would lead one to expect. She attributed to the eminent not only "above average" heredity but also "superior advantages in early environment," persistence, confidence, and strength of character (C. Cox, *The Early Mental Traits of Three Hundred Geniuses*, 1926, pp. 215–218. Readers should also check, on pp. 68 and 72, Cox's figures and explanation of the reasons for eliminating two raters against Gould's account of them).

Giving the Army Beta to his undergraduate class, Gould discovered some of the absurdities of the procedure that had plagued the Army testers. The grades his class received, incidentally, were above the distribution for World War I officers, in spite of the datedness of some of the test items. Altogether, Gould's treatment of Cox's work, of the Army tests, and of some other specifics may be a bit more polemical and whiggish in tone than it needs to be, and not always quite accurate. But more space than is available here would be needed to spell out such judgments.

Beneath this history of "mismeasures" lies, Gould thinks, the fallacy of "reification," of turning "intelligence" into a single measurable thing. But this diagnosis remains blurred, since Gould's emphasis seems to shift about. Exactly what does he object to: A single "general intelligence" (*g*), instead of multiple abilities? Measuring the unmeasurable? The "thingness" of *g*, as contrasted to nonmateriality, process, or what? But no tester, however obsessed with the importance of IQ, ever thought of it as literally an independent "thing in the head." The hypotheses linking intelligence to brain processes, energy, and the like may have been wrong and naïve, and "reductionist" to boot. But, though Goddard was clearly naïve (and wrong) in linking feeblemindedness to a single gene, to accuse him of a logical fallacy seems less like wisdom than like being wise after the event. As for reductionism, some link between intelligence, however defined, and brain processes would be assumed by most biologists I know (which does not make it true).

Come to think of it, Gould never tells us directly what his own proper, unreified conception of intelligence is. He does use "mentality" (p. 24), even has "no doubt . . . that IQ is to some extent 'heritable' " (p. 155). He also says: "Causal reasons lie behind the positive correlations of most mental tests" (p. 251), and believes that "a factual reality exists . . ." (p. 22). All that does not solve my problems with his "thingness;" it also makes me wonder if Gould's problem is to have reified the testers' metaphors. As for measurability, testers have indeed often taken their numbers too seriously. But Gould does not seem to reject all testing.

There remains the question of "singleness." After a clear exposition of the basics of factor analysis, Gould points out that it does not provide a unique solution; hence it cannot decide between theories of a single intelligence (*g*) and of multiple abilities. But that may not be too helpful either, as extramathematical considerations, including usefulness, now become relevant. The National Merit exam measures two separate abilities—then adds them together to award scholarships, without worrying much about a reified *g*. Reification may be a bad thing indeed. But Gould's diagnosis seems too formalistic; I think it misstates the issue.

Of course, there is another definition of reification, as abstracting a part of the concrete, interconnected world, isolating it from its context, and turning it into an "object," a commodity. This alternative seems to come closer to the point. It also links reification to the other root fallacy identified by Gould: "ranking," on a single dimension in terms of worth. Now ranking remains no longer the strange obsession of individuals, presented by Gould in thumbnail sketches without much explanation why and how they became so involved. Instead, it puts the whole problematic back into its social-historical context. Not just individual hangups but technological and political decisions of all kinds demand a (unidimensional) ranking of multifaceted realities, together with a legitimation of the ordering used.

A few days after reading Gould's book, I received my October (1981) number of the *American Psychologist*, a special issue on testing, with some 20 articles by experts (and one "outsider"). Reading this issue was like stepping into a world different from Gould's. A lead article proclaims a well-grounded science of human abilities to be alive and well, another comments on the recent growth of testing, and on p. 1129 we are informed of the ultimate reification, in the second sense: that a computer programmer rated at the 85th percentile on job performance is worth $20,800 per year more to the employer than one at the 15th percentile and that cognitive ability (read: intelligence) tests will select the better programmer.

In short, ability testing is out there, a sizable industry in the "real world," and a smaller one in academia. And all Gould's incisive thrusts at "finagling" and "fallacies" seem to be almost irrelevant to it; Burt's name appears only once in the 20 reference lists in the *American Psychologist*. Let me not be misunderstood. None of these experts are "racists." (Almost all of them step gingerly around the issues of heritability and race differences.) There is much concern with legal issues, affirmative action, special education, and the social responsibility of testing—together with complaints about the bad, and largely unfair or at least exaggerated, press testing has received. In all, there is hardly any direct contact between Gould's arguments and the issues occupying the experts. I am not sure just what to make of this contrast; and this is not the place to speculate. It does seem to mean, though, that whatever intellectual victories over the (mostly dead) testers Gould's eminently readable book achieves, its categories may not be particularly helpful in dealing with present realities; the real action seems to be elsewhere.

FRANZ SAMELSON
Department of Psychology,
Kansas State University,
Manhattan 66506

The Beetles

The Biology of the Coleoptera. R. A. CROWSON. Academic Press, New York, 1981. xii, 802 pp., illus. $139.50.

It is often said upon the retirement of a taxonomist that he or she should write a general natural history of his or her particular group, so that years of unpublished observations and speculations will not be lost. R. A. Crowson, the world's leading authority on Coleoptera, has far exceeded this expectation by producing a modern reference work covering virtually every aspect of biology that in any way relates to beetles. The magnitude of the task completed may be appreciated when one considers that the order Coleoptera includes more described species than there are vascular plants and has an evolutionary history dating from the Permian. Crowson's exhaustive treatment of beetle biology is based not only on his own experience but on an up-to-date survey of the literature (the bibliography contains almost 1200 entries, of which 40 percent are later than 1970).

The three chapters on the morphology of adults and immatures will be particularly useful to those concerned with problems of beetle classification, because many commonly used terms have not been properly defined or illustrated previously. The next several chapters

include a variety of topics falling under the headings of physiology, behavior, development, life cycles, cytology, and genetics. In these sections, Crowson combines an extensive literature review with his own grasp of variation within the Coleoptera and places the results in an evolutionary framework. The remaining chapters cover special topics of interest to ecologists and systematists alike, such as adaptations to aquatic habitats, beetles in dung and carrion, predation and defense, symbiotic relationships, and herbivorous beetles.

The chapter "An ecological triangle: beetles, fungi and trees" deals with the complex relationships between Coleoptera associated with decaying plant material and the fungi, whose activities make this food source available. Included is a wealth of information on wood borers, cambium feeders, and the inhabitants of leaf litter, as well as those species feeding on fungal spores or fruiting bodies. In this and the previous chapter ("Symbiotic and parasitic relations"), considerable attention is paid to small and often setose cavities on the surface of adult beetles and their possible role in the transport of fungal spores. Although such structures (called mycangia) have been demonstrated in ambrosia beetles (Curculionidae: Platypodinae and some Scolytinae), it is premature to assume that the variety of cavities mentioned and illustrated by Crowson have a similar function.

The chapter on geographical distribution begins with the expected coverage of dispersal powers, global distribution patterns, and faunal characteristics of the major regions. The topic of recent extinctions and changes in distributional limits in northern Europe leads to an interesting discussion of conservation practices and the dangers facing species with limited ecological niches. In the final chapter on evolutionary history, Crowson incorporates his own ideas on the phylogeny of beetles, mostly published elsewhere, with the growing body of fossil evidence coming out of the Soviet Union. At the end of the chapter, he examines certain evolutionary generalizations, such as Dollo's law, in the light of knowledge about the history of beetles. Some cited examples of evolutionary reversal, however, might actually reflect the need for reexamination of the original phylogenetic hypotheses.

Students of Coleoptera may be confused by the classification adopted for the book and included as an appendix. Certain family concepts, such as Empelidae, Hobartiidae, Cryptophilidae, and Megalopodidae, are either entirely novel or have not been used previously by Crowson and his students. The placing of Stylopidae (Strepsiptera) in the superfamily Lymexyloidea is discussed on pp. 71–75, but in this reviewer's opinion the evidence is not convincing.

There are quite a few redundancies and typographical errors, and the book would have been more readable if the chapters had been subdivided. Regardless of minor faults, however, *The Biology of the Coleoptera* is and will remain for years to come the definitive work on the subject.

JOHN F. LAWRENCE
Division of Entomology, Commonwealth Scientific and Industrial Research Organization, Canberra 2601, Australia

Insects and Adaptations

Insect Life History Patterns. Habitat and Geographic Variation. Papers from a symposium, Denver, Colo., Nov. 1979. ROBERT F. DENNO and HUGH DINGLE, Eds. Springer-Verlag, New York, 1981. xii, 226 pp., illus. $29.80. Proceedings in Life Sciences.

Within the last few decades, there has been a shift in the types of organisms most frequently studied by animal ecologists, from vertebrates (especially birds) to insects (especially herbivorous insects). At the same time ecology has increasingly embraced an adaptationist paradigm, in which the life histories of organisms are viewed as strategies deployed to meet differing environmental challenges. This collection of papers is a product of these new emphases; it uses insects to challenge several ecological platitudes (such as the primacy of competition) and exemplifies the promises and limitations of an adaptationist viewpoint. Through an introductory chapter and three synoptic essays, the editors weave together an unlikely blend of life history studies, ranging from demography to diapause to community organization to island biogeography.

Several papers in this volume examine environmental variability, highlighting its consequences for the success and evolution of insect life histories. Among the most provocative of these is a contribution by Whitham, in which he suggests that within-plant somatic variation is an antiherbivore adaptation. Casual observation of almost any plant-insect association will reveal that herbivorous insects concentrate their feeding on a small subset of any particular plant's tissue. Whitham takes a fresh look at this ordinary observation and shows that insects often suffer from increased competition or predation as a result of aggregating at restricted feeding sites. He does not, however, provide evidence indicating that variability by itself is an adaptation; to do so would require data on the fitnesses of plants that differ only in their degrees of somatic variability. Edmunds and Alstad also consider the challenge that plant variability presents to specialized herbivores, focusing on between-plant (as opposed to within-plant) variation in ponderosa pines. This chapter is a rehash of a paper (*Science* **199**, 941 [1978]) that is widely cited as providing an example of a herbivorous insect (the black pineleaf scale) with low mobility that has evolved counteradaptations to the defenses of individual plants. This interpretation may be premature since evidence of genetic differentiation among demes of scale insects is lacking. Furthermore, the transplant experiments performed by Edmunds and Alstad do not rule out an alternative hypothesis that involves neither differentiation nor adaptation on the part of scale insects. In particular, scales transferred from infested trees to uninfested trees may fail not because the scales are uniquely adapted to particular infested trees but because all uninfested trees are unsuited to all scale insects. Such unsuitability could well be a result of historical or environmental factors acting on the trees. Before it is concluded that genetic differentiation and adaptation are responsible scales should at least be transferred among different infested trees. By sampling insect populations from geographically distant areas, other authors are able to document striking examples of heritable differentiation in life history traits. Istock offers an especially tempting sketch of his extensive studies of life history variation in pitcher-plant mosquitoes. Combining quantitative genetics, field observations, and a simple model, Istock first demonstrates the genetic basis of inter- and intrapopulation variation in diapause strategies and then explains this variation as a bet-hedging tactic in response to a fluctuating environment. In some instances, the insights that make sense of life history patterns derive from careful natural history and not from expectations of theory. For example, after finding classical *r*- and *K*-selection theory to be unhelpful, Blau argues that adjustments in body size for thermoregulatory purposes underlie the life history differences between temperate and tropical swallowtail butterflies. One theme