually intriguing ecological model - the iconic equilibrium model. Its strong emphasis on the impact of varying rates of colonization and extinction on species richness, and the implied levels of turnover in community composition, led quickly to the development of quantitative ecological models that were applied not only to islands but to island-like habitats and habitat fragments, contributing greatly to the early development of conservation biology. These perspectives and insights continue to ramify, leading ever further into broader and broader aspects of ecology and evolution, with no end apparent. In hindsight, it seems that MacArthur and Wilson’s 1967 book, and the earlier but less influential 1963 paper, mark the boundary of an epic sea-change: there is biogeography before the equilibrium model, and there is biogeography after the model.

Or so it would seem, and so it is viewed by some of the authors who participated in the conference held in 2007 that celebrated the contributions of MacArthur and Wilson’s book and the paradigm shift it brought about, which led to the edited volume that is the subject of this review. But this narrative is certainly too simple in several ways.

Perhaps it is inevitable that when a powerful new idea becomes widely accepted and forms

the basis of a dominant conceptual paradigm, it becomes iconic - a symbol of a world-view, a way of placing all new information into a single framework. In the case of MacArthur and Wilson’s book, it was not the volume in its entirety that became iconic so much as one specific component, one very specific graph - the crossed, curving lines of the equilibrium model of island biogeography. For many researchers, that one graph *became* the MacArthur and Wilson theory, and gradually most other parts of the book were forgotten. And in the effort to test, refine, and apply the implications of those two curving lines, sometimes blinders came into existence, causing application of the theory to be limited only to those questions that arose from equilibrium model and the assumptions on which it relies.

These two viewpoints - MacArthur and Wilson’s theory as the well-spring of broad and continuing conceptual insights, and the equilibrium model as constraining blinders that have inhibited the development of new questions and entirely new insights - are polar ends of a spectrum of attitudes that are implicit or explicit in all of the 16 chapters that make up the new volume that celebrates 40 years of research in the MacArthur - Wilson paradigm. It is the breadth of world-views in the chapters that, for me, made the book a fascinating array of ways of viewing biogeography and its place in science, and the place of the 1967 book in biogeography. Perhaps more than anything else, this edited volume makes it clear that biogeography and MacArthur and Wilson’s view of biogeography are not the same thing.

Some of the differences in perspective are immediately apparent in the way the new book is introduced. The brief foreword, by Robert May, describes the 1967 book as a seminal contribution to theoretical ecology, and specifically to community ecology, to the extent that he uses the words “island” and biogeography” only in reference to

the title of MacArthur & Wilson's book. In contrast, the Preface by Losos and Ricklefs also emphasizes the role of the 1967 book in the transition from descriptive to analytical approaches, but they explicitly emphasize islands, biogeography, and the changing conceptualization of the dynamics of island biotas.

It is to the credit of the editors that they adopted a liberal approach to the selection of contributors to this volume, in which this contrast between "island biogeography as metaphor for theoretical ecology" and "islands as real places with complex dynamics" is on display. Even so, there is much about island biogeography as a field of study that is left out. Examination of the index illustrates well what is and is not covered here: among the topics generously represented in the index are the terms *archipelagoes*, *birds*, *colonization*, *competition*, *divergence*, *diversity*, *equilibrium*, *exploitation*, *extinction*, *food web*, *habitat fragmentation*, *immigration*, *metapopulation dynamics*, *neutral theory*, *population biology*, *predators*, *trophic regimes*, and *turnover*. Not appearing in the index, and absent conceptually from many chapters in the volume, are the terms *adaptive radiation*, *geology*, *historical*, *persistence*, *phylogeography*, *phylogenetics*, *reproduction*, *speciation*, *tectonics*, and *vicariance*.

Not all of MacArthur and Wilson's insights continue to bear fruit. It is striking that a field of study that blossomed and became prominent in the 1970s and 1980s as an outgrowth of the 1967 volume, that of life history tactics as typified by the terms *r*- and *K*-*selection*, has seemingly disappeared, supporting the contention of Lomolino et al. that a contraction of the 1967 theory took place. Another, greater contraction is discussed below: the exclusion of speciation as an intrinsic, dynamic process in island biogeography.

The volume begins with a brief retrospective by E. O. Wilson on the origin of the 1963 paper and 1967 book, followed by two insightful chapters that focus on the early development and tests of the equilibrium model, by Lomolino et al. and Schoener; these should be required reading for any student of island biogeography. All three chapters make it clear that the intent of the 1967

volume was to de-emphasize the role of geological change and the long-term historical development of island biotas, and to develop a theoretical framework that was ecological, mathematical, and subject to direct experimentation and testing. It was, in my personal estimation, an effort to model the maintenance, not the origin, of biodiversity, because aside from brief sections that Lomolino et al. point out as having been lost in a subsequent contraction of the theory, it explicitly excluded "deep-time" and evolutionary phenomena.

It is tempting to divide the remaining chapters into two groups, those that fall within the framework of the 1967 volume, and especially that of the iconic equilibrium model, and those that strike out into new territory. That, however, would be badly misleading. Chapters by Hanski, Holt, Hubbell, Laurence, Simberloff & Collins, Terborgh, and Velland & Orrock all deal with issues intrinsic to the equilibrium model and theoretical aspects of ecological dynamics of species at a community level, and in that sense they carry on the tradition. However, those by Hanski and Laurence, and Holt to a substantial extent, explicitly view patterns of species richness and community composition as representing the sum of disparate parts that must be analyzed independently, and they decry an "excessive emphasis on species-area relationships" (Hanski, p. 209), and advocate analysis of single-species incidence functions and the complex and hugely varied differential colonization and extinction patterns, based on species' autecology. The contrast could hardly be more extreme with the perspective of Hubbell, who largely accepts the implication of the equilibrium model that "species [are] ecologically equivalent ... in their probabilities of immigrating ... and going extinct" (p. 264). Hubbell sees the simplicity of the equilibrium model as one of its greatest strengths, and states that we should "... add complexity slowly, ... when absolutely necessary, kicking and screaming the whole time" (p. 288). I believe that, based on chapters in this volume, a strong case could be made that, whether or not there has been kicking and screaming, a new paradigm of community ecology has developed that no longer relies on the equilibrium model as the pri-

mary framework, with great opportunity for new advances that lie beyond its iconic simplifying assumptions.

What, then, has changed since 1967? We can identify, somewhat simplistically, changes that have taken place for two different reasons. First, as shown in many of the chapters, there has been a great deal learned, through application and testing of the equilibrium model and its assumptions. This leads inevitably to recognition that the model is limited and imperfect, perhaps typified by Hanski's statement that "... expectations regarding the MacArthur-Wilson model were too high ... and distracted attention from single-species incidence functions, which would have provided much richer material for research ..." (p. 209), and Holt's comments that MacArthur and Wilson's "... island biogeography continues to provide a powerful metaphor for thinking about habitat fragmentation, ... [but] ... this metaphor can be limited, and at times highly misleading. Habitat fragments in some ways are like islands, but in some ways are radically different" (p. 174). These and other chapters point the way to a new and different paradigm of ecological studies of fragmented populations in island-like ecosystems.

A second change since 1967 took place outside the context of the MacArthur-Wilson theory. In 1967, the concept of plate tectonics was new and hotly debated, but soon came to provide a powerful, widely-accepted model for the evolution of the earth's surface features. With it came evidence of "hot-spots" where plumes of magma give rise to chains of oceanic islands over millions of years. Beginning in the late 1970s, the first intimations of what became molecular phylogenetics appeared, leading eventually to today's robust phylogenies. The islands themselves can have their origins and histories dated with increasing precision, and the phylogenies can be temporally calibrated, though the degree of precision is admittedly currently contentious. All of this allows direct investigation of the processes of speciation in ways that were undreamed of in 1967, and it is the process and impact of diversification that is the basis for the second and perhaps more revolutionary of the changes in perspective.

A number of the authors pointed out that it is somewhat ironic that the 1967 volume made so little of the role of speciation in island faunas, given the prominence of islands in the development of the theory of natural selection and Wilson's research on the systematics of ants; indeed, Wilson's earlier paper on taxon cycles relied heavily on phylogenetic diversification as a primary process influencing ants on Pacific islands. In this volume, speciation and diversification processes are highlighted in many of the papers, along a spectrum of conditions. Clegg, Ricklefs, and Grant & Grant show the process of diversification at early stages, demonstrating genetic changes that immediately follow colonization of islands, anagenetic speciation on isolated islands that results in speciation but not in diversification, and the early stages of diversification among closely-related species, respectively. Gillespie & Baldwin and Losos & Parent document the extensive role of diversification as an intrinsic, dynamic element among biotas on remote islands and archipelagoes, and explicitly emphasize the necessity of studying diversity patterns and community composition over large scales of time and space, and they make a strong case that diversification must be considered as a process that is the conceptual equivalent of colonization and extinction in the formation of island biotas. In what makes explicit a substantial paradigm shift, the chapter by Whittaker et al. takes these observations to their necessary conclusion with their recently developed "General Dynamic Model" of oceanic island biogeography. They present this as an extension of the MacArthur & Wilson 1967 model, but their model is so different from the equilibrium model in its assumptions, components, and predictions that it leads biogeographers to ask an entirely different set of questions from those that naturally emerge from the equilibrium model. Rather than setting aside speciation, the new model places diversification in a central role; and rather than treating islands as static entities, oceanic islands are presented as having "life histories" that have direct, measurable, and predictable impacts on every aspect of the dynamics of island biogeography. Deep history and evolution have been re-incorporated into island biogeography.

It is exciting to see such dynamic development of new approaches to island biogeography; whether one studies metapopulation dynamics, conservation biogeography, or adaptive radiations on oceanic islands, the field of island biogeography continues to offer broad horizons. If it is true that the paradigm initiated by MacArthur and Wilson is being supplanted in both ecological and evolutionary island biogeography, the new paradigms owe much of their development to the robust research agenda laid out 40 years ago.

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Edited by Markus Eichhorn and Joaquín Hortal

book review

Past environmental change in Latin America

Late Cretaceous and Cenozoic History of Latin America Vegetation and Terrestrial Environments, by Alan Graham

Missouri Botanical Garden Press, *Flora Mesoamericana* Vol. 4(1), June 2010, 617 pp. ISBN: 978-1-930723-68-9

Price: \$95.00 (Hardback)

<http://www.mbgpress.info/>

This authoritative text by Prof. Alan Graham of the Missouri Botanic Gardens is based upon nearly four decades of research in the Neotropics. The volume provides a comprehensive foundation for understanding the past environmental change that underlies the biogeographic patterns found in Latin America (defined as 32°N to 55°S), and is a companion volume to his earlier work which focused upon North America (Graham, 1999), in addition to another recent book on the palaeoecological history of the New World (Graham, 2010¹). The introductory note to the Latin America book states that the target level is advanced undergraduate and graduate students, and that the aim is to be appropriate for use within university courses and seminars. In my opinion this book achieves this goal by providing up-to-date information in a well organised, concise and interesting manner.

The book is logically set out and written in an easily-digestible style which should be understood by most undergraduate students. Divided into three sections (Background, Database and Synthesis) and eight chapters, there is a natural progression in the format, which means that the text could easily be utilised as a support for a seminar series or lecture course. The background section contains a wealth of basic information on the climate, geography, geology and vegetation, which provides the scene-setting details which would be required for any undergraduate lecture series on the past environments of Latin America. The clear and consistent geographic organisation of chapters and subsections also lends itself to the teacher or student who wishes to extract information on a particular region of interest; Mexico, the Antilles, Central America or South America.

¹A review of this book will appear in a future issue of *Frontiers of Biogeography*.

The appendix and database section provide primary data on modern plant distribution, and micro- and mega-fossil fossil discoveries respectively. The lists of plants and fossil finds are a useful collation especially for any new research student embarking on a project focused upon Latin America. The database chapters also include clearly reproduced maps, pollen diagrams and images of key pollen taxa and the discussion assesses the evidence for change at particular sites and across the regions. The databases section is not merely names and numbers; it contains far more commentary and scientific debate than I can hope to cover in this review. Therefore, to give you a flavour of the text I will focus on one of the areas with which I am more familiar to hopefully provide an idea of the level of detail presented.

In the region-specific 'South America' chapter, the Quaternary history of environmental change on the Bolivian Altiplano is given specific attention because of the relative abundance of information (Chapter 7, p. 418-424). Understanding the likely response of water resources to predicted future climate change is critically important today because of the millions of people living in, and around, La Paz (Bolivia) and the fragile, highly biodiverse, ecosystems which are reliant on the persistence of glaciers and lakes. Past fluctuations in climate on the Altiplano and the extent to which these controlled the size, duration and timing of mega-lakes have been the focus of much recent research. Graham sets out clearly the basic understanding of late Quaternary moisture balance change which has been gathered from sedimentary records, ice cores and glacial features. The review of the literature in the book is comprehensive up to 2006; I will summarise the evidence discussed and add some context from recently published research.

One of the main issues that must be considered when thinking about the waxing and waning of mega-lakes on the dry central Altiplano in Bolivia is: how do you get the positive moisture balance to 'grow' the lakes? Following the examination of sediments from Salar Uyuni, Baker et al. (2001) evoked an elegant model which suggested that orbitally forced movement of the Inter Tropi-

cal Convergence Zone conveyed more (or less) moisture to this region dependent on the angle of the rotation of the Earth's axis relative to the Sun. Assuming that movement of the ITCZ is key to getting moisture onto the Altiplano we can anticipate that a c. 20,000 year cyclicity would be evident in records of moisture change found across the Andes and Amazon. As Graham points out, Burbridge et al. (2004) find a similar pattern in the southern Amazonian lowlands and suggest a similar mechanism. However, on the Bolivian Altiplano, recent work by Placzek et al. (2011) on tufa barrages, assumed to record the age of lake high stands, suggests that the timing of lake high-stands does not necessarily fit the pattern anticipated from precessional forcing. A pertinent, yet not often considered, question related to this is raised by Graham in reference to discrepancies in the timing and extent of lake level changes: what influence have subterranean tectonic shifts had, and how could these have altered ground water flow and therefore influenced the surface lakes and water drainage systems. Evidence from new fossil pollen records obtained from Lake Titicaca suggest that a further complicating factor may be the impact of local, regional or extra-regional climate feedbacks. Bush et al. (2010) point out that today Lake Titicaca modifies its local climate, particularly rainfall, and that feedbacks associated with this are likely to have magnified past environmental changes on the Altiplano during the last 370,000 years. Reconciling the seemingly conflicting records of lake level change and understanding the biogeographic implications is an on-going challenge for researchers. This is one of many short sections within Graham's book which provide an excellent foundation for setting up debate, well-supported by references and with a growing body of new literature available for students to discover for themselves.

The final section of this book continues to present key issues in a clear manner but focuses on bringing together data to understand the broader picture. This section covers major biogeographic events and issues, including the closure of the isthmus of Panama, the origins of Neotropical rain forest, and debate surrounding

the existence, or otherwise, of Quaternary glacial forest refugia in the Amazon lowlands. The insight and coverage offered throughout this impressive text reflects Alan Graham's experience and interests. What is more, the digestible way in which the vast array of complex data and discussion is conveyed demonstrates his breadth of knowledge and literary skill. In summary, this book contains a wealth of useful information for those wishing to get to grips with the key issues of surrounding past vegetation and terrestrial environments in Latin America. The text would be an excellent support for a lecture course or sections could be used as a springboard for a related student research project.

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Edited by Markus Eichhorn

New editorial policy for book reviews

Frontiers of Biogeography will publish in-depth reviews of recently published books (typically less than one year old) on biogeography or of interest to biogeographers, alongside a 'Noted with Interest' section providing brief details of new publications. Authors, editors or third parties are invited to suggest books for review to the Book Review Editor, Dr Markus Eichhorn, School of Biology, University Park, Nottingham NG7 2RD, United Kingdom; telephone ++44 (0) 115 951 3214; e-mail markus.eichhorn@nottingham.ac.uk. We welcome offers to review books for *Frontiers of Biogeography*, but will not accept an offer to review a specific book. Anyone wishing to review books should send a brief *curriculum vitae*, description of competencies, and a statement of reviewing interests to the Book Review Editor. Reviews should be in an essay style, expressing an opinion about the value of the book, its focus and breadth, setting it in the context of recent developments within the field of study. Textbook reviews should consider their utility as resources for teaching and learning. Avoid describing the book chapter-by-chapter or listing typographical errors. The length should normally be 1000 words (1500 words for joint reviews of related texts) including a maximum 10 references. Authors may suggest a short heading for the review, followed by the title of the book(s), the authors/editors, publisher, publication date, price, hbk/pbk, pages, ISBN and website (where available). Figures or tables will not ordinarily be included. Authors of reviews must verify that they have not offered (and will not offer) a review of the same book to another journal, and must declare any potential conflict of interest that might interfere with their objectivity. This may form a basis for editorial decisions and such disclosures may be published. Book reviews will usually go through a light editorial review, though in some circumstances also will be considered by one or more referees.

book review

Census of marine life

Life in the World's Oceans: Diversity, Distribution and Abundance, ed. by Alasdair D. McIntyre
Wiley-Blackwell, October 2010, 384 pp. ISBN: 978-1-4051-9297-2

Price: £120 / € 144 (Hardcover)

<http://www.wiley.com/wiley-blackwell>

This is a book about which superlatives are needed. It contains 17 chapters which summarise results to date in the Census of Marine Life, which as a bald statement perhaps under-emphasises the important point that these chapters are invaluable reviews of knowledge about a very large programme and about widely different aspects of the ocean's life. This particular programme has been in progress for a decade at least, depending on which species groups are referred to, but which started mostly in the early 1990s following increasing concerns about loss of diversity in the oceans of the world.

The book itself is beautifully produced and is in colour throughout, containing innumerable clear maps, which is especially important for such a subject. The graphics for the volume have added much to its attractiveness, value and to its ability to convey biogeographic and biodiversity information to a very wide readership. This, of course, is the whole point of a summary or of a review, especially a collected volume of such reviews, but with printing costs of colour such effective use is too commonly sacrificed by the use of much less clear and less communicative black and white images. It seems clear that if books are to retain a place in a pdf world, then colour in such volumes is essential, and when a collection of highly illustrated articles like this is included in one volume, its success is surely assured. The publishers too are to be complimented in this case.

The chapters start with an introduction by Fred Grassle, one of the pioneers of marine biodiversity studies, followed by 17 chapters (by a total of about 140 authors) which systematically review different aspects of marine biodiversity, by region and by both pelagic and benthic habitat, including that of the very deep ocean. Without listing all the chapters, the different parts are collected into several parts: Oceans Past, Oceans Present – Geo-

graphic Realms, Oceans Present – Global Distributions, Oceans Present – Animal Movements, Oceans Future and, finally, Using the Data. None of the component chapters slip from providing very useful, well-summarised and informative information on themes connected with oceanic diversity. They also manage to include large numbers of photographs illustrating the wide range of biodiversity that exists. Perhaps microbial diversity is under-represented, microbes contributing as they do some 90% of oceanic biomass. However, this is a subject whose literature I'm sure will explode in the next decade as we humans realise more and more that, whatever the interest, appeal and observability of macro life are to us, macro-species are just one rather small offshoot of Earth's life as it has existed over billions of years.

This volume is of great value to those of us whose research has been necessarily focussed on particular aspects so that we feel we are losing the battle to keep abreast of all these innumerable findings in so many other related marine biology disciplines. This book is, quite simply, very good. It is a pity it is £120 which means it is mostly going to be confined to libraries, rather than be used in countless private homes and office shelves, especially of younger researchers. It will be a benchmark for several years.

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Edited by Markus Eichhorn

books noted with interest

Soils of South Africa

Martin Fey

February 2010, Cambridge University Press, 287 pp.

£60.00 (Hardback) ISBN: 978-1107-000-506

http://www.cambridge.org/gb/knowledge/isbn/item5741477/?site_locale=en_GB

This lavishly-illustrated book lists the main soil groups of South Africa, their distribution, properties, classification, genesis and uses. A whole section is devoted to the often under-appreciated role of soil-transforming animals. The final chapter provides analytical details of 37 illustrative soil profiles. What could be a dry tome is lightened by an idiosyncratic style, with the inclusion of soil-themed poems coming as a surprise. There is generous and occasionally excessive use of colour throughout, including numerous colour plates which vary considerably in their quality. The glossary is short and contains little basic terminology, making the book most suitable for specialists.

Bats of Southern and Central Africa: A Biogeographic and Taxonomic Synthesis

Monadjem, A., Taylor P.J., Cotterill F.P.D. & Schoeman M.C.

September 2010, Wits University Press, Johannesburg, 596 pp.

£52.50 (Paperback) ISBN 978-1-86814-508-9

<https://sites.google.com/site/fpdcotterill/bats-of-southern-and-central-africa>

There are 116 known bat species in southern and central Africa. This book presents the state of current knowledge regarding all aspects of their natural history, taxonomy and ecology. Each species description contains a distribution map, based on over 6000 museum records, most of which have been personally checked by the authors, from which potential ranges have been projected using MaxEnt. The diagnostic characters are thoroughly described, accompanied by colour pictures of live bats and their skulls, and sonograms of echolocation calls when available. There are also introductory chapters on bat biology and biogeography. Overall this is an excellent resource.

From Populations to Ecosystems: Theoretical Foundations for a New Ecological Synthesis

Michel Loreau

2010, Princeton University Press, 328 pp.

\$99.50 / £69.95 (Hardback), \$45.00 / £30.95 (Paperback) ISBN: 978-1-4008-3416-7

<http://press.princeton.edu/titles/9238.html>

Ecologists are sub-divided into those working on populations, communities, ecosystems or evolution. While it is clear that these fields are necessarily connected, and ultimately give rise to biogeographical patterns, a unified approach has seldom been attempted, and never without controversy. Michel Loreau is a theoretical ecologist whose contributions to the link between biodiversity and ecosystem services have made him one of ecology's most recognised authors. This is a bold attempt to build bridges between disciplines operating within different conceptual frameworks, often presented in mathematical terms, though clearly explained throughout. It will reward the conscientious reader with new insights and inspiration.

Flower and Fruit: Morphology, Ontogeny, Phylogeny, Function and Ecology

Leins P. & Erbar C.

October 2010, Schweizebart Science Publishers, Stuttgart, 439 pp.

€ 69.00 (Hardback) ISBN 978-3-510-65261-7

<http://www.schweizerbart.de/publications/detail/isbn/9783510652617/Flower-and-Fruit>

This translation of the textbook 'Blüte und Frucht' provides a thorough treatment of the reproductive organs of angiosperms, including chapters on floral structure and classification, along with how specific adaptations are connected to pollination and seed dispersal. The translation is accurate albeit with occasionally faltering prose. The text is illustrated by over 250 high-quality figures, many in colour. Despite some overlap with another recent book (*Understanding Flowers and Flowering*; Beverley Glover, OUP 2007), this book goes into considerably greater anatomical detail, and will prove particularly valuable by providing access for English speakers to the wealth of botanical literature in German.

A List of the Terrestrial and Marine Biota from the Azores

Borges, P.A.V., Costa, A., Cunha, R., Gabriel, R., Gonçalves, V., Martins, A.F., Melo, I., Parente, M., Raposeiro, P., Rodrigues, P., Santos, R.S., Silva, L., Vieira, P. & Vieira, V.

November 2010, Príncipe, Cascais, Portugal, 432 pp.

€ 15.00 (estimated, Hardback) ISBN 978-989-8131-75-1

<http://cita.angra.uac.pt/biodiversidade/publicacoes/ver.php?id=311>

This collaborative work documents the terrestrial and marine (mainly coastal) biota of the Azores archipelago, listing over 8,000 resident taxa and 300 non-breeding birds. The groups considered are: fungi, lichens (and associated fungi), diatoms, bryophytes, vascular plants, terrestrial flatworms, earthworms, terrestrial nematodes, molluscs, arthropods, terrestrial and marine vertebrates, marine macroalgae and coastal marine invertebrates. The presence of each taxon on all nine islands is documented, along with whether they are endemic, native or introduced. The checklist follows a consistent format throughout and includes a comprehensive alphabetical index of all species. A free PDF version can be downloaded at http://www.azoresbioportal.angra.uac.pt/files/publicacoes/Listagem_ml.pdf.

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Climate Change Impacts on Freshwater Ecosystems. Kernan M., Battarbee R.W. & Moss B. (eds) Wiley-Blackwell (2010).

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Tropical Rain Forests: An Ecological and Biogeographical Comparison. 2nd Edition. Corlett R.T. & Primack R.N. Wiley-Blackwell (2011).

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Interview with John C. Avise, recipient of the 2009 Alfred Russel Wallace award

by Brett R. Riddle¹

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Brett R. Riddle. Wallace is considered the “Father of Biogeography”, Darwin the “Father of Evolution”, and Linnaeus the “Father of Taxonomy”; and now you are the “Father of Phylogeography”, which admittedly is a young discipline compared to the other three. If you were to look perhaps 100 years into the future, do you have a sense that phylogeography will become a discrete discipline alongside those other three pillars in the biodiversity sciences?

John C. Avise. I'm flattered (and also a bit embarrassed) that you would even imagine placing my name in such lofty company. Nevertheless, I do believe that phylogeography has prompted a revolutionary way to look at the world, important enough to merit at least a bookmark in the history of science. Prior to phylogeography's emergence as a recognizable discipline in the mid-1980s, two dominant halves of evolutionary biology had remained quite disconnected. On the one side was population genetics, a well-established microevolutionary discipline that deals with various forces (such as natural selection and genetic drift) that can shape allelic and genotypic frequencies in conspecific populations. On the other side was phylogenetic biology, a discipline that aims to estimate historical relationships mostly of supra-specific taxa. Ever since my early days in graduate school, I perceived as incredibly ironic the fact that population genetics and phylogenetic biology had extremely little contact, despite the reality that both of these academic traditions deal ultimately with the histories of genetic lineages, albeit across different timescales.

BRR. Ironic, I agree, and phylogeography certainly has filled the role as an integrator between those disciplines.

JCA. Well, that's how I like to think of it, anyway. In my opinion, a proper reading of history will

credit phylogeography with having forged some of the earliest empirical and conceptual bridges between micro- and macro-evolutionary thought in genetics. A century from now, I suspect that phylogeography will still be recognizable as a discipline, although perhaps so thoroughly integrated into evolutionary research that most biologists will simply take for granted that an emphasis on genealogy in space offers a seamless approach for connecting the evolutionary-genetic histories of organisms below and above the level of species.

BRR. Right, and that synthesis certainly is becoming more clear all the time. In your view, has phylogeography been more successful in generating new questions and avenues of research because of that power of integration, or really as a new approach to addressing older questions?

JCA. I think it's some of both. Phylogeography is a conceptually bounteous discipline in the sense mentioned above, but it's also a data-rich field that has opened highly practical ways to address biogeographic questions, many of which had existed before but others of which (especially within species) had been formulated poorly at best. Beginning with the introduction of mitochondrial (mtDNA) data to population biology in the 1970s, geneticists finally gained empirical access to gene genealogies within as well as among species, and that's something that had not been practicable (nor even much dreamed of) prior to the phylogeographic revolution. So, phylogeography has become a spirited mix of some things old but many things new.

BRR. And how do you see the conceptual and the empirical sides of phylogeography interacting with one another to drive the field's development?

JCA. I see them as synergistic or mutually reinforcing. For example, mitochondrial data provided much of the original motivation for developing coalescent theory, but the latter in turn now stimulates ongoing efforts to gather additional genealogical information from multiple loci and species. So, I see extensive feedback between data and theory in phylogeography. Indeed, such synergism probably characterizes nearly all young and vibrant fields of science, some other examples being astrophysics and molecular biology.

BRR. Have there been any limitations or dead-ends that have surprised you, or questions that you might have seen 10-20 years ago developing but really have not come to fruition the way you might have envisioned?

JCA. Yes, definitely. For example, I'm very surprised that the field has not yet moved with much greater effectiveness into analyses and interpretations of nuclear genealogies. During its first 20 years and beyond, the field of phylogeography has been tied closely to cytoplasmic genomes because of the relatively clear genealogical signals that these non-recombining haploid segments of DNA provide. Thus, a major challenge for the field now is to extend genealogical concepts and empiricism to the nuclear genome, where the vast majority of each species' total genetic history must in principle reside.

BRR. Has that been mostly a methods constraint, or a theory constraint? Is it technological, or just that people have not figured out analytically how to deal with nuclear genomes?

JCA. The constraint is definitely technical in part. For example, one immediate logistic hurdle is how

to isolate and assay nuclear haplotypes, one at a time, from diploid tissue. There are several ways to do this in model systems, but in general the challenge has proved to be stiffer than many people probably anticipated. However, the constraint is also conceptual in part, because a multi-locus coalescent theory for structured populations will have to be developed before we can gain a clear interpretation of the multitudinous pathways that haplotypes at unlinked nuclear loci undoubtedly have traversed during a species' history. So, the challenges are both conceptual and technological. Eventually, the field of phylogeography will have to come to grips with dissecting nuclear genomes in ways that are at least somewhat analogous to how mtDNA data traditionally have been handled.

BRR. Do you see any technological thresholds on the horizon that will overcome some of those problems? Genomics based technology?

JCA. Yes. I think the field is moving gradually in several directions to address these shortcomings, and genomics-based technologies will certainly play key roles. But let me frame the situation in the following way: If I were a young researcher, just starting my career now, I would probably strive to pioneer a new approach that I might label "haplotypic genetics" or perhaps "gamete-based genetics". The basic idea, in part, would be to take full advantage of nature's own methods for purifying nuclear haplotypes. Every time a gamete is produced, nature in effect has isolated a discrete piece of DNA at each and every nuclear locus. If these could be isolated and sequenced in large numbers, a vast new category of genealogical information might be tapped and then interpreted (using a multi-locus coalescent theory) in an expanded phylogeographic framework. This

Two special issues on the 4th IBS Meeting are now out

The December 2009 issue of *EcoHealth* (<http://www.springerlink.com/content/1612-9202/6/4/>) included a special feature on the biogeography of disease, the outcome of a symposium organized by Kate Smith and Sam Schneider at the 4th IBS International Meeting (Mérida, Mexico, 2009).

Ecography published in July a special issue with a selection of contributions that arose from the Mérida meeting, representing the diversity of current biogeographical research. The issue is available at <http://www.wiley.com/bw/journal.asp?ref=0906-7590>.

would be a truly revolutionary development. Long before the phylogeographic era, much of traditional population genetics dealt with diploid genotypes, allele frequencies, and populations as units of analysis, but there wasn't the explicit genealogical perspective that a gamete-based genetics might promote. The net result could be a fresh way of thinking about the biological world that should compliment and enrich not only phylogeography but also much of traditional phylogenetics and population genetics.

BRR. Would you envision, if you just took that a little bit further, and you were thinking about a model system to start developing that sort of genetics, going straight to a diploid based system, or would you start with something simpler, something that's haploid?

JCA. If I had the necessary resources (time, personnel, and money), I would approach the problem from a variety of different angles. I might indeed start with a haploid (or perhaps a haplo-diploid) genetic system for tractability, but eventually I would want to expand the methods and concepts to diploid sexual creatures. Although certainly challenging, the whole approach should be technologically feasible, as evidenced for example by the work of Norman Arnheim at the University of Southern California, who has forged a highly successful career by applying gametic assays to questions in human population genetics. In many publications over the years, Arnheim and his colleagues have demonstrated a remarkable diversity of applications for this kind of gamete-based approach. If it can be done in humans, it surely must be do-able more generally.

BRR. Going back to phylogeography as it exists today, can you briefly think about some of the greatest successes you've seen – I think you've probably written about them before – or perhaps some of the successes that have been the most surprising to you?

JCA. I've written several review papers and books on the many unanticipated discoveries from phylogeography. Especially in the early years, when

the field was still in its infancy, it seems like my students and I encountered one big surprise after another, almost on a monthly basis. A lot of that had to do with stunning findings about mtDNA structure and evolution, but some of it had to do more broadly with genealogical patterns, processes, and principles. One example of the latter was our discovery that particular phylogeographic patterns sometimes recur across multiple co-distributed species in nature. Before then, I had supposed that nearly every species (and perhaps nearly every locus in sexual taxa) might tend to display a unique and idiosyncratic genealogical history, so I was genuinely amazed when my laboratory (and many others) began to uncover rather consistent phylogeographic themes or patterns at several levels of biological organization. This in turn led to our elaboration of "principles of genealogical concordance" that have become centerpieces of phylogeographic thought.

BRR. Does that expectation of idiosyncrasy derive from the expectation that the recent and shallow intra-specific world works differently from the deep and across-species world where, since the 1970's, people have certainly been expecting to see such recurring patterns derived from a sort of vicariance-based biogeographic perspective?

JCA. You're quite justified in suggesting that my astonishment might merely reflect my naivety or ignorance at that time. My training had been mostly in population genetics and ecology, so perhaps I would have been somewhat less surprised had I been trained instead as a vicariance biogeographer. In any event, in most cases I still prefer to deem the simplest null hypothesis as being a general paucity of extreme consistency in nature.

BRR. So what you're doing is really confirming your previous discussion about the extreme conceptual differences between the micro-evolutionary population-genetic and macro-evolutionary phylogenetic-biology worlds that had existed until then.

JCA. You make a valid point. But regardless of whether these and many other phylogeographic eye-openers about nature were discipline-wide revelations at that time or merely personal insights for me then, they are now thoroughly woven into the basic fabric of modern phylogeography.

BRR. Getting back to those early days, you talked a little at the IBS meeting in Mexico last year about the role of serendipity in your role in discovering phylogeography. What I'd like to ask is for you to talk a bit more about serendipity in terms of being in the right place at the right time and having the right molecule to work with.

JCA. In many respects, I can be thought of as the poster-child for serendipity in science. My entire career has been one fortuitous break after another. Let me give you an example. [I've written about this before, but the story bears repeating.] In the mid-1970s, I became involved with mtDNA by pure good fortune. I had just started as an Assistant Professor at the University of Georgia when I gave a departmental seminar about an allozyme project that I had conducted on fish. I concluded my talk by telling the audience that although it might be fine to study protein products of structural genes, the real stuff of evolution was probably genetic regulation. [This was at a time when Allan Wilson and others had first proposed key evolutionary roles for regulatory genes.] So, I queried the audience for any suggestions about how I might shift the focus of my research to gene regulation. One person raised his hand and asked, "Have you tried restriction enzymes?" I replied, "What are restriction enzymes?" [I honestly didn't know; they had been discovered only recently.] So, I soon read up on these proteins and learned that they are like little scissors that snip DNA at specific recognition sites. Wow, wouldn't it be great if I could harness restriction enzymes to examine repetitive nuclear

DNA, which was by then widely suspected to play some role in gene regulation? Accordingly, I approached several biochemistry professors at UGA to ask whether I could work in their lab for a few weeks to learn restriction techniques. Except for one person, they all politely said "no". The one exception was Robert Lansman, who welcomed me into his laboratory but who also lamented that he knew little about the nuclear genome, having instead devoted his career to the biochemistry of mtDNA. "What is mtDNA?", I asked. Well, I soon found out, and, as they say, "the rest is history". In Bob's lab, we soon began purifying restriction enzymes (because they weren't yet commercially available) and digesting mtDNA, which I never imagined would hold any special evolutionary interest in its own right. Of course, the mitochondrial genome turned out to have many wonderful properties that make it nearly ideal for what later would become known as phylogeographic analyses. Such studies on a wide variety of vertebrate and invertebrate animals were to become my occupation and preoccupation for the ensuing 20 years.

BRR. I'm sitting here thinking that this is loosely analogous to contingency in evolution and adaptive radiations. I don't want to take that too far, but my question would be: mitochondrial DNA really was an important part of the story of getting phylogeography off the ground to begin with, and if you had not been in Bob Lansman's laboratory at the right time, working with the right person who himself did not really know the power of this molecule in an evolutionary and biogeographic sense ...

JCA. Right, Robert was a biochemist and physiologist, so his academic background was entirely different from my training as a population geneticist and natural historian.

You can find information about the **International Biogeography Society** at <http://www.biogeography.org/>, and contact with other biogeographers at the **IBS blog** (<http://biogeography.blogspot.com/>), the **IBS facebook group** (<http://www.facebook.com/group.php?gid=6908354463>) and the **IBS twitter channel** (<https://twitter.com/biogeography>).

BRR. Yes ... totally different view, and so have you ever thought about, had you not been there, and Bob not been there, how long, if at all, it might have taken phylogeography to develop and what it might look like today?

JCA. I've often mused about that, and I've also wondered whether a cleanly recognizable field of phylogeography would even exist today? Honestly, I rather doubt it. Although it is true, of course, that many people would still be using cytoplasmic genomes as powerful genealogical tools, it also seems unlikely to me that such approaches would inevitably have become wrapped into a broader and cohesive conceptual evolutionary framework. How the word 'phylogeography' itself was coined came about as follows. In the late 1970s, as we began writing scientific articles about the spatial distributions of matrilineal haplotypes in various species, I was forced to use cumbersome phrases to summarize a straightforward observation: that there seemed to be intelligible geographical patterns in the distributions of interrelated genetic lineages.

BRR. So you needed a word!

JCA. Yes, I needed a word! I coined it in an article for the Annual Review of Ecology and Systematics. And once that was accomplished, I began almost immediately to think in quite different ways about biogeographic topics-- for example, I became more conscious that I was really attempting to use gene trees to decipher the demographic and genealogic histories of conspecific populations-- indeed, that I was thereby actually developing quite a new discipline from the ground up! So, that little lexical exercise offers a fine example of how a well-timed word or phrase can itself profoundly shape one's own thinking. Several other such influential words come to mind. For example, E.O. Wilson coined the word biodiversity about 20 years ago, and it has since become an evocative label for whole areas of biological inquiry. Global warming is another utilitarian phrase that has promoted the recent emergence of several novel research arenas.

BRR. Well, 150 years ago, I don't think anybody was even talking about biogeography per se.

JCA. Exactly, so I suspect there probably was some kind of rather fundamental transformation in the minds of Alfred Russel Wallace and other biogeographic pioneers when the object of their attention first became seen as a formal discipline. "Biogeography" did indeed constitute a beautiful marriage between the formerly separate realms of biology and geography.

BRR. Before we leave this whole idea of serendipity, you call yourself a poster-child for serendipity in your career, but to the extent that might be true, what do you think is the general role of serendipity in the progress of science?

JCA. I think it's often underestimated. I love to read biographies, of scientists especially. When people reflect candidly on their lives, almost without exception serendipitous incidents at key junctures can be seen to have had profound ramifications. Indeed, given that life is inherently sequential, how could it not be the case that idiosyncratic events have huge but unforeseeable impacts on careers?

BRR. And even the people we've been talking about had events in their lives that, if played again differently, perhaps, our science would have evolved in a very different way.

JCA. For sure.

BRR. So, you were already an accomplished evolutionary geneticist before this serendipitous point in your career. Have you thought about, had this not happened, the path your career would have taken?

JCA. Yes, I've often thought about that. If I had never been introduced to mtDNA or become captivated with phylogeography, I'd still like to think that I would have found a useful niche in some other grand scientific arena, such as, perhaps, gene regulation, gamete-based genetics, or con-

ervation genetics. But I certainly have no regrets. I've always been a naturalist at heart, a geneticist in mindset, and an incurable academic who feels forever compelled to marvel at the biological world from novel intellectual vantages. Thus, phylogeography played perfectly into several of my most basic natural dispositions.

BRR. Do you have a message in all of this, then, for today's graduate students?

JCA. Well, just be sure to be lucky as well as diligent! Seriously, if there is any broader message to be taken from my scientific good fortune, perhaps it is to always nurture your deepest interests but also remain prepared to capitalize fully on any unforeseen opportunities that might happen to come your way.

BRR. But you won't know there's an opportunity unless you bring your prior experience to it.

JCA. Quite true! So, here's one more piece of professorial advice: Even as you focus on the daily details of your research, always try to keep at least a part of your brain working in a broadly strategic or synthetic mode as well.

BRR. What's your feeling about the role of a natural history background in this whole business and being able and having the desire to get out and see your system?

JCA. Well, the natural-history side is incredibly important. For example, if I hadn't been raised as an outdoor biologist and natural historian, I wouldn't have had much of any clue about the glorious opportunities that molecular biology provides to study wildlife. To me, one huge problem for the modern world is that fewer and fewer young people have any real opportunity to experience nature firsthand and thereby become enthralled by its wonders. How can we truly care about what we do not emotionally know and love?

BRR. Some people might say that they're even afraid of nature.

JCA. I can attest to that from personal teaching experience. Here I am in the Los Angeles basin, with its 12 million people. Most of my students at UCI have grown up in this impossibly crowded environment and have never, in their entire lives, truly experienced the great outdoors. I find this situation very sad. The good news, however, is that many of these same students do quickly come to appreciate biodiversity after being introduced to even a fraction of its wealth via organismal courses such as ecology or ornithology.

BRR. Yes, and I really wish we could convey that message to certain administrators and developers of curricula these days.

JCA. Yes, for sure. For those students who do have a solid foundation in organismal biology, every other course in a biology curriculum (including such seemingly disparate subjects as genetics, biochemistry, physiology, mathematics, and statistics) almost inevitably becomes far more relevant and meaningful.

BRR. Just in terms of phylogeography being a great integrator between disciplines, I'm wondering what you think its role has been in integrating biogeography back into ecology and evolutionary biology? In other words, what do you think biogeography would look like today without phylogeography?

JCA. I think biogeography would be a poorer discipline in several regards. For example, by focusing on historical in addition to contemporaneous evolutionary processes, phylogeography provides a useful conceptual counterbalance to ecogeography, another major branch of traditional biogeography. Another useful heuristic is that phylogeography fully incorporates and embraces both vicariance and dispersalist perspectives, which traditionally have seemed totally at odds to many biogeographers.

BRR. Have you read, a little bit of a controversial area, over the last decade, a few of the die-hard vicariance or cladistic biogeographers have argued that phylogeography is simply a resurrection of the old dispersalist biogeography that they tried to deconstruct? Do you have any response to that?

JCA. I have a very different take on this topic. I see phylogeography as being an umbrella discipline that encompasses vicariance and dispersalism, both of which clearly can act and interact in particular circumstances to affect the spatial distributions of genealogical lineages.

BRR. I agree with you, and I've argued that it's not resurrecting old dispersalist biogeography because in fact phylogeography's population genetic basis actually makes dispersal analytically tractable.

JCA. And it makes vicariance more tractable too, I might add. Phylogeography doesn't necessarily support one biogeographic worldview against the other.

BRR. Just briefly, if we can look at the broader importance, and ongoing development of phylogeography, we get into this world of theoretical and empirical discordances between gene trees and species trees that we're all very aware of these days, but on the other hand, it seems that mitochondrial or chloroplast DNA trees do frequently capture, as you showed us several decades ago, plausible signals of geographic history that seem to make good biogeographic sense and as such they still seem to nearly always be used as at least a first approximation, even by the most ardent advocates of multiple gene tree approaches. What more do you think we'll learn about biogeography and biodiversity when every-

body's sampling hundreds to thousands of gene trees, and hundreds of co-distributed species?

JCA. Well, that will be a wonderful day if and when it arrives, because everything then should be on more secure empirical and conceptual footings. As you say, much of phylogeography to date has been based on cytoplasmic genomes, merely for reasons of tractability, and yet the vast majority of any species' total genetic history must be ensconced in its nuclear DNA. On the conceptual front, however, most people already have come to appreciate the key distinction between gene trees and species trees, and indeed I take this intellectual conversion to be one of phylogeography's major contributions to evolutionary thought.

BRR. And it's exciting that there are some really bright people working on those issues right now.

JCA. For sure.

BRR. I'd like to finish with you responding to a few broader issues about the impact of phylogeography. The first I was thinking of was the impact of phylogeography in making progress toward developing a unified and operational species concept.

JCA. I think we already have gained a much richer understanding of "species" than we had before the phylogeographic era, and a lot of this improvement goes back to the basic gene tree – species tree distinction. Personally, I've always been quite content with the basic sentiments of the BSC (biological species concept), but I'm also cognizant that we can probably learn some things from the PSC (phylogenetic species concept) as well. Actually, I think that the BSC and PSC can and will ultimately be reconciled, at which time the whole debate may be remembered as little more than a semantic brouhaha. If such a reconciliation does indeed eventuate, I think we can then thank phy-

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logeography (and the clarity of its basic distinction between gene trees and species trees) for having contributed substantially to a final resolution of "the species problem".

BRR. Related to species concepts and biodiversity, what do you think of phylogeography spinning off concepts such as ESUs and initiatives such as DNA barcoding? Are these worthwhile and valuable endeavors?

JCA. I'm quite happy with such developments. The ESU concept is important because it emphasizes that there can be rather deep historical subdivisions within species that may warrant special conservation and taxonomic attention. The related but different concept of the MU (management unit) is important too, because it brings contemporary demography into the picture as well. Taken together, such concepts have stimulated deeper thought about what population structure means at different temporal levels and spatial scales, and from different evolutionary etiologies. With respect to DNA barcoding, I have more mixed feelings. On the one hand, I've long been an advocate for greater standardization in biology, especially in the field of systematics, and DNA barcoding is one practicable route to such an end. On the other hand, a potential downside is that people might become unduly enamored with single-gene data that sometimes may misrepresent broader genetic relationships.

BRR. Related to those concepts is the overall contribution to conservation of biodiversity. Do we know more about biodiversity and how to conserve it because of phylogeography?

JCA. Yes, definitely. For example, we can now appreciate more fully how biodiversity relates to phylogeny, and that these terms can apply at multiple levels, ranging from the gene at one end of the biotic continuum, to species, and to regional biotas and ecosystems at the other end. Most fundamentally, biodiversity is simply the richness of life, which ultimately is the richness of the planet's genetic heritage. So, I think phylogeogra-

phy has been extremely helpful in revealing that richness. More specifically, I like the whole idea of implementing a nature reserve system focused on biodiversity, much as our current national parks seek to preserve rich geological features such as Yosemite's scenery or Yellowstone's geysers. Wouldn't it be wonderful if society could get its act together well enough to put together a series of parks that feature biodiversity as well?

BRR. It would be fantastic. Even concepts such as wilderness areas tend to be areas that are not very useable for other things that other people wanted to use them for.

JCA. Yes. If the nation were to establish a series of what we might call biodiversity parks, an important component to understanding where those parks should be placed is not only extant biodiversity but also historical considerations— where did this modern biodiversity come from? Much as national monuments recognize important events in human history, key happenings in the Earth's natural history could be captured by appropriately placed biodiversity parks. Such phylogeographic reserves might even spark a new societal conservation ethic, when enough people finally learn to enjoy, appreciate, and cherish the Earth's biological heritage.

BRR. So are you thinking something along the lines of phylogeographic hotspots?

JCA. Yes, that could certainly be a part of the biological rationale underlying choices about where to place the parks. You would want to take into consideration not only what's currently there but also how what's now there came to be.

BRR. So the whole biodiversity parks concept is very integrative and synthetic in its own right?

JCA. Yes.

BRR. That feeds into my last question, which is the contribution to scientific literacy as a whole, and I think that you've kind of come up with a boots-on

-the-ground idea about converting what we all think about into broadly something that people can see and increase their scientific literacy. Do you have any other thoughts on this?

JCA. Imagine people visiting one of these biodiversity parks. I would assume that they might go first to the visitor's center where they would be exposed to films and exhibits on how the geographical and biological histories of the region mesh. And then as they further explore the park, the entire learning experience would be highly enriching. People might then want to tour more such biodiversity parks, to get a fuller sense of history of a continent (or even of the world when an entire network of parks becomes internationally linked). I can see endless educational opportunities, and the more the public learns about biodiversity, the more enthused it is likely to become about protecting the planet's biological heritage for future generations. Especially in democratic societies, widespread public awareness about biodiversity can only help to move governments toward wiser conservation policies.

BRR. And the fact that our appreciation for biodiversity is not conflicting with an appreciation for human well-being?

JCA. The two are in fact essentially one-and-the-same.

BIOSKETCH: John C. Avise

Even as a youngster I was an enthusiastic naturalist, determined someday to translate that fascination into a profession. Toward that end, in 1970 I earned a B.S. in Fishery Biology at the University of Michigan in Ann Arbor. Two more degrees followed: a M.A. in Zoology from the University of Texas at Austin in 1971, and a Ph.D. in Genetics from the University of California at Davis in 1975. I spent the next three decades at the University of Georgia in Athens, where I worked my way up the academic ranks before moving in 2005 to my current faculty position at the University of California at Irvine.

Temporally, my career paralleled the rise of molecular biology in the latter half of the 20th century, and I have consistently tried to capitalize upon that good fortune by applying molecular markers to intriguing questions in natural history and evolution. Thus, I am a molecular ecologist and evolutionary geneticist with an abiding love of nature and an endless intellectual compulsion to unravel its many mysteries. My rather atypical career in the natural sciences is detailed in an autobiography published in 2000: *Captivating Life, A Naturalist in the Age of Genetics*.

In addition to conducting research in such areas as phylogeography, conservation genetics, and molecular natural history, I have tried to communicate the wonders of science to a broad audience by publishing more than 20 textbooks and tradebooks on various evolutionary, ecological, and genetic topics. I have been blessed throughout my career not only with fabulous students and colleagues but also with a wonderfully supportive wife and daughter. Ultimately, these people deserve much of the credit for any lasting scientific contributions I hope to have made.



Picture from <http://today.uci.edu/>.

Job announcements

Postdoc position

University Toulouse 3, France

A 2-yr postdoc position is available in the Evolution and Biological Diversity group (<http://www.edb.ups-tlse.fr>). The postdoc will work on 'Predicting future patterns of freshwater fish extinctions' within the project FISHLOSS.

The aim of the project FISHLOSS (ANR "Sixth Extinction") is twofold. First, to draw an ecological and evolutionary framework for both natural and human driven freshwater fish extinctions at several spatial scales (from the site to the globe) by using a multidisciplinary approach combining ecology, biogeography, phylogenetics, paleontology, and biometry. Second, to use this framework as an explanatory and predictive tool to identify species that are extinction-prone and to provide expected extinction rates under different scenarios of future climate change. As available evidence suggests that area of occupancy is a good predictor of background extinction rate, the FISHLOSS consortium intends to establish a general relationship between background extinction rate and area of occupancy (EAR, extinction-area relationship). EAR could then be used to predict how much the extinction probability of a population will increase if its area of occupancy decreases.

The successful applicant will apply a range of species distribution models, General Circulation Models (GCM) and greenhouse Gas Emissions Scenarios (GES) to a set of stream fish species occurrences. Then he will produce fish species distribution forecasts and will combine these forecasts (i.e., ensemble forecasting) with previously defined extinction-area relationships. The final outputs of this work will be to identify/cartography the combination basin-species having the highest predicted extinction probability (France), and the basins having the highest predicted extinction probability of endemic species (Africa).

The applicant should hold a PhD in quantitative ecology or biostatistics, and knowledge on species distribution modeling and working with large databases. Please send a CV (no more than 5 pages) outlining research experience and interests, publications, a list of skills and other relevant professional information to gael.grenouillet@cict.fr. Closing date for application: 1st April 2011.

Assistant professorship

University of Hawai'i at Mānoa

Assistant Professor in Marine Geography at the Department of Geography, College of Social Sciences (<http://www.geography.hawaii.edu>). 9-month full-time appointment, possibility of tenure track, to begin August 1, 2011, subject to position clearance and availability of funds.

Duties: Teach undergraduate and graduate classes; advise students; conduct an active research program and publish results; seek extramural funding; collaborate on interdisciplinary research and field courses in marine geography; engage in departmental governance and service activities.

Minimum Qualifications: Earned Ph.D. in Geography or a closely related field from an accredited college, university or foreign equivalent. Demonstrated ability as a teacher; record of scholarly achievement or promise of future achievement in chosen area of research. Desirable Qualifications: Teaching and research interests in spatial analysis of marine environmental systems, anthropogenic impacts, and conservation of marine species and habitats; ability to teach statistics and research methods. Secondary interests in applying GIS to integrate human and environmental systems and analysis.

Applications will be accepted online at <http://surveys.socialsciences.hawaii.edu/ework/>. Application shall include a current curriculum vitae, cover letter stating your teaching and research interests and indicate how you satisfy the minimum and desirable qualifications and expected fit within the department, the College of Social Sciences and University of Hawai'i at Mānoa; contact information for three references; evidence of teaching effectiveness; and three representative publications (pdfs). Any other correspondence about the position should be addressed to Professor Alison Rieser, Chair of Search Committee, Department of Geography, 440 Saunders Hall, Honolulu, HI 96822; e-mail: rie-ser@hawaii.edu. Review of applications will begin on April 1, 2011 and will continue until the position is filled. Applications received by that date will be given priority.

Postdoctoral position

University of Zürich

A postdoctoral position is available in the group of Owen Petchey, beginning in the spring / early summer of 2011. Research in Petchey's group addresses the causes and consequences of extinctions, taking an integrative approach across multiple levels of ecological organisation: individual, population, community, and ecosystem. Extinctions occur in changing environments, so research addresses the ecological consequences of environmental change, such as changes in individual behaviour and interspecific interactions caused by temperature variation. Other projects include measurement of functional components of biodiversity and exploration of the determinants of food web structure. Empirical research in the group mainly involves experiments with laboratory-based microbial communities, but also includes analyses of existing large-scale datasets. These analyses often include the development of new theoretical models of ecological communities. The successful applicant will combine his or her own interests with these research themes and methods.

The postdoc will be held in the Institute of Evolutionary Biology and Environmental Studies at the University of Zürich, Switzerland (<http://www.ieu.uzh.ch>). Funding is available for at least two years. Applicants must have a PhD in Ecology or a closely related subject. Informal enquiries can be made to o.petchey@ieu.uzh.ch. Applications should be emailed, as a single PDF document to Sabine Marty (sabine.marty@ieu.uzh.ch). Applications should include a CV, including the names and contact details of three academic referees, a one-page research proposal, and a brief statement of plans for career progression. Review of applications will be from 1st April and continue until the position is filled.

Postdoctoral positions

Three postdoctoral research positions are available with the PaleON project (A PaleoEcological Observatory Network to Assess Terrestrial Ecosystem Models), an interdisciplinary research group of paleoecologists, ecological statisticians, and ecosystems

modelers working together to study how climate variations shape forest dynamics across a range of timescales (<http://www.paleonproject.org>). All postdoctoral positions have a desired start date of May, 2011.

Specific PaleON goals include developing a coherent inferential framework with rigorous estimates of uncertainty for paleoecological data, applying these techniques to reconstruct variations in forested ecosystems for the last 2000 years from the Great Lakes to New England, and then assimilating these datasets into a suite of regional-scale ecosystem models to infer presettlement biogeochemical cycles. PaleON has recently received funding from NSF-Macrosystems to begin a two-year effort towards these goals, with an emphasis on initial development of methods and datasets, community-building, and interdisciplinary training in paleoecology, statistical ecology, and ecosystem modeling.

Position 1: Paleoecological and Paleoclimatic Data Synthesis and Analysis. The primary responsibilities of this position are to coordinate the assembly of the witness tree, fossil pollen, charcoal, and paleohydrological datasets and analyze these datasets for intra- to interregional patterns of variance and synchrony. The postdoc will work closely with the other postdocs and the rest of the PaleON team towards the objective of fitting a full space-time statistical model to the paleoecological data and assimilating these reconstructions into the ecosystem model experiments.

Minimum qualifications are a doctoral degree in a relevant ecological or environmental science. The ideal candidate would have a strong familiarity with Quaternary paleoecological and paleoclimatic data, skills in paleoecoinformatics, knowledge of scripting languages such as Matlab and R, and experience with multivariate statistical methods. Experience with Bayesian hierarchical models, spatial models, and/or ecosystem models is also desirable.

This position will be based at the Department of Geography at the University of Wisconsin and will be jointly supervised by Drs. Jack Williams (University of Wisconsin) and Steve Jackson

One of the benefits open to IBS members is the opportunity to have job openings posted on the biogeography.org website. If you have a position you would like to have advertised, please contact Karen Faller (faller@wisc.edu) or Michael Dawson (mdawson@ucmerced.edu) for details.

(University of Wyoming). This position is up to two years with a preferred start in early May 2011. Interested applicants are encouraged to email a CV and cover letter with the names and contact information of three references to Alice Halfen (ahalfen@wisc.edu) with the subject line: PaleON Postdoctoral Application. For more information contact Dr. Jack Williams (jww@geography.wisc.edu) or Dr. Steve Jackson (jackson@uwyo.edu). Evaluation of applications will begin April 4 and continue until the position is filled.

Position 2: Ecological Statistics. This researcher will lead the development of statistical models, based on spatial statistics, state space, and data assimilation methods for the PaleON initiative, interacting with statisticians, paleoecologists, paleoclimatologists, and ecosystem modelers. Specific modeling challenges may include spatio-temporal modeling of paleoecological data, state-space modeling informed by ecological models, modeling uncertainty in radiocarbon dating, and spatial modeling of vegetation based on colonial settlement-era historical records. Strong applicants will possess a background in Bayesian statistical modeling, especially spatial modeling, state space modeling, or data assimilation. Applicants must be interested in working at the interface of statistics and ecology.

The postdoctoral researcher will be based at the University of Notre Dame's new Department of Applied and Computational Mathematics and Statistics and is supported in part by the Notre Dame Environmental Change Initiative (ND-ECI). This position will be supervised by Dr. Jason McLachlan at Notre Dame, with extensive input from Dr. Chris Paciorek at UC Berkeley, and interaction with other PALEON team members. The position is available for a two-year period, subject to annual performance review. We will consider applications on a rolling deadline. Funding is available for an immediate start, but we will consider start dates as late as summer 2011. Please email your CV and a cover letter with the names and contact information of three references to Jason McLachlan (jmclachl@nd.edu).

Position 3: Ecosystem Modeling and Model-Data Synthesis. The primary responsibility of this position is to coordinate the model-data inter-comparison activities and shared data among the modeling teams, to analyze model dynamics to make inferences about presettlement biogeochemical cycles, and to assess model-data fidelity across multiple models. The secondary responsibility is to help complete the Ecosystem Demography model runs for the model-data inter-comparison. Research questions focus on validating ecosystem models at centennial time-scales, making inference about pre-settlement ecosystem dynamics and biogeochemical cycles, and exploring the sensitivity of models to historical vegetation. Position will be supervised by Dr. Michael Dietze at the University of Illinois.

Minimum qualifications are a doctoral degree in a relevant ecological or environmental science. The ideal candidate would have experience with more than one of the following areas: ecosystem models, paleoecological data, Bayesian statistics, R, linux, computer programming, data assimilation, and climate downscaling techniques. Salary is commensurate with experience and qualifications with two years of funding available. Evaluation of applications is rolling with a preferred start May 2011. Interested applicants are encouraged to send a CV and cover letter with the names and contact information of three references to Melinda LaBorg (laborg@igb.uiuc.edu). For more information contact Dr. Michael Dietze (mdietze@illinois.edu).

If you want to announce a meeting, event or job offer that could be of interest for (some) biogeographers, or you want to make a call for manuscripts or talks, please contact us at ibs@mncn.csic.es and frontiersofbiogeography@gmail.com.

Upcoming events

Evolution of Life on Pacific Islands and Reefs Conference

26-30 May 2011 – Honolulu, Hawaii, USA

http://botany.si.edu/events/2011_pacific/index.htm

VIth European Congress of Mammalogy

19-23 July 2011 – Paris, France

<http://www.alphavisa.com/ecm2011/>

3rd International Conference on Climate Change

Impacts and responses

21-22 July 2011 – Rio de Janeiro, Brazil

<http://www.Climate-Conference.com>

XVIII International Botanical Congress

23-30 July 2011 – Melbourne, Australia

<http://www.ibc2011.com/>

Ecological Society of America 96th Annual Meeting

7-12 August 2011 – Austin, TX, USA

<http://www.esa.org/meetings/>

Conference on Frontiers in Historical Ecology

30 August-2 September 2011 – Birmensdorf, Switzerland

http://www.wsl.ch/frontiers_historical_ecology

BES Annual Meeting

British Ecological Society

12-14 September 2011 – Sheffield, UK

<http://www.britishecologicalsociety.org/meetings/>

European Society for Evolutionary Biology Meeting

20-25 August 2011 – Tuebingen, Germany

<http://www.eseb2011.de/>

12th EEF Congress

European Ecological Federation

25-29 September 2011 – Ávila, Spain

<http://www.eefcongress2011.eu/>

6th International Conference of the International Biogeography Society

January 2013 – Florida, USA

<http://www.biogeography.org/>

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