

Fads and fallacies in the name of ecology

The Moon in the Nautilus Shell: Discordant Harmonies Reconsidered by Daniel B. Botkin. Oxford University Press, 2012. US\$29.95/£18.99 hbk (424 pages), ISBN 978 0 19 991391 6

Jeffrey C. Nekola

Biology Department, University of New Mexico, Albuquerque, NM 87131, USA



In broad brush strokes, the principle messages relayed in the *The Moon in the Nautilus Shell* by influential ecological modeler Daniel Botkin are: (i) ecological systems are both spatially and temporally dynamic, making equilibrium or steady-state assumptions routinely invalid; (ii) stochastic processes are critical to ecological process; and (iii) resource management and policy based on assumptions of system stability are doomed to failure.

These points are anything but novel for anyone who has been paying attention to the ecological field over the past 30 years. Back in the early 1990s, when I was in graduate school, an understanding of the individualistic nature of communities was expected of all undergraduate ecology students, seminars in ‘disturbance ecology’ were being offered to both upper-level undergraduates and graduate students, and nonlinear dynamics (aka chaos mathematics) was impacting the field on both theoretical and popular levels (e.g., [1,2]). Over the intervening decades, an even greater emphasis has been placed on these and related topics.

Why then was this book written? Clearly, the author’s motivation is part memoir, with much of the text detailing seminal personal experiences that helped him understand the lack of balance in nature. However, other motivations also exist: Botkin spends more than a chapter musing about the philosophical reasons that steady-state perspectives have proven so appealing, even though they are easily falsified. He also considers the importance of keeping an open mind and continually confronting current paradigms with data, as well as the poor track record that the ecological sciences have generally displayed in letting data, and not belief, dogma, and politics, drive our understanding of the world.

These are critically important insights. It is thus particularly discouraging how easily Botkin falls prey to the exact same flaws. First, little of the relevant ecological literature published since his 1990 book *Discordant Harmonies* [3] is mentioned. Although early on he states that only ‘a small group of ecological scientists’ share his outlook on the dynamic quality of ecological systems, this belief seems to be based on an inadequate reading of the current literature. The voluminous work done on neutral community assemblage, nonequilibrium disturbance, complex ecological systems, fractality and other forms of nonlinear dynamics, and the role of stochasticism and

entropy (e.g., [4–7]) is simply not discussed. Given these topics include some of the most active areas of ecological research over the past two decades, it is hard to justify why they were not reviewed.

Second, Botkin spends almost two chapters talking about how essential disturbance, particularly fire, is to the maintenance of diversity in many USA grassland and conifer forest communities. However, he then chooses to ignore the accumulating empirical evidence documenting catastrophic biodiversity losses across many taxa groups following the reintroduction of fire into reserves (e.g., [8,9]). Such works suggest that the widespread improper use of fire management represent one of the single most harmful immediate threats to biodiversity within the USA today.

Perhaps the most embarrassing lapse relates to the title of the book itself. As Stephen Gould did earlier in *The Panda’s Thumb* [10], Botkin uncritically recounts a now 35-year-old paper that purports to show the importance of multiscale interactions, with planetary orbital factors influencing the construction of individual cephalopod shells. Unfortunately, immediately upon publication, this work was shown to be based on faulty assumptions and has now been widely discounted [11].

It is thus hard to see how *The Moon in the Nautilus Shell* is ‘poised to be a core text’, as the dust jacket predicts. At least, not for ecologists. However, perhaps we are not the anticipated audience. Based upon the solicited blurbs on the back cover, it seems likely that this book was principally written for resource managers and policy makers. If this is so, it suggests a troubling disconnect between pure and applied ecology, with the latter field lagging perhaps a quarter of a century or more behind the former. Too often I have seen resource managers ignore empirical findings when these contradicts strongly held beliefs based on what they were taught decades before. Too infrequently have I seen such managers keep an open, inquisitive mind that continually confronts their existing worldviews with both the current literature as well as their own quantitative observations. The result is almost always disastrous for the systems involved.

If this book can help modernize resource management pedagogy and catalyze a more rapid response of applied ecology to new concepts stemming from pure ecological research, then it will have indeed affected a much-needed revolution.

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Bill Hamilton: a portrait with warts and all

Nature's Oracle. The Life and Work of W.D. Hamilton by Ullica Segerstrale, Oxford University Press, 2013. US\$35.00/£25.00, hbk (440 pp.), ISBN 978-0-19-860727-4

Brian Charlesworth

Institute of Evolutionary Biology, School of Biological Sciences, University of Edinburgh, Edinburgh, EH9 3JT, UK



As readers of *TREE* are undoubtedly aware, William (Bill) Hamilton was one of the most influential evolutionary biologists of the second half of the 20th century. Ullica Segerstrale's *Nature's Oracle* is the first full-length biography to appear since Hamilton's untimely death early in 2000, and provides a mass of details about both his professional and personal lives. It conveys a vivid impression of a brilliant but

flawed person, who had a very strong sense of what he wanted to achieve, and a lack of social skills that seems unusual even by the low standards of many scientists.

From a very early age, Hamilton had a deep curiosity about the living world, and an intense interest in understanding it in terms of evolution by natural selection. As an undergraduate, he soon discovered Fisher's *The Genetical Theory of Natural Selection* [1]. This inspired much of his future work; the problems of altruism, sex ratio evolution, selection in relation to age, sexual selection, and the evolution of sex and recombination, were all discussed by Fisher. He decided to work on the problem of altruism during his final year at Cambridge University. He was fortunate to be taken on by the London School of Economics and the Galton Laboratory at University College London, and allowed to pursue this goal under light supervision. Hamilton clearly felt isolated and misunderstood during his PhD work, and the letters from this period that are quoted convey feelings of self-pity that seem to have persisted throughout his life. He complained that he was never invited to give a presentation on his work, but from my own experience this was quite normal at the time; PhD students were then largely to be seen and not heard.

Segerstrale makes it clear that, from the start of his career, Hamilton was unusually sensitive to what he saw as a lack of due recognition. At times, he displayed an

almost paranoid view of some of his colleagues. The most notorious example of this was his reaction to John Maynard Smith's 1975 review of E.O. Wilson's *Sociobiology* [2] in the *New Scientist* [3], which is covered in considerable detail. To be brief, John mentioned that J.B.S. Haldane had made a remark in a London pub conversation to the effect that 'he was prepared to lay down his life for eight cousins or two brothers'. Hamilton refused to accept this as true, even after it had been pointed out that Haldane had published something very similar in 1955, in an article in the popular science publication *Penguin New Biology* [4]. Hamilton all but accused John of having invented the story in an attempt to minimise the importance of his contributions. This led to several years in which he was deeply hostile to John, and even refused to attend scientific meetings at which John was present. In reality, John was always ready to express his admiration for Hamilton's work on kin selection, and I remember his expressing this very clearly in a lecture to a Cambridge undergraduate society in 1965. It was only when H.J. Eysenck confirmed that he too had heard Haldane's remarks that Hamilton relented, in a letter to John in 1980 quoted by Segerstrale.

These examples illustrate the major strength of the book: it provides revealing details of how Hamilton worked and thought, and how he interacted with his peers. One of its weaknesses is that Segerstrale does not seem to have a broad knowledge of evolutionary biology, and so does not do a very good job of putting Hamilton's work into the context of developments in the field as whole. Although the areas in which Hamilton worked were indeed very significant growth points, and were driven forward by his insights, there were several others that were also advancing rapidly over this period, to which Hamilton made little or no contribution. For example, no mention is made of molecular evolution, a hugely important area of modern evolutionary biology that was largely ignored by him. In discussing Hamilton's visit to Japan in 1986, Segerstrale implies that there was then no important Japanese

Corresponding author: Charlesworth, B. (Brian.Charlesworth@ed.ac.uk).