of from 200 to 800 cc. per minute. In this range the float moves through a distance of 125 mm. This

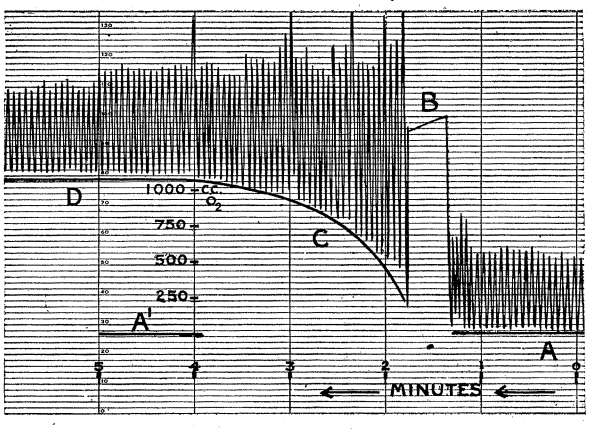


FIG. 2. A kymographic recording of the effect of a stair-climbing exercise on metabolism. Note recovery period as shown by the curve (C).

Rotameter was designed to measure small flows of liquids used in aviation equipment. Obviously, a tube of greater length with a similar range of delivery would aid in more precise reading.

Corrections for temperature and pressure for the Rotameter delivery are negligible and may be disregarded. Such corrections, however, are necessary for gas in the spirometer.

My principal purpose in designing the modification was for use in connection with class work of a course called "Applied Physiology." The report is given here with the thought that investigators more especially interested in metabolism studies may be better able to further develop the present modification. Our experience with the apparatus as shown in Fig. 1 indicates the desirability of redesigning other parts of the apparatus, especially for use with the Rotameter. One modification we have tried is the substitution of a respiratory bellows in place of the water spirometer. This makes respiratory movements much easier on the subject but increases difficulty of computations. Refinements in the construction and assembly of the apparatus would obviously contribute to greater accuracy and also further broaden its application.

# *Letters to the Editor*

### The McDonald Observatory

Your announcement of appointments at the Yerkes and the McDonald Observatories (*Science*, 1946, 103, 80), should be corrected to the effect that the McDonald Observatory is a part of the University of Texas and not of the University of Chicago. Appointments for both observatories are made by the University of Chicago, but the University of Texas owns the McDonald Observatory and approves the appointments.

OTTO STRUVE, *Director*
*The W. J. McDonald Observatory, Fort Davis, Texas*

### Museum of Science and Industry

In *Science* (1946, 103, 17) you refer to our sister institution in New York as "The Museum of Science and Industry." This is misleading even though you add the words "Rockefeller Center, New York." We feel, in view of the history of the two institutions, that we are entitled to have our museum associated with that name.

The New York museum was founded under the name of "The Museum of the Peaceful Arts," while ours carried the name "The Rosenwald Industrial Museum." When we changed our name to "The Museum of Science and Industry," they changed to "The New York Museum of Science and Industry." At that time we protested the close copy of our name, but they declined to change, claiming that the qualifying words "New York" made the distinction clear. Certainly, to leave off these words is unfair to this institution, which considers itself entitled to its proper designation before the public. We will appreciate your consideration of this point.

RUSSELL H. ANDERSON
*Museum of Science and Industry, Chicago*

### Malaria and Rainfall Periodicity in Palestine

The war has demonstrated the vital necessity of medico-geographical work. Yet, in order to be useful, it seems to us that future work in this field should avoid some of the pitfalls found in Dr. H. de Terra's "Rainfall periodicity in relation to malaria and agriculture in the Near East" (*Science*, 1945, 101, 629–631).

Studies in medical geography should be based not only on geography, but equally on medicine. Of course, excessive or insufficient regional rainfall is one of the causal factors of malaria epidemics, and such correlations have been attempted since the time of Hippocrates. (A. Hirsch has provided a survey of such attempts, which were particularly numerous about a hundred years ago, in *Handbook of Geographical and Historical Pathology*. London, 1883, Vol. I, pp. 258 ff.) It is not possible, however, to predict malaria epidemics simply on the basis of probable maxima of rainfall, for to do so is to reduce the complicated process underlying a malaria epidemic to one simple factor: the quantity of anopheles. (I. J. Kligler, who is one of Dr. de Terra's main sources, makes the follow-

ing important strictures on the use of rain statistics in *The epidemiology and control of malaria in Palestine* (Chicago, 1930): "The importance of rainfall as a factor in the formation of breeding places depends as much—sometimes more—upon the distribution as upon the total volume" (p. 6). "Most of the anopheles breeding places in Palestine are due however either to century old neglect by man or to man's carelessness" (p. 11).) We know today that fluctuations in the *immunity* of the population are an essential factor in the genesis of a malaria epidemic (L. W. Hackett. *Malaria in Europe.* London, 1937, pp. 217 ff.). Fluctuations in spleen mass which express the amount of "immunity" or "premunition" seem to offer a far more reliable factor for prediction of malaria epidemics than rainfall curves. Preceding years of drought, which reduce endemicity and therefore premunition, and which break down the cattle barrier, may in this sense contribute at least as much to an epidemic as the rainfall period itself. There are other possible factors also, e.g. increased gametocyte production, which should be taken into consideration.

In a region such as Palestine, where the rate of malarial infection is always high, the mass importation of nonimmunes is likely to be more important in setting off an epidemic than the importation of carriers. This is applicable to the situation in Palestine in 1917 when soldiers, and to other years when colons and laborers, were moved about in large numbers. In addition to these, the social dislocations of war bring many other disturbances which are conducive to spreading malaria. It seems more likely that Greece and the Balkans suffered from similar epidemics in 1917 because of similar sets of causes, some of which were social, than because of "similar rainfall regimes" with Palestine, which they do not have. (Palestine has a typical Mediterranean regime of winter cyclonic rain and summer drought. Except for Greece, none of the Balkans has such a rainfall distribution. The Adriatic coast is an area of high year-round rainfall. The rest of the Balkans, except Greece, are marked by a strong concentration of summer convectional rains.) One should note in this connection that parts of France and Flanders, certainly not similar to Palestine in rainfall regime, also had epidemics in 1917.

The weakness of a unitary explanation of malaria in Palestine is illustrated by the impossibility of predicting malaria in Palestine from de Terra's graph. The graph would, for instance, suggest epidemics at least in 1914 and 1929. But no such epidemics were observed. It will be shown below that the evidence is not good for epidemic malaria in the years noted on the graph. These contradictions can be understood only if all factors contributing to malaria epidemics are taken into account. One of the most important is social change. Even if excessive rain should occur in cycles of 10 to 12 years over the Holy Land, its chances of producing a malaria epidemic would depend largely on its always encountering equally miserable socioeconomic conditions and equally susceptible populations. This, fortunately, is neither necessary nor likely.

These methodological considerations are independent of the validity of the data on epidemics and rain cycles. Unfortunately, the evidence advanced for epidemics in 1904, 1911–12, and 1926 seems inadequate. That Cropper found a high rate of infection in 1904–05 is no reflection of an epidemic in this period, but simply of the fact that he made his inquiry in those years. The same holds good for the data of Bruenn and Goldberg in 1911–12. The epidemic of 1917–19 did not actually conform to rain statistics and stop in 1919, as de Terra assumes, but continued into 1920 and 1921 (see Kligler, p. 157). The data concerning the "epidemic of 1926" which we have been able to collect are far from indicating a "rapid rise of malaria infections" in 1926. For example, G. J. Schneider (*Organisation und Erfolg der Malaria Bekämpfung in Palästina.* Jena, 1928) has collected the following data:

| Percentage of malaria cases in all clinic patients | | Infections in region supervised by Malaria Research Units |
|---|---|---|
| 1922 | 7.17 | 68.4 |
| 1923 | 4.90 | 34.8 |
| 1924 | 4.12 | 13.2 |
| 1925 | 2.27 | 9.8 |
| 1926 | 2.04 | 11.0 |

Spleen index census

November 1925 ..... 4.6 per cent
November 1926 ..... 1.7 per cent

The rainfall cycles are equally open to doubt. The theories of correlation of sunspot maxima and rainfall, and of the cyclic occurrence of rainfall maxima, are too speculative for anyone to be able as yet to interpret unanalyzed curves in accordance with them.

Although de Terra states that for Palestine "meteorologic recordings go back sufficiently in time to permit of definite conclusions as to the periodicity of rainfall and water supply," such periodicity is neither evident in his figure nor demonstrated in his text. He shows rainfall curves for three stations for the years 1874 to 1939 and for the fourth station an unbroken record only from 1920 to 1938. The last is too short to be useful except in comparison with the other curves with which, however, it shows poor correlation. The three long-range stations also show poor agreement with each other. A very wet year at one station is a very dry year at another. It is difficult to see any way of predicting rainfall from such irregular curves.

The agreement with the solar cycles shows the same picture. Wet periods of individual stations occur both at sunspot maxima and minima. One must ask if there is even a rough correlation. De Terra states that he has under way a special study on possible periodic shifting of the cyclonic storm tracts in the Eastern Mediterranean. It is to be hoped he will demonstrate more clearly any periodicity he may find in his data.

It may well be that one of the necessary conditions for a malaria epidemic in Palestine is maximum rainfall. But even if rainfall periodicity could be "better recognized and substantiated," it is unlikely that this alone could "lead to the prediction of conditions conducive to malaria epidemics." No one expects Dr. de Terra to be a malaria expert. We regret, however, both that he did not analyze his climatic data further and that he did not collaborate with other scientists who could have helped him better to

evaluate the role of weather and climate in malaria. Any effort in the field of medical geography is highly desirable, and should be encouraged; but we feel that in order to contribute effectively to a scientific understanding, medical geography will have to study the phenomena in all their complexity and keep clear from premature and oversimplified conclusions.

ERWIN H. ACKERKNECHT, M.D.
GEORGE F. CARTER, Ph.D.
*The American Museum of Natural History*
*The Johns Hopkins University*

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Supplementary statistical data which I received, after publication of my article, by courtesy of the president of the Hebrew University of Jerusalem, and which are at present not available to me in Mexico, show rather well the relationship between rainfall maxima and peaks of malaria mortality. Even if the data given for previous malaria incidence in Palestine are inadequate, it is nevertheless suggestive to see the epidemics of (if I remember correctly) 1935 and 1939–40 occurring in years of greater rainfall. As for the irregularity of the rainfall cycles, seemingly apparent for certain stations, it must be remembered that such discrepancies may well be due to local microclimatic factors arising out of peculiar topographic location. However, this cannot distract from the general impression of periodicity. As for the cyclic control of these epidemics, it would seem that late spring rains in between the major rainfall peaks possibly introduce another climatic element which deserves further study in relation to malaria epidemics. While it is apparent that present data are suggestive rather than conclusive, I would personally only welcome a fuller investigation of this important problem of medical geography, not only through closer cooperation with medical specialists but through an effective and intensive field study in the Near East. Only thus will it be possible to eliminate the inadequacies disclosed by this discussion.

H. DE TERRA
*Mexico, D. F.*

### Virus Encephalomyelitis in Buffaloes

In the western part of the Szechuan province, China, there is a disease among water buffaloes known as "Szegiao-han" (meaning "four-legs-cold") to the farmers. It occurs more in wintertime than in the hot seasons, and young and old buffaloes seem to be equally affected. Its occurrence in other provinces is also suspected, though not proved. A report of the disease in the scientific literature has not appeared before.

Onset of the disease is usually abrupt. The first symptom is a paralysis of the hind legs and inability to stand. The paralysis quickly extends to the fore legs and may be of either a spastic or flaccid type. Animals apparently well the day before may be found lying ill in the stable the next morning with all four legs rigidly extended. The body temperature is usually subnormal from the beginning of the illness. As death approaches, it often drops to below 35° C. Only occasionally is a middle-grade fever observed. The surface temperature of the legs feels colder than normal and thus the name. Sensation of the skin is dulled, especially over the paralyzed parts. In severe cases there is complete anesthesia of the whole body surface except over the head. Appetite and rumination are decreased and later entirely lost. Other rarer symptoms are retention or incontinence of urine, colic pains, bloody discharge from the anus, tremors of the muscles, and rigidity of the neck. The respiratory and circulatory systems are comparatively less affected. Consciousness is retained and excitement absent. The course of the disease is usually short, most animals dying within one to two days. The mortality rate is very high (probably over 95 per cent).

On post-mortem examination, the central nervous system is usually found to be highly congested. There may be hemorrhages under the meninges and petechiae in the spinal cord. The cut surface of the brain, however, looks apparently normal. Occasionally there is a gelatinous infiltration in the epidural space of the vertebral canal. Besides these changes in the nervous system there are always signs of septicemia of the whole body. Hemorrhages may be found in different organs. Constant and especially marked are hemorrhagic gastroenteritis, cystitis, tracheitis, and endocarditis, and occasionally effusions in the serous cavities. The parenchymatous organs show signs of albuminoid degeneration. Cultures taken from the blood and central nervous system are invariably negative.

Histological examination of the central nervous system reveals congestion and capillary hemorrhages in addition to the petechiae already visible to the naked eye. The nerve cells of the brain and spinal cord are mostly degenerated. The cytoplasm may be either swollen or, more often, shrunken so that the cells become irregular in contour. The nucleoli are masked by the deep staining of the pyknotic nuclei. In some cells there is complete dissolution of the nucleus and tigrolysis of the Nissl substance. The degenerated nerve cells are soon invaded by glial cells. This phenomenon of neuronophagia is especially marked in the pyramidal cells of the cerebral cortex and hippocampus, but less so in the Purkinje cells of the cerebellum and ganglion cells of the spinal cord. Perivascular spaces of the blood vessels of the brain are often dilated owing to an accumulation of edematous fluid. Adventitial proliferation is frequently seen, but perivascular infiltration is rarely encountered. There may be a mild degree of gliosis in the brain substance. Occasionally a small artery under the pia mater is thrombosed. Otherwise, the meninges are essentially negative. Inclusion bodies, whether intracytoplasmic or intranuclear, are not found.

In view of the facts stated above, the author believed this disease to be an encephalomyelitis caused by a filtrable virus. Experiments were soon planned and conducted to test its transmissibility to other animals. Brains and spinal cords of two buffaloes which had died of the disease were ground up in sterile normal saline and filtered through Seitz E. K. filters. The bacteria-free, clear filtrates were injected into goats, guinea pigs,