ESP, "on this side we are not deceived because we observe so clearly and our logic works," then deception has a very specific meaning. It means the other side, where the trickster is simply the man who *intends* to deceive. Such an intentional trickster may well have played his role in every ESP experiment. But if we say that there is a more universal trickster at work, and that unconscious deception is always a likely occurrence in all attempts to discover truth, then who can guarantee the absence of deception in any experiment?

Hansel seems to feel that the sloppiness of the ESP experiments could be removed if the experimenters used mechanical devices in shuffling cards, transmitting messages, and so forth. He may be right, for every experiment that was ever run is subject to improvement; but it is doubtful if mechanization is the sole answer, as everyone knows who has tried to work with computers in a man-machine symbiosis. It is amazing how easily deception occurs on such occasions.

But even if "clearcut" experiments could be devised by more controllable mechanisms, we would learn very little from them about deception or ESP. A parapsychologist could assert that the very existence of the mechanisms "cuts off" ESP. I think it would be much more to the point to test ESP against a very astute and self-conscious trickster—for example, a professional magician. Could such a person achieve scores that are "highly significant" without the experimenter's being able to tell how he did it? If the method was revealed (at the price of retirement for the magician) wouldn't we then learn something about the role deception plays in ordinary as opposed to extra-ordinary science?

At the risk of overworking the etymology of two terms with the same very common Latin root, I note that perception and deception are two modes of "capturing" nature, "through" and "away." It is sound to say, in the mood of Hegel, that there is no perception without deception. For Descartes, deception was an evil, created by a perfectly malevolent being. To assure himself that deception cannot occur in the simplest and clearest of the facts we humans accept, he argued that we must first prove that the perfect trickster cannot operate in this domain because there exists a benevolent god who prevents him from doing so. We have found Descartes' solution unacceptable. Instead, we could say that the most there is to be learned from an experiment is to be found in the ways in which the experimenter was deceived. If so, the "purpose" of science is to create a satisfactory theory of deception. It is doubtful if modern science has accomplished all there is to accomplish in this direction.

But what of ESP? Is it "true"? It is surely true as a human feeling, and no amount of criticism of Hansel's type will have much effect on this feeling. As can be seen, I'm in favor of choosing the second tactic mentioned above: to interpret ESP in a "satisfactory" manner, for example, to call it nonintentional deception in perception. Of course I realize that this definition has the advantage and the disadvantage of being unacceptable to the parapsychologist, who wants to keep the mystery and the reality at the same time. He wants to say that there "really" is a mysterious linkage between some minds and some events. But I am saying that what we humans take to be "really" the case always has elements of deception in it. Thus I can't help asking whether the "wonder" about ESP is no more than a manifestation of an unreflective and pure acceptance of the findings of modern science.

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Biology and Taxonomy of the Protozoa

Protozoology (Thomas, Springfield, Ill., 1966. 1188 pp., illus. $15.95) by Richard R. Kudo is now in its fifth edition. This fact alone attests to the book's lasting value as a text and reference. As in previous editions, the bulk of the reference value lies in the second of its two parts, Taxonomy and Special Biology. This section has now been expanded to include newer genera and species, and the nomenclature has been revised to include the new system of uniform endings for names of higher taxa. For the biologist who is not a protozoon taxonomist, Part 2 is possibly the best and cheapest source (in English) for brief descriptions and relationships of Protozoa.

For the more serious student, however, the book will be a starting place rather than a definitive work. (In the preface Kudo describes it as a university text.) Certain errors of omission occur which will seem inexcusable to specialists in the groups involved. For example, the lower Trypanosomatidae are inadequately handled (p. 420), neither the well-justified separation of *Blastocrithidia* from *Crithidia* being recognized nor the status of the literature-infesting name *Strigomonas* being mentioned. In addition, the peculiar and indiscriminate use of the words *Trypanosoma*, Trypanosoma, and Leishmania (capitalized, italicized, and not italicized) for generic names and Trypanosoma, Leptomonas, Leishmania, leptomonas, and leishmania (capitalized and not capitalized) to designate body forms will do little to help students understand this family. (The widespread use of non-italicized generic names, even for hosts of parasitic protozoa, is a regretful legacy from previous editions and is distracting to the reader.) Likewise, many malariologists will deplore references to such things as a "motile ookinete." The handling of such confusing organisms as *Sarcocytis* and *Toxoplasma*, however, is to be commended. Perhaps the author should have justified his placing of these genera in the Haplosporida, but his reluctance to assign them to lower taxa is a true reflection of their present status. The expanded information on *Toxoplasma*, emphasizing its role as a human pathogen, is an important addition.

Perhaps careful scrutiny of every taxonomic group would reveal inadequacies, and even beginning protozoologists may wonder at the arrangement which seems to include the Opalinida in the subclass Peritricha. Any failings, however, should be interpreted in light of the fact that this book is the only current text to present descriptions and a taxonomic scheme of all groups of protozoa, down to genera and representative species.

Part 1, General Biology, is perhaps subject to sterner criticism. This sec-

tion is characterized by a heavy infusion of pre-1940 literature, which is both a virtue and a disappointment. Protozoology is a huge, growing, and complex field, and Part 1 provides the beginning student with a much-needed background. Many classic papers are cited in the chapter bibliographies (that is also true of Part 2). On the other hand, these feats are accomplished at the expense of newer knowledge (a drawback if the present work is to stand for another 12 years). A case in point is the discussion of mitochondria (p. 93). Certainly the present knowledge of mitochondrial function and the characteristic structure of protozoon mitochondria is not so new that it could not have been included. Likewise, the section on nutrition (p. 115) seems to ignore the advances of the 1950's in the growth of protozoa in defined media. As in previous editions, the value of some illustrations is limited by insufficient labeling (Figs. 1, 25*b*, 25*c*, 26, 32), and one may find references to authors' names but not to specific papers.

Many of the shortcomings of both parts are offset by the expanded chapter bibliographies and improved English usage (the book reads much better than previous editions). The production of a satisfactory text on either the general biology or the taxonomy of the Protozoa is very difficult. In general the author has again succeeded admirably in combining the two aspects in a single relatively inexpensive volume. It is indeed hard to imagine anyone who must deal continually with protozoa, either in the field or in the literature, without his worn copy of "Kudo." I see no reason why the fifth edition should not continue the tradition.

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Scientists as Diplomats

The nuclear test-ban treaty of 1963 marked a turning point in the nuclear arms race. Study of the decade-long negotiations provides an opportunity to examine the whole web of world political change. Equally important, the negotiations were an important setting in which men of science acted as diplomats. Four conferences were conducted primarily by scientists—the Conference of Experts (1958), the two Technical Working Groups (1959), and the Seismic Research Program Advisory Group (1960)—and at least one scientist was always assigned to the American negotiating team.

Diplomats, Scientists, and Politicians: The United States and the Nuclear Test Ban Negotiatons, by Harold Karan Jacobson and Eric Stein (University of Michigan Press, Ann Arbor, 1966. 548 pp. $8.50) tells the tale and is a useful addition to the literature on the impact of science and technology upon public affairs. The authors had access to the full minutes of formal sessions, and they here weave a chronological narrative which, though complicated, is always absorbing and lucid. An authors' analysis provides background, but in general the facts are allowed to speak for themselves, a method which tends to refute the tendentious and facile generalizations about scientists as "a new breed."

At one point in the technical talks, James B. Fisk responded to a Soviet remark by rejoining that "science is not the servant of political expediency" (p. 227). The perspective in this account proves exactly the opposite. The technical issues were very real, but they could not be separated from the facts of bargaining positions and assessments of national interests.

Throughout the negotiations, U.S. policymakers tried to make this separation. At each step, the scientists found themselves forced to act as political bargainers, frequently (as far as the U.S. was concerned) with inadequate political guidance. The role of "experts" was political from first to last. In effect, the technical men provided an alternate forum where the nuclear powers could resume their dialogue when other forums broke down. The U.S. resort to this forum was often an effort to evade the fact that American disarmament policy was divided and uncertain. Emphasis on the technical unknowns of detection served to gain time in the face of deep conflict in Washington over the whole role of nuclear weapons in diplomatic and military global strategy. It remained for a new president, John F. Kennedy, to wrestle the dilemma into a coherent military capability which, by 1963, permitted adoption of the unsupervised partial test-ban as part of a general stabilization of the nuclear arms race.

As an epilogue, the authors provide a perceptive, closely reasoned essay which limns the moral of the tale. The role of the scientists differed little from that of conventional diplomats; the talks were always primarily political. Scientific expertise was essential but lacked any singular magic or authority. Effective decision-making requires that "policy makers . . . receive advice from several scientists rather than one" (p. 483), in order that personal and political values may be isolated. In this way responsible politicians reserve the right of decision and indeed are forced to face up to such judgments as should be neither delegated nor obscured.

If this book has a weakness, it lies in the explanation of key departures from policy during the negotiations. Because they stick closely to the formal sessions, the authors are often forced to speculate on the sources of policy change and fail to go beyond official rationalizations. The internal politics of decision-making are not yet completely on the record. But this does not detract from the value of their work in making the record more complete. Here are no startling revelations, no inside dope, few fresh insights into the behavior of nation-states. Instead, we find a solid account of America's adjustment to its role as superpower and nuclear giant trying to find its way through a variety of highly technical and highly political bargaining situations.

By 1960, nuclear testing had become a kind of shadow-war. While negotiators sat across the table from each other in Geneva, the thunder rumbled from the wings, each side testing, raising the background radiation of the planet, and daring the other to raise it still higher. Testing became in effect a demonstration of force to support other forms of diplomacy. The period of reprisal testing was happily ended by the treaty of 1963.

This account of the negotiations vividly demonstrates the contradictions of U.S. policy in what might be termed the classical period of the nuclear age. The inability of the West to develop a consistent diplomatic strategy and military capability prevented a unified negotiating posture. Added to all the other factors of world diplomacy, this prevented an earlier assessment of Soviet intentions, obscuring the changed nature of the Soviet threat in the late 1950's and the need to revise U.S. military strategy in the direction of nonnuclear forces. The authors' work provides another opportunity to continue the process of national introspection in a changing and challenging world.

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