

NIH Fact-Finding

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I had explained, demonstrated and mathematically investigated the physical cause of the slipperiness of the articular cartilage of joints by 1962 (McCutchen, 1962). (See “Background” in “Biphasic lubrication is weeping lubrication.”) But starting in 1973 further progress was blocked for two decades or more in the U. S. A. by two nonsensical lubrication theories from the group of Professor Van C. Mow, then at Rensselaer Polytechnic Institute and from 1986 at Columbia University. The first of the two, the mechanical pumping effect, was said to result when Torzilli and Mow (1976) corrected a grave but nonexistent error in M. A. Biot’s (1941) consolidation theory. They predicted enormous flows of fluid out of and back into cartilage as it was loaded and unloaded, flows that would have generated heat by viscous losses a hundred million times as fast as mechanical work was done on the cartilage (McCutchen, 1977).

Never withdrawn, mechanical pumping was replaced by the self-generating mechanism (Mow and Lai, 1980), which arose when Mow and colleagues “improved” Torzilli-Mow consolidation theory until it was again Biot theory, renaming the result the KLM model for their initials (Kuei, Lai and Mow, 1978). In imagination they rubbed joint cartilage with a “free-draining” indenter, an idealized tea strainer, and the theory, probably correctly, predicted flows somewhat like those in the mechanical pumping effect, though enormously smaller in magnitude. But because the indenter was free draining there was zero hydrostatic pressure at the rubbing surface, and thus no lubrication.

Also in 1980, their graduate student Adimora Uzowihe assumed that the indenter was impervious and found high pressure beneath it, as weeping lubrication predicts (McCutchen 1959). W. Michael Lai, Uzowihe’s supervisor, told me that this research existed, and that it was never submitted for publication. I do not remember any citation of Uzowihe’s thesis by Mow’s group.

Mow was still promoting the self-generating mechanism in 2004 (Mow and Huijskes).

Perhaps the first mention of mechanical pumping was by Mansour, Mow and Redler (1973). The porosity of cartilage entered their calculations explicitly, though it should only have entered via its effect on the flow permeability and stiffness of the solid.¹ Its explicit presence probably stemmed from confusion in their bookkeeping of the stresses in fluid and solid in a porous material, for which they cite Biot (1941) and Biot (1955), which use systems that differ from each other. Further warning of trouble was a prediction by Mow, Lai and Redler (1974) that the surface of working cartilage would develop ripples, which they thought they had found experimentally, a claim that rested on the misapprehension that cartilage is viscoelastic rather than poroelastic. See McCutchen (1975a). Such errors led me, as a reviewer for the National Science Foundation in 1975, to advise that support for theoretical work by the Mow group not be renewed unless Professor Mow were replaced as principal investigator.

After the mechanical pumping effect appeared in the *Journal of Biomechanics* (Torzilli and Mow, 1976), and its authors did not reply to my published comment debunking it (McCutchen, 1977), the only way to force a scientific debate seemed to be to warn Mow's Government paymasters that they were buying nonsense. Mow's confusions lay in physics, so I went first to NSF. There George Koo Lea gave me an advance copy of "Selected unresolved problems in synovial joint biomechanics," by Van C. Mow and W. Michael Lai, the Keynote Lecture that Mow was to deliver at the 1979 Biomechanics Symposium of the American Society of Mechanical Engineers, June 18-20, 1979.

"Let's you and him fight" (in 1950's vernacular), Dr. Lea seemed to be saying. As Mow's text denigrated weeping lubrication I asked the conference editor, Professor William C. Van Buskirk of Tulane University, for ten minutes to reply, and was given it. A week later Van Buskirk said he had been told he had done wrong, and withdrew my ten minutes.

Professor Mow's NSF support continued.

Next I tried my employer, the National Institutes of Health. In 1982, with the help of Joseph E. Rall, NIH Deputy Director for Intramural Studies, I squeezed an unwilling Stephen L.

¹Jan. 17, 2013. I might have cited McCutchen, C W. 1998. Consolidation theory derived without invoking porosity. *Int. J. Solids Structures*, 35, 69-81.

Gordon, the Program Director in the Arthritis Institute responsible for Mow, into commissioning an outside review of written criticisms I had prepared of the mechanical pumping effect. (See “The Mow-NIH Alliance.”) Reviewer Lyle Mokros agreed with me. Gordon’s telephone notes of the review, which I received only years later, read, “Agree technical part.”

Mokros said no misconduct was involved -- I had charged incompetence, not misconduct -- and he and Dr. Gordon agreed that Mow might do better in the future. Gordon reported this review in pussyfooting language in a memo to me, copy to Lawrence Shulman, Director of the Arthritis Institute, with advice that I “consider a positive rather than negative approach.”

Professor Mow’s NIH support continued. Those who disagreed with him about the consequences of the porosity of cartilage were defunded, also those, among them David Swann, who were studying the prime mystery remaining in joint lubrication; why did synovial fluid lubricate cartilage better than physiological saline?

I continued squeezing, which resulted in the memo, reproduced in “The Mow-NIH Alliance,” from Ileen Stewart, Executive Secretary of the Orthopedics and Musculoskeletal Diseases Study Section of NIH, to Dr. Shulman, our mutual boss.

The memo suggested that I publish my theory in peer-reviewed journals, which, as I had already published it, would have been misconduct. I did submit to the *Journal of Biomechanics* an article which included the derivation of Equation 1 in McCutchen, (1975b), a “diffusion plus side-leakage” equation for a load-carrying disk of cartilage. It was rejected.

My articles on joint lubrication were all rejected by biomechanics journals but I could sometimes publish comments. However when I explained that Van Mow had renamed Biot’s consolidation theory for himself and colleagues Editor Rik Huiskes of the *Journal of Biomechanics* took Mow’s advice and rejected my manuscript. Later, Bruce R. Simon, over objection, published the fact in a journal remote from biomechanics (Simon, 1992).

Having failed to get Ms. Stewart to learn about joint lubrication, I submitted two written complaints to Katherine L. Bick, Deputy Director for Extramural Studies, who relayed them to the misconduct office. I heard nothing for two years. A determined effort found that

they had been dropped a year earlier.

A further push, probably with the help of Dr. Rall, resulted in two panel investigations, one into Mow's plagiarizing Biot's theory, the other into NIH's role in his rise. Panel members totaled four NIH employees, one NIH grantee and one ex-NIH grantee.

Before the first investigation was fully under way Mow's dean at Columbia, Herbert Pardes, threatened NIH Acting Director William F. Raub, the investigation's executive secretary Craig K. Wallace and myself with suit,² and Raub greatly weakened the investigating procedure that he had proposed.

The panel lined up two to one against me. Executive Secretary Wallace then dragooned my supporter, an NIH employee, into making it unanimous that no misconduct had occurred. In his letter to me of January 23, 2009, Arthritis Institute Director Katz described this decision thus, "All members of his panel categorically agreed that there was no evidence of fraud or scientific misconduct by the investigator."

It came out later that opposing panel member Wilson C. Hayes had been co-editing a book with Mow while the panel met. Conflict of interest by a member of a Government-paid advisory panel is a crime with substantial penalties. NIH frightened Hayes into hiring a lawyer, but it then discovered that the Government had paid him via a "chairman's grant," which made everything all right. Robert Lanman, NIH's chief lawyer, determined that Hayes's conflict of interest had not influenced his judgment.

Four years earlier I had telephoned Hayes about Van Mow. He said, according to my telephone notes, that people knew that KLM was Biot but could not do anything about it. After the investigation, I asked Hayes about his decision. He tried to avoid talking about it and then said I was "unreasonable." "You mean I am wrong?" I asked. No, he answered, that was not what he had said, and he said again that I was unreasonable. He would not repeat directly to me the judgment he had made as a panel member.

The second panel found no evidence of favoritism by NIH for Professor Mow. In

² Wallace told me of the threat to Raub and himself. Next day he denied having told me.

response to a Freedom of Information Act request for something else, I received a memo that Tommy L. Broadwater, Chairman of the panel, had sent to the panel members. It included: “This table [of various scientists’ success at getting NIH grants] will not be part of the final report because of the ‘high potential’ for misinterpretation based on the numbers reflected in each category.” The table showed that Mow was remarkably well-rewarded. Also doing well were journal editors Wilson Hayes and Richard Brand.

Life went on. Co-authorship by co-authorship, and no discoveries that I know of, Mow moved smoothly upward into the stratosphere of honors and advisory positions. NIH did not change course. Indeed my nagging may have goaded it into flaunting its support of Professor Mow, further greasing the descent of joint lubrication research into a Devil’s comic opera of pomp, fear and sycophancy.

About 1995 nonsense propagation by the Mow group stopped, as far as I know, and recently, under Ateshian, the group has made what look like two real discoveries (Albaro, et al., 2008; Irene M. Balaso *et al.*, 2007). The mechanical pumping effect and the self-generating mechanism are now expunged (Ateshian, 2009) from its list of accomplishments,³ so why now re-stir the pot?

Because we should learn if the Mow-NIH alliance is typical of NIH granting. If so, research should be supported in some other way. Or was there was something special about the alliance that could be guarded against in the future? As the NIH people involved were retired or dead the time seemed ripe to find out.

Afterword

I posed a dilemma for mid-level NIH people. They depended on their salaries, and I, with a private income, wanted them to hazard their futures by bucking the bosses. Some did, but before you think ill of the rest remember how much I was asking.

Harder to excuse are the bosses who played politics with joint lubrication research, letting it sink to a level where Wilson Hayes found me “unreasonable” for wanting an honest

³ The mathematics of cartilage remains, even though Biot and I did it years earlier.

decision in the face of political pressure, and likewise bosses who now maintain that nothing strange happened in joint lubrication research.

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