The authors' justification for making this kind of comparison is implicit (another possible justification, that it is traditional in the behavioral sciences, is worthless). They have *imagined* a socialist policy alternative, which would operate directly to equalize incomes, working conditions, and so on. Such a policy could presumably operate to offset the effects of luck as well as of systematic social forces such as family background. But comparing a real cause in the world with the strongest cause one can imagine, rather than with other causes actually operating, gives an artificially deflated estimate of the importance of the real cause. Thus much of the argument of the book comes down to arguing, for one systematic social cause after another, that it is unimportant compared to luck.

Aside from the fact that such a procedure is a proposal for the abolition of behavioral science, it seems to me not justified for policy analysis. The equalizing effects of the most socialist policies ever instituted in the United States, the progressive income tax and the social security system (which equalizes among ages, not among social classes), are relatively small. I doubt if they much exceed the effects of equalizing education over the past four decades. Thus it seems to me more reasonable to compare the effect of a particular systematic social cause to the total effects of all the systematic causes we can find, excluding luck. In my summary, I will therefore try to assess the size of various effects as compared with the total *explained* inequality, rather than as compared with total inequality. This means, for example for income, that I will ignore the 77 percent of the variance that is, as far as we know now, due to luck (see fig. B2, p. 339) and try to assess the importance of causes relative to the *socially patterned* inequality, the other 23 percent.

The most striking finding is that, no matter how schools are assessed, *which* school a child goes to has a negligible effect on success, however measured. Schools may be integrated or segregated, expensive or cheap, with rich students or poor students, or merely ranked by degree of success, but differences between them make very little difference to students' success. The idea that schools make a big difference is a statistical illusion. Schools whose students have high IQ or achievement scores, or that have high percentages of students going on to college, do so almost entirely because the students in them *come* to school with high scores and with family backgrounds that lead to college. A student may possibly be disadvantaged by going to school with all smart kids, because it makes him feel dumb, but the effect is trivial in size. He may on the other hand be advantaged because he learns more from his smarter peers, but that effect is also trivial in size.

What happens *within* schools has a large effect. In particular, whether a student ends up on a college preparatory curriculum is the dominant immediate determinant of whether he goes on to college. This in turn is strongly influenced by his intelligence test scores and his grades, and influenced some much smaller amount by his social background *aside* from aspects of social background that determine IQ and grades. Schools hardly discriminate at all by pure racial or social class background. Almost all the apparent discrimination is due to social influences on test scores and grades. However, most of the slippage between high school preparation and college attendance is explained by sex and social background. That is, college preparatory students who do *not* go on are largely working class, or women, and those on other curricula who *do* go on are largely from richer families, and men.

Children's intelligence is the dominant determinant of adult cognitive abilities, with years of education (nowadays much of this is a measure of the college/noncollege distinction) an important supplementary cause. Since children's intelligence scores are a dominant determinant of years of education, this means that in a practical sense years of education and adult intelligence are almost the same variable. Whatever that mixed variable is—certificates or true competence—it is by far the dominant cause of what level of job people get. Tests of adult mental competence allow us to explain a little bit more of differences in jobs above the amount explained by years of education.

The dominant determinant of income is, of course, whether or not a person holds a job, with old people, women, children, people just entering the labor market, people in seasonal, capital goods, or weapons industries, and Blacks being principally disadvantaged. Like most recent studies of inequality, this book systematically ignores the causes of being out of the labor force or being unemployed, although it does for a change include women in the analysis. Once a person has a job, the dominant cause of his income is what kind of job it is, with some smaller effect from adult measures of cognitive competence.

But income is poorly predicted by sociological or genetic IQ variables. That is, luck plays less of a role in whether or not a person becomes a physician than it does in whether he becomes a very rich or only a well-to-do physician; luck plays less of a role in determining that a person becomes a factory operative than it does in determining whether he works all year in the high-wage chemical industry or only part of the year and in the low-wage cannery industry.

Of course, by the time the originally weak effects of differences between schools on children are further attenuated by luck in getting more education, luck in getting a good job, and luck in getting a high income out of that good job, they are completely trivial. Anything we now know how to do to elementary and secondary schools, including spending money on them, integrating them or resegregating them, grouping according to ability within them or not, adding preschool and kindergartens to them, will have trivial effects on the eventual incomes of the children in the schools.

The policy implications the authors draw from this are, first, that school policies and expenditures should be evaluated by what kind of life they give children, rather than by what effect they might conceivably have on the life of 50-year-olds 40 years from now; and second, that if one wants to equalize incomes, give the poor money, not education.

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Civilian Thinkers

The Private Nuclear Strategists. ROY E. LICKLIDER. Ohio State University Press, Columbus, 1972. xiv, 214 pp. $11.

During recent years civilian students of nuclear strategy have been the targets of severe attacks from both the right and the left. Critics on the right have typically argued that such matters are too important to be entrusted to civilians, and they point, with more than a touch of envy, to the fact that "in the Soviet Union, the strategy recommended to the Politburo is made by military professionals and not by a comparable cortege of economists, sociologists, psychiatrists, and comptrollers, the groups that have sought to monopolize strategic thinking in the United States." Attacks from the left often convey the image of a tightly knit, secret coterie of Dr. Strangeloves posing a threat both to world peace and to democratic institutions in America. Roy Licklider's systematic study of "the private nuclear strategists" sheds light on the personal and professional attributes of civilian, nongovernmental strategists, as well as on some of their beliefs and attitudes. His findings provide little if any support for the more extreme charges from either the right or the left. But this book is a sociology of one segment of the strategic community; it was not the author's intention to address all the troubling questions about the defense establishment.

Of 491 private civilian strategists—identified through authorship of at least one book or three articles on problems of nuclear strategy—to whom Licklider sent his long questionnaire, 191 responded. In an appendix the author carefully compares the respondents with those who failed to return the questionnaire. Political scientists and younger students of strategy were oversampled, but otherwise the 191 respondents are representative of the selected population. Their answers to 65 questions provide the data base for the book.

The central hypothesis informing Licklider's study was the suspicion that private strategists clustered into several homogeneous groups of like-minded persons. He expected to find professional communication within these groups but little between them. The data failed to support this image in any important respect. As a group the civilian strategists have many attributes of a pluralistic community.

The empirical core of the study consists of six chapters that explore differences between "influentials" and others, the effects of military service, motivations and frustrations, type of employment, academic discipline, and professionalism. Each chapter is liberally documented with tables—there are 50 in the book—most of which are characterized by an absence of statistically significant relationships. That is, in most respects one cannot predict the distribution of the strategists' attitudes, beliefs, or policy preferences from their other attributes.

In light of the controversies surrounding government-sponsored research, some of the most interesting findings concern the type of research the respondents would *refuse* to undertake for moral or political reasons. Examples range from research on techniques for staging a military coup in the United States (67 percent would refuse) and for establishing a Doomsday Machine (64 percent), to work on plans for unilateral disarmament by the United States (34 percent), and for establishment of world government (8 percent). Although those with experience on contract research were somewhat more willing to undertake what the author calls "right wing" projects of the Doomsday Machine genre, they were no less willing to do research on unilateral disarmament or world government.

In the concluding chapter Licklider suggests that strategic thinking has reached a plateau, partly as a result of widespread acceptance of some seminal ideas of the 1950's and 1960's, partly as a consequence of the Vietnam war. He predicts, however, that military technology—specifically the MIRV and the ABM—will lead to a revival of work on strategy. The book closes with a brief discussion, and perhaps overly optimistic dismissal, of the "garrison state" hypothesis that "specialists in violence" will come to dominate public policy.

Licklider has effectively achieved his goals, but his book is more likely to be of interest to sociologists than to political scientists or strategy buffs. We learn a good deal about the civilian strategy community, but relatively little about the linkages between the ideas and controversies that marked the development of strategic thinking since 1945, and their progenitors. We are told that these strategists—whether among the "influentials" (as identified by their peers) or not—feel highly efficacious, but we learn little about the complex political process by which their ideas may or may not have an impact on strategic doctrine, weapons procurement, and the like. Licklider's data support the contention that the community of private strategists is a pluralistic one, for example, but it does not necessarily follow that the "politics of defense" can be so characterized. In short, *The Private Nuclear Strategists* performs a useful service, but the real significance of Licklider's findings will be most apparent when they are systematically linked to the entire process by which strategic decisions are made.

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Questions about DNA

Evolution of Genetic Systems. A symposium, Upton, N.Y. H. H. SMITH, Ed. Gordon and Breach, New York, 1972. viii, 580 pp., illus. Cloth, $20; paper, $6.95.

One of the current central problems of biology is to arrive at a better understanding of the structure and function of the genetic material. We know that DNA is the basic source of information that is passed from generation to generation, but curiously it is present in hugely different quantities per cell as one proceeds from one kind of organism to the next. We expect viruses to have less nucleic acid than bacteria (which they do), because the free-living bacteria have more functions to perform than viruses. But it appears that in evolution from the primitive prokaryotic state to the eukaryotic the amounts of DNA per nucleus have increased tremendously (from 10- to 100,000-fold). This is certainly all out of proportion, since it is difficult to conceive that eukaryotes have literally thousands more different functions to carry out in their metabolism than do prokaryotes. Furthermore, related plant and animal groups show great differences in DNA content. For example, among the terrestrial vertebrates the Amphibia have the most DNA while the birds, on the average, have the least, and mammals are in the middle. These observations along with the apparent high degree of redundancy of DNA in the eukaryotes pose perplexing questions to be answered about the evolution of genetic systems and their functions.

This book is a written report of the proceedings of the Brookhaven Symposium in Biology held in the summer of 1971. The preface states that "the purpose . . . is to bring together recent