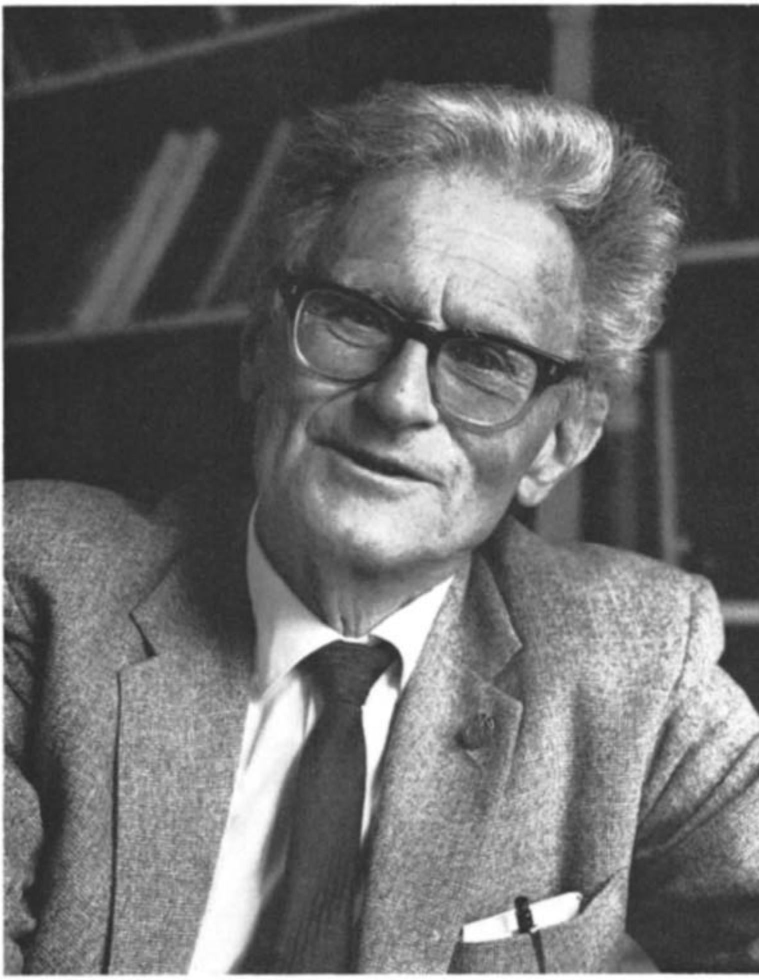

Max Kleiber

PREFATORY CHAPTER: AN OLD PROFESSOR OF ANIMAL HUSBANDRY RUMINATES

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When an old professor indulges in reminiscences some of his listeners may fear that he has reached the stage of rumination, and has therefore started to waste their time by rechewing old stuff. But a cow who indulges in chewing the cud assists her microbial co-workers by making material digestible which otherwise would be useless. I hope the readers of this preface will look at this more positive aspect of rumination and will bear in a friendly mood the frequent "I" and the seriousness with which I take myself and my cud.

Efficiency of Energy Utilization and Body Size

In the third volume of the *Annual Review of Biochemistry* (1934), Samuel Brody (1) discussed a German paper of mine on animal size and feed utilization [*Tiergrösse und Futterverwertung,* 1933 (2)] in which I deduced that the total efficiency (gain/food) of two animals is equal when, with equal partial efficiency (change in gain/change in food intake), their relative food intake (food intake/basal metabolic rate, or food intake per unit of the ¾ power of body weight) is equal. Since empirical data indicated that neither partial efficiency nor relative food capacity (maximum food intake per unit of the ¾ power of body weight) is correlated with body size, I concluded that in general the total efficiency of animal energy utilization is independent of body size.

J. Mayer (3) later confirmed this generalization and called it Kleiber's law. I keep this recognition by a colleague of Mayer's stature gratefully as a spiritual tonic for periods during which I am bothered by the awareness of my various obvious shortcomings. In 1961 my connection with law was officially cinched by the University of California with the degree Doctor of Law.

In the fourth volume of the *Annual Review of Biochemistry,* Brody (4) extended the discussion of body size and efficiency of energy utilization. At his request I sent him a copy of a paper which I had submitted to the *Journal of Nutrition.* It was mainly a critique of the Palmer-Kennedy efficiency quotient (5) (food consumed/gain in weight times weight), which obviously makes the efficiency (the reciprocal of the Palmer-Kennedy quotient) proportional to body weight, an assumption that makes no sense.

The *Journal of Nutrition* did not publish the article; my critique appeared therefore only as a personal communication in Brody's review. Brody wrote me to Switzerland that Palmer was angry, would I please explain to him my criticism. This led to the exchange of several letters which I thought had clarified the matter. To my amazement I noticed later that Palmer et al. (6) in 1946 were still using their ill-conceived efficiency quotient, finding it satisfactory.

The Lilliputian's Calculation of Gulliver's Food Requirement

An average Lilliputian was as long as Gulliver's middle finger, say 7 cm (7). If Gulliver stood 6 feet or 180 cm he was as tall as 26 Lilliputians piled up feet on heads. With the same density and isometric build Gulliver weighed as much as $26^3 = 17600$ Lilliputians. The Emperor of Lilliput decreed that "the said Man Mountain shall have a daily allowance of meat and drink sufficient for the support of 1724 of our subjects."

To find out how the Lilliputians calculated Gulliver's food requirement we can formulate for Gulliver (G) and Lilliputian (L) as folows:

$$\frac{\text{food }(G)}{\text{food }(L)} = \left(\frac{\text{weight }(G)}{\text{weight }(L)}\right)^p = 1724$$

therefore

$$p = \frac{\log 1724}{\log \dfrac{\text{weight }(G)}{\text{weight }(L)}} = \frac{\log 1724}{\log 17600} = 0.76$$

The Lilliputians calculated the food requirement as proportional to the ¾ power of body weight. This anticipates the results of Kleiber of 1932 (8), $p = ¾$, and those of Brody of a few months later in 1932 (9), namely $p = 0.734$, by at least 233 years since the Lilliputians applied this calculation at the time Lilliput was discovered, A.D. 1699.

If the Lilliputians in their remarkable anticipation of the "man mountain's" knowledge concerning body size metabolism and food requirement had stopped at A.D. 1839 when Sarrus & Rameaux (10) proclaimed the surface law of animal metabolic rate or even as late as 1883 when Rubner (11) or 1889 when Richet (12) rediscovered this law empirically and independently, they would have fed Gulliver only the equivalent of 675 Lilliputian rations since the square of the length ratio, 26^2, is 675.

Idea and Action

Two souls, alas! reside within my breast.
Goethe, *Faust,* Part I, Scene 2

Ulrich von Hutten confessed, "I am not a finely planned book, I am a man with his contradiction." Most people have splits in their souls. One of these splits is a discrepancy between their ideas and their actions. When they preach brotherly love but exploit or suppress or kill fellow human beings we call the split hypocrisy. People differ in degree of sensitivity to internal contradictions and in degree of tolerating or even liking such disharmonies. I seem to have a somewhat greater than average allergy against mental schisms. This means an enhanced tendency to express my ideas through my actions, to do as I preach, or one might turn this upside down and call it to think in harmony with what I am doing or preach as I do. Two incidents in my life seem to show this.

1. *An experiment in asocial isolation.*—Like many, perhaps most, young people I went through a phase of antisocial feeling during the first year of college. I assume that out of such a loathing of the social environment with its restrictions, but accepting it for practical reasons, Schiller wrote his "Robbers", and Goethe "Goetz von Berlichingen" and "Werther's Leiden". Most express their rebellion in less creative ways. In Zurich we wore sandals and went around without a hat and with soft "Schiller-collars", which at that time was just as much frowned upon by the solid citizens of the second decade of this century as long hair and beards are loathed today. But that protest in appearance was not enough. I said to myself: since I hate society I should get out and prove my independence as a hermit, so I searched for a place (accessible for my limited means) where the population density was especially low and emigrated with two younger friends to the West of Alberta, Canada. Each of us took up a homestead near the McLeod River about 100 miles west of Edmonton, less than 100 miles northeast of Jasper Park. This experiment was valuable. It made me aware of the degree to which *Homo sapiens* is a social animal. For a couple of months I worked on a farm near Red Deer to earn money for buying tools. Returning to my homestead I found a chance to teach a very intelligent twelve year-old girl who frequently rode on a huge black retired race horse to where I was building my blockhouse about a mile away from her home. Teaching arithmetic and geometry soon interested me more than digging out willowshrubs and cutting down poplar trees.

2. *Conscientious objector.*—After less than a year of homesteading and teaching in the Wild West, this experiment in asocialism was stopped by the first World War. I was ordered to return to Switzerland as a soldier for guarding the frontier. Partly supported by a generous contribution of my pupil's family, I traveled to New York and was sent to Marseille on an old ship called the *Germania,* loaded with French reservists.

Between periods of military service I returned to the College of Agriculture, having passed my experiment in radical individualism and being willing to prepare myself for useful service in human society. I was an ardent Swiss patriot and I hated to find in the army an undemocratic militarism which I thought was imported by the Swiss professional officers from their training in Prussia. I became an officer in order to fight against that un-Swiss undemocratic spirit. But more and more I realized that a democratic army may be an impossibility; military training, after all, must be training for war, for inhuman slaughter, for deception and therefore for unconditionally obeying orders rather than acting according to one's own judgment and following one's own conscience.

When a superior officer, obviously drunk at a party, ordered me to drink so much that I would have lost my judgment I refused and was later reprimanded. This incident was of course silly but it serves as an example of a serious problem. It must be admitted that once military discipline is broken, even at a point of no consequence, it may crack at another perhaps critical occasion. A lot of people recognize the schism between the freedom of the personal conscience and the dictate of military discipline but the degree to which one accepts this contradiction varies and here my allergy against a split between idea and action became especially active.

My internal struggle came to a crisis when, at a particularly dangerous turn of the war, two top-rank Swiss officers were caught spying for the German High Command. In Court they explained that they had acted as patriots because Switzerland was at the mercy of the Kaiser's *Wehrmacht*.

I began to suspect that the Swiss army under the command of officers trained in Prussia was not protecting Swiss freedom and democracy and Swiss neutrality, that to the contrary the army might become a threat. Freedom and democracy in Switzerland could only be secured by abolishing tyranny anywhere—by abolishing war and militarism.

I joined a youth movement whose slogan was "no more war" [that slogan is the title of a recent book by Linus Pauling (1958) (13)]. Regarding the means to achieve our goal our movement was split; some of us followed the advice of the Bolscheviki to go into the army in order to secure access to the weapons for the coming social revolution which the capitalists would try to suppress by force. Others, including myself, maintained we should fight against militarism, refuse participation in the armed services, and work for our cause by nonviolent means. Refusal of military service never was officially recommended by the Social Democrats, because it does not fit into disciplined party action; a conscientious objector always acted strictly as a person. I thus retained a considerable amount of individualism even after my experiment in the Wild West. I refused to follow the next order to active service and was sentenced to four months in prison which I regarded as a mild punishment for an officer. The prison term was even generously delayed to let me finish an exam. But the College of Agriculture kicked me out. Many students protested against this academic punishment of a conscientious objector which they felt was a political degradation of an academic institution.

Dr. Georg Wiegner, Professor of Agricultural Chemistry, sent Rubner's book *Die Gesetze des Energieverbrauchs bei der Ernährung* to me in prison and later remarked with a congratulation that I probably was one of a few students who had read that book through, because Rubner, a genius as an experimenter, was not a good writer. For some inmates, prison is a school for better crime, for me it helped my education in physiology, especially in animal energetics.

After release from prison I got a job in the Agricultural Department of the city of Zurich and married. Later I tried to direct a small production cooperative on a farm near Zurich. Professor Wiegner visited me there and told me I could render better service to mankind if I spent my effort in a way for which I was especially qualified, namely in scientific research. This sounded good to me since I painfully realized that despite training and good intentions I was not a good farmer. So I followed Professor Wiegner's advice gratefully. His prestige re-opened the door of my Alma Mater for me, and I became an assistant in Agricultural Chemistry.

The Swiss poet Kurt Guggenheim has written a fine historical novel, *Alles in Allem,* on Zurich's recent history (14). He makes the daughter of a German businessman in Zurich express sympathy for the conscientious objector, but also voice her disappointment that the man, after refusing military service, begs his Alma Mater to let him go on with his education. This seems to mar her picture of a proud hero. All I can say is that I did not want to create the picture of a proud hero. I just feared to live with a bad conscience. This is not the feeling of pride. A haughty refusal to follow my professor's friendly advice and offer might have satisfied the aesthetic sensibility of a rich young lady but for me it was more important to demonstrate that a conscientious objector does not have to be a holy man apart from others but can become a useful research worker and academic teacher.

Words and Meaning

(Semantic fuss)

In the beginning was the word

Gospel of John 1

In the beginning was the meaning

Goethe, *Faust,* Part 1, Scene 3

As ideas are preserved and communicated by means of words it necessarily follows that we cannot improve the language of any science without at the same time improving the science itself; neither can we, on the other hand, improve a science without improving the language or nomenclature which belongs to it. However certain the facts of any science may be, and however just the ideas we have formed of these facts, we can only communicate false impressions to others while we want words by which these may be properly expressed.

Lavoisier, *Elements of Chemistry,* Preface (15)

1. *Energy.*—The chapter on energy metabolism in the sixth volume of the *Annual Review of Physiology* (16) (1944) gave me an opportunity to fuss about bad language concerning energy. At the end of the 17th century, scientists distinguished between ponderable (with weight) and imponderable (without weight) substances. According to Lavoisier's law of conservation of matter (the basis for stoichiometric calculation in chemistry),

ponderable substance can be neither produced nor destroyed. It can only change the form in which different kinds (elements) are combined. According to Joseph Black's caloric theory, heat is an imponderable substance which also can be neither produced nor destroyed but can only change from sensible to latent heat. The caloric theory was disproved by Benjamin Thompson (who in 1792 became Count Rumford) who showed that indeed heat can be produced, for example by boring a cannon (17). Nuclear physics later revealed that matter is not conserved but can change to radiant energy. The Einstein equation $E = Mc^2$ should not be interpreted as indicating that energy can be produced. It indicates that mass, now to be regarded as a special form of energy, can be transformed to another form, namely radiant energy. Heat, work, chemical energy, or mass can be produced or destroyed by changing to other manifestations of energy; and energy itself remains the only generalizing concept for what, by definition, can be neither destroyed nor produced. Therefore expressions like "the energy production of cancer cells" by O. Warburg (18) or 'the energy-producing' mechanism" by H. Krebs (19) are self-contradictory.

2. *Turnover rate.*—In kinetic studies, especially investigations of intermediary metabolism in animals, which have been, and are, greatly helped through the use of isotopes as tracers, we define the *turnover time* as the duration of time during which a metabolic pool is renewed or turned over. It means the time interval during which as many molecules of which the pool consists have entered or left the pool, whose size remains constant, a condition which we presume when we speak of turnover in contrast to growth or decay. The *transfer rate,* or *flux rate,* indicates the number of molecules which enter or leave the pool per unit of time. This rate has the dimension: number of molecules per unit of time, or grams per unit of time.

Some authors use as an additional synonym for flux rate *turnover rate,* which other authors reserve for the reciprocal of turnover time. In a short note I argued that the use of turnover rate as a synonym for transfer rate was illogical because in tracer work we are not concerned with the rate at which molecules turn over (or rotate), which would be a problem of molecular physics. In tracer work, we are concerned with the rate at which a pool turns over. The letter in which an editor of *Science* declined to publish my note alleged that I was not sufficiently aware of Hévèsy's work, but the note appeared in *Nature* (20). The year before (1954) I had made a trip to Stockholm to visit Hévèsy and his laboratory. That proved my admiration and respect for the major pioneer of tracer work.

Zilversmit (21) countered my critique of the use of turnover rate as a synonym of transfer rate by the suggestion that a good analogy for his use of turnover rate of pools was the turnover rate of patients in a hospital. But this is just an excellent analogy to make my point. The mean turnover rate of the patients (number of patients turning over per unit of time) would indicate how many times on the average each patient turns over (in his bed or otherwise) per unit of time.

The analogy of what we mean by the turnover rate of a metabolic pool is not the rate at which patients turn over but the rate at which the patient population (the analogy of the pool) turns over. Mawson (22) agreed with the use of turnover rate as the reciprocal of turnover time and suggested flux rate to express the number of molecules which enter or leave a pool. In a report of May 1, 1966 to the International Commission on Radiological Units the U. S. task group on tracer kinetics accepted the term turnover rate as the reciprocal of turnover time.

3. *Degrees of freedom.*—A mean of repeated measurements with a given variance has the smaller standard error the greater the number of measurements from which the mean is calculated. The smaller the error the more accurately the mean is determined, but in using statistics in biological research we say that with increasing number of measurements the degrees of freedom increases. The mathematician may have degrees of freedom when he constructs artificial series of figures but the experimental scientist has no such choice. He is bound by the results of his measurements. To call *degrees of freedom* what for us are more nearly *degrees of determination* rubs me the wrong way, perhaps all the more because freedom is so much and so disgustingly misused anyway.

4. *A billion.*—I am also bothered, in fact more so, when I have to call 10^9 a billion. The syllable "bi" means double. Some of us are bilingual when we speak two languages; we bisect, make bilateral arrangements, and ride bicycles. In Europe a billion means 10^{12} with twice the exponent of 10 for a million, $10^{6\times2} = 10^{12}$ is a billion and $10^{6\times3} = 10^{18}$ is a trillion; 10^9 is a milliard, 10^{18} a billiard which, as above, is also a trillion. That all makes sense but why is 10^9 a billion in America?

5. *Mol or mole?*—Wilhelm Ostwald called the quantity of a substance equal to its molecular weight expressed in grams a *mol.* He properly created a new word for a new concept. The spelling most common in this country, *mole,* spoils this good terminology. A mol stands specifically for one clearly established concept but mole can mean a lot of things. We might read that a mole digging in a mole of a castle consumed ⅙ of a mole of oxygen per day, or that the injection of a thousandths of a mole of a carcinogen produced a mole in the uterus.

I am grateful to the editors of John Wiley and Sons that they allowed me to use mol in my book, and I testified gladly that this was not an oversight on their part.

6. *Large and small calories?*—I was not so lucky with the editors of the *American Journal of Physiology* who changed my *kcal* to *Cal* which embarrasses me when I explain to my students that to call a kilocalorie a large calorie is just as silly as to call a mile a large foot, and they find Cal in one of my own and rather recent publications. I understand that a cer-

tain amount of uniformity in symbols used in journals is desirable but I feel there should be the possibility of progress in terminology and authors who give good reasons for a deviation from an accepted type of expression should not be suppressed by a rigid editorial adherence to the status quo. The author should be entrusted with a certain amount of semantic responsibility if he asks for it.

7. *Specific dynamic action.*—Specific dynamic action is an erroneous translation of Rubner's *spezifische dynamische Wirkung*. The proper translation would be specific dynamic effect but dynamic has to do with work or movement whereas the effect is an increase in heat production, that is a calorigenic effect.

Linear Regression Equations

Writing usually means a great effort to me. When I have a sheet of paper in front of me and a pencil in my hand or when I sit in front of a typewriter, my mind seems to begin to labor like a motor with a lot of internal friction. Maybe this explains why I have a tendency to become belligerent when I read arrogant statements which seem to have come to paper without hesitations. Maybe I just envy the authors who seem so free of the type of writer's constipation which delays my own work.

When a belligerent mood is aroused, writing seems to go more freely. Sometimes a note thus produced is sarcastic and not friendly enough to be printed but at one such occasion a paper resulted which I still like and consider useful, since with the IBM machines taking over a lot of our routine calculations we will soon be even more swamped with empirical linear regression equations than we are already. The paper appeared in the *Journal of Applied Physiology* (23).

The California Respiration Apparatus for Cows

(Amateur engineering)

In 1929 I was imported to California from Switzerland to build a respiration apparatus for big animals, especially dairy cows.

Before I left Europe I visited Professor Møllgaard in Copenhagen whose respiration apparatus for dairy cows I had studied a few years before. He said that I was welcome as a person and a guest in his house as I had been earlier but he would not show me his laboratory since now I was representing the University of California.

Later I learned that Professor Møllgaard himself had earlier been approached but had demanded too high a salary for himself and an assistant whom he planned to bring along. Dr. Hart, then the head of the Department of Animal Husbandry, was not only a good scientist but also a shrewd businessman. He wondered if there were not perhaps cheaper European scientists on the market. He got together with C. B. Hutchison, about to become Dean of the College of Agriculture, but at that time still talent-hunting in Europe for an American Foundation. He knew my teacher Professor Wiegner at Zurich and had seen the respiration apparatuses for rabbits and one for sheep which I had built mainly to learn how to design one for cows. I was offered a temporary position as Associate in Animal Husbandry with a salary of $4500, I believe, per year for a period of three years. For me that was a wonderful opportunity and in May 1929 I arrived in Davis with my wife and our nine-year-old daughter.

To engage a beginner instead of Møllgaard, then at the height of his career, was of course a risk, but Dr. Hart took that risk and reassured me later at a time when one or the other of my attempts did not work out as I expected. "If one way does not work as you like go right ahead and try another", he would say. Dr. Hart could be a powerful and helpful friend.

To get our work started Dr. Hart introduced me to Professor Walker, the head of the Department of Agricultural Engineering where to my delight I saw excellent machine shops and met competent mechanics. We planned to build the respiration apparatus as a cooperative project. Unfortunately, this plan was nipped in the bud by a young man who was then business manager at Davis. This was my first great disappointment and I was shocked to witness how two internationally recognized professors were bossed rather crudely by a bureaucratic upstart who had no academic standing. The apparatus was to be built not by the obviously ideally suited Department of Engineering but by the administrative division, Construction and Repair, directed by an old carpenter. For metal installations, blueprints and specifications had to be worked out so that the Sacramento businessmen had a chance to bid for making this equipment. So, following my inclination to accept challenges, I learned to make drawings for blueprints and with Dr. Hart's help, worked out specifications. Since my skill as a draftsman remained rather limited I had to go over to machine shops and sheet-metal works in Sacramento and explain my blueprints. The Construction and Repair Department was mainly equipped for carpenter work. The double chamber of our apparatus is a 2 × 4 structure with wooden walls, later lined with galvanized iron. The chambers are ventilated by moving four copper pipettes, each holding 225 liters of air, up and down in a water trough. The pipettes were built at Sacramento but the driving mechanism was assembled in Davis, also the air sampling and air collecting devices, CO_2 absorbers, and methane combustors. By a special concession Dr. Hart was able to provide me with a vise, a drill press, and even a small lathe, and the glassblower of Dr. Lewis' department in Berkeley gave me an oxygen blow torch. I designed the ventilation, air sampling apparatus, and absorbing system so that they would work even if, with rather limited skill as a mechanic, I should construct the apparatus myself. I had a chance later, however, to get the help of a farmer who in his youth had worked as a gunsmith. After two and a half years the apparatus worked (24). We could burn up three liters of alcohol a day and get the proper result within less than 2 per cent error but the machine made an awful racket. When a highly trained engineer became my assistant, we changed water valves in the airline to ground in stopcocks and also changed from a switch which reversed the motor at the end of each aspirator motion to a rack-and-pinion drive.

I succeeded in coming to California because I was cheaper than Professor Møllgaard, and the California apparatus is, I believe, cheaper than most other similar apparatuses for big animals. Ventilation with a blower and testmeter may be cheaper, though, than my air-pipetting machine, and there are good reasons for using blower and testmeter for ventilation; I cannot so well understand a return to the old extremely expensive Blackslees mercury pump which makes an extra air-moistening tower necessary and has to be calibrated by a gas meter (25) (Møllgaard and Anderson, 1917). I also fail to see the justification for using closed chambers for respiration trials with large ruminants which not only use up daily the equivalent of 50 to 100 kg KOH but also make temperature, pressure, and airtightness especially critical. A closed chamber also causes methane to accumulate so that the experiments have to be interrupted every day. I suspect that some designers of apparatus assume that the most expensive must be the best.

TELEOLOGY

1. *In the past.*—Teleology is the doctrine that the world has been created according to a plan and that nature works toward a purpose (*telos*) or purposes. This theory plays a major role in the philosophy of Aristotle (384-322 B.C.). Through his great influence it remained important in the philosophy of the Christian era and its ghost still haunts the thinking of modern scientists and plays spooky tricks in many of the writings even of those who deny its justification. The doctrine was taught long before the time of Aristotle and had already been refuted by Empedokles (490-430 B.C.), a great thinker later worshiped by Galenos (130-200 A.D.) as the founder of the Italian medical school. Recently he was hailed by the followers of Mazzini as the "democrat *par excellence* of antiquity" who honored his democratic principles by refusing an offer to become king of Agrigentum. According to Empedokles the processes of nature occur by chance but among the products formed, especially plants and animals, only those survive which fit the conditions; misfits, such as horses with human heads, are weeded out. Thus survival of the fittest among offspring occurring by chance, Empedokles' alternate to classical teleology, anticipated by nearly two and a half millennia Darwin's *natural selection* as the alternate to the teleology of our time.

Galenos was more vague in his statements than Empedokles had been; according to Verworn (26) he suffered under the dualism of the natural acceptance of causality and a teleology inherited from Aristotelian philosophy.

Descartes (1596-1650) declared that we cannot know God's purposes but Boyle (1627-1691), of gas law fame, maintained that some of the divine ends are "readable", such as the marvelous adaptation of living creatures, and that "hence it is foolish to reject the teleological proofs for the existence of God".

To Galileo (1564-1642) the teleological terminology of the scholastics was, as Burt (27, p. 134) put it, no longer serviceable, but Newton (1642-1727) maintained a mystic credo and declared that the motions which the planets now have could not spring from any natural causes alone but were impressed by an intelligent agent (27, p. 289). As a faithful former student helps an old professor by keeping him active, so Newton tried to "keep God on duty searching the Universe for leaks to mend." This idea was repugnant to Huyghens and to Leibniz who postulated that God had created a world which did not need supervision and repairs (27, p. 299).

In 1742 a French medical doctor, Julien de la Mettrie, wrote a book on the natural history of the soul followed in 1748 by another, *Homme Machine,* man a machine. Both books got him into trouble with the clergy who accused him of atheism, and he had to escape to Holland. When he was no longer safe even in Holland, Frederick the Great of Prussia, the monarch who proclaimed that in his kingdom every man was free to save his soul in his own fashion, offered asylum to the persecuted atheist (28).

In an attempt to clear science of theology, the postulate that man is a machine is a rather tricky analogy because an essential characteristic of a machine is that it is planned for a purpose which implies a designer and that the best, or possibly the only, way to understand a machine is to understand the purpose which the designer had in mind. The study of man as a machine thus leads to teleology and that leads naturally to the question of the mind of the designer of man. This mind must work in a way similar to that of the human mind, if we are to understand its planning; we understand the planning of a machine because the designing engineer thinks as we think. So we are back at theology. Some atheistic teleologists of the period of enlightenment solved this problem by a switch from a moralizing stern biblical lord to a bright goddess, Nature; and as the priests claimed they learned God's purposes through supernatural revelation, so the atheistic scientists claimed that Nature revealed her purposes to those who were asking her properly through scientific research.

That type of naturalistic teleology, or nature theology, was clearly the frame of mind of Sarrus & Rameaux when they wrote (29) in 1839: "When nature can achieve an aim by various means she never uses one of the means exclusively to the limit, she makes these means compete so that each one of them produces an equal part of the total effect."

The attitude of a natural teleologist is not essentially different from Priestley's frankly theistic approach, not only to questions of ethics and religion in a narrower sense but to everything he did including his scientific research. He writes: "The most pleasing views of the unbounded power, wisdom and goodness of God are constantly present to his (the naturalist's) mind"—(30, p. 193).

In the preface to volume II of *Experiments and Observations on*

Different Kinds of Air (1774-1777) Priestley wrote as follows: There is nothing capital in this volume from which I can hope to derive any other kind of honor than that of being the instrument in the hands of Divine Providence which makes use of human industry to strike out and diffuse that knowledge of the system of nature, which seems, for some great purpose that we cannot as yet fully comprehend, to have been reserved for this age of the world . . . —(30, p. 246).

In Priestley's mind there were no different compartments, as for example in Faraday's where science and religion coexisted in complete separation (31, p. 110), for rational scientific thinking and for religion. Priestley was a consistent Unitarian also in this sense. The science of this great chemist and physiologist was integrated with his theology and his theology was permeated by the courage and freedom in the search for truth which characterizes a great scientist. One may question, though, whether this unity was not achieved at the cost of clarity wherefore it took Lavoisier, presumably an agnostic, to free Priestley's great scientific discovery from the confusion of the phlogiston theory, to show that Priestley had discovered an elementary gas, to name this gas oxygen, and to formulate the theory of combustion and metabolism as oxidations, a theory whose overall aspect is still valid today.

2. *Teleology today.*—Boyle's main argument for teleology was the marvelous adaptation of living creatures but Reichenbach wrote (32, p. 200), "Darwin's theory of natural selection is the tool by which the apparent teleology of evolution is reduced to causality. The need for teleology is eliminated by Darwin's principle."

Selection of the fittest among offspring whose characteristics result from gene inheritance and chance mutations makes the postulation of a plan superfluous. Bernatovics (33) describes the present situation as follows: "For most teachers of science, teleology and anthropomorphism are not issues to be debated but to be deplored."

Bernatovics presents a selection of samples of teleological language taken from modern textbooks of biology, chemistry, physics, astronomy or geology. Here is another example from Lehninger's excellent and lucid *Bioenergetics* (34, p. 159): "Just as the whole cell *had to evolve* active transport mechanisms located in its outer membrane *in order to* preserve the constancy of composition of the intracellular solutes . . . " [my emphasis]. A nonteleological formulation might read as follows: "Cells in whose membranes active transport systems operate may maintain intracellular solutes at a constant composition. If the latter is biologically advantageous, active transport becomes a criterion of natural selection and thus a factor in evolution."

(*a*) Teleological slip of my own.

"Let each man watch just where he stands and if he stands, beware of falling." This advice to Goethe comes to my mind when I find teleological slips in my own publications such as the following passage (35, p. 369):

"This increase in katabolic rate observed in magnesium deficiency may thus be related to an increase in the breakdown of tissue because of an inability to secure sufficient magnesium out of intact tissues after the stores have been depleted."

"Inability to secure" implies a frustrated will to obtain magnesium; it has a teleological flavor which was noticed by Blaxter & Rook (36). I should have cautioned the reader by "as if" to make him realize that my statement was meant metaphorically, for example as follows: "A lowering of the Mg level in the blood may lead to a lower Mg content in the tissues and this in turn may lead to an increased breakdown *as if* these tissues were sacrificed as a source of Mg for the rest of the body."

(*b*) An erroneous accusation.

A review (37) of a paper on metabolic rates of rats as a function of age by Kleiber, Smith & Chernikoff (38) is summarized by the statement, "The work is a valuable addition to the sum of information on the metabolic rate of rats but the teleological discussion, thinly disguised as physiology, is unhelpful."

This passage caused a good deal of hilarity among my students and co-workers familiar with my occasional exhibitions of a missionary zeal in preaching against teleological explanations of scientific observations (in fields other than sociology or psychology where of course human purposes are a legitimate object of research).

I wrote to the editor of *Nutrition Reviews:* "I do not claim that the criticized discussion is helpful, that depends on the reader, but I claim 1) That it is not disguised and 2) It is not teleological, being based on natural selection, the alternate to teleology." The article contains the statement: "*It is* as if an agent stimulating metabolic rate increased in old rats at a constant relative rate" and the sentence: "Our rats would then be *naturally selected* for high metabolic rate." True, the sentence "It [the agent involved] might be related to the condition that favors development of spontaneous tumors in ageing rats" contains "favors" which has a teleological flavor, but how could a condition "favor" in any other way but metaphorically?

3. *Don't eliminate metaphors!*—I think it would be a pity to eliminate from scientific language those poetical metaphors which are recognizable as such. I would feel sorry to miss such meaningful and stimulating statements as Fenn's (1924) that the muscle adjusts the extent of catabolic processes to the load which it "discovers" it must lift after the stimulus for contraction is over (39; 40, p. 16).

It would be deplorable if a statement like the following by J. B. S. Haldane (41) were banned from biological literature as teleological heresy. "If the insects had hit on a plan for driving air through their tissues instead of letting it soak in they might well have become as large as lobsters."

4. *Taking teleology seriously.*—The case is different when teleology is taken seriously as, for example, by H. Krebs (42) in his lecture at Johns Hopkins Hospital, 1954, under the title "An expansion into the borderland

of biochemistry and philosophy", when he states "that the wholesale dismissal of teleological consideration in biology is unjustified, that it is the primary purpose of the oxidative degradation of foodstuffs to generate phosphate bond energy."

This would make scientific research an effort to understand nature in terms of a creator's purposes, that would be theology, and for some scientists this may be a favorable condition for research as it was for Priestley; but others, and they are probably the majority of scientists, regard teleology with Reichenbach (32) as analogism and pseudoexplanation and see an open problem of causality where teleologists are satisfied with an answer based on analogy. The *Annual Review of Physiology* 18 (43) gave me an opportunity to voice disagreement with teleology.

5. *Function and purpose*.—Two years after Krebs' excursion into the borderland of biochemistry and philosophy, another Nobel laureate, A. V. Hill, joined Krebs' recommendation for a return to teleology. In an article "Why Biophysics?" (44) he states that "the idea of function of oragnization of design is an essential part of biology as it is of engineering . . . it is sensible for a physiologist to ask what the functional significance of an organ is . . . its relation to other parts of the machinery, its purpose in connection with behavior, survival, or inheritance . . . "

There is no argument that it is sensible for a physiologist to study the function of organs if we mean by function the relations of one organ to other organs as a part of an organism, that is, when we use function the way it is used in mathematics, expressing relation. What most of the physiologists object to is the use of function in the teleological sense as a synonym for purpose. Purpose has a proper meaning only in connection with a creator who designs organisms. A student of engineering properly speaks of the purpose of a part of an engine. He understands why the inventor of the machine had designed a part in a particular shape and position because the student of engineering has learned to think as the designer thinks. But can a biologist learn to understand what the inventor of a fish or a man had in mind when he designed these creatures?

A professor of engineering, in explaining a machine, properly starts with the purpose of the machine, but should a professor of biology likewise start with the purpose of an organism—and does a biologist whose conscience (or politics as Hill adds) forbids him to ask for purposes, who as a biologist avoids teleology and limit himself to causality, really "miss most of what is interesting"? Did Darwin make his successors miss most of what is interesting when he showed that evolution can be explained by natural selection, as an alternate to teleology?

To the contrary, instead of accepting an analogy between a creator of organisms and a designer of machines and hunting for divine blueprints, the darwinistically oriented physiologist is stimulated to search for causes and even if he does not completely succeed he usually finds a lot of what is interesting on his way.

The Davis Tracer Team

1. *Peaceful use of atomic energy.*—The third epoch in the evolution of man started on December 2, 1942 when at the end of an abandoned football field of the University of Chicago "Man achieved the first self-sustaining chain reaction and thereby initiated the controlled release of nuclear energy"—(45, p. IX).

I wrote a jubilant letter to Switzerland, Science works! But my old friend, a well known educator, Fritz Wartenweiler, remarked I was crazy, man was not ready morally for that much power. I must admit he may be right. When warhawks direct the governments of nations, all increase in power increases evil, with or without nuclear bombs, though, Hiroshima . . . Vietnam. But I still hope and believe that the good in human nature will prevail over the evil, that free intelligence will defeat conformist stupidity, that reason will be victorious over nonsense, that generals will be subordinated to statesmen.

I was eager to participate in the peaceful uses of nuclear energy, in particular to use isotopes in metabolic research, especially research on dairy cows, the most important farm animal in California which can become an almost ideal tool for a physiologist who, supplied with isotopes as tracers, is eager to measure biochemical processes in a normally functioning animal.

The use of isotopes as tracers started before Fermi and his co-workers ushered in the nuclear age; it started A.D. 1911 when Hévèsy, working in Rutherford's laboratory, could not separate radium D from lead and, as a genius, turned failure into a success by using radium D as an isotopic tracer for lead. In Davis the use of isotopic tracers started in the Botany Division in 1942 when Robert N. Colvell used ^{32}P in translocation studies. For me it started with a Chemical Society lecture by Dr. E. A. Evans, Jr., at Davis and a following talk with him in our home in October 1944. Four years earlier Evans and Slotin had demonstrated carbonate fixation in liver slices incubated with ^{11}C.

In the fall of 1945 discussions among physicists, chemists, and biologists at Davis resulted in a project entitled "Metabolic research with isotopes". This project was activated by a proposal for cooperative research directed jointly by Drs. H. Young and M. Kleiber and accepted by Dean C. B. Hutchison in May 1946.

I wanted to supplement my earlier investigations on energy utilization involving respiration trials with dairy cows by research on the intermediary metabolism using a carbon isotope as a tracer. The isotope of carbon, ^{14}C, was not yet sufficiently available, though it was first discovered in 1940, arising from bombardment of carbon with deuterium in the cyclotron (46, p. 168). The ^{11}C was too short lived for extended trials with dairy cows; therefore, at that time the choice was the stable ^{13}C.

In 1946 I arrived in Chicago as visiting professor of applied biochemistry in order to learn tracer methods, especially work with ^{13}C, which usually involves mass spectrometry. During the period when I studied the mass spectrometer in Rittenberg's Laboratory at Columbia University in New York, the friendship and hospitality of Konrad and Lore Bloch made me feel at home in Cedarhurst.

By 1947, ^{14}C had become sufficiently available and the measurement of its soft radiation sensitive enough that we postponed the acquisition of a mass spectrometer for the Davis campus for the future when we would extend our tracer work to ^{15}N.

2. *A lucky accident.*—The work under our project began by an accident. ^{32}P was used in the Donner Laboratory at Berkeley to treat leukemia patients. A bottle with ^{32}P broke and the radioactive solution had to be recovered from the floor. The physicians declined to inject this sample into veins of human beings, so it became radioactive waste to be enclosed in concrete for dumping into the ocean. Fortunately, Dr. Garden, in charge of waste disposal, is according to unofficial information of Scotch ancestry, which involves characteristics similar to the Swiss. Anyway the idea of wasting so much of so valuable a material as that sample of ^{32}P hurt his soul. He called up Dr. Reiber at Davis, asking if the contaminated ^{32}P solution might possibly be of good use in agriculture. Again, fortunately, Dr. Reiber was one of the chemists who had helped to initiate our tracer discussion group. He called me up and asked, "Could we use ^{32}P?" I said, "Yes!" True, we had not planned to work with ^{32}P but with ^{13}C or ^{14}C. However, luck is a proud lady and should not be told to come at some other time or with something else on and I am glad that I said yes. Dr. Garden phoned "Come and get it."

My assistant A. H. Smith interrupted his Ph.D. thesis on microrespiration trials with mammalian egg cells, and we drove to Berkeley discussing en route what we might investigate with that ^{32}P and a cow. Our good friend Dr. H. B. Jones in the Donner Laboratory taught us how to handle radioisotopes. Fortunately Dr. Hewitt of Plant Pathology had a Geiger counter and Drs. Gardner and Patten of Physics knew how to operate it, and an undergraduate student with an unusually inquisitive and quickly grasping mind learned the tricks of that Geiger counter faster than the rest of us. That student, Arthur Black, was later endearingly called "Bones" because he prepared radioactive cow bones for radioassay in a self-constructed crusher with extraordinary devotion and corresponding noise.

On December 23, 1946, Dr. George Hart, Head of the Division of Animal Husbandry, thrust a bleeding needle into the jugular vein of the chosen cow and I, somewhat trembling I must confess, connected the needle to a bottle which contained half a liter of the awe-inspiring solution, and the infusion of the radioactive phosphate into the cow began. Dr. Loosli from Cornell University and Dr. Reiber from our own, as well as Drs. Patten and Gardner from our Physics Division, shared with us the joy and excitement of this first trial which included intensive laboratory work even over Christmas.

3. *Chicago meeting on isotopes in agriculture.*—Tracer work aroused the interest of progressive officers of the American Society for Animal Production early. The Investigating Committee (Wise Burrow, Chairman, George Davis, G. E. Dickerson, Max Kleiber, Paul B. Pearson) organized a panel discussion in the Sherman Hotel at Chicago, November 27, 1948, with the following program:

Isotopes in Animal Experimentation

Max Kleiber, University of California: "Phosphorus exchange in cows, an example of using radioactive isotopes for investigating turnover rates and or specific metabolic rates".

Konrad Bloch, University of Chicago: "Some applications of isotopic tracers to the study of intermediary metabolism in animals".

C. L. Comar, University of Tennessee: "Radioisotopes in animal nutrition research".

W. F. Libby, University of Chicago: "Availability of isotopes in the production of labeled compounds: Isotope farming".

James H. Jensen, Div. Biol. Med. United States Atomic Energy Commission: "Further considerations in using isotopes in animal experimentation".

4. *Atomic Energy Commission.*—Listening to Dr. Jensen's talk and getting acquainted with him may have saved the life of our newly born Davis Tracer Team when our project seemed doomed by withdrawal of financial support in the Division of Animal Husbandry. A desperate call for help was answered favorably by the U. S. Atomic Energy Commission (AEC). My assistant, who would have lost his job, was awarded a postdoctoral fellowship in 1948, and in 1949 an AEC contract "Studies on metabolism and biosynthesis in farm animals" was started. The reluctance of the administrators of the College of Agriculture to accept our AEC contract was fortunately overcome by the influence of Dr. Hardin B. Jones whose personal standing among his colleagues at Berkeley, together with the prestige of the Donner Laboratory and its chief Dr. John Lawrence, apparently outweighed the hostile attitude at Davis.

Of great importance for the development of our project was a fine cooperation with members of Professor Calvin's Bio-organic Group of the Radiation Laboratory where the labeled metabolites used in our experiments were synthesized under the direction of Dr. Bert M. Tolbert. Later, bio- as well as chemosyntheses were achieved in a laboratory organized for that purpose in the School of Veterinary Medicine at Davis by Dr. Georg Brubacher from Basel, Switzerland. Dr. Jerry Kaneko, an early member of our team and a technician and graduate student, acquired a good deal of skill in chemosynthesis and so did Dr. Black.

The youthful Tracer Team owed a great deal to the friendly and effective help of Dr. Donald Jasper in all functions requiring the skill and know-how of a veterinarian, from liver and bone biopsies to diagnoses of disease and treatment of sick cows.

In 1951 the Davis Tracer team suffered a major loss when Dr. N. P. Ralston left Davis to start his career as head of the Department of Dairy

Science at Michigan State University. He combined the grassroot interest of an Animal Husbandman intimately acquainted with the practical problems of the farmer with the vision, skill, and curiosity of a progressive physiologist. Fortunately some of the functions of Dr. Ralston on the team were soon taken over by Jack Luick, a rather rare combination of a skillful chemist and competent dairyman. His ability in dealing with people was manifest in his research, undertaken together with a graduate student, G. McLeod, on ketosis which required a close cooperation between dairy farmers, farm advisors, practicing veterinarians, the School of Veterinary Medicine, and the Tracer Team.

5. *The emblem of the team.*—After struggling through infancy with its hazards, the Davis Tracer Team showed such a lusty growth that I have to refrain here from even listing the names of its members, let alone discussing their achievements. It warms the old professor's heart to contemplate how far the knowledge of his academic offspring exceeds his own without making them strangers. Information on the activity of our family can be found in the literature and it may be advantageous to mention in a prefatory chapter mainly what is not elsewhere found in print. Among these items is the origin of the tracer team emblem. Mr. Chernikoff who had started his career as a pharmacist in the old army of the Tsar, had escaped through China and then earned his living as a technician in the Physiology Department at Berkeley. When Professor Schock switched his field of action from pediatrics in California to Geriatrics in Pennsylvania, Mr. Chernikoff came to Davis as my technician. When we started to work with radiophosphorus, Cherni appeared with a piece of sheet lead hung where sculptors usually put a fig leaf. From that time on a departing member of our team is decorated with a fig leaf of lead.

End of This Rumination

I am happy that I have chosen the right profession. A scientist is not only free to seek the truth, it is his job. An academic teacher has not only the right to tell the truth, it is his professional obligation. This fits especially human beings who are allergic to schisms in their soul. Scientific research and academic teaching saves them from the difficulty which was encountered by a prominent author who was said (47) to have written the strict truth as a historian but had to tell lies as a White House aide. I am happy that I found my place as a scientist and an academic teacher, a profession which fits my charactersitics.

I enjoyed teaching basic ideas in my special field of animal energetics and writing a book on that subject, *The Fire of Life* (48). I enjoyed lecturing also on subjects of general interest to students such as: "What you should take with you from your University", and on subjects of general human interest: "What science means to me" and "Scientists are human". The latter may have stimulated a group of my friends to donate a beautiful picture of Albert Einstein by Karsch for the entrance hall of the physics building. I enjoyed a public debate with Dr. Teller on the question: "Should the U.S. resume testing?" I had my soul in sermons to Unitarians like: "The conscience of an agnostic" and "Freedom and truth". I am grateful for the opportunity to continue some of these activities, happy to discuss philosophical questions in monthly meetings at my home.

I feel satisfaction with the memories of some good fights during the time when the McCarthy spirit invaded our Campus at Davis during the period within which the University suffered through "the year of the oath" (49).

Just as a cow cannot chew her cud continuously, so can an old professor not put into a prefatory chapter all he has to ruminate. Some items left out may make a good cud to chew on at another occasion of contemplation.

LITERATURE CITED

1. Brody, S., *Ann. Rev. Biochem.*, **3,** 295 (1934)
2. Kleiber, M., *Z. Tierernährung*, **5,** 1 (1933)
3. Mayer, J., *Yale J. Biol. Med.*, **21,** 415–19 (1948/49)
4. Brody, S., *Ann. Rev. Biochem.*, **4,** 383 (1935)
5. Palmer, L. S., and Kennedy, C., *J. Biol. Chem.*, **90,** 545 (1931)
6. Palmer, L. S., Kennedy, C., Calverley, C. E., Lohn, C., and Weswig, P. H. *Univ. of Minnesota Agr. Expt. Sta. Bull.*, 176 (1946)
7. Swift, J., *Gulliver's Travels* (1726) (Reprinted in Great Books, **36,** p. 19, *Encyclopedia Britannica,* 1952)
8. Kleiber, M., *Hilgardia,* **6,** 315–53 (1932)
9. Brody, S., *Missouri Res. Bull.*, 166 (1932)
10. Sarrus et Rameaux, *Bull. Acad. Roy. Med.*, **3,** 1094 (1839)
11. Rubner, M., *Z. Biol.,* **19,** 535–62 (1883)
12. Richet, Ch., *La Chaleur Animale* (Alcan, Paris, 1889)
13. Pauling, L., *No More War* (Dodd Mead & Co., New York, 1958)
14. Guggenheim, K., *Alles in Allem* (Artemis Verlag, Zürich, 1952)
15. Lavoisier, A., *Elements of Chemistry* (Great Books, **45,** *Encyclopedia Britannica,* 1952)
16. Kleiber, M., *Ann. Rev. Physiol.,* **6,** 123–43 (1944)
17. Brown, S. C., *Count Rumford* (Doubleday Anchor, New York, 1962)
18. Warburg, O., *Science,* **123,** 321 (1956)
19. Krebs, H., *Bull. John Hopkins Hosp.,* **95,** 19 (1954)
20. Kleiber, M. *Nature,* **175,** 324 (1955)
21. Zilversmit, D. B., *Nature,* **175,** 863 (1955)
22. Mawson, C. A., *Nature,* **176,** 317 (1955)
23. Kleiber, M., *J. Appl. Physiol.,* **2,** 417–23 (1950)
24. Kleiber, M., *Hilgardia,* **9,** 1–70 (1935)
25. Møllgaard, H., and Anderson, A. C., *Forsøgslab. Vet. K. og Lands bohojskoles,* **94,** 1–180 (1917)
26. Verworn, M., *Allgemeine Physiologie,* 10 (Fischer, Jena, 1922)
27. Burtt, E. A., *The Metaphysical Foundations of Modern Science* (Doubleday Anchor, Garden City, N.Y., 1932)
28. Lange, F. A., *Geschichte des Materialismus,* **1,** 323 (Brandstetter, Leipzig, 1914)
29. Sarrus et Rameaux, *Bull. Acad. Roy. Med.,* **3,** 1094–1100 (1839)
30. Brown, I., *Joseph Priestley* (Pennsylvania State Univ. Press, 1962)
31. Ostwald, W., *Grosse Männer* (Akad. Verlagsges., Leipzig, 1927)
32. Reichenbach, H., *The Rise of Scientific Philosophy* (Univ. of California Press, 1951)
33. Bernatovics, A. J., *ETC: A Review of General Semantics,* **17,** 63–75 (1959)
34. Lehninger, A. L., *Bioenergetics* (Benjamin, New York, 1965)
35. Kleiber, M., Boelter, D. D., and Greenberg, D. M., *J. Nutr.,* **21,** 363–72 (1941)
36. Blaxter, K. L., and Rook, J. A. F., *J. Physiol. (London),* **121,** 48 (1953)
37. Anonymous, *Nutr. Rev.,* **15,** 77 (March 1957)
38. Kleiber, M., Smith, A. H., and Cher-

nikoff, Th., *Am. J. Physiol.*, **186,** 9–12 (1956)
39. Fenn, W. O., *J. Physiol. (London),* **58,** 373–95 (1924)
40. Kleiber, M., and Rogers, T. A., *Ann. Rev. Physiol.,* **23,** 15–36 (1961)
41. Haldane, J. B. S., On Being the Right Size, *World of Mathematics,* 954 (Newman, J. R., Ed., Simon & Schuster, New York, 1956)
42. Krebs, H. A., *Bull. Johns Hopkins Hosp.,* **95,** 19–51 (1954)
43. Kleiber, M., *Ann. Rev. Physiol.,* **18,** 35 (1956)
44. Hill, A. V., *Science,* **124,** 1233–37 (1956)
45. Fermi, L., *Atoms in the Family* (Univ. of Chicago Press, 1954)
46. Kamen, M. D., *Radioactive Tracers in Biology* (Academic Press, New York, 1947)
47. Anonymous, *Progressive,* 9 (Jan. 1966)
48. Kleiber, M., *The Fire of Life* (Wiley, New York, 1961)
49. Stewart, G. R., *The Year of the Oath* (Doubleday, New York, 1950)