tional and genetic changes that follow as consequences of insular colonization. In addition, the nature of such changes on continents and on islands is compared. This is postcolonization evolution, and two kinds of selection are delimited: *K* selection, whereby a more efficient utilization of available resources is favored, and *r* selection, which results in a higher population growth rate and productivity. Selection for *r* is dominant in new and growing populations in the early stages of colonization, and in unstable habitats where species may be repeatedly colonizing because their numbers are being regularly and dramatically diminished by environmental vicissitudes. Old and stable populations exhibit the results of *K* selection, as it were, a closer fit to the environment. M. L. Cody's analysis [*Evolution* **20**, 174 (1966)] of the variation in clutch size among birds of different biologies and environments is discussed as a good example of the interplay of *K* and *r* selection in different environmental situations. The average clutch size in birds is known to be large in the higher latitudes or elevations of continents, and smaller in the lower latitudes and elevations of continents and on islands. This suggests that natural selection in density-dependent species operates for quantity of offspring produced in the unstable environments, but more for quality of the (fewer) offspring produced in the more stable environments. Since changes in *r* and *K* indicate changes in life-history parameters, MacArthur and Wilson's discussion of the possible evolutionary significance of selection by the two methods raises important problems for all ecologists. This discussion, derived logically from the previous discussions by L. Cole and R. Lewontin of life-history phenomena and the role of *r* in population adaptation, is a useful one. It may well be that the recent arguments over density-dependent and density-independent regulation of population size and over individual versus group selection can now be reexamined in a more realistic manner. Presumably this revaluation would be made not in "either-or" terms, but in equilibrial terms according to which one process may override the other in certain circumstances, with environmental variation or certain historical factors regulating which process predominates.

In addition to advancing new ideas and updating old ones, the book directs attention to many problems in biogeography, the accounts of which have previously been scattered in various journals or books. One would hope that all ecologists and evolutionists will take the time to read it in its entirety. The authors are clearly attempting to stake out a new area in the biological sciences. In order to do this, they are deliberately bypassing many conventional and standard problems in ecology. They are calling for a new approach utilizing the principles of population genetics and ecology in a more precise approach to evolutionary ecology. Undoubtedly some biologists will say that the authors, and MacArthur in particular, are simply posing untestable models or setting up straw men against the only model that is valid in the context of the data advanced. This is not true, and indeed if it were true it would be an unimportant criticism. The authors are practicing a hypothetico-deductive approach to their scientific interests, not unlike L. Slobodkin's effort for community ecology some years ago. They appear to be very successful in this practice.

Hopes and Prospects

There is in science a natural selection of new ideas. The success of an idea expressed in a paper or in a book is tested by the frequency with which it, as compared to other, related ideas advanced previously or at the same time, appears in future years or generations. As the recent increase in numbers of papers concerned with species diversity, species-area phenomena, and character displacement that have been appearing in journals of ecology and evolution testifies, the research of MacArthur and Wilson during the past five to ten years has generated a marked enthusiasm and response on the part of many contemporary biologists. This response in itself can be considered a sign of selection for their ideas. It is to be hoped that, in the future, experimental studies of single species as well as of the biotas of small islands or of special environmental situations on continents will result in major advances in our knowledge of "the biogeography of the species." And, in their concluding chapter 8, the authors call for specific and detailed studies of this type. They emphasize that investigators can create "miniature Krakatus" on small islands by removing, by poisoning or by manual means, certain or all elements of the biota. Thereafter the immigration, colonization, and turnover rates can be measured for the propagules reaching an island. For certain insects and other organisms having short life cycles, investigators may well simulate biogeographic phenomena.

If MacArthur and Wilson's hopes are realized, we may see a new day for the science of ecology. My only reservation about this is whether or not such experimental studies can provide precise information on the composition of the gene pool and the genetic changes that are manifested during the "biogeography of a species." Population geneticists are presently having a field day describing isozymic variation in natural populations, however, and are finding a wide array of allelic diversity at many if not most loci of the genome. If these geneticists or others can discover the adaptive significance of such isozymic variation and its contribution to the fitness of the individual whose phenotype is the unit of natural selection, and if these techniques of genetic analysis can be successfully applied to populations of "manipulated biotas," we may see a development of population biology or community ecology without a parallel in the biological sciences since the rise of molecular biology a decade or so ago. Finally, at the minimum, it is clear that this book by MacArthur and Wilson is the most significant and original contribution to biogeography since P. J. Darlington's book *Zoogeography: The Geographical Distribution of Animals* of 1957.

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Beginnings of Hydrology

On the Origin of Springs. PIERRE PERRAULT. Translated from the Paris, 1674, edition by Aurele LaRoque. Hafner, New York, 1967. 213 pp. $15.

As far as is known, this is the first translation into English of the full text of the work that O. E. Meinzer said established Perrault as one of the principal founders of the science of hydrology. That distinction stems from Perrault's measurements of rainfall over the upper basin of the Seine, his estimates of discharge from that part of the basin, and his calculation that yearly precipitation was about six times the runoff. The study was the first quantitative demonstration —albeit crude—that precipitation is adequate to account for the flow of springs and rivers. About a decade later Marriotte confirmed Perrault's results. Between the two, the ancient idea that precipitation was quantitatively inadequate to account for runoff largely was laid to rest.

Perrault's discourse includes also the first, or at any rate a very early, description of (i) sublimation of ice and snow, documented by loss of weight from a continuously frozen block of ice; (ii) interception of snow by trees and consequent loss of moisture to the forest floor; (iii) bank storage and alternate recharge to and discharge from aquifers in hydraulic continuity with rivers (of course he did not use the modern terms); (iv) fresh-water-salt-water relations, with some concept of the importance of the differing densities of the two fluids in explaining fresh-water wells near sea coasts; and (v) the role of slow drainout of saturated earth materials in maintaining the low flow of rivers. Oddly enough, Perrault persisted in believing that springs are fed by rivers and steadfastly refused to believe that precipitation could wet the soil and rock to depths sufficient to allow what we now would call recharge. From sources obscure, he also maintained the existence of a universal layer of clay at varying depths below land surface and attributed to the clay a major role in control of movement of water underground.

A discussion of that clay layer and a taste of the turgid style that permeates the book follows:

> The cause of these fluctuations is due to the arrangement of the continuous clay that is under the low plains and under the mountains, I mean that when the slope of this clay is not toward the current of the River, and that there is a cavity there in which much water remains which cannot flow into the River with the rest, or else that there is one or many of these basins we have mentioned, larger than in other places, the waters that remain in them can supply the continual evaporations, and the springs they produce flow with a continuous and almost even flow, because there is enough material to keep them in that state: but when this clay slopes toward the River, and there are only a few of these basins, or they are small, or there are none at all; and that by these means the water that is in the sands flows to the River, and these basins being small are soon emptied, the springs thereby suffer various decreases, and sometimes dry up entirely: so also on the contrary when the overflow water of Rivers has risen enough to enter these basins and on top of these beds of clay higher than the usual ones, which does not happen often; then since there is more material for evaporation, the springs become stronger and have more than usually copious and abundant flow.

The style, clearly, is Perrault's. The translator has leaned toward the literal rather than the interpretative translation in order, he says, to avoid reading into his statements more than Perrault knew or intended.

Soil scientists will be fascinated by a section (pp. 78–81 of the translation) that describes Perrault's expansion of Magnanus' studies in capillary rise, flow in unsaturated media, and demonstration that passage of salty water through soil does not—as many ancients maintained—remove the salt. The translator suggests that Perrault deserves a place as a precursor of soil scientists.

This is an important book in the history of earth sciences. Its appearance in English is a tribute to the diligence and scholarship of the translator—it is copiously and excellently footnoted—and to the zeal of George W. White, recently elected vice-president for North America of the International Commission for the History of Geological Sciences. White "practically commissioned" the work by presenting LaRocque with a copy of Perrault's book on the condition that he translate it, as he also stimulated A. V. Carozzi to translate J. B. Lamarck's *Hydrogeology* (University of Illinois Press, 1964).

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Nuclear Structure

Unified Theory of Nuclear Models and Forces. G. E. BROWN. North-Holland, Amsterdam; Interscience (Wiley), New York, ed. 2, 1967. 271 pp., illus. $9.25.

The second edition of Brown's book calls for a second edition of this review [the first appeared in *Science* **148**, 622 (1965)]. The book has grown by simple accretion, by addition of chapters with no molding of the earlier chapters to the new need. The original title "Unified Theory of Nuclear Models" implies an explanation, from a common ground, of why the shell model, the optical model, and the collective model work; but there is very little of the fascinating applications to nuclei that give life to the models. The "and Forces" added to the title in the second edition is not descriptive of the added content, which is mainly a treatment of nuclear matter with its "effective forces" and alludes to the meson theory of nuclear forces only as it relates to these.

It is helpful to have impressive and difficult aspects of nuclear structure elegantly and concisely presented, but the book has too high a ratio of mechanics to philosophy, of superb mathematical tricks to needed transitional thought. It may help the very exceptional student soar, but in the hands of others it could contribute to the lamentable process of bringing up a generation of physicists adept in applying specialized tools but without a facility for approaching new problems.

The dubious pedagogy of compressing very involved nuclear-matter calculations into three additional chapters is illustrated by the question the reader encounters: What does it mean to create two holes in the single state *m*? One learns how to evaluate diagrams containing them, but not why. The answer would involve the additional bookkeeping of canceling disconnected diagrams, an operation which in a lecture might be called tedious but without which one wonders why one should be dazzled with the diagrams at all.

There is a lot of hard work being done and to be done to further our understanding of nuclei. The book is ambitious and perceptive in showing where hard work is being done and in doing some of it. Understanding could be improved if the link with first principles were firmer.

At one lighter point, with a lack of subtlety not typical of the serious parts, the author garbles de-Shalit's familiar introduction to a talk on finite nuclei: "Some of my best nuclei are finite."

Despite shortcomings, despite some disunity in the presentation of a "unified" theory, it must be said that nowhere else is to be found in so little space so much of the main thread of the far-flung and rapidly evolving modern theory of nuclear models.

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On Telling

Speaking and Writing in Medicine. The Art of Communication. CLIFFORD F. HAWKINS. Thomas, Springfield, Ill., 1967. 177 pp., illus. $8.50.

The broad appeal of this book is partly hidden by the "in medicine" of the title. Two chapters, which occupy only about one-sixth of the text, are in the particular province of the medical practitioner: "Listening to patients" and "Talking to patients." Their central focus can be described in a phrase: the doctor himself as a therapeutic