temperature sufficiently high to destroy the poison if present.

As regards low temperatures and cold storage effects, the bacteria may remain alive at zero temperatures and below, and even continue to multiply as long as the medium is liquid. But if meat is stored at temperatures low enough to produce a solidly frozen substratum it will keep indefinitely since there can be no bacterial growth or activity in ice. Fowls have been found perfectly good after four years storage at —10° F. But the public prejudice against cold storage products leads the market men to thaw the birds or meat before placing them on sale. This thawing is done by soaking in cold water, and as fresh water is not used for each piece, the water becomes foul, and well preserved material becomes infected. Unsold birds or meat, after thawing and hanging in the air for longer or shorter time, are frequently returned to cold storage and re-frozen to keep until the market demands them. Such re-refrigerated stock always shows marked deterioration. By purchasing original frozen stock, and allowing it to thaw slowly in the air, the consumer can insure himself perfectly good material at practically no risk of toxic poisons. Soaking frozen stock is always to be condemned.

In the lengthy discussion it was brought out that pure sepsin always shows the same degree of toxicity, no matter how prepared. When combined with albumen, certain animal and vegetable poisons appear to act more quickly than do their pure toxins. This is true particularly of ophiotoxin or snake venom. Meats and fowl should be placed in cold storage at once after killing, to insure long keeping. But for economical reasons, the animal heat is allowed to dissipate before putting the meat in the cold room. Meats that are "high," have already begun to decompose, but their habitual use appears to render the consumer more or less immune to the effect of toxins. In support of this theory successful experiments have been made to immunize animals against sepsin.

Following the discussion, a vote of thanks to the speaker, and to the faculty of the Harvard Medical school for the courtesies extended to the section, was passed. The members were then shown through the laboratories and inspected the equipment of the Chemical Building.

FRANK H. THORP,
Secretary

DISCUSSION AND CORRESPONDENCE

TOWER'S EVOLUTION IN LEPTINOTARSA

IN SCIENCE for July 19, 1907, Professor T. D. A. Cockerell gives a very appreciative review of Tower's investigation of evolution in chrysomelid beetles of the genus *Leptinotarsa,* and incidentally points out some defects. Professor Tower's work is of such scope that it seems desirable to call attention to certain errors and shortcomings which it contains. Above all one misses a clear presentation of the facts upon which the work is built up and which alone can give it standing among scientists. The value of the evolutionary discussion, which makes up the bulk of the work, must rest upon the accurate presentation of data and if these data are weak the deductions can not hold. It is my purpose herewith to point out such statements touching upon the biology and systematic aspect of these beetles as seem to me to call for criticism. Even a slight acquaintance with the literature of the subject would have saved Professor Tower from errors which are surprising in a man who claims to have devoted eleven years to his subject.

On page 1 is a tabulation of genera and species of Chrysomelini, abstracted from the "Biologia Centrali Americana." Although this purports to include the forms found in "America north of the Isthmus of Panama" the species found to the north of the Mexican boundary, with the exception of a few species of *Leptinotarsa,* are omitted. Thus several additional genera, and a large number of species, should be included in such a consideration. It is stated that of the 13 genera enumerated all but *Phædon* are peculiar to America, while in fact *Plagiodera* and *Melasoma* are likewise circumpolar. To these circumpolar genera must be added *Timarcha, Entomoscelis, Prasocuris, Chrysomela, Gastroidea, Gonioctena* and *Phyllodecta.* Professor Tower states that "with the exception of *Phædon,* all of these genera are closely allied." In fact, *Plagiodera* and *Melasoma* fall into a well-marked group with *Phædon.*

On page 2 Professor Tower states that three species of *Leptinotarsa* are found in the United States. The following species are known to occur north of Mexico: *decem-lineata* Say, *juncta* Germ., *texana* Schaeff. (*defecta* Linell, not Stål), *defecta* Stål, *lineolata* Stål, *dahlbomi* Stål, *haldemani* Rog., *rubiginosa* Rog.

Tower states that the life histories of *Leptinotarsa* are almost entirely undescribed; those of the following species have been published: *cacica* Stål, *behrensi* Harold, *undecim-lineata* Stål, *decem-lineata* Say, *texana* Schaeffer, *juncta* Germar, *calceata* Stål, *lineolata* Stål.

The list of the species of *Leptinotarsa* is simply garbled from the "Biologia" without reference to any other sources. Stål's classic work on the group is not even cited in the bibliography! The following two species are altogether omitted: *peninsularis* Horn and *multilineata* Stål. Most likely this last is the "*intermedia*" proposed by Tower, but not described. Under *L. defecta* at least two species are confused; quite probably the quoted record from Yucatan applies to still another species. *Leptinotarsa modesta* Jacoby = *L. behrensi* Harold, as Professor Tower might have discovered by more careful consultation of his one source of information—the "Biologia." No attempt is made to settle the status of doubtful species. Thus *L. violacesceus* Stål and *L. libatrix* Suffrian occur in the same localities and appear to be forms of one species. Tower visited these localities and collected these forms and a little attention should have settled this point. *Leptinotarsa puncticollis* Jacoby is merely a color-variant of *L. behrensi.*

Of Tower's five new species there is no descriptive matter whatever. Of four of them a single elytron is figured—however, no specific characters are shown in these figures. More useful for future identification will be the larvæ which are figured in three cases. Professor Tower asserts that the specific distinctions lie mainly in the colors of the beetles in life, and which disappear after death; he, however, studiously avoids any statement of what these color-differences are. And what are we to think, then, when on page 238 we read that *L. oblonga* is dimorphic and has a red and yellow form! In truth, several of the forms which Professor Tower enumerates as species are invalidated by the evidence which he presents in the body of the work. Thus *L. melanothorax,* if Professor Tower's observations are correct, can in no sense be termed a species; it does not exist independently in nature and is merely a color-variant of *L. multitæniata.*

While there is no direct statement to that effect, one is led to infer that the elytral pattern is of the greatest importance for specific differentiation. Yet the figures, if the forms are correctly associated, directly contradict this view. It remains to be proven, however, that such forms as appear under the same name in plate 14, Figs. 38 and 39, and in plate 23, Figs. 20 and 21, really belong together. On page 77 it is stated that the subcostal stripes are the least variable part of the elytral pattern; in *L. juncta* the two subcostal stripes present the most striking variation in that they are either independent, except at their extreme ends, or fused throughout, forming one heavy black stripe.

Plate 14, Fig. 35, shows a variation of *L. undecim-lineata* with the outer stripe wanting, but this form is omitted from the table of variation on page 78.

With Tower's seven varieties of *Leptinotarsa decem-lineata* matters stand even worse. Two of them are figured; of the rest there is nothing whatever to indicate their nature—unless one accepts the names themselves as aids to the imagination. Some of these forms are stated to show a number of characters which are specific; it would certainly be of interest to learn what these distinctive characters are. To give his work standing, Professor Tower must publish satisfactory descriptions of these forms. Furthermore, he should deposit series of all his species in a public museum, such as the National Museum, where they would be accessible to students and their preservation and authenticity guaranteed.

Plate 16, Fig. 9, represents *L. tortusa* Tower, a "variety" of *decem-lineata;* it appears to be a sport such as is sometimes produced by malformation or slight injury to the pupa. Such oddities of color-pattern, and much more striking ones, produced in such manner, are of frequent occurrence in the Coccinellidæ.

The statements regarding the distribution of *Leptinotarsa* on page 3 are at variance with those in the table on page 1.

But it is when we come to the discussion of *L. decem-lineata,* and the book may almost be said to be a treatise on this one species, that the superficiality of the author becomes most apparent. *L. multitæniata,* which occupies central Mexico, is stated to have extended its range to the northward along with its food plant, *Solanum rostratum,* in the wake of the Spanish conquerors in their progress northward. In its new habitat (northern Mexico and Texas) it was transformed into the form "*intermedia.*" The introduction and dispersal of *Solanum rostratum* into northern Mexico along the lines of early Spanish travel, and its spread from there farther north by the bison, are discussed at great length. *L. intermedia* of northern Mexico, after it reached the eastern slopes of our Rockies, was transformed into *decem-lineata.*

The whole argument turns upon the hypothetical dispersal of *Solanum rostratum* and the assumption that this plant is the original food plant of *L. decem-lineata.* We are virtually asked to believe that since the days of the Spanish conquest *L. multitæniata* has produced the two species *intermedia* and *decem-lineata.* Professor Cockerell has made the claim that in New Mexico *Solanum eleagnifolium* is the normal food plant of *L. decem-lineata.* As Dr. Chittenden has pointed out to me, the species of *Leptinotarsa* will feed upon various species of *Solanum,* preferring the more succulent ones.

On page 24 we learn that "the original distribution of *decem-lineata* was on the eastern slope of the Rocky Mountains northward to the Canadian boundary, eastward into western Kansas and Nebraska, and southward into Texas and New Mexico. In this habitat it was found by Say in 1823. Then, as now, it was probably sparsely distributed over the area, feeding upon *Solanum rostratum.*" It would be interesting to know from what sources Professor Tower obtained all this information regarding the original habitat and food plant of the species. After the original description of the species by Say, we find the remark: "This species seems to be not uncommon on the Upper Missouri, where it was obtained by Mr. Nuttall and by myself. The variety I found on the Arkansas." The variety in question, in which "the two outer intermediate lines are united at base and tip" is undoubtedly *L. juncta;* therefore the original habitat record is from the upper Missouri only, and certainly very little was added to our knowledge of the beetle until the time when it became of economic interest.

The eastward spread of the beetle and the factors that controlled it are presented in detail. Beginning with p. 44, the effect of the wind on the dispersal of *L. decem-lineata* during its progress eastward is discussed. On page 47 it is stated that the prevailing southerly winds greatly retarded the southward progress of the beetle. The scarcity of its favorite food plant in the south is much more likely to have been a barrier to its progress. The facilities for dispersal offered by the boat traffic on the Mississippi and its tributaries (see p. 30) would more than offset the influence of the unfavorable prevailing winds.

Tower states that the species of *Leptinotarsa* are double-brooded. This point, it seems to me, needs further investigation. My own experience with species of many genera of Chrysomelidæ has shown that they are all single-brooded and that the newly developed beetles do not become sexually mature until the following season. I have had no experience with *Leptinotarsa,* but abundant data which show that such closely related genera as *Calligrapha, Lina* and *Gastroidea* are only single-brooded. Such a physiological difference in alternating generations, of quick sexual maturity in one brood and of a long period of sexual inactivity in the other, would be most remarkable. The seeming double-broodedness of *L. decem-lineata* may be due to the difference in the time of emergence from hibernation of different individuals. We have no exact data regarding this point in *L. decem-lineata,* but some very pertinent ones on the boll-weevil which elucidate this subject. In Bulletin 51 of the Bureau of Entomology, p. 108, it is shown that the boll-weevil continued to emerge from hibernation during a period of more than two months (March 18–May 26). It is only reasonable to suppose that there would be an equal irregularity in the time of copulation, oviposition and larval development—amply sufficient to account for the two apparent broods.

Leptinotarsa juncta is taken up on page 49, and its retreat before *decem-lineata* discussed. The original distribution of *juncta,* as given in the text and on the accompanying map, is incorrect. Originally the species extended along the Atlantic as far north as the New England states and west of the Alleghenies at least northward into Ohio. As to the present distribution, I know of its recent occurrence at Richmond, Va., New Richmond, O., and St. Louis, Mo. The observation of Professor Quaintance that *juncta* and *decem-lineata* "hybridize freely in nature, although the eggs that are laid are not fertile," is ingeniously turned to account.

"The full explanation of the extinction of *juncta* is to be found in the fact that the two species cross freely in nature, and that this natural crossing has resulted in a most interesting and peculiar case of prepotency in one species and of submergence in the other." In other words, according to Tower, *L. juncta* in crossing with *decem-lineata* has been eliminated through Mendel's law. Does Tower realize that *juncta* has a number of specific characters and that these can not all be "recessive"—at least not according to the usual interpretation of the Mendelian law? In fact, *juncta* continues to exist as a distinct species, even upon the same plant with *decem-lineata.* Furthermore, Tower states (p. 20) that his *L. oblongata* and *L. multitæniata* Stål occur together upon the same food plant but do not interbreed. The same statement is made with reference to *L. multitæniata* and *L. rubicunda* Tower. These three forms appear to be much more nearly related to each other than are *L. decem-lineata* and *L. juncta* and it is unreasonable to believe that the last two interbreed freely while the first three do not. A similar case to that of these two species of *Leptinotarsa* is that of the crowding out of *Pieris oleracea* by *Pieris rapæ. Pieris oleracea* still persists in the mountainous parts of New England, where it is found associated with *P. rapæ,* and it would be going far afield to invoke the "recessive" principle of Mendel's law to explain its disappearance from its former territory. In the Chrysomelidæ the crossing of closely related species is quite common when the two forms occur upon the same food plant. I have observed it repeatedly in *Gastroidea polygoni* and *G. cyanea,* but I am not aware that such crossing has had any appreciable effect upon either species.

In the chapter on the habits and instincts of *Leptinotarsa* we find, on page 236, the following statement: "the eggs, although they may be fully formed and fertilized, are not laid, but are retained in the passages of the female reproductive organs until they are resorbed, or, as more frequently happens, until the female dies." As the process of egg fertilization in insects is generally understood, the seminal fluid is deposited in the receptaculum seminis and the eggs are only fertilized when they pass this organ during oviposition.

The statement, on page 260, that all the species of *Leptinotarsa* feed upon Solanaceæ is certainly incorrect; this may be true of the *lineata* group, but it is certainly not for the whole genus.

Chapter III., which deals largely with the physiology of color-pattern production, is the most scholarly part of the work, and most interesting and instructive. It is, however, in great part a repetition of the previously published investigations of the author.

Many pages of the book are taken up with tables which look very impressive. As the figures are, however, for the most part only in averages, and there is nothing to show how extensive or complete are the data back of them, they are not convincing. Thus in the table of data on page 237 relating to oviposition, the maximum, minimum and average for any given species may be made up of more or less complete observations on two beetles or on a thousand; at all events, new observations will change the figures. Data on the oviposition of *L. decem-lineata* which Mr. A. A. Girault is about to publish will change the aspect of this table very materially.

It goes without saying that there is much excellent material in Professor's Tower's work. The observations on habits are most interesting. A point well worth the attention of experimental biologists is that tropical species, being less subject to fluctuating conditions than those of more northerly regions, respond more readily to change of environment.

The work, along with other Carnegie publications, suffers very materially through the absence of an index.

FREDERICK KNAB
WASHINGTON, D. C.

SPECIAL ARTICLES

AGE OF A COOLING GLOBE IN WHICH THE INITIAL TEMPERATURE INCREASES DIRECTLY AS THE DISTANCE FROM THE SURFACE

KELVIN's famous and epoch-making paper on the secular cooling of the earth was published in 1862.[1] His problem was to find the time which would elapse before a globe completely solid from center to surface and having throughout a certain uniform initial temperature would cool so far as to reduce the surface gradient of temperature to any given value. He assumed an initial temperature of 3,900° C., a diffusivity of 0.01178 in c.g.s. units and a final surface gradient of 1° C. in 27.76 m. or 1° F. in 50.6 feet. These data discussed by one of Fourier's theorems give for the age of the earth 98×10^6 years. Kelvin, however, expressly directed attention to the fact that the effect of temperature in modifying diffusivities is almost unknown, and that the original distribution of temperature is uncertain. He also referred to the great differences in the surface gradient of temperature, which varies with the locality, as he stated, from 1° F. in 15 feet to 1° F. in 110 feet. He, therefore, allowed very wide limits in his estimate and placed the age between 20 million and 400 million years.

In 1893 Clarence King made a very important contribution to the subject[2] by introducing the criterion of tidal stability. Mr. Barus determined for him the melting point of diabase in terms of depth. If in any hypothetical earth consisting solely of diabase the temperature in any couche were to exceed the melting point of diabase, then tidal instability would set in, the crust would break down and chaos would reign for the time being. In a real earth the same result would follow provided the couche were in a region where diabase or equally fusible rocks are to be expected. Excluding such cases, King found that the age of the earth could not exceed 24 million years when Kelvin's values for diffusivity and surface gradient are assumed. He also found that the corresponding initial temperature of such a globe would be 1,950° C.

Kelvin's last paper on a cooling earth[3] was read in 1897 and he there stated that after having worked out the problem of conduction of heat outwards from the earth by an elaborate method, he was not led to differ much from Clarence King's estimate. This he adopted as the most probable age and reduced his limits to between 20×10^6 and 40×10^6 years.

While King's earth is tidally stable, I confess that his solution of the problem seems to me to be fatally defective. He himself gives a temperature curve for the same earth at an age of 15 million years and this earth shows a couche at a temperature above the melting point of diabase, this layer extending from a depth of 34 miles below the surface to 66 miles. According to Laplace's law of densities these two levels correspond respectively to densities of 2.85 and 2.93, and it seems certain that the material must consist chiefly of basaltic rocks. Thus the 15-million-year

[1] *Trans. R. S. Edinburgh*, reprinted in Thomson & Tait, "Natural Philosophy," Pt. II., p. 468.

[2] *Am. Jour. Sci.*, Vol. 45, 1893, p. 1.

[3] *Trans. Victoria Institute*, Vol. 31, 1899, p. 11.