each newly designed component before moving on to the advanced designs than he was with high-altitude flights that might catch popular fancy but prove nothing. The Smithsonian officials were not unsympathetic; their dilemma simply mirrored a problem common to virtually all funding agencies. They wanted to foster sound research, but they also needed results that would impress those individuals, including congressmen, who could contribute to the Institution's endowment. Threatened with loss of support, Goddard reluctantly undertook to design for altitude by a 20-fold increase in scale, a move he was to regret because it increased costs and upset his careful step-by-step approach.

Goddard was ambivalent about publicity for his researches. A painfully shy man, he understood but recoiled from the necessity for record flights: "no support until results are had, and no results unless sufficient support is had." Moreover, publicity brought requests for details from investigators and government officials in Russia, Japan, Germany, and elsewhere. He responded when Hermann Oberth asked for a copy of his Smithsonian paper, only to be incensed when the German investigator made use of his fundamental ideas but disparaged the feasibility of his designs and ignored the priority of his work. This and several similar experiences intensified Goddard's tendency to secretiveness. Ironically, it was newspaper publicity that led the Daniel and Florence Guggenheim Foundation to consider supporting his research just as the Smithsonian phased out. Through the mediation of Charles A. Lindbergh, then at the peak of his popularity and prestige, the Guggenheims in 1930 made the first of the many large grants which were to sustain Goddard for more than a decade of rocket research at a site near Roswell, New Mexico, where climate and terrain were ideal.

The record of the years in New Mexico overflows with passages that give insight into the creative process, including diary entries on how the investigator's subconscious mind came up with novel theories and designs. Supported only by three or four machinists and helpers, Goddard was both scientist and engineer, projecting theories and then designing and building the apparatus in his own shop to test them. Step by step he worked his way through the problems: combustion chamber design for fuel efficiency and effective cooling, fuel flow (by tank pressure or pumps), stabilization during and after burn, and so on. This incremental approach led to the development of ever more complex rockets, by 1940 an 18-foot liquid oxygen and gasoline model producing 2800 horsepower, as well as to dozens of flights, the best of which reached approximately 7000 feet and a speed of 700 miles an hour. Seen in perspective, this was remarkable progress; Goddard all but single-handedly tackled problems sufficient to busy a corps of engineers.

While Goddard's fertile imagination and insistent drive were crucial, the contributions of others are not to be overlooked. President W. W. Atwood of Clark granted Goddard repeated leaves of absence in the face of departmental chafing; the Guggenheims provided not only money but remarkable understanding and patience. In this, Lindbergh's periodic assessments favoring continued support appear to have been decisive. (Was the reason behind them the appreciation of one loner for another?) Yet even Lindbergh was unable to persuade Goddard to cooperate effectively with others in the academic world, notably the Caltech group, which was beginning to take an interest in rockets in the late '30's. All such schools the lonely investigator regarded as threats to his priority; instead of training students and publishing his findings in scholarly journals, he secured patents, eventually 214 in all, on every major feature of his designs.

When the war came Goddard repeatedly approached the military services for financial support to press on with his rocket research, only to be disappointed. He was coldly furious when the National Research Council granted funds for research not to him but to Von Kármán's students at Caltech, whom he rated as 15 years behind himself. The military authorities were more than willing to employ Goddard as a consultant to pick his brains, but the only support he was able to secure was a contract to develop a variable-thrust device for jet-assisted airplane takeoffs. Though cruelly disappointed, he loyally poured his energies into this project, on which he was still working when the appearance of German V-2 rockets heralded a new era of warfare. Subsequent investigation revealed that in principle and in design the V-2 bore a remarkable similarity to the rockets for which he had vainly sought military funding. Even so gentle a soul as Goddard could be forgiven for harboring bitter thoughts at this turn of events, but the diary, at least as published, gives no hint of recrimination down to the date of his death from a malignancy in 1945. It is easy to blame the military for folly in dealing with Goddard, but his own papers suggest another view. The very qualities that drove him so relentlessly, committed him so completely, and unleashed his creative energies so fully imposed serious limitations on his capability for scientific cooperation; he was a man doomed by the sum of his own virtues.

This magnificent compilation offers a treasure trove for a multitude of readers. Historians of science and technology, rocket specialists, space buffs, students of the psychology of creativity, military officers, aerospace executives, and foundation administrators can mine insights almost at random from these fascinating pages, including the bibliography, patent checklist, chronology, and other information in the appendix. While the Guggenheims deserve credit for funding the publication of such a full record, lavishly illustrated with photographs, drawings, and diagrams, there can be no mistaking the primary role of Goddard's widow, who poured years of her life into the task of editing. For any who may question her exclusions and elisions at some points, the full manuscript record is available at Clark University.

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Darwin as Seen from Paris

La Sélection Naturelle. Etude sur la Première Constitution d'un Concept (1837–1859). CAMILLE LIMOGES. Presses Universitaires de France, Paris, 1970. 184 pp. Paper, 25 F. "Galien."

When, in 1878, Darwin became a corresponding member of the French Academy, his supporters found it necessary to have him elected to the botanical section. This episode has virtually become a conventional symbol of the curious history of evolutionary biology in France. Evolution was accepted most reluctantly, and natural selection even now seems to baffle the French mentality. The appearance, in the French tongue, of a serious work on the history of evolutionary theory thus presents us with an opportunity to con-

sider some aspects of national culture, as well as to profit from a somewhat different perspective.

Typically French is a high regard for the intellect. True to form, Limoges deals with the growth of an idea, natural selection. He not only exploits the rich supply of Darwin's marginalia and notes preserved at Cambridge, but also considers the works that Darwin used while he was developing his theory. It is especially pleasing to have information about Darwin's indebtedness to the French literature, particularly to Milne-Edwards and de Candolle. Perhaps the most important contribution is the demonstration that ecological ideas were crucial to the discovery of natural selection. This aspect of Darwin's work has been dealt with by earlier students of his work, but never in such detail or depth. Hence Limoges's book focuses upon a crucial point, and is most welcome.

Equally French is a deep concern for language. The positive benefit of literary excellence is sometimes offset by an excessive placing of style before content. Rhetoric is confused with logic, and words become more important than concepts. Such excesses would seem to have affected Limoges's analysis, so that his interpretation, although basically correct, sometimes deals with matters that others would consider epiphenomenal.

Limoges's thesis that Darwin's biogeographical thinking played an important role as his ideas developed can scarcely be denied. Yet precisely what biogeography contributed to the theory is open to question. Much of Darwin's concern for such matters is perhaps better explained as an effort to refute the idea of special creation than as a search for an evolutionary mechanism. Furthermore, the ecological aspect of Darwin's work becomes a procrustean bed, to which documents are fitted in a rather questionable manner. Historians now seem to be reaching a consensus in agreeing that Darwin did not become an evolutionist until after he had returned from his voyage on the *Beagle*. Limoges accepts this view, but perhaps goes too far. Time and again, he invokes purely negative evidence to show that Darwin had no evolutionary interests during the voyage. But he fails to mention the positive evidence that clearly demonstrates an early interest in "centers of creation" and in temporal changes in faunas.

The fallacy of negative evidence is again apparent in Limoges's criticism of the notion that the study of artificial selection helped Darwin to discover natural selection. He rightly observes that Darwin's consuming interest in artificial selection was a late development. However, we need not infer that it had no significance, merely because the term "selection" does not occur in Darwin's earlier notebooks. The perniciousness of such negative evidence is apparent when we find Darwin using the term "picking" in his second notebook on the transmutation of species. The concept is there, even though the word is absent.

Like so many historians, Limoges finds it necessary to explain away the influence of Malthus. At first sight this seems odd, for the main thrust of Limoges's argument is to show that Darwin developed a new conception of the natural economy. That a work on political economy might provide some crucial insight is therefore only to be expected. Limoges maintains that Darwin owed Malthus only an impression of the intensity of the struggle for existence. It seems to me that Malthus led Darwin to see who was struggling with whom and for what: the struggle involves a reproductive competition between members of the same species. This insight, the fundamental event in the Darwinian revolution, has been quite generally overlooked. Perhaps the resistance to natural selection, in France as elsewhere, derives from an insufficient appreciation of how much such a change in outlook implies. As Ernst Mayr has pointed out, Darwin's insight not only demolished the old conception of the natural economy, it refuted the whole system of metaphysics from which that conception derived. But older ways of thinking tend to linger on, particularly when fundamental to educational practices and religious beliefs.

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Laboratory Pharmacology

Importance of Fundamental Principles in Drug Evaluation. Proceedings of an American Pharmaceutical Association symposium, May 1968. DAVID H. TEDESCHI and RALPH E. TEDESCHI, Eds. Raven, New York, 1968. xvi, 496 pp., illus. $18.95.

These proceedings are concerned with the *laboratory* evaluation of drugs. They review and discuss the assets and liabilities of most of the methods that have appeared in the last several years and that are changing the scope and increasing the potential of pharmacological research. Investigations of drug metabolism, drug interactions, drug receptors, behavioral effects of drugs, pharmacokinetics, and pharmacodynamics are all represented. The value of studies of inter- and intraspecies variation in drug responses is stressed in clear, honest, and sometimes humorous language (Ahlquist). Newer discoveries in catecholamine synthesis and metabolism, dose-dependent kinetics, disposition of drugs as a function of drug action, morphologic and biochemical factors contributing to the differences in responses between organs, and observations on drug elimination by the lungs, all fundamentally important in modern-day drug evaluation, are well reviewed and referenced. The value of multidisciplinary approaches to drug evaluation is clearly demonstrated in chapters by Giarman and Beyer.

The professor of pharmacology or medicine could not hope to find more convenient and concisely written analyses of the merits and shortcomings of in vivo and in vitro assays in selected studies or better reasoning and examples to show his students why both are needed to solve most problems. The student will be particularly gratified by the spectrum of coverage; most chapters cover the subjects from their ideals and philosophy to the mathematics necessary to begin analogous experiments. The book will probably be most valuable as a reference text to be consulted for help in experimental design and in assessing the scientific merit of technical presentations. It will also be useful to the student interested in the new currents in pharmacologic research, who with its help will be able, for example, to assess the advantages and shortcomings of methods using isolated organs or parts of organs, administration of drugs directly into parts of the central nervous system, and electrophysiologic techniques, and to appreciate the complexity of approach in the new and exciting studies of drug interactions.

The book does have its deficiencies. There is little organized discussion of the principles of drug evaluation in man, and, as may have been surmised, no effort is made to present the difficulties of the evaluation of drugs as they affect disease (after all the *raison d'être* of pharmacology). Clearly the book may help the clinically oriented pharmacologist to understand, simulate, or try to