of Geneva, walked into LeRoyer's pharmacy there and asked J. B. A. Dumas (then 18 years of age) to determine for him whether sponge, especially burnt sponge, contains iodine. On receiving Dumas' report that iodine was present, "Dr. Coindret no longer hesitated to consider iodine as a specific against goiter."[1]

The *Scientific Monthly* for September, 1935 (pp. 263–5), has an article by Dr. Norman Tobias entitled "Making Malaria Work for the Doctor," in which reference is made to the work of Dr. J. Wagner-Jauregg, who received the Nobel prize in 1927 for his discovery that malarial infection may be used to combat certain nervous sequelae of syphilis. In this connection, I would draw attention to certain facts as stated in a chapter entitled "The Savage as Scientist," by Fulahn, in No. 11 of a series of books called "Tales from the Outposts," published by Blackwoods, Edinburgh.

Kinga, who was one of the most famous chiefs and rain-doctors in East Africa, refused to be moved from his kraal at Mandi on the Daua Plateau down to Sekenke in the Wembare Plains, as medicine-man Mgendu urged; and Mgendu came to ask advice of the writer, who was then administrative officer in charge of the Iramba tribe. [Kinga was suffering from general paralysis.]

Said Mgendu: "The *vidudu* of paralysis must fight with the *pilintu* of malaria so that the *pilintu* may be devoured: then must Kinga eat of the *nzizi chungu* (bitter roots), and he will be strengthened." . . . *Vidudu* are mysterious insect-like things; a *pilintu* is a strange unknown worm-like thing . . . that half-naked savage doctor was prescribing the most up-to-date medical treatment for paralysis based on the most recent discoveries of medical science. . . . Sekenke is one of the worst malarial districts in all Africa. . . .

Many tribes, not only the Masai and Nandi of Kenya, knew the cause of malaria. The Somalis knew, for a British traveller in their country was told by Somali tribesmen thirteen years before Ross's discovery, that the *kan'ad* or mosquito was a bad insect, biting a man and making his blood boil with fever. Chief Kitandu of the Iramba tribe knew four centuries ago, for his minstrels sang to the twang of the *lusembi*, a primitive calabash guitar, "*Ni aza kusengila pana nu imbu; nu imbu mbii masaka masenkila!*" (Do not build huts where mosquitoes live; for mosquitoes are evil, and make your blood hot!). And that song, with others full of savage wisdom, is to be heard to this day in the kraals of Tanganyika.

I wonder whether any reader of SCIENCE can say where the statement occurs that the early British explorers, Speke and Burton, commented on what seemed to them a silly superstition among the natives of the Congo, that the African "sleeping-sickness" was associated with the coming of the tsetse fly. I remember having read of this as a boy, but can not recall where.

[1] A. W. von Hofmann, *Berichte*, 1884, 17, 637, referate.

In an address on "Biochemistry and the Manufacture of Fine Chemicals,"[2] Dr. F. H. Carr said that real scientific medicine, in so far as concerns biochemical aspects, began forty-one years ago, "when the use of thyroid gland in the treatment of myxoedema was discovered." In a paper on "The Contributions of China to the Science and Art of Medicine,"[3] Dr. Edward H. Hume, then of the College of Medicine (Yale-in-China) at Changsha, China, stated (p. 349): "Organotherapy is described as early as the 6th century, A.D., when sheep's thyroids were used for cretinism. The practice is familiar to housewives throughout the land."

In a preceding paragraph, Dr. Hume said: "Inoculation against small-pox was practised early, records being available of the transfer of virus from person to person in the 7th century, though the routine use of the method was not common until the 11th. A century before Jenner, the standard materia medica mentions the use of cow fleas for the prevention of small-pox." (Note that insect transmission is involved).

Professor J. J. Abel's isolation of bufagin and an adrenaline-like substance from the skins of toads, and our wide use of ephedrine from Ma Huang, are further instances of the fact that science very often explains or rediscovers practices long ago established.

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THE MEANING OF "MASKING"

THOMPSON[1] has recently objected to Davis and Derbyshire's[2] use of the term "masking" to designate the phenomenon that occurs when one sound obliterates another, stating that "to call this 'masking' introduces confusion into the literature, 'masking' having already been preempted to designate a central phenomenon, postulated by Robert[3] and demonstrated (we think) by us." Thompson's experiment shows that electrical excitation of the radial nerve masks the sensation resulting from deep pressure applied to the ulnar nerve and that the mechanism of this masking is in the central nervous system.

This contention of Thompson's can not be permitted to pass unchallenged, since the term "masking" had already been in general use to designate the auditory effect for some years before it was preempted by him. Wegel and Lane[4] used the term in reporting their study on the dynamics of the ear in 1924, and defined it on the phenomenological level.

Minton,[5] in discussing an auditory experiment re-

[2] *Chemistry and Industry*, 53: 123, 1934.
[3] SCIENCE, April 18, 1924.
[1] I. M. Thompson, SCIENCE, 82: 221, 1935.
[2] H. Davis and A. J. Derbyshire, *Am. Jour. Physiol.*, 113: 34, 1935.
[3] Robert, *L'Union Médicale*, 12: 487, 1858.
[4] R. L. Wegel and C. E. Lane, *Phys. Rev.*, 23: 272, 1924.
[5] J. P. Minton, *Phys. Rev.*, 22: 506, 1923.

ported in 1923, stated, "This test . . . is surely convincing proof that the masking effect described is not necessarily a function of the cochlea of the same ear . . . but may more probably be a property of the auditory nerves, or even of the acoustic centers of the brain itself." Davis and Derbyshire's recent work[2] indicates that the mechanism is peripheral. They define auditory masking as "the diminution of audibility of one sound caused by the presence of a second sound." This is obviously a phenomenological definition, and makes no implication concerning the mechanism. Troland,[6] in his "Psychophysiology" (p. 217 *ff.*) has used the term in much the same way. This usage is typical; many other cases could be cited.

Reference to Robert's report[3] reveals that he did not use "masking" in a restricted sense, for in discussing his electrical anesthesia he says "Y a-t-il eu véritablement anesthésie; ou faut-il considérer l'action de l'électricité dans ces cas comme masquant simplement la douleur?" and further mentions that "Un soufflet, par exemple, donné à un malade au moment de l'ouverture d'un abcès, masquera la douleur du coup de bistouri, etc." His conclusion seems to have been that there was "véritablement anesthésie" rather than "anesthésie de diversion" or masking.

It would seem that confusion could best be avoided, particularly in auditory terminology, by using the term "masking" when appropriate, as descriptive of a common phenomenon and without any implications as to where the phenomenon is produced.

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A CENTRAL TREE SEED LABORATORY

THE movement to establish a central laboratory for research and testing of tree seeds and seeds of woody shrubs of all kinds is gaining impetus. The Botanical Society of America, Ecological Society of America and American Society of Plant Physiologists at their respective business meetings at St. Louis early in January, 1936, passed resolutions favoring the establishment of such a laboratory.

The need for such a central agency has been present for a long time, as pointed out by the writer in previous years. Ever since reforestation became an important activity of public agencies, and private planting increased in volume, it has become very important to know, for instance, the danger, if any, of movement of seed from different origins, and presumably different local races. The evidence is slowly accumulating that even indigenous American trees may have local adaptations, and that indiscriminate mixture of seeds of different origins not only endangers the success of plantations, but may even eventually deteriorate natural stands by cross pollination. In any case failure to start investigations into such matters 20 or 30 years ago is most unfortunate. Many aspects of seed origin investigations involve the seed itself; physiological and serological tests have been used with some success to identify seed of different origins. Since such problems are country-wide, not to say world-wide, and can be attacked and directed best from a central station, a laboratory devoted exclusively to seed studies is highly desirable.

Control of origin of seeds and certification of origin involves some sort of machinery for delimiting zones, inspection of collection, etc. Such organizations exist in many European countries, but at present no officially certified seed can be obtained in this country in spite of wide-spread foreign demand for such American seed. Legislation directed towards providing for control of forest seed origin is urgently needed. A central seed laboratory forms the logical nucleus around which a control service can be developed. Provision has been made for control of seed origin in a bill prepared for presentation to this session of Congress,[1] but none for its administration. Such a duty might properly fall to a central seed laboratory.

Shrubs and other forest plants of value as food for game have been little cultivated artificially, and almost nothing is known of the peculiarities of their seed. Recent use of such plants in wild-life sanctuaries and many rarely cultivated species in erosion control work and for shelterbelts has caused an urgent demand for information on their seed habits.

Routine testing of purity and viability of forest seeds would form an important function of such a central laboratory. Such service should be available at a regular fee to commercial seed dealers, so that the purchaser would have some guaranty of the quality of seed he purchases, and nurserymen and other consumers would have data by which to guide them in the amount of seed to use. Existing state seed laboratories handling agricultural seed have neither the equipment nor experience required for uniform results in tree seed-testing.

There are other problems, such as kiln-drying and extraction, which are peculiar to tree seeds and would naturally belong to the same organization handling seed-testing. At present each regional forest experiment station does some seed-testing, and most of them have some projects aimed at the importance of seed origin. These should be continued, but coordinated with a comprehensive national investigation. Testing can be standardized and improved only by a central agency.

[6] L. T. Troland, "The Principles of Psychophysiology," Van Nostrand, New York, 1930, pp. 397.

[1] National Forest Conservation Bill. Sec. 9, Amendment to Act of June 7, 1924—43 Stat. 653, Sec. 11.