Island Biogeography: Implications and Applications for Archaeology

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In his ornithological notes prepared in 1835 while he was homeward bound, young Charles Darwin made what is now seen as a prescient remark about the mockingbird specimens he had gathered in the Galápagos Islands: “When I see these Islands in sight of each other, & possessed of but a scanty stock of animals, tenanted by these birds, but slightly differing in structure & filling the same place in Nature, I must suspect they are only varieties. . . . If there is the slightest foundation for these remarks the zoology of Archipelagoes will be well worth examination; for such facts would undermine the stability of Species” (quoted in MacArthur and Wilson 1967: 3, note 1). Such was Darwin’s later fame and the value of his evolutionary theory that the Galápagos Islands are now iconic symbols of the power of islands to open our minds to the mysteries of nature and the wisdom of evolution. However, islands as archaeological landscapes have long been problematic. Has the circumscription and possible remoteness of islands made island societies exceptional or deviant in some respects from the normal run of things in continental regions such as Asia, Europe, or the Americas? Or can we use islands as naturally defined “units of study” in landscape archaeology to explore questions about the ecology and evolution of human societies that might be overwhelming or utterly confusing if tackled on the scale of continents?

Rhetoric is a goodly part of scholarship (Gross 1990). It is currently fashionable in archaeology to complain about island biogeography and the idea of islands as “laboratories” as anachronisms that should probably be shoveled into the grave of failed ideas and discredited worldviews as too inhuman and disrespectful of human agency to be taken seriously. In this chapter I want to challenge current wisdom to suggest not only that islands are fine places to do archaeology but also that island biogeography is alive and well—although it could be argued that to survive it has had to morph into something called metapopulation biology.

Anthropology and Islands

What islands have meant to human beings has differed with time and place as well as with the texture of historical experience. As John Donne’s poetry and Sir Thomas More’s Utopia alike inform us, islands can be imagined as “other worlds” cut off from the corruption of foreign influences, invasions, and social responsibilities. Yet, life tells us otherwise. It is their geographic definitiveness and the challenges of access that define them, not their isolation—certainly not if isolation is taken as “other-worldliness,” or life lived in Nature’s equivalent to a laboratory petri dish (see Broodbank 1999).
Islands may be places you want to escape to, especially if the islands have palm trees and it is the dead of winter in higher latitudes, but this tourist’s fantasy won’t work if, having reached their shores, a sailor cannot come ashore. The claim made by some that a previous generation of archaeologists thought islands could be used as natural laboratories, because they are isolated cases of human adaptation to presumed peculiarities or simplicities of island ecosystems is either exaggerated or false (Keegan 1999). Remember this: when it comes to archaeology, landscapes, and island biogeography, the word “isolation” needs to be read as relative, not absolute, and certainly not as “out of the ordinary” (Terrell 1986: Chapter 6).

If we must have a villain in our tale, then anthropologists rather than archaeologists will do (Kuklick 1996). It is not farfetched to say that after World War II, especially in the Pacific, the islands-as-isolates idea did have apparent standing among anthropologists for a while (e.g., see Goodenough 1957; Mead 1957; Sahlins 1957, 1958; Sharp 1956; Suggs 1961: 194; Terrell, Hunt, and Gosden 1997; Vayda and Rappaport 1963; Watson 1963). This was the heyday of positivism (Popper 1959), the quantitative revolution in the social and biological sciences (Haggett 1966; Kingsland 1985), and of functionalism, structuralism, and comparative studies in anthropology (Lévi–Strauss 1963; Murdock 1949). In sociocultural anthropology, fieldwork was often reported to the world in narrative ways that made it sound as if anthropologists were privileged persons: each one had “my village” to talk about, and these little microcosms could be treated for comparative purposes as if they were indeed human isolates—cut off not only from the world but evidently from their neighbors, as well.

However appealing these imagined unworldly places may have been to anthropologists and other social scientists for a few decades after World War II, most would nowadays acknowledge that human societies, like human languages, are not discrete entities sealed off from contact with others (e.g., Leach and Leach 1983). Minimally, it is accepted that cultures and societies are “mixed” in the sense that their characteristics and distinctive features stem both from their own history and from their participation in broader ecological, social, economic, and political relationships (Lape 2004; Terrell, Hunt, and Gosden 1997). This is not to say, of course, that everyone in the academy has yet heard the word. In one of the more transcendental corners of biology, the field of phylogenetics, there are those who still unblushingly announce that for their purposes “languages and although there has been a vigorous debate about how far we can treat cultures as discrete, bounded units, similar to species” (Mace and Holden 2005: 116). This shows that there is nothing new under the sun.

Looking over what was written about island archaeology 20 or 30 years ago (e.g., Clark and Terrell 1978; Evans 1977; Glover 1977: 59; Hunt 1986, 1987; Irwin 1973; Kaplan 1976; Kirch 1986, 1987; Rouse 1977: 7; Shutler and March 1975; Terrell 1976, 1977a, 1977b) lends little to the claim (e.g., Rainbird 1999a: 223–26) that archaeologists thought very differently then about such places than they do nowadays (see Keegan 1999). As I commented in the mid 1970s:

The value of islands as natural laboratories does not lie in their supposed uniqueness, isolation or divergence from the main paths of history and evolution. On the contrary, islands are good places to study the world because they are numerous, because they occur in many sizes, shapes and degrees of ecological complexity, and because they can be found both in secluded locations where evolution may proceed virtually free from outside intervention and also near continents which are sources of new immigrants and ideas. (Terrell 1977b: 79)

In other words, there is scant evidence supporting the idea that archaeologists once upon a time were naïvely seeing islands as laboratories merely on the shaky grounds that such places are cut off from the main currents of history. Even those whose theoretical orientation toward data then as now led them to privilege isolation as an analytical simplification (e.g., Kirch 1984) were normally careful not to overdo the thrust of the oversimplification (e.g., Kirch 1986; Kirch and Green 1987, 2001).

### Island Biogeography

Island biogeography (Whittaker 1998) is a scientific calling nested within the broader field of biogeography (Lomolino and Heaney 2004; Lomolino et al. 2005). Both have long been a mixed bag of concerns, field methods, laboratory procedures, and analytical techniques (Lomolino 2000; Whittaker 2000). They are alike readily defined as the study of the geographic distributions of organisms, past and present (Brown and Gibson 1983: 557). Both now add up to a more coherent body of thought than was certainly the
Chapter 12: Island Biogeography

2004; Sauer 1977). I would add that neither island biogeography nor biogeography writ large is a simple or singular template (Brown and Lomolino 2000) for defining what an archaeological island biogeography is or should strive to become as a discipline among the other sciences (or among the humanities, if one prefers).

Critiques of island biogeography and of seeing islands as natural laboratories for studying the patterning and processes of biological evolution have long been a concurrent part of the business of evolutionary biology (Gilbert 1980; Hanski and Simberloff 1997; Sauer 1977; Walter 2004; Williamson 1989). These critiques are worth reading. It is my guess, however, that this criticism is not well understood outside the biological sciences. The thought, for example, that gene pools, groups of organisms, or places can be usefully modeled as island-like (for example, as in metapopulation biology today; see Hanski 1999; Hanski and Gilpin 1997) exploits rather than refines the idea of islands. In any case, it is relationships among such seemingly definitive things or places, not their presumed character as closed systems, high-security laboratories, or inaccessible plots of ground, that lend this kind of model building its biological cachet. In other words, the landscape of choice in island biogeography—as Darwin commented—is normally not an isolated island but an archipelago, real or imagined (Lomolino 2000; Terrell 1999).

Furthermore, it makes a difference whose biogeography you are talking about. Ernst Mayr (1963) pushed the paradigm of allopatric speciation as far as it would go, but not all agreed with him a generation ago (Sauer 1977: 322); nor is it now any longer advisable to dismiss sympatric speciation as improbable and therefore basically irrelevant to the evolution of life’s diversity (Jiggins and Bridle 2004; Via 2001). Yes, Mayr’s kind of biogeography has informed anthropology (e.g., Sahlin 1957; Watson 1963) and archaeology both then and now (e.g., Kirch and Green 1987, 2001). Yet I suspect that for many social science practitioners, old and new, the concept of allopatric speciation and the citation of processes such as adaptive radiation have probably always sounded far too biological to be employed productively in what they do.

However, it is true that archaeologists working in the Mediterranean (e.g., Broodbank 2000; Cherry 1981; Evans 1977), the Caribbean (Keegan and Diamond 1987), and the Pacific (Hunt 1987, 1988; Irwin 1992; Kaplan 1976; Terrell 1974, 1976, 1977a, 1977b) were captivated at least for a Robert MacArthur’s and Edward Wilson’s kind of biogeography as heralded in their famous equilibrium theory of island biogeography (1967).

MacArthur’s and Wilson’s basic proposition was simple (as it turned out, too simple). They suggested that island species diversity could be predicted mathematically as a dynamic equilibrium between the immigration of species from elsewhere and local species extinction. Their theory, of course, had much more to offer than just this (see Whittaker 1998: Chapter 7; also Quammen 1996). It became famous for its apparent elegance, its mathematical models, and its emphasis on such seemingly direct and easily acquired field data as island size (area) and inventories of species found on islands under scrutiny.

What attracted me to this brand of biogeography was not so much its mathematical side or its seemingly predictive powers, but rather the emphasis this approach placed on being honest and direct about the ideas being advanced and used. As it turned out, this was a prescient position to adopt, for in a devastating article published in 1980 in the Journal of Biogeography, F. S. Gilbert (1980: 230–31) gave the coup de grâce to seeing this theory as an accurate explanatory device with these now classic closing words: “The qualitative use of the equilibrium concept has stimulated a great deal of valuable research, and is clearly of use as a way of approaching an appropriate problem . . . Quantitatively, however, it would seem that the model has little evidence to support its application to any situation.”

This may sound like a weak endorsement of a bad idea, but when even important science journals such as Nature are willing to publish poorly explained models purporting to detail how ancient “express trains” or “slow boats” once upon a time carried prehistoric colonists out to virgin islands in the Pacific (e.g., Austin 1999; Diamond 1988; Gibbons 2001; Gray and Jordan 2000), I continue to admire how open MacArthur and Wilson were in going public with their willingness to analyze obviously complex issues by first describing them in clear, relevant, and simple terms so that actual work could be done to start teasing them apart.

However tarnished the MacArthur–Wilson equilibrium theory of island biogeography may now be (Brown and Lomolino 2000), the lesson for me remains unchanged: archaeologists ought to be as direct and open in showing us their cards and playing their hands. The fact that some archaeologists now criticize the efforts of their predecessors, because they feel they had not paid sufficient attention to an elusive causal variable called “human
the point I had thought was obvious. No, of course, the simple models favored by MacArthur and Wilson, mathematical or graphical, are not entirely realistic models. They weren't really intended to be (see Levins 1966, 1993; Orzack and Sober 1993). They were mostly what C. H. Waddington (1977) in the 1970s called "tools for thought."

Implications for Archaeology

Here is a basic question: what is an island? As biogeographers see them, islands are what they are because they are living spaces (habitats) surrounded by radical shifts in habitat—so radical that (1) few species of plants and animals are able to live for long in more than one of these radically different habitats; and (2) consequently we must pay close attention not only to what these habitats are like at any one moment but also to how and how often what is living there (plants, animals, and humans) comes and goes. This, at least as MacArthur and Wilson saw things, is basic island biogeography. "Islandness" may thus be seen as a common property of life rather than as a peculiar property of unusual places.

If islands are habitats surrounded by radical shifts in habitat, then islands have much to offer us. We may use the property we are calling "a radical shift in habitat" to determine the boundaries of "our places of study" rather than, say, using more artificial parameters such as latitude, longitude, or the dimensions of an arbitrary "sampling grid" or geopolitical unit. Seen this way—contrary to what the famous poet John Donne told us—all men are islands. So, too, are berry bushes in a cow pasture; cow pastures beside an interstate; cornfields great and small; and so on. All are "islands," since the world, as ecologists like to say, is "patchy." Once more, this is basic island biogeography. From a biogeographer's point of view, paying scientific attention to this patchiness is one way to get our arms around our earth's diversity. In short, islands are not special because of what they are; they are just great places to think about and study.

By the 1980s, MacArthur's and Wilson's equilibrium theory of island biogeography had fallen from favor in population biology. Depending on your point of view, it had by then either been replaced by, or had begun evolving into, a related way of thinking about and mathematically modeling Nature called metapopulation biology (Hanski 1999; Hanski and Gilpin 1997). Loosely described, this alternative approach relies on the idea that organisms are unlikely to be uniformly distributed in space. If so, then they can often local populations that are spatially related to one another by migration (or in other ways, too) into larger regional populations. When this is the case, efforts to analyze and understand what is happening locally (or archaeologically speaking, happened) probably need to take into account these broader interactions.

Two of the leaders of this relatively new approach wrote a decade ago: "An important reason for the appeal of the metapopulation concept comes from our subjective conviction that natural landscapes truly are, for many species, patchworks of one or several habitat types" (Hanski and Gilpin 1997: 3). Given this statement, it may be obvious that this way of thinking about biogeography has much in common both with older MacArthur–Wilson island biogeography (Hanski 1999: 11) and also with modern landscape ecology (e.g., Lomolino and Perault 2001). Like the former and unlike the latter, metapopulation models often focus on idealized habitat patches in more or less featureless worlds (Urban and Keitt 2001); like the latter and possibly less like the former, students of metapopulation biology favor real-world applications, especially in the arena of conservation biology (e.g., Patterson and Atmar 2000)—an arena within which classical island biogeography tried to help but largely failed (Gilbert 1980: 230; Hanski and Simberloff 1997: 17–19).

It might be possible for landscape archaeologists to morph along with island biogeography and try to emulate metapopulation biology, but instead of proposing that what archaeologists now need is metapopulation archaeology, I would like to return to a thought I had when I first came across The Theory of Island Biogeography in Staver's Bookshop on the south side of Chicago in 1971. Recently Hartmut Walter (2004: 189), who is highly critical of MacArthur's and Wilson's ideas as expressed in this classic text, observed nevertheless that both were right to stress the importance of islands. I thought so in 1971, and still think this. I also continue to believe—as I did when I bought a copy of Haggett's Locational Analysis in Human Geography (1966) in Cambridge, Massachusetts, in 1968—that the formation processes at all levels that have led to what is now the "archaeological record" obviously had their real-world spatial dimensions.

Combine these two thoughts—the analytical importance of islands (Renfrew 2004) and the spatial character of life's processes—and you have a basis for island biogeography.

A few years ago Robert O’Neill (2001) at Oak Ridge National Laboratory took a critical look at a
like equilibrium island biogeography, was once all the rage but that is now enjoying a much more muted coexistence with rival ways of thinking about Nature. Like MacArthur–Wilson modeling, this concept once looked like a practical approach to the complexity of natural systems. In both cases, however, it was too easy; some would say necessary, to leave history out of our equations, since both approaches made much of equilibrium, feedback, and homeostasis.

O’Neill observes that using a tool such as the ecosystem concept is not cost free: such tools are grounded on assumptions that can limit our thinking and predetermine the research questions we think to ask. In the case of ecosystems, he writes, the spatial distribution of the component populations may be much larger than the presumed boundaries of the system being examined, and this may lead to anomalies. Said differently, the stability properties of an ecosystem may not be explicable by a concept that considers only the dynamics occurring within those boundaries. Ecosystems, like islands, may be more “open” than is scientifically convenient.

At first, it might seem that MacArthur–Wilson equilibrium models do not suffer from this limitation, because they make so much of species coming and going from one place to another. This difference, however, is more semantic than real. It is just a question of how you draw your boundaries, or if you like, it’s just a question of scale. In this regard, O’Neill has offered a number of observations about ecosystem modeling that can be applied more broadly. Here are several of them restated in some-what more generic terms (2001: 3280–82):

1. An ecosystem works on an array of spatial scales going from the local up to the full potential dispersal range of the species within the local system being considered;
2. The potential scale of an ecosystem is set by the environmental constraints on each species, the character of barriers to their dispersal, and the particular ways that the various species involved get about from place to place;
3. The potential size or scale of an ecosystem is not constant or uniform over time and can change, for example, with changes in climate, geological events, and the like—these changes may be slow and thus fairly easy to adapt to, or they may be rapid and cataclysmic;
4. Furthermore, conditions all across the

are unlikely to be uniform, either locally or on a larger scale;
5. Over the long run, what maintains the sustainability of ecosystems is heterogeneity in time and space. “Stability to smaller scale impacts depends on the system’s ability to resist change and recover with resilience. But long-term stability or sustainability depends on a flexibility of response that can only be maintained in an environment that varies with time and space” (2001: 3281).

It is unnecessary, I think, to spell out how directly these observations can be applied also to human systems of interaction, economic engagement, and political control.

These are not the full measure of what O’Neill has to say about the character of ecosystems. I have selected these observations because by making these observations O’Neill is trying to make this concept, this tool for thought, work better. As he concludes: “Is it time to bury the ecosystem concept? Probably not. But there is certainly need for improvement before ecology loses any more credibility” (2001: 3282).

By way of summary, he identifies three key elements that he suggests cannot be ignored in model building if we want to understand how the world works, because the real-time processes and constraints thus hallmarked are unlikely to be encompassed within the boundaries of the local ecological system: spatial pattern, extent, and heterogeneity. Each of these dimensions, he maintains, is critical to ecosystem stability (2001: 3282). He is probably right. The biogeographer Lomolino (2000) would add that individuals, groups of individuals, and species differ in their abilities to respond to variation in the spatial patterning, extent, and heterogeneity of the world around them, and, furthermore, their abilities to do so evolve over time. Said colloquially, individuals, groups, and species do not stand still—and the importance of including a dimension that many nowadays call “agency” in our model building holds true for species other than just Homo sapiens (Terrell 1986: Chapter 7; Terrell and Hart 2002; Terrell et al. 2003)

One major implication for landscape archaeology, island or otherwise, that may be drawn from these observations is fairly certain. In thinking about the stability of ecosystems, it won’t do to see Homo sapiens as an external disturbing force. Instead, we are a keystone species within ecosystems. Looking to our own endurance as a species, he observes: “In the long term, it may not be the magnitude of extracted goods and services
our disruption of ecological recovery and stability mechanisms that determines system collapse” (O’Neill 2001: 3282).

I think this observation may also be applied retrospectively. We are taught as young archaeologists that to be successful at explaining prehistoric cultural stability and change we cannot neglect that we are part of—not external to—the world we live in (Terrell et al. 2003). Therefore the challenge we face cannot be ignored: how are we to grapple with this fact in a successful way? If we are part of the problem, so to speak, how can we expect to see our way to a solution?

This is why I think it is a mistake to throw away tools for modeling the issues we want to study as archaeologists on the grounds that they have become no longer fashionable or overlook something we now emphasize more strongly than we once did (Terrell 1997). Speaking personally, I do not consider the education of young archaeologists complete without advising them to read about and evaluate for themselves such venerable (or new) tools for thought as the ecosystem concept, the equilibrium theory of island biogeography, locational geography, landscape ecology, and metapopulation biology. A fly fisherman knows that different fish and different fishing conditions call for different kinds of flies. Instead of throwing away the investigatory tools we have acquired over the years, we need to focus instead on deciding which of these tools may be able to help us do whatever it is we want to do right.

Conclusions

The great thing about islands for archaeology is that they invite, even beg, for comparative study—which in the eyes of many remains a primary concern of anthropology broadly defined (e.g., Curet 2005; Kirch and Green 2001; Rainbird 2004). In the right hands (e.g., Curet 2005; Irwin 1992, 1999), islands lead us to confront the essential diversity that is so characteristic of our species. And as Broodbank has said: “Although island biogeography must not (and has no pretensions to) set the main agenda for island archaeology, we merely impoverish ourselves by entirely denying its relevance out of a faddish distaste for Darwinism in the social sciences” (1999: 237; also 1999: 28).

Note


References


O’Neill, R. 2001. Is it time to bury the ecosystem concept? (with full military honors, of course!).
Part III: Thinking through Landscapes


