flow from scientific discoveries are a matter of random chance and that nothing can be done to accelerate the conversion of new knowledge into new technology. The second, and the more prevalent view today, is that there is a systematic relationship between scientific and technological growth; the advance of the former by necessity contributes to the advance of the latter. Ben-David's position is that, though there is no necessary connection between scientific and technological activities, steps can be taken to increase the probability that scientific advances will be exploited by technologists. This means increasing the motivation and the opportunity to find uses for science.

"The purpose of policy should, therefore, be to influence the likelihood of these chance occurrences [between scientific knowledge and practical needs] by increasing the density of both kinds of activities and the velocity of the circulation of ideas and problems from both areas of activity in spaces which ensure interaction. Increasing the density is a matter of investment, velocity is the result of entrepreneurship, and creating the properly enclosed spaces is a task for organization" (p. 61).

The creation in Europe of these necessary conditions for rapid scientific and technological advance is the third concern of Ben-David's study. He begins by noting a widespread consensus in Western Europe with respect to the need for reforming scientific research and education: the establishment of larger research facilities, especially of a multidisciplinary nature, the suppression of the chair system of university organization in favor of the American departmental system, and the development of graduate training in science. But there is little consensus when it comes to the reform of the university itself or of the overall national system for the support of science. With respect to these larger and basic issues Europeans may be divided into the "reformists" and the "revolutionaries." The "reformist" position is that the European system for the support of science is basically sound; the problem is principally one of inadequate financial support and of establishing new institutions to support new fields of research which cannot find a home either in the university or in industry. In other words, Europe's backwardness can be overcome by tinkering with and adjusting the existing system. The "revolutionaries," on the other hand, believe that Europe's problem is long standing and inherent in the European science system itself. "The relative backwardness of European science in certain fast-growing fields is, according to this view, only a single instance of the chronic weakness of the system to take up fast-growing fields altogether." What is required, therefore, is not minor organizational alterations but structural change. The overthrow of the existing system in favor of a system resembling that of the United States should be the goal of national science policies.

What Europe needs, in Ben-David's view, is a strategy which over time will transform European scientific and technological institutions. Specifically, he advocates that governmental funds for research and higher education be allocated on the basis of institutional merit and in a manner which would leave the details of policy-making to the institutions themselves. Secondly, he advocates exchange and mobility, especially among the European states, in order to stimulate competition and entrepreneurship.

As Ben-David cogently demonstrates, and I have sought to show elsewhere with respect to France [*France in the Age of the Scientific State* (Princeton University Press, 1968)], the task of transforming the European system will not be an easy one. What is involved is the altering of essential features of European social-political life: limited social and geographical mobility, overcentralized decision-making for science and the absence of strong, financially autonomous universities, the existence of national boundaries which prevent the creation of a European Common Market for brains, and the separation between the university and the economy. Unfortunately, the creation of an environment which would benefit science and technology is strongly resisted throughout Western Europe. Powerful academic and administrative interest groups prevent needed reforms of the European university system. One of the ironies of the present situation in fact is that those groups which are on the left politically and represent change are most conservative with respect to reform of the university. One cannot expect, therefore, to see the needed reforms come out of the protests and upheavals currently convulsing European universities.

ROBERT GILPIN
Woodrow Wilson School, Princeton University, Princeton, New Jersey

Accounting for Aborigines

Origins of the American Indians. European Concepts, 1492–1729. LEE ELDRIDGE HUDDLESTON. Published for the Institute of Latin American Studies by the University of Texas Press, Austin, 1967. x + 179 pp. $5. Latin American Monographs, No. 11.

The quiet title of this book gives no hint of the painstaking care with which the author has found, studied, and compared the works of the bickering European writers who, misquoting and plagiarizing each other for over two centuries, sought to explain the presence of man in the New World. While some might call the book history and others might call it anthropology, it emerges as an important essay in the history of ideas.

The most reasonable and economical explanation of the presence of red men in the Americas is the now commonly accepted one that the human population was derived from Asia. In addition to being commonsensical the idea is supported by many biological, archeological, and geological data. This is the "modern" view, resting on current research. Among laymen another popular and widely held belief is that the original American population was derived from Europe or the Near East, with the ten lost tribes of Israel usually being credited with the colonizing voyage.

Thanks to Huddleston we can know that the theory of an Asiatic origin, supported by logic applied to scant but empirical data, was first put forward by Joseph de Acosta, a Spanish Jesuit scholar, in 1589. So much for modern thought. And the ten-lost-Hebrew-tribes theory was not proposed by Vespucci, as is often claimed, but was proposed much later.

The history of speculation on American Indian origins, as Huddleston traces it, goes thus:

The earliest writings about the American Indian (by Columbus and Vespucci, among others) reveal no curiosity about origins, but by 1530 the fact that a New World—a land unaccounted for in theological knowledge—had in fact been discovered led to great scholarly uneasiness and the necessity of finding an explanation. Moreover, this explanation must fit into theological dogma, including both the creation myth and the story of Noah and the ark. In those days, of course, scholarship was largely the province of the clergy. Inevitably

the writing was theological in nature; the basic problem was to explain how the children of the ark reached the new continents that were unconnected to the Eurasiatic land mass. Most of the scholars who considered the question were from Mediterranean countries, but it was an English poet, John Rastell, who, in 1520, first posed the question of American Indian origins.

By 1535 G. F. de Oviedo suggested Carthaginian (Phoenician) origins, a theory ultimately going back to Aristotle. This theory persisted for centuries, although Oviedo himself, for political reasons, favored an ancient Spanish origin.

Francisco López de Gómara, who despised the Indians, was first to invoke Atlantis as their possible place of origin; this theory was picked up by Agustín de Zárate by 1555 and was developed by many later writers. At least two Portuguese writers—Antonio Galvão and Pero de Magalhães—by 1575 had suggested the Chinese as probable ancestors of the Indians.

By 1567, however, the Hebrew-tribes theory had been clearly enunciated and touched off a whole new series of largely theological debates. Apparently it should be credited to Joannes Fredericus Lumnius and Peralta, although it was soon confused with the Canaanite theory, which involves the curse upon Ham, not the Hebrew children. In many versions the Jewish-origin theory is viable today, but it developed much later than is generally believed.

By 1570, then, most of the theories (including some not mentioned above) were developed. All relied on authority, cultural comparisons, and some empirical data. It remained for Acosta in 1590 to utilize available evidence—"experience is more reliable than philosophy." Using valid cultural arguments against the comparisons of the earlier writers, and dismissing Atlantis and the Jewish theories, Acosta insisted on continuous close land connection between the Old and New Worlds and a separate culture history for the Americas. His argument rested in part on the distribution of animal species other than man. (Acosta's argument still had a theological base, in that he accepted as a controlling factor man's restriction to the Old World because that was where the ark landed.) According to Huddleston, Acosta's views draw most of their importance from the suggestion of a culture history and origin independent of Europe and in providing a set of logical ground rules for study of the problem.

Soon after Acosta came García (1607), whose aim, according to Huddleston, was to identify all possible origins for the Indians—and he evidently believed all of them to be possible. His techniques for argument seem to have obscured the fact that he supported all theories. The Acosta tradition is contrasted with García's because the latter reverted to, and reinforced, the comparative method.

Acosta had great influence in shaping thought among scholars in the northern European countries, while Spanish thought on the problem stagnated, García having essentially exhausted the possibilities for debate. Therefore during the 17th century the arena of argument shifted to northern Europe. Here the names of Grotius, de Laet, and Horn are well known. Using Acosta's zoological and distributional approach, de Laet and Horn had before 1700 concluded that Siberia was the ancestral home of the Indians.

There are many good things in this book, more than can be summarized here. Suffice it to say that its thoroughness and scholarship make it a contribution to both the literature of ideas and that of anthropology. The author shows great restraint in five short pages of general conclusion as he demonstrates that several respected Americanist authors have perpetuated mistakes (as did the Mediterranean writers), overlooking most of the primary material so carefully dealt with in this volume. Huddleston, understandably, does remind us that the Acostan tradition persists in modern anthropological thought, which is rereaching conclusions already widely supported during the 17th century.

JESSE D. JENNINGS
Department of Anthropology, University of Utah, Salt Lake City

A Study of a Primate Society

Social Organization of Hamadryas Baboons. A Field Study. HANS KUMMER. University of Chicago Press, Chicago, 1968. x + 189 pp., illus. $8.95.

This document is based upon a year's observation by the author and Fred Kurt of wild *Papio hamadryas* (Cercopithecidae) in their native habitat in Ethiopia. It is the first field study of this arid-land baboon. It was largely upon the behavior of members of this species in the London Zoo that Zuckerman based a theory of primate sociality which engendered a 30-year controversy over the role of mating bonds in the organization of social groupings of primates. Observations by other workers on other species in which the "harems" seen by Zuckerman were not prominent cast doubt upon his assumption that sexual attraction between adults was the primary basis of primate social organization. Some workers even doubted the validity of Zuckerman's observations, or attributed his results entirely to the crowded conditions of the zoo colony.

Kummer studied the ethology of hamadryas baboons in a colony in the Zurich Zoo before proceeding to the field and confirmed Zuckerman's observations that adult males collected and maintained exclusive groups of females. The field observations also revealed the existence of these groupings of one male with several females, but showed that they were merely the smallest units in a complex hierarchy of social units within the hamadryas population. The field study gave indications that the one-male units originate in the maternal behavior of adult males toward kidnapped juvenile females and that only later do sexual motivations become manifest. This finding adds a fascinating twist to hypotheses about the processes of social organization, and should warn against mono-causal explanations for complex social phenomena. The history of speculations about the social organization of hamadryas baboons, which have been finally resolved by the current study, shows the absolute necessity of placing observations within the context of the natural population living in an unrestricted habitat if they are to be understood.

Kummer's field study illustrates a particular concern with such context. Although much attention is given to the details of the one-male unit, the stated purpose of the study was to survey the entire system of social organization within the population. Accordingly the study began with an extensive survey, then narrowed down to increasingly intensive observations of smaller units. The picture that emerges of one-male units combining into two-male teams, which are themselves integrated