would be available for oral consultation; the systematic and periodic publication of up-to-date compilations dealing with each branch of research; and the creation through Unesco of an international organization for the preparation of card files based on the application in every country of laws prescribing the printing of abstract cards at the same time the work is published. It will require some legal compulsion to overcome inertia, nationalism, individualism, and competition, and thus promote the most rapid advance of research on an international scale.

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Vitality of the Aged

THE writer presented statistical evidence in 1929 (SCIENCE, 70, 85) of a persistent retrogression in vitality among the aged of both males and females of this country—in marked contrast with the distinct improvement at all earlier ages. Similar results were obtained by others, and this retrogression was reported to be generally accepted by the representatives at the recent International Gerontological Congress at St. Louis.

The writer now wishes to present evidence based upon succeeding years of a surprising improvement at these advanced ages. This report is brief and preliminary because we still await an analysis of the data from the 1950 U. S. census for further assurance that improvement has come to stay.

Table 1 gives the death rates (per 10,000) at ages 70 and 80 for both males and females for the earlier decennial years, as well as for the later years 1930 and 1940.

TABLE 1
DEATH RATES (PER 10,000)

Year	Males (age)		Females (age)	
	70	80	70	80
1890*	556	1227	502	1127
1900	606	1323	549	1206
1910	630	1387	569	1259
1920	605	1319	568	1225
1930	600	1309	517	1188
1940	599	1290	469	1138

* Seven states.

It should be noted that the maximum rates seem to have been reached sometime between 1910 and 1920 for both males and females, and that the rates have now fallen to about what they were at the beginning of the century, when official records of deaths began. The females enjoy lower rates throughout.

The rates tabulated in Table 1 apply only to the ten original registration states (the six New England states, and Indiana, Michigan, New Jersey, and New York), which were the only states to supply official mortality data from the beginning of the century, and were computed by a technique used in constructing abridged mortality tables.

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Applications of Ecology and Economics to Fisheries

NEWCOMBE (SCIENCE, 114, 27 [1951]) holds up halibut management as a model to the California pilchard industry and says that pilchard investigators "were quite well aware that a major catastrophe was imminent because the *catch per unit of effort* was on the decline, even though total catches continued to increase. . . . Numerous warnings were issued by the Division of Fish and Game as early as 1930, but, seemingly, research evidence was in itself insufficient proof."

This is naïve, to say the least. Sette (in a profound and authoritative paper included in Newcombe's list of references) was of the opinion in 1943 that "at the present stage of research on the pilchard population, we do not have any notion" what fishing intensity would be desirable, and this evidently is still true. The same may be said of halibut (Burkenroad. *Bull. Bingham Oceanog. Coll.*, 9, [4], 81 [1948]; *Texas J. Sci.*, 2, 438 [1950]; *Bull. Inst. Marine Sci., Univ. Texas*, 2, 1 [1951], which extends the discussion to general principles).

The disaster that overtook the pilchard fishery in the late 1940s was evidently connected with unfavorable natural changes. Newcombe's view that the occurrence could have been "averted" by fishery restrictions thus implies belief that, if the catch had been smaller, the recruitment, growth, natural survival, and/or availability would have been greater. Otherwise he must mean merely that, if the marketable catch had been sacrificed to permit more of the fish that came into range of the fishery to remain in the water, some of these might for a time have been included in subsequent catches to cushion the effects of shrinking renewals on raw-material costs.

However, Newcombe cites no evidence that the pilchard stock has been driven below its level for maximum equilibrium yield. It has to be borne in mind that events accompanying and following the temporary obliteration of a number of great fisheries for clupeids (e.g., for Gulf of Maine menhaden in the nineteenth century, and for herring in one or another Swedish area during the thousand years of record) do not suggest that these local changes in abundance could have been significantly affected by any human act.

It remains to be seen whether even the present immense expansion of the pilchard research program can furnish proof that restrictions would have procured (or are now procuring) a net gain to society through effort saved without countervailing sacrifice of catch. The summary by McHugh and Ahlstrom (*Sci. Monthly*, 72, 377 [1951]) is hardly encouraging.

For example, if distinction still cannot be made between natural mortality and availability of adults (p. 381), how is it possible to distinguish between recruitment and availability in attributing the decline in catch per unit effort in 1935–38 to "rapidly increasing fishing pressure" (p. 378)?

In any case, a better appreciation of the formidable nature of the equations of biological and social equilibria that are to be written and solved deductively is important. Elementary misconceptions, like Newcombe's and the conversely used but comparable ones of Taylor (*Survey of Marine Fisheries of North Carolina,* Chapel Hill: Univ. North Carolina Press [1951]) impede administrative realization that biologically effectual and socially advantageous restriction of some sea fisheries is theoretically indicated to be feasible, but that reliable results at reasonable costs can as yet hardly be expected in practice without use of the *inductive* method. Inductions permitted by the accidents of wars have led to a relatively assured resolution of the problem of effects of North Sea trawling on production costs. The method could be made a regular tool of fishery investigation through design of regulations in forms equivalent to *controlled* experiment, a procedure that would often be practicable if thinking about fishery situations were more careful.

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THE subject of Burkenroad's letter is a symposium paper entitled "The Application of Ecological Research to Aquiculture and the Fisheries." This paper includes, for purpose of illustration, a "review [of] events in the development of this [the sardine] fishery" based upon certain findings and opinions of Pacific coast fishery biologists. Burkenroad takes exception to some of their interpretations and explanations of known fluctuations in fish populations.

In the symposium article, hope is expressed "that, when the full story is written, it will prove to be an analog of the halibut fishery of the North Pacific Coast developed so skillfully under the leadership of W. F. Thompson." The article clearly indicates in what respects the two fisheries may be considered to correspond—namely, they both present a major challenge to administrators, biologists, and fishermen alike; they are both intrinsically ecological problems; and they both present an example of unified, balanced, cooperative effort in scientific fishery management.

What disturbs Burkenroad most is the reference to assumptions based upon the *catch per unit of effort.* Although it is impossible, in a short communication, to discuss all the varied ramifications of this oft-debated subject, opinions of several long-time students of fishery biology may properly be indicated. Scofield (*Calif. Fish & Game,* **24,** (3), 219 [1938]) states "The *catch per unit of fishing effort* is significant and is one of the best ways of measuring abundance in the ocean." Taylor, in a 1951 book entitled *Survey of Marine Fisheries of North Carolina,* described by one reviewer (Thompson, E. F. *Ecology,* **32,** (3), 566 [1951]) as an "analysis [that] should provoke much needed clear thinking in the muddled theory of fishery management," states (p. 135) that "the best indication of an over-fished population is in a decrease in catch-per-unit-of-effort." Also, the 1948 *Bulletin I* of the Pacific Marine Fisheries Commission points out (p. 53) that "The biologists of the three states [California, Oregon, and Washington] agree in the interpretation that heavy fishing intensity and poor spawning survival have reduced the population [of sardines] to a dangerously low level and that management of the fishery should not be postponed."

Burkenroad quotes Sette's 1943 statement that, as of that date, "we do not have any notion" what fishing intensity of sardines would be desirable, and states that it "evidently is" true in 1951. In August 1948, however, the research staffs of California, Oregon, and Washington recommended "an annual bag limit of sardines, initially of not more than 100,000 tons and preferably of 50,000 tons" (*Bull. Pacific Marine Fisheries Com.,* **1,** 53 [1948]). It seems reasonable to assume that such a recommendation would be based on a substantial amount of data, recognizing, of course, that subsequent data might well warrant a modification of this recommendation. Significantly, according to the 1950 *Progress Report of the California Cooperative Sardine Research Program* (p. 8), "The Division of Fish and Game has accumulated almost half a million individual observations on the size of sardines." This mass of records, together with the well-known, exemplary system for collecting and compiling fishery statistics employed at the Division's Terminal Island Laboratory, provides ample basis for assuming an official recommendation to be based on the best available fishery data and seasoned judgment.

Burkenroad also takes apparent exception to the halibut findings of W. F. Thompson and his collaborators, although the record suggests that, even in comparatively recent years, he may have entertained some reservations on this particular viewpoint. Thus, in a 1948 *Bulletin of the Bingham Oceanographic Collection* (**11,** (4), 62, 63), Burkenroad states

> It has been generally considered that the Pacific halibut affords a clear and proven case both of serious reduction of a population of marine fishes by fishing and of the subsequent rehabilitation of the population through restriction of exploitation. [Continuing, he states that the purpose of his paper is] to demonstrate that . . . the magnificent studies published by Thompson and his collaborators are insufficient to exclude the possibility that natural fluctuation has been the major factor in the long-time decline and recovery in abundance of at least one of the stocks.

Despite being somewhat limited in scope, this interesting and searching paper of Burkenroad's constitutes an exceedingly valuable contribution; but, like Thompson's monumental work, it will not find universal acceptance. In fact, Burkenroad (p. 127) generously admits "I am perhaps prejudiced in favor of the view that natural fluctuation may be the cause of most of the changes in our fisheries because this proved to be the case in my first effort in the field of population study."

In the case of marine fisheries for species that have a high reproductive potential, such as the sardine, environment may properly be assigned a relatively important role, if not a dominant one, in designating the possible factors that determine numerical population changes. Supporting this side of the debate, Merriman concludes (*Sci. Monthly,* **68,** 13 [1949]) ". . . in fisheries with large numbers of eggs the fluctuations in abundance are likely to be due more to the environment than to the size of the adult stock."

The symposium review article does not overlook the importance of ecological factors in their relation to natural fluctuations. Quite the contrary, it points out (p. 2) that "Knowledge of how the ecological factors of the marine environment influence the size and composition of the catch is quite as important as an understanding of how fishing intensity affects the population," and again (p. 4) that "In the ocean, interest centers on the effect environment exerts on population numbers." Here, we are clearly in agreement. In fact, from what I am able to glean from Burkenroad's communication, we may be more in agreement than in disagreement. Obviously, one should hesitate to jettison, arbitrarily and without comparable supporting record, the findings and hypotheses of some of the most widely experienced fishery investigators—such as Thompson and Taylor—much less to relegate a serious consideration of their contributions to the realm of naïveté.

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Erratum

In the article entitled "The Use of an Ion Exchange Resin for the Hydrolysis of Casein and Coffee Proteins" (SCIENCE, **115,** 95 [1952]), we have inadvertently omitted glycine 4.53 and 4.38 mg of nitrogen for the HCl and resin hydrolysates, respectively, in Table 1.—G. E. UNDERWOOD and F. E. DEATHERAGE.

Book Reviews

Biochemistry and Physiology of Protozoa, Vol. I. André Lwoff, Ed. New York: Academic Press, 1951. 434 pp. $8.80.

In the introduction the editor states, "For the future development of protozoan biochemistry, it seems of utmost importance that an atmosphere develop in which more biochemists may feel, without external pressure, that many problems of the biochemistry of Protozoa are now ripe for further investigations. . . ." The book aids in creating such an atmosphere.

A section on phytoflagellates by S. H. Hutner and L. Provasoli discusses, with extensive references, the comparative biochemistry and photosynthesis, including suggestions for demonstrating in phytoflagellates types of photosynthesis similar to those found among bacteria, and for testing postulated first products of photosynthesis by supplying them as nutrients to obligate phototrophs; evolution and biochemistry of photoreceptors; biochemistry of "acetate" flagellates; induction of apochlorosis with streptomycin; vitamin requirements, with particular reference to B_{12} nutrition and assays of bound vitamins using phagotrophs; mineral requirements and chelating agents for mineral buffering; and sexuality in *Chlamydomonas.* Many speculative ideas are developed that should stimulate experimental investigation.

In "The Nutrition of Parasitic Flagellates (Trypanosomidae, Trichomonadinae)," M. Lwoff reviews the requirements of trypanosomes for hematin and the evidence that ascorbic acid is an essential growth factor. The effect of changes in chemical configuration on thiamin activity is of particular interest. The anaerobic nature and the sugar fermentation of *Trichomonas* are described. Cailleau's detailed work on the cholesterol requirement of *T. columbae* is reviewed, including the experiments on the influence of chemical configuration on activity. These experiments constitute some of the most convincing evidence that protozoa may contribute notably to biochemistry.

Von Brand's extensive researches on the metabolism of parasites qualify him for the authoritative discussion of the "Metabolism of Trypanosomidae and Bodonidae." The high oxygen consumption of certain trypanosomes and its variation with developmental state are described. The carbohydrates fermented and their fermentation products are given, and fat and protein metabolism is briefly reviewed. The mechanism of host injury and the action of drugs on trypanosomes are discussed in detail.

A short chapter on the "Nutrition of Parasitic Amebae" by M. Lwoff emphasizes the oxidation-reduction potential and the requirement for cholesterol.

In "Biochemistry of *Plasmodium* and the Influence of Antimalarials," R. W. McKee discusses the blood changes accompanying infection, *in vivo* and *in vitro* nutrition, metabolism, natural immunity, and antimalarials. Investigations in this field have proceeded so rapidly since the war that this organization of results is particularly appropriate.

The section on "Biochemistry of Ciliates in Pure Culture" deals chiefly with *Tetrahymena,* the most studied ciliate. G. W. Kidder and Virginia Dewey, themselves active contributors in the field, have pro-