

## SOULE'S CULTIVATION OF THE LEPROSY BACILLUS

Referring to the recent editorial on this subject<sup>1</sup> Dr. John H. Hanks<sup>2</sup> has entered a "contrary opinion." He tells of having heard, apparently before he came to Culion, of six unsuccessful attempts to repeat the work of Soule and McKinley. He apparently did not discuss the matter with Soule.

He tells of a peculiarity of the Huntoon medium in that it contains lipids which, when the tubes are incubated, tend to creep to the surface, where they combine with lipids liberated from an implanted tissue suspension, the two kinds of lipids coalescing in tiny droplets or pseudocolonies. When mycobacteria are present in the inoculum they, being lipophilic, are "swept into the lipid aggregations." He relates an experiment of his in which coded tubes, after incubation, were graded as positive or negative *by inspection*, and it turned out that the media to which 0.5% phenol or 1.0% formalin had been added had as many "positives" as did the unmodified medium. He says nothing of microscopic findings, or of subcultures.

Granting that all this is correct, it is not evident why it follows that the factor cited should be held to negate the work of Soule and McKinley. It is suggested that that factor might have fooled an amateur worker, but not a professional bacteriologist. Soule was under Professor Novy at the University of Michigan Medical School, later succeeding him in that position. And McKinley, too, was a professor of bacteriology. It seems quite possible that they both would have been aware of the lipid factor described by Hanks.

At any rate, about his Culion work Soule reported that, of 42 specimens worked with, only 25 produced cultures which he regarded as positive, which means that 17 of his specimens gave negative results. Furthermore, to meet an objection that had been raised, he controlled each of the inoculations by boiling before culturing a portion of the seed material used; the killed bacilli, he reported, disappeared entirely after a few transfers.

That Soule and McKinley were conservative in their claims is evident from the citations given in the editorial; yet informally (in correspondence with Lowe) McKinley expressed the opinion that their cultures were the true germ of leprosy. To deny them any success in this work implies, for one thing, that McKinley was wrong—to put it mildly—in his reports of taking some of the Puerto Rico cultures with him to Washington, where with Verder he attempted—unsuccessfully—to get better growths in a liquid medium containing minced chicken embryo. It also implies that Soule was equally wrong in saying that he took several of his Culion strains back to Ann Arbor with him to carry on further subcultures.

It is not clear just what the implication is regarding my own

<sup>1</sup>WADE, H. W. The cultivation of *M. leprae* by Malcolm Soule, THE JOURNAL **32** (1964) 201-204 (editorial).

<sup>2</sup>HANKS, J. H. Cultivation of *M. leprae* by Soule, THE JOURNAL **33** (1965) 104-105.

account of a personal follow-up with Soule in 1938. Legalistically speaking, it is of course only hearsay, based on Soule's statement to me, that of the two strains that he was carrying on at that time (others having been discarded), one represented the Puerto Rico cultures isolated 7 years before, and the other represented the Culion cultures isolated 5 years before, both after many subcultures. At any rate, my statement about the appearance of these cultures and about the observations on the smears made from them is factual. I remain of the opinion that McKinley was correct in the statement cited above.

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