

board member represented approximately 250 constituents. Citizens exerted substantial control over school policies by regular and personal contact with these officials. In response to arguments made on grounds of efficiency by professional school administrators, the number of districts has been so reduced that today there exist fewer than 17,000 districts. Each school board member, on the average, now speaks for approximately 3000 constituents. The opportunity for local citizens to exert control over schools has suffered as a result. The condition is exacerbated by the growing tendency to elevate educational policy decisions to the state level and to permit teachers to engage in collective bargaining. Not only are there fewer school boards, they have less authority. Into the power vacuum have stepped professional educators. From the fundamentalists' perspective, the educational professionals have abused this power by promoting a sectarian doctrine, "secular humanism."

Ironically, the very processes that have placed the large majority of Americans at a greater distance from school decision-making permit a determined minority to have disproportionate influence. The concentration of school decisions in the hands of fewer officials, often at the state level, as in statewide adoption of textbooks, enables highly organized groups, no matter how small, to exert great leverage. Their energies can be concentrated upon a single target, the state board of education or the state legislature. If they had to try their case in hundreds of school districts, the slender numbers and resources of the creationists would be swamped by the larger majority who support evolution, either actively or by being willing to leave professional educators and academics to teach as they see fit.

Nelkin's book, though somewhat abbreviated in parts, is by any measure readable and by most measures accurate. In the absence of clear empirical evidence regarding the teaching of evolution in high schools it may be, however, that she has overdrawn the case. Without question, antievolutionists continue to succeed in their efforts to provoke political conflict. Those of us who work closely with public schools, however, continue to be impressed by the distance between policy-making bodies and the classroom. Most school board members at both the local and the state level know, even if they cannot afford to say it loudly, that their decisions are only loosely coupled to the organizational dynamics of schools and the activities that take place in classrooms. Public school

policy-makers have too few sanctions, either positive or negative, at their command to influence teachers widely. Consequently, it may be that several generations of science teachers, having been steeped in evolutionary theory by their college teachers, are now instructing in the Darwinian vein with little concern for the political efforts of creationists. Moreover, courts exhibit increasing reluctance to grant creationists equal textbook space. The courts have found such tactics to be in violation of the First Amendment's prohibitions against the establishment of religion.

Regardless of the extent to which Nelkin makes the case that evolution is being neglected in the classroom, she conveys a larger message. As is clear from the controversies that swirl around such undertakings as nuclear energy projects, science is now an integral part of public policy decisions, and we will henceforth be observing the degree to which science and democracy can coexist. If science is increasingly a concern of public policy, then science instruction in public schools will increasingly be politicized. School politics reflects social controversies as surely as species originate by natural selection, and for that reminder we can be grateful to Nelkin.

JAMES GUTHRIE

School of Education, University of California, and Board of Education, Berkeley

The Upward Path

Scientific Elite. Nobel Laureates in the United States. HARRIET ZUCKERMAN. Free Press (Macmillan), New York, and Collier Macmillan, London, 1977. xvi, 336 pp. \$14.95.

American Nobel laureates in science (persons who did their prizewinning work in the United States) tend to come from professional or business families, to go to elite schools, to be recognized early, to work under leaders in their fields, to publish more and to work harder than their unprized contemporaries, to receive their prize in early middle age and to have their lives changed by it. None of this, as Zuckerman says, is very surprising. "Ascent into the ultra-elite [of prizewinners] follows an almost commonplace script." Her contribution is to quantify it.

Here are some of her numbers. Eighty-two percent of the 61 laureates educated in the United States came from professional or business families as against 90 percent of Supreme Court jus-

tices and 48 percent of holders of doctorates in science. Eighty-five percent of the laureates got their Ph.D.'s at one of 13 institutions; 52 percent worked under prizewinners past or future. They earned their doctorates 4.7 years earlier than the average scientist, published more than four times as many papers, did their prizewinning work at an average age of 39 and received their prizes 12 or 13 years later. The prize made them public figures, lured some into punditry (a number here would be intriguing), ended collaborations prematurely (joint winners still working together at the time of the award continued together another 5.4 years, single winners and their prime collaborators parted after 3.6 years), and reduced by 35 percent the productivity of those who had not previously enjoyed the attentions of the public.

Some interesting points emerge. The elevated age at which the prizewinning work is accomplished suggests that science is not, as it is often said to be, a game for late adolescents. The age has not changed much over time: in the case of physics, 36.7, 33.9, and 37.0 for prizes awarded during the years 1901-25, 1926-50, and 1951-72, respectively. Another point concerns recognition before the prize. Citations in the professional literature to work by laureates-to-be just before their awards averaged 222 a year as against 6.1 for the common scientist. Seventy-eight percent of winners were members of the National Academy of Sciences when they were called to Stockholm. As Zuckerman observes, the Nobel prize does not go to unknowns. A third point is collaborative research. In the years 1901-25, 41 percent of the prizes were awarded for work done in collaboration; in 1926-50, 65 percent; in 1951-72, 79 percent. Here, as in much else, the laureates led the masses. During the same three periods, the percentages of published papers with multiple authors were 25, 51, and 71.

The world is no doubt richer for these numbers. It could have been still richer. Francis Galton found that great Victorian scientists had small heads. How does the matter stand with American Nobelists? Are they longer or shorter than the average scientist? Have they fewer children, more wives, odder hobbies, stranger pets? Zuckerman's line of inquiry does not run to such questions; she seems to take not biological or personal characteristics but early and continued access to "advantages"—excellent instruction, elite institutions, all the necessary resources—as the chief determinant of scientists of prizewinning quality. She wonders whether other scientists, if

equally advantaged, would not have done as well. She does not ask whether the average Nobelist is smarter than other people.

Another quibble might be raised. Zuckerman relies primarily on material from biographical dictionaries, from formal Nobel proceedings, from interviews she has conducted with surviving laureates, and from sociometric studies. These resources are not always reliable; a wider reading of history might have prevented blunders at important points in the argument. To take but one example, the award of the prize in chemistry in 1904 to William Ramsay (rather than to Ramsay and Soddy) is not, as Zuckerman thinks, recognition of only the senior member of a collaboration. Ramsay won for his detection and isolation of the rare gases during the years 1894 to 1899; Soddy first worked with him in 1903, on the chemistry of radon.

Zuckerman's statistical tables are well constructed and easily read. Her elucidations are commonsensical. They are also often banal ("There is evidence that a small number of scientists contribute dis-

proportionately to science"; "Their responses in the long run and the impact of the prize on their careers depend in some measure on how old they were when they got it.") The cause might be that her numbers do not sum to a distinctive group portrait: they might also characterize Supreme Court Justices, officers of the American Medical Association, or members of the President's cabinet. Or, as Zuckerman puts the point: "It may be that evocative environments [elite schools, distinguished professors, good resources] enhance opportunities for doing excellent science in ways that are formally akin to the mutually reinforcing effects of environments with high crime rates where vulnerable individuals become criminals." Precisely. But the problem is not to report the rates, but to characterize the vulnerability, to look for the traits (if any) that predispose people to commit the sort of crime that brings the Nobel prize.

J. L. HEILBRON

*Office for History of Science and Technology,
University of California, Berkeley*

Unfoldings of Mental Life

Intelligence in Ape and Man. DAVID PREMACK. Erlbaum, Hillsdale, N.J., 1976 (distributor, Halsted [Wiley], New York). xiv, 370 pp., illus. \$16.50.

Language Learning by a Chimpanzee. The LANA Project. DUANE M. RUMBAUGH, Ed. Academic Press, New York, 1977. xxii, 312 pp., illus. \$17.50. Communication and Behavior.

In 1885 Sir John Lubbock, Lord Avebury, reported to the British Association for the Advancement of Science a "Note on the Intelligence of the Dog":

Hitherto we have tried to teach animals, rather than to learn from them: to convey our ideas to them rather than to devise any language or code of signals by means of which they might communicate theirs to us. The former may be more important from a utilitarian point of view—though even this is questionable—but psychologically it is far less interesting. Under these circumstances, it occurred to me that some such system as that followed with deaf mutes, and especially by Dr. Howe with Laura Bridgman, might prove instructive, if adapted to the case of dogs.

So it came about, nearly a hundred years ago, that an English gentleman attempted to pass on the torch of language not to

a chimpanzee but to a black poodle called Van. Lord Avebury's method of teaching his dog to "read" and "write" was remarkably close to that which was later to be used by David Premack in his early experiments with the chimpanzee Sarah. In honor of the dog and man, it seems proper to quote the original paper at some length:

I have tried this in a small way with a black poodle called Van, by taking two pieces of cardboard, about ten inches by three, and printing on one of them in large letters the word "food," leaving the other blank. I then placed two cards over two saucers, and in the one under the "food" card I put a little bread and milk, which Van, after having his attention called to the card, was allowed to eat. This was repeated until, in about ten days, he began to distinguish between the two cards. I then put them on the floor, and made him bring them to me, which he did readily enough. When he brought the plain card I simply threw it back, while when he brought the "food" card I gave him a piece of bread, and in about a month he had pretty well learned to realise the difference. I then had some other cards printed with the words "out," "tea," "bone," "water," and a certain number also with words to which I did not intend him to attach any significance, such as "naught," "plain," "ball," &c. He soon

learnt that bringing a card was a request, and to distinguish between the plain and printed cards; it took him longer to realise the difference between words, but he gradually got to recognise several. If he were asked whether he would like to go out, he would joyfully pick up the "out" card, choosing it from several others, and would bring it to me, or run with it in evident triumph to the door. The cards were not always put in the same places, but were varied indiscriminately, and in a great variety of positions. Nor could the dog recognise them by scent, for they were all alike, and continually handled by us. Still I did not trust to that alone, but had a number printed for each word. When, for instance, he brought a card with "food" on it, we did not put down the identical card, but another bearing the same word; when he had brought that, a third, then a fourth, and so on. For a single meal, therefore, eighteen or twenty cards would be used, so that he evidently was not guided by scent. No one who has seen him look down a row of cards and pick up the one he wanted, could, I think, doubt that in bringing a card he feels he is making a request, and that he can not only distinguish one card from another, but also associate the word and the object. This is, of course, only a beginning, but it is, I venture to think, suggestive, and might be carried further, though the limited wants and aspirations of the animal constitute a great difficulty. [*Report of the British Association for the Advancement of Science*, 1885, p. 1089; see also *The Life-Work of Lord Avebury* (Watts, London, 1924)]

Fortunately the wants and aspirations of the chimpanzee are not so limited. Indeed, the chimpanzee's eagerness to learn and to exploit its new-found skills has contributed as much as its native intelligence to the success of recent attempts to teach it language. For Premack's and Rumbaugh's chimpanzees the playroom has been the schoolroom, the schoolroom the playroom. When Rumbaugh and his colleagues have wished on rare occasions to punish their chimpanzee Lana the worst thing they could think of doing to her was to switch off her electric "typewriter."

But another, more insidious limitation has until recently shackled the efforts of psychologists to teach language to non-human animals: lack of faith that anything would come of it. Before the Gardners' pioneering experiments with Washoe few people seriously believed that a chimpanzee could be trained to communicate with human beings in human language. A few eccentrics might have dreamed of it, but almost no one who valued his scientific reputation would have committed himself to a full-scale research program to demonstrate it. The reason lies deep within us all: our lack of faith in the linguistic abilities of animals is founded on our faith in the uniqueness of ourselves.

Such faith is not of course unjustified. It is a fact of nature, no mere fiction of human ideology, that people are pro-