

Correspondence

FAILURE OF THE SWEAT MECHANISM IN THE DESERT

To the Editor:—It was with interest that we read Captains Wolkin, Goodman and Kelley's article "Failure of the Sweat Mechanism in the Desert: Thermogenic Anhydrosis" (*THE JOURNAL*, February 19, p. 478). The reason for our interest is that we have observed 2 patients who presented the same syndrome. These men, however, were not subjected to the hot dry climate of the desert but to the hot highly humid climate of the South Pacific.

The first patient who came to our attention was a cook who had been in the tropics about seven months, where the temperature is high and the humidity is constantly near the saturation point. In addition to being exposed to this extreme heat, he carried out his duties as a cook. He was in good health and sweated in a normal fashion for the first six months. Then he noticed that in a short time he stopped sweating from the neck down and developed a dry scaliness of the skin. Associated with this was generalized weakness. Examination at this time showed a striking distribution of sweating. The day of examination was extremely hot, so that all of the examiners and the people in the dispensary were sweating profusely. This man was sweating in the same manner only about the head and neck. At the base of the neck the sweating stopped abruptly. The rest of the body was dry. Examination of the axillas and inner aspects of the groins showed the skin likewise perfectly dry. The skin presented a nondescript, dry, scaling, slightly erythematous eruption. There was no evidence of ichthyosis. He appeared weak and pale. Physical examination otherwise revealed nothing of importance. He was sent to a hospital in a cooler climate. On examination there nothing unusual was found. After two weeks of rest he began to sweat normally.

The second patient also was in the same type of climate; namely, hot and highly humid. He had no particular difficulties for the first four months; then he began to notice weakness while working and the appearance of a dry, scaly eruption and soon after this the failure to sweat. Because of the weakness that he developed on the slightest exertion he also was sent to a hospital in a cooler climate. Examination there showed nothing unusual except the anhydrosis. After three weeks of rest he was sweating normally and felt much stronger.

These 2 cases follow closely those described by the authors, with spectacular sweating of the face and neck, which stops abruptly, the rest of the body remaining dry. The eruption over the body is very similar to the stationary "goose flesh" that they describe: in other words, a dry papulofollicular, nonerythematous eruption. We were unfortunately unable to study their cases further. We feel that the authors have described a unique form of anhydrosis and that our patients also fall into this group.

The one difference between their patients and ours is that of climate. Their patients were subjected to extreme desert heat with low humidity, and ours to tropical heat with high humidity. It would seem, therefore, that only the high temperature plus some unexplained failure of the sweat mechanism are the principal factors in bringing out this syndrome.

Undoubtedly, with more troops being subjected to high temperatures over long periods of time, more of these cases will be seen. The only apparent treatment is removal to a cooler climate.

Further investigation as to the cause of this form of anhydrosis should be carried out.

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SUGAR A "DILUTING" AGENT

To the Editor:—The Council on Foods and Nutrition (*THE JOURNAL*, Nov. 7, 1942, p. 763) gave as its view that "the consumption of sugar and of other relatively pure carbohydrates has become so great during recent years that it presents a serious obstacle to the improved nutrition of the general public." It was emphasized that refined sugar is a food deprived of vitamins and minerals; however, "if sugar could be always used only as a means of making highly nutritious foods like milk or whey more appetizing, much less odium would attach to it than does. . . . But even here one is essentially 'diluting with calories' the food which is sweetened."

The Council thus presents the concept of sugar "diluting with calories" the foods of our dietary. It seems we should study the full meaning of this concept, what it implies and note some of the biochemical events it involves as they appear to be related to our national health and war effort.

From the Council report it is made clear that when sugar is "diluting with calories" it is displacing nutritionally superior foods from the dietary, while at the same time it is increasing the requirements of nutrients in the foods displaced. Vitamins B₁, riboflavin and niacin are necessary for the oxidation of dextrose. Sugar does not supply these vitamins, yet it increases their requirements. When "diluting with calories" sugar is thus using vitamins from other foods or from the reserves of body tissues. Wilder (*Handbook of Nutrition, THE JOURNAL*, Oct. 17, 1942, p. 531) expressed the idea in these words: "Sugar is not among the recommended foods. Its recent rationing will not provoke a hardship, for sugar supplies nothing in nutrition but calories, and the vitamins provided by other foods are sapped by sugar to liberate these calories."

Cowgill (*THE JOURNAL*, Dec. 9, 1939, p. 2151) pointed out that there is an increased requirement of other food essentials like calcium, phosphorus and iron with an increase of calories. We may say, then, that sugar "diluting with calories" decreases the vitamin (or other nutrient)/calory ratio and increases the difficulty of obtaining a diet nutritionally adequate to meet the daily allowances recommended by the Food and Nutrition Board of the National Research Council (*THE JOURNAL*, June 7, 1941, p. 2601).

Little concern would be felt for the role of sugar as a "diluent" of foods and nutrients and a "sapper" of stored vitamins if only about 1 ounce (120 calories) per person daily were consumed, as was the case a hundred years ago. However, according to Gubin (*Hygeia*, January 1944, p. 15), the monthly per capita civilian consumption of sugar as food for 1935-1939 averaged 8 pounds (or 483 calories per person daily); for 1942 it was 7½ pounds (452 calories daily); for 1944 it is estimated to average 6½ pounds (or about 400 calories per person daily).

Four hundred calories of sugar consumed by a person on a diet of 2,000 calories represents a "dilution" with calories of 20 per cent. The diets of many people contain less than 1,800 calories per day: as, for example, 8.8 per cent of the aircraft workers in southern California (Wiehl, Dorothy G.: *Milbank Memorial Fund Quart.* 20, October 1942). Even on a 4,000 calory diet the 400 calories of sugar dilutes the nutrients 10 per cent. Probably if the 400 calories of sugar were supplied by superior foods, all the nutrients would be more "concentrated" in the diet by 10 or 20 per cent, depending on whether the diet contained 4,000 or 2,000 calories.

It may sound superficial to say that "diluting" our foods with this great consumption of sugar is "diluting" the physical strength, national unity and war effort of our country, yet the Council on Foods and Nutrition of the American Medical Association (*THE JOURNAL*, Nov. 7, 1942), the Committee on Nutrition in Industry of the National Research Council (Reprint and Circular Series No. 110, April 1942) and members participating in the Symposium on Nutrition in Industry at the Annual Congress on Industrial Health (e. g. see discussions of

Wilder and of Pett, *THE JOURNAL*, March 13, 1943, pp. 869-871) have already expressed concern over the consumption of sweetened carbonated beverages and candy by workers in industrial plants.

It seems to me that more publicity should be given to this concept of sugar as a "diluting" agent; and more research is needed to find out the extent of harm our present consumption of sugar is causing.

Our patients and the general public must be made to realize that, while sugar supplies energy, it also "dilutes," weakens with calories, and that other carbohydrates and all proteins and fats are sources of energy.

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DIAGNOSIS OF EARLY SYPHILIS

To the Editor:—Clinical teaching has always stressed the fact that a painless sclerotic solitary ulcer or "hard chancre" is the typical primary lesion of syphilis. While it is true that all textbooks and teachers allow for some variation in the clinical appearance of the initial local sore, my personal observation is that its characteristics, somewhat like those of later syphilitic manifestations, are more protean than is generally considered by our profession.

In several years of both military and private practice I have had occasion to see relatively few cases of early syphilis. But of 11 cases that my associates and I were able to confirm positively by dark field examinations only 3 presented the classic type of primary lesion. The 8 others resembled ordinary chancreoid or balanitic ulcerations; they were not indurated, often were multiple and sometimes were painful. The most recent case in my personal experience was almost, and perhaps hardly inexcusably, overlooked as syphilitic, for the patient presented the typical picture of an ordinary nonspecific balanoposthitis complicating a phimosis with several tender ulcerated areas on the preputial mucosa and, furthermore, he maintained that he had had the same condition several times in the past. Only the history of recent exposure prompted me to have a dark field examination made, and it was quite a surprise to receive a laboratory report that the smear was found teeming with *Treponema pallidum*.

This naturally brings to mind the important question How many of the latent or advanced cases of syphilis we find were not detected in their early, more easily treated, stage simply because the primary lesion was so atypical or minor as not even to suggest the ruling out of syphilis to the examiner? If the threat of syphilis to mankind is to be further reduced, not only must a high "index of suspicion" be maintained on the part of the individual practicing physician but an abrupt *volte face* is in order on the part of our clinical teachers to emphasize duly the varied appearance of initial syphilitic lesions. Only thus can the practitioners of medicine be influenced to subject each and every clinical case presenting a genital ulcer of any description to the trustworthy oracle of the dark field laboratory examination.

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To the Editor:—On page 278 of the May 27 issue of *THE JOURNAL* there is an editorial comment on the recent celebration of the Lancaster city and county medical societies. It speaks of Dr. Jonathan M. Foltz as being Surgeon General under President Grant. Dr. Foltz was Surgeon General of the U. S. Navy, and I am sure there was a Surgeon General of the U. S. Army at the same time, so that this branch of the service should have been designated. I happened to notice this because in

writing up my grandfather's diary I used a biography of Dr. Foltz, who during the Civil War was ship surgeon on Farragut's flag ship while my grandfather was paymaster on one of the other vessels in the same fleet.

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BILATERAL OOPHORECTOMY WITH RADICAL OPERATION FOR CANCER OF THE BREAST

To the Editor:—Your editorial on "Bilateral Oophorectomy with Radical Operation for Cancer of the Breast" in the May 20 issue, was interesting.

Oophorectomy may favorably influence the control of mammary cancer, but such a blunt statement is not desirable at present without an adequate number of cases with sufficient length of follow-up, as it may lead to premature conclusions. Can one judge on the observation of 1 case of mucoïd cancer in a series of 25 cases out of which only 2 had a five year follow-up period?

In an earlier article on "Mammary Cancer of Youth" (*Surg., Gynec. & Obst.* 77:55 [July] 1943) 73 cases of breast cancer under the age of 30 years were reviewed. The records of these patients were collected from six leading hospitals of New York and Boston, with follow-up periods over five and ten years. From this study it was learned that the previously held belief that the prognosis for women with breast cancer under 30 years of age is fatal is untenable. Results in such younger groups could be favorably compared to the general series (Eggers, Carl; de Cholnoky, Tibor, and Jessup, D. S. D.: *Ann. Surg.* 113:321 [March] 1941).

The five year survival was found to be 40.8 per cent and the ten year arrest 37 per cent in all cases in which operation was performed. Included in this series is a patient 23 years of age with bilateral breast cancer. She gave birth to a child five years after the first radical operation and a year after that had a radical operation for cancer of the other breast. This patient is well twelve years after the original operation.

In patients with small tumors less than 2 centimeters in diameter lymph node involvement was rarely found. Among these patients 10 out of 11 survived the five year period, although no oophorectomy or x-ray castration was performed.

Early diagnosis, therefore, is imperative when dealing with small solid tumors. Often this can be made only by a frozen section. In such cases oophorectomy may be omitted, even if further observations on a larger series with a longer follow-up period should indicate its beneficial influence.

Endocrine influence on tumor formations is a possibility, and it was also mentioned in one of our articles (*Arch. Surg.* 38:79 [Jan.] 1939). It is not exactly known as yet whether the elimination of ovarian function alone could reduce carcinogenesis or promote cancerous growth. At present it may be hasty to advocate oophorectomy in all cases of early breast cancer in the premenopausal stage. Clinical evidence with few recurrences points to satisfactory results following a carefully performed radical operation, after which no "small amount" of cancer cells should remain which could be stimulated by estrogenic substances. Additional mutilating operations therefore ought to be limited to more advanced cases with metastasis or recurrent lesions until further well controlled investigations confirm the beneficial effect of bilateral oophorectomy in breast cancer.

Finally I would like to point out the statement in the editorial that "mucoïd cancer . . . probably arises from the stroma. . . ." This is obviously a mistake.

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