

A Question of Balance

By Constance E. Putnam

“What ifs” are always perilous. But sometimes powerful lessons can be learned from the past.

A close examination of perhaps the most cataclysmic period in DMS’s history suggests that sometimes upheaval will temper rather than topple an institution.



DARTMOUTH
MEDICAL
SCHOOL

Founded 1797

DMS’s verdant campus gives no hint of the turmoil that beset it in the mid-1960s. It’s stronger today as a result, according to this excerpt from a new book.

On March 20, 1961, Dartmouth Provost John Masland sent a memo to the College's president, John Sloan Dickey. Its topic was a new "Medical Science Graduate Program." Masland shared with Dickey a resolution adopted by DMS's department chairmen two weeks earlier: "Be it resolved that graduate education leading to the Ph.D. degree within the Dartmouth Medical School should take form as interdisciplinary programs under the broadest categories of major faculty research interest and capability. These categories are: a) molecular biology, b) physiological mechanisms in health and disease." Though this program would have its roots in the Medical School, the resolution went on, it would involve collaboration with the College's science departments.

Two months later, DMS Dean Marsh Tenney, M.D., created a Molecular Biology Graduate Committee and asked Clinton Fuller, Ph.D., the School's new chairman of microbiology, to head it. The January 1962 issue of the *Dartmouth Alumni Magazine* made public the fact that DMS was "joining with the Science Division of the College to offer a new Ph.D. program in molecular biology, a step in the development of graduate education that was inherent in the growth of the School and the type of faculty and research now flourishing there."

Yet long before the pieces of the new program fell into place, tensions arose. Those surrounding molecular biology, in particular, color all memories of DMS during the 1960s. What was meant to be the crowning glory of the refounded school all too soon looked like gold turned to dross. A number of faculty (many of them researchers meant to be jewels in the institution's new crown) left DMS feeling sad or disappointed or angry or bitter. Or all of the above. Those who stayed shared a similarly wide range of emotions—including relief, triumph, vindication, and a deep sense of loss.

Some have characterized the core of the problem as whether a medical school should be in the business of training doctors or of educating scientists. Some, to be sure, were convinced that the next logical step for DMS—now that it had been successfully refounded—was a return to M.D. degree-granting status. Yet most did not see "doctors" and "scientists" as an either/or choice. Others saw the disagreement as evolving from differences between the faculty and the administration over the role that the faculty should play in governance. Still others believed that it stemmed from Dartmouth's historic ambivalence about whether it was a "small college" or a university.

Granting some ambiguity about when the difficulties began, one might point to the recruitment of Manuel Morales, Ph.D., in 1957 to head the biochemistry department. Critical to Tenney's vision of renewal was that DMS's science teaching be strengthened—yet perhaps not even he recognized how great the culture shock would be once the faculty contained a significant number of Ph.D.'s. Their focus differed from that of faculty who held M.D.'s, even those who were serious scientists (like Tenney, who was a noted pulmonary physiologist).

Certainly when Shinya Inoué, Ph.D., was hired in 1959 as the chairman of anatomy and cytology, signs of a mismatch might have

Medical historian Constance Putnam has written about many aspects of Dartmouth medical history for this magazine. She holds a Ph.D. from Tufts University and is the coauthor, with the late Oliver Hayward, M.D., of the definitive biography of DMS's founder. This feature is excerpted from her newest book, a 200-year history of the Medical School. Source citations are available upon request to the editor of DARTMOUTH MEDICINE.

Peering into the past

In 1961, Dartmouth Medical School was both the nation's fourth-oldest medical school and, in a way, one of its youngest. Founded in 1797, the institution was a leader in U.S. medical education through much of the 19th century. Its decline and near-disappearance during the first half of the 20th century had many causes, a lot of them accidents of time and place. Suffice it to say, as context for the adjacent story, that its M.D. program was reduced in 1914 to two years of preclinical training, the institution dwindled over the next few decades in size and impact, and it was put on "confidential probation" in early 1956 by the body that accredits U.S. medical schools.

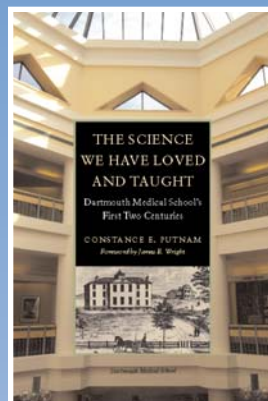
The Dartmouth Trustees were faced with deciding whether to close their medical school or to "refund" it. They chose the latter course, recruiting 1944 DMS alumnus Marsh Tenney, M.D., to revitalize and expand the faculty, facilities, and student body. He arrived in Hanover late in 1956.

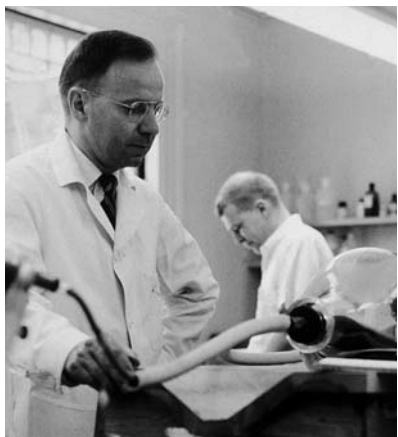
By the early 1960s, it was clear the Trustees had made the right choice. DMS was a growing, vibrant institution. The stigma of probation had been removed, new financing was falling into place, an ambitious building program was under way, the faculty had quadrupled, and the student body had doubled. Nevertheless, in almost all respects save its founding date, DMS was a young school—full of promise, but short on both fiscal and human resources. This was the stage on which arguably the most turbulent period in the School's history played out.

This saga is condensed from one chapter in a newly published history of DMS. The book, titled *The Science We Have Loved and Taught*, was commissioned in the mid-1990s as part of the celebration of DMS's bicentennial. Medical historian Constance Putnam has spent the better part of a decade digging into Dartmouth's archives, interviewing dozens of sources, and studying the fortunes of similar institutions. The history is a companion volume to a biography of DMS's founder, Nathan Smith—*Improve, Perfect, and Perpetuate*. The titles of both books come from Smith's 1806 valedictory charge at DMS.

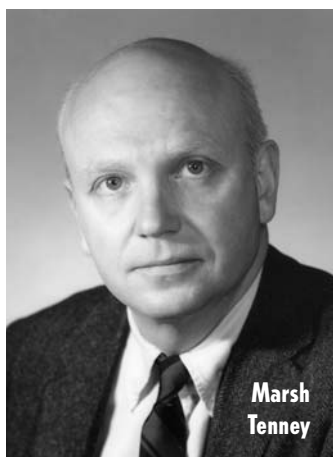
The portion of the history excerpted here offers a thorough, even-handed assessment of a period that has been fodder for national headlines and covert whispers alike—but that has never before received a dispassionate analysis. What is clear from the events of succeeding decades—as well as from Putnam's balanced treatment of a dispute that, at its heart, was about balance—is that often an institution comes out stronger for having weathered a tempest like the one at DMS in the 1960s.

The Science We Have Loved and Taught, a hardcover 384-page book, was published by University Press of New England. It can be ordered at any bookstore or purchased for \$35 plus shipping at www.upne.com or by calling 800/421-1561.





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been seen by anyone alert to them. A devoted researcher, he was drawn by the chance to join other up-and-coming scientific stars rather than to be on the faculty of a medical school.

Shortly after Fuller's 1961 appointment as chairman of microbiology, Morales was offered an endowed professorship elsewhere, an offer that Dartmouth was not yet in a position to match. The chairmanship of biochemistry fell to Lafayette Noda, Ph.D., already a member of the department. Three other biochemists (including Jean Botts, Ph.D., one of the early women on the faculty) left with Morales, and another three members of the department left the following year. However, those who remained made their own strong contributions. Furthermore, such turnover was not shocking; the 1960s were marked by the greatest academic mobility of the 20th century.

Anyone asked to describe the upheaval that followed resorts quickly to describing the personalities of those involved. What is striking is how differently the same person is described by different individuals. Tenney, for example, has been praised for his “scintillating” intelligence and for having “held things together.” Yet a “lack of confidence” in him has also been reported. And he is by no means alone in having been so variously described. Nor is the language used always temperate. One and the same person has been labeled an “ogre” and a “very likeable person.” Another was both “a sweet guy” and “absolutely hostile to all scientists.” A faculty member said by one

colleague to have “absolute integrity” was called “devious” by another. A “very wise man” was also termed “arrogant.” Words like “ineffective” and “inept” and phrases like “harsh, powerful, and proud of it” have been bandied about. One of the more measured comments was that “the controversies appear to have been blown out of proportion,” followed by the wistful remark, “If we hadn't been so young, if we had all been a little smarter . . .”

But they *were* smart. Yet most were also quite young and committed to their own visions of what DMS should be. They may not all have fully understood the route that Tenney was following. Like Dickey, Tenney wanted to build bridges between the College and the Medical School. In his 1959 “Second Progress Report,” he wrote that the “graduate program leading to a doctorate in the basic medical sciences is considered an important component in bringing the Medical School's educational responsibility to full maturity. . . . It is likely that a ‘cross-field’ program will emerge—‘molecular biology,’ for example—which will utilize the combined resources of the Medical School and liberal arts faculties of Dartmouth College.”

Sure enough, in his 1961 “Third Progress Report,” Tenney announced that the molecular biology program, Dartmouth's first Ph.D. program, would “be offered through a joint effort of all the basic science departments of the [Medical School] and certain departments of the Division of Science in the College.”

That year, Tenney also announced his intention to step down as dean; Gilbert Mudge, M.D., the chairman of pharmacology and associate dean at Johns Hopkins, was named as his successor. A few months before his arrival at Dartmouth, Mudge wrote to Allan Tisdale, M.D. (who had been recruited from Harvard earlier in 1961) to express his concern about the lack of clear definition of “clinical investigation” at Dartmouth. Mudge insisted he was trying “not to have too many preconceived notions” and understood that DMS faced “a large number of very closely interrelated problems, and it will be better in the long run to go slowly rather than in the wrong direction.”

Certainly Mudge's attempt to get on the same page with Tisdale was a good idea. Tenney had called Tisdale's appointment—by DMS, not by the Hospital or the Clinic—a “step of signal importance.” Tisdale's responsibilities, explained Tenney, included “the supervision of all clinical instruction in the [Medical School], not only in the first two years of medicine, but in the internship and residency programs as well. . . . The large and very active clinical group is a magnificent asset, but without someone who is full-time . . . it is difficult to

maintain essential continuity and perspective. Dr. Tisdale has brought this kind of leadership to our clinical programs.”

Tenney later reflected that during his own deanship, he dealt “with arguments over familiar subjects of concern in any faculty group: governance; tenure; finances; space. These matters grew in importance subsequently.” Soon after Mudge’s arrival, Tenney went on, “the School was divided over a number of contentious issues.”

Mudge’s first meetings with the faculty and the Advisory Board (made up of the DMS department chairmen and several key College administrators) did not go well. At least that is the impression one gets from notes Mudge made. Years later, Mudge said he had been well briefed by Tenney and was “fully aware of a divided point of view within the School.” Yet he seems to have been surprised and distressed at the extent to which rapid growth had produced a faculty lacking in collegial experience. Complicating the situation was the fact that Mudge wanted department chairmen to play a role in running the School, but to see the wisdom of his own views. Repeatedly, in memos to the file and to others, Mudge wrote of his desire that the chairmen “accept responsibility in a mature way.”

At an Advisory Board meeting in October 1962, he announced that “we did not want to set up separate research institutes here” nor “have a postgraduate faculty.” He also said “all individuals should be appointed for their ability to both teach and perform research.” It is easy to see how some of those in the exciting, innovative “cross-field” molecular biology program might have taken umbrage.

The handwritten notes on which Mudge based his remarks at a November 19 special meeting of the Advisory Board indicate that he was by then aware of the seriousness of the situation. He knew for sure after receiving a detailed response the next day. In a nine-page memo, one department chairman responded item by item to 14 points Mudge had made. Occasional conciliatory remarks—“I cannot agree with you more” or “that is certainly true”—were for the most part followed by “but” clauses. What the dean presumably still did not know is whether the memo reflected the views only of its author or of the entire “dissenting minority.”

Looking back, it is difficult to tell how much the increasing tensions were caused by Mudge’s authoritarian management style. The difficulties he faced may equally possibly have been caused by a combination of poor information and communication or by bad luck and bad timing. A further blow came in a post-Christmas letter to Mudge from Clinton Fuller, explaining that he felt compelled to resign as chairman of the Molecular Biology Graduate Committee.

The letter is an important one, for it shows what a key faculty member believed was the focal point of the differences. The issue, as Fuller saw it, was a fundamental distinction between how Mudge thought DMS should be organized and how many faculty thought the new graduate program should be organized. “I am sorry,” Fuller wrote, “that graduate education was the victim of faculty organization problems in the Medical School—but by the very nature of your ideas of the organization and operation of a classical medical school and my ideas on the organization and operation of a hopefully special kind of graduate program, this clash was inevitable.” The crux of the matter, according to Fuller, was Mudge’s conviction that the Medical School had to have a “highly departmentalized structure with permanent, strong department chairmen,” whereas the cross-field



John Sloan Dickey

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molecular biology program was by its very definition a “non-departmental and non-school oriented program.”

Six days later, Mudge distributed a document on “The Organization and Responsibilities of the Faculty” and asked for responses in writing. Fuller, Inoué, and Noda all replied, independently asserting that alterations in the dean’s proposal were essential. Fuller, for example, pleaded, “I really feel you will have your faculty behind you if you can . . . show them you are willing to let them participate in the overall operation of this school directly.”

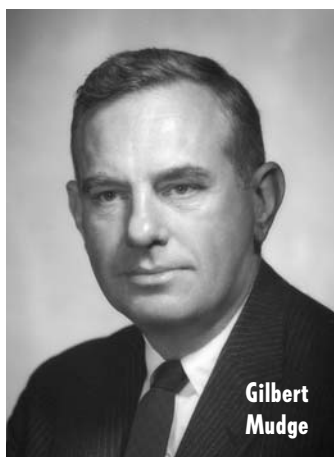
The extent to which the distress of the three chairmen was shared by members of their departments is unclear. Tenney later treated them as one. “The dissident group,” he said, “were members of the Departments of Biochemistry, Cytology, and Microbiology. They had all come to the Dartmouth Medical faculty from positions in university or institute departments. None had had a medical school experience. Therefore, it was natural that their orientation was different in many regards from that which is traditional in a medical faculty. Faculties in the liberal arts have a greater concern for academic government as a participatory democracy; medical faculties are more inclined to accept a good deal of authoritarian management.” This may explain why Mudge—with his medical school experience—came into conflict with faculty members used to self-governance.

Bit by bit, the three central departments of the “exciting” cross-field program were being viewed by the administration—starting with Mudge but by no means ending with him—as problematic. Or, to be more precise, the three chairmen were seen as difficult. Mudge came to see Inoué and Noda especially as contrarians and “dissidents.”

President Dickey perceived the disagreements between Mudge and the three chairmen as serious. Anticipating that leadership changes might be needed, Dickey—in a startlingly candid memo to Mudge—expressed the hope that Fuller could be “saved” as a chairman even if



Less than two months later, in a formal letter to the dean (“Dear Dean Mudge” was its salutation), Dickey reported that “neither the ad hoc committee nor any other individual or group has been able to suggest a comprehensive course of action which is wholly wise and feasible.”



Inoué and Noda could not. (The unpleasant possibility exists that some anti-Asian prejudice was at play here. But the views of those who speculate, or even insist, that such prejudice existed are offset by those of at least as many observers, even some of those possibly affected, that nothing of the sort was true.) Among the reasons was that this would make the Advisory Board a more cohesive unit, building a “growing sense of solidarity for the future in their collective action with the dean.” There was clearly more than a little anxiety over a possible unraveling of all the good that had been achieved at DMS in the past few years.

On April 26, 1963, Dickey sent a memo to “The Dean and Faculty of the Dartmouth Medical School.” He intended, he wrote, to ask the Trustees Planning Committee (TPC) to form a subcommittee, “made up mainly of Medical School faculty members,” to “undertake a thorough review of the professional aims, principles, and policies governing all teaching and research activities of the School.” And, Dickey added, in “a parallel but independent undertaking, I propose to appoint an ad hoc committee to examine the organizational structure and the current administrative problems of the School.” The TPC unanimously approved the creation of a subcommittee on May 10, 1963.

Establishing a committee is one thing; achieving the desired result is another. Less than two months later, in a formal letter to the dean (“Dear Dean Mudge” was its salutation), Dickey reported that

“neither the ad hoc committee nor any other individual or group has been able to suggest a comprehensive course of action which is wholly wise and feasible in all eyes.” The next day, he sent a personal letter (“Dear Bert”) to Mudge’s home, saying, “I think the most basic thing for you and me to be clear about is that we are at a point in the affairs of the Medical School where the deanship must be able to contribute positively to the unity of both the School and the larger biomedical community.” Dickey left it to Mudge to decide whether he could keep the deanship from being “a prisoner of either that [unhappy] past or of a present that gives any indication of being a repetition of that past.” Mudge stayed on as dean.

Roughly a year later, a new but related issue emerged. A proposal to establish clinical departments in medicine and in surgery under the Medical School’s aegis, when coupled with requests from the molecular biologists for more space and funding, highlighted the differing priorities among members of the faculty. In March 1964, a document about the new departments was prepared for the Advisory Board, and Shinya Inoué circulated it in the cytology department. One reaction is preserved in an unsigned memo back to Inoué, with 21 pointed and sometimes sarcastic questions about the implications of the proposal. An undercurrent of anxiety over who had championed the proposal makes it evident that disaffection and distrust were by no means a thing of the past.

Then in late March, a memo from Inoué to Leonard Rieser (in the latter’s capacity as director of graduate study) made the case that Dartmouth should apply for an experimental “Centers of Excellence” grant from the National Science Foundation (NSF). A response from Mudge (who had been sent a copy) shows cautious support for the idea in principle but questions where the necessary additional funds would come from.

Mudge’s “Report on the Dartmouth Medical School to the Trustees of Dartmouth College,” covering the first 18 months of his deanship, came out in April 1964. References to the molecular biology Ph.D. program (then in its second year) were matter-of-fact. He more than once noted “the conviction that research and education were opportunities of equal importance.”

On April 6, 1964, Mudge sent Dickey a proposal concerning the new Departments of Medicine and Surgery, which had been unanimously endorsed by the Advisory Board. The next step was Trustee approval. As Mudge said in a letter to Dickey five months later, establishing these departments was “tangible evidence that the Hitchcock (either Clinic or Hospital) and the School

will in the future be collaborating towards a common goal”—something that could not be taken for granted in earlier years.

Mudge then turned his attention to Inoué’s proposal about the NSF grant. In a long memo to Rieser, the dean spelled out why DMS should not support graduate education as such, since it had been agreed that was the College’s responsibility. It followed, Mudge said, that requiring greater commitments of time, money, and personnel from DMS for the molecular biology program was not advisable. “The Medical School has medical education as its mission,” he concluded, not “graduate education (Ph.D.).” He did not explicitly oppose having molecular biology be a “Center of Excellence,” but he laid a foundation for not supporting an application to the NSF.

Three weeks later, Fuller, Inoué, Noda, and John Copenhaver, Ph.D. (of the College biology department), submitted to Rieser a proposal for the NSF application. Their cover memo included a statement obviously intended to show their support for other DMS initiatives: “Concurrent with the strengthening of clinical medicine, the improvement of the medical curriculum, and research efforts, the time has then come when graduate education in the basic science departments needs to be placed on a sounder financial basis and to be provided with necessary space and facilities to fulfill this aspect of our academic mission in medicine and science.”

They were not to get their wish. Dartmouth concluded it was too risky to get involved in a program that would require the College to pick up the tab after the initial funding ran out. Tenney’s summary of the decision was that although the proposal had “considerable merit,” its “future financing was a problem. . . . The School was also dubious about fitting an institute into its structure, and eventually a question of ‘balance’ among the programs of the School was raised.”

The rejection of the NSF overture was not the only blow the molecularists received. Around the same time, the National Institutes of Health (NIH) established a new interdisciplinary program in molecular and cellular biology. The Dartmouth group jumped at the opportunity to submit one of the first grant proposals. To their delight, the funding they sought was approved by the NIH, but not for the full amount. A revised budget was requested. Once again, the College administration balked at the requirement that it assume financial responsibility after the five-year grant ran out; this made submitting a revised application a waste of time.

Despite these setbacks, the success of the new program was evident when the first Ph.D. in molecular biology was awarded in June 1964. And the College clearly had not decided to altogether block new initiatives in graduate education, for a second Ph.D. program—in physiology and pharmacology—was soon announced. But this only intensified the molecularists’ frustrations.

Things came to a head early in March 1965. Mudge became convinced that opposition from Inoué and Noda was standing in the way of progress on governance. He demanded that they resign their department chairmanships. Mudge then met with Fuller and asked him to stay on as chairman of microbiology. By his own account, Fuller was astonished and outraged. He insisted that he, too, would resign his chairmanship.

The same day, Mudge wrote Dickey “to report on policy matters.” Far more revealing, however, are personal notes Mudge made in the days following the resignations as he met with some faculty members



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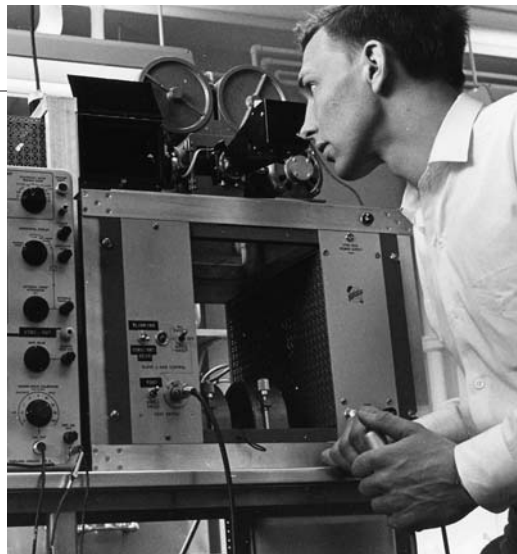
and tried without success to meet with others. What emerges from these notes is that virtually everyone on the faculty seems to have been distrusted at one time or another by at least one other person.

For nearly two weeks, Mudge searched for solid ground. None came into view. In personal letters, he stressed how “infinitely complex” the “academic squabble” had become. Finally, he announced his resignation as dean effective September 1, 1965. He elected to stay on at DMS as a member of the faculty—testimony to his loyalty to the School—and later was named chair of medicine. He must, however, have been monumentally disappointed.

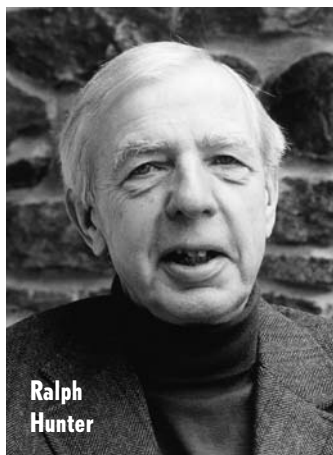
On May 15, 1965, the long-awaited report of the TPC subcommittee was submitted. A cover letter explained that the subcommittee had “attempted to evaluate objectively the needs of the Medical School now and in the near future without restriction.” The report, two years in the making, was a model of careful work. The main section opened with a statement on “The Principle of Balance”; this turned out to be the sticking point and, eventually, the breaking point.

It was “apparent that trouble lay ahead” early in the subcommittee’s work, recollected Tenney—who was a member of the group, along with biochemist Peter von Hippel, Ph.D., biophysicist Andrew Szent-Györgyi, M.D., and others. The first item on the subcommittee’s agenda, Tenney continued, “was introduced in complete innocence: it was a simple statement that the primary purpose of the Medical School was to educate medical students. This provoked an immediate objection by von Hippel and Szent-Györgyi that the word ‘primary’ was unacceptable, that graduate education and research were of equal importance.”

In August 1964, Tenney wrote to Dickey to express his concern over the continuing difference of opinion on the matter: “I think the most important aspect of the problem derives from our still unsatisfactory incorporation of graduate education into the concept and operation of a basic science medical school; and now, even



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more significantly, into the biomedical center.”

Yet the minutes of a September 1964 Advisory Board meeting state that “it was nevertheless generally agreed that the primary mission of this medical school is medical education” and that “whereas the primary mission of DMS and each of its departments is medical education, the specific role of individual faculty members may be variable.” But agreement on these points was not universal, as “added comments” from von Hippel and Szent-Györgyi on the subcommittee’s report made clear. While stressing the importance of a medical school having medical students and “active research programs, graduate students, postdoctoral fellows, etc.,” they registered “philosophical objection” to labeling one part of the DMS mission as “primary.”

Tenney, however, saw no inconsistency in identifying a “primary purpose” while insisting that faculty engage in both research and teaching. A decade earlier, Tenney had written to Donald Morrison, then the provost, as follows: “Why can’t a faculty be created which is interested in teaching and research? Many of the qualities of a good research worker are just those things that make him a stimulating as well as a critical teacher. Conversely, investigation can easily wither in the absence of teaching—the plague of research institutes. I am fundamentally opposed to faculty appointments of the either/or choice—teacher or researcher. The superior faculty will contain qualities of each in the individual, though it will rarely be 50/50 or whatever balance is deemed ‘best.’”

Despite the ferment generated by the subcommittee report, remarks by John Masland to the Medical School faculty on May 21 focused almost entirely on the need to find a new dean and new (acting) chairmen for biochemistry and anatomy-cytology. Nothing was said about the report. The vacancy in microbiology had been filled by Clarke Gray, Ph.D. His willingness to step into the chairmanship disturbed his colleagues, however, since up to that point the faculty in the three departments had remained loyal to the resigned chairmen by refusing to replace them. Gray’s acceptance may have made Masland’s evident frustration at the stance taken by members of the other two departments all the greater.

Many of those reminiscing some three decades later about the tensions of the time say there was fear among those not in the molecular biology program that it was growing disproportionately to other departments. One of the “dissidents” who stayed admitted that they paid little attention to the rest of the Medical School and didn’t realize how ambitious their plans looked to others. Another said “the department grew out of any proportion to its teaching responsibilities.” Yet another said it was viewed by some “as the tail of avant-garde research wagging the dog of medical tradition.”

In a memo to Dickey five days after he met with the DMS faculty, Masland reported that the issues of “primary purpose” and “balance” had come up again. He said of the dissidents, “Basically, they kept coming back to a central theme, to the effect that the School has been ‘stacked’ against the molecularists. Thus they once more argued that some kind of reorganization at this time was called for.”

Mudge thought Masland was making matters worse and noted that the provost had “attempted a greater degree of reorganization than ever previously proposed by anyone . . . and this without consultation with the dean, either present or dean to be appointed.” Mudge’s anger led him to prepare “A Chronology of recent events relating to Dartmouth Medical School, with a few comments” (it is not clear to whom—if anyone—he sent or intended to send this memo). In it, he attacked the provost, saying his action “has been arrived at in such a manner as to virtually guarantee failure.”

Mudge’s remarks at an Advisory Board meeting a week later were even stronger. He referred to “an ‘academic Junta’” and said, “It would be imbecilic to assert that tranquility is the order of the day.” Some sensible observations got thoroughly buried under his rhetorical excesses. “The affairs of the School are now in a state which can not become worse, for indeed if they do worsen there will in fact no longer be a Dartmouth Medical School,”

he intoned. He would be proved wrong the next April, when things did get worse, but without the School collapsing.

A calming influence came with the appointment of Ralph Hunter, M.D., as acting dean, beginning September 1, 1965. A 1925 DMS graduate, he had been on the faculty since 1939. No one could doubt his loyalty, and he knew the ins and outs of academic medicine. As one of the “dissidents” later remarked, Hunter stabilized the place with “enlightened leadership.” And in October 1965, the Trustees passed three resolutions based on the TPC subcommittee report, having to do with “Purpose,” “Future Expansion,” and the “Possibility for a Tutorial M.D. Program.”

When Hunter decided that four months of leadership were enough, Marsh Tenney was asked to step into the breach, beginning January 1, 1966. Thus it was he who was dean at the time of the dramatic dénouement to the tensions in April 1966. The drama had two dimensions—one very public (and widely recounted, if in fragmentary fashion) and one mostly private (and poorly understood). The public part was triggered by media coverage in April 1966 of “mass resignations” of faculty from the molecular biology group; in fact, the resignations took place over a period of many months. The private part began with a December 16, 1965, letter to the American Association of University Professors (AAUP) from 14 members of the DMS Departments of Biochemistry, Cytology, and Microbiology. (At least some of them decided not to wait to see how events would turn out. In January, Inoué and two other cytologists announced their departure for the University of Pennsylvania.)

Letters flew back and forth among faculty members and from various faculty members to the AAUP, demanding to know the nature of the complaint to the AAUP. Charges and countercharges were leveled. Mudge was especially upset. The request for AAUP “assistance” seemed to some like a gauntlet thrown.

Fortunately, there was a voice of reason at the AAUP. Staff Associate Philip Denefeld’s responses to the sometimes “demanding” letters were models of patient explanation. “First,” he informed the DMS faculty, “no formal ‘investigation’ of Dartmouth Medical School has been requested, and none is presently contemplated.” Only after studying the situation and deciding whether “the allegations are accurate and reflect serious departures from our recommended standards,” would advice be offered. (This is a critical point. Having the AAUP look into a matter is by no means the same as a formal investigation.) Denefeld then dealt firmly with requests that he reveal who had said what to the AAUP. “I do believe,” he wrote, “that our record of a half century warrants the assurance that we do not abuse information submitted to us.”

But on April 8, 1966, both *The Dartmouth* and the local *Valley News* made internal affairs suddenly very public: “11 Professors Quit Posts in Med School Controversy” was the headline in *The Dartmouth*, and “11 Med School Profs Resign: Doctors Quit In A Hassle Over Complex Policies At Dartmouth,” said the *Valley News*. Other papers soon picked up the story. The *Boston Globe* put it on page one on April 9 (“11 Quit Dartmouth Med Staff”), and the *New Hampshire Sunday News* carried it on April 10 (“Claims Dartmouth Resignations ‘Timed’”). Predictably, no two versions of the story completely agreed. The number of faculty members resigning, their reasons, and what it all signified were variously reported. A *New York Times* story on April



The drama had two dimensions—one very public (and widely recounted, if in fragmentary fashion) and one mostly private (and poorly understood). The public part was triggered by media coverage of “mass resignations.”

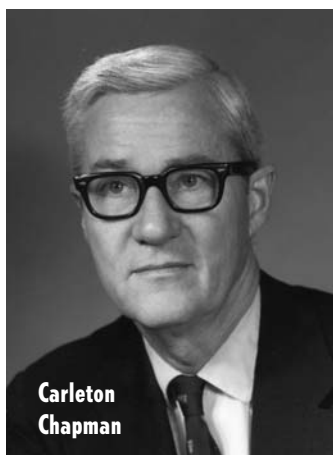
15 (buried on page 41) contained perhaps the best concise statement of the views of those who left: Szent-Györgyi was reported to have said, “Dartmouth ‘has done the impossible’ by recruiting a strong faculty and rebuilding a medical school ‘but it has failed to do the possible—keep it together.’”

The mystery was who had leaked the story to the press. Mudge was among those who held Inoué responsible. In a long letter to Masland, Mudge mentioned his own attempts to “keep the Medical School situation out of the press” compared to “Professor Inoué’s epidemic of press conferences.” (Yet at least with respect to the *Sunday News* story, in which it was claimed the news was “timed” to coincide with a national scientific meeting, Inoué and eight others signed an affidavit denying responsibility.) Mudge’s main concern was to assist Masland in the damage control he assumed would be necessary. “It would be absurd to deny that the nationwide publicity has been harmful to the School,” he wrote. While saying his letter was “not intended for the public press,” Mudge sent copies to several administrators at Dartmouth, the president of the DMS Alumni Association, the president of the Association of American Medical Colleges, and the members of DMS’s Policy Group (a largely external advisory body). Several of the latter replied. Barry Wood, M.D., of Johns Hopkins was “sure that the long-range reputation of the Medical School will not be injured by the flurry of public complaints.” Francis Dieuaide, M.D., of Columbia wrote, “I can’t believe the recent publicity and the departure of your late colleagues will really injure the School.”

In early May, Philip Denefeld informed Dickey that the AAUP had received communications from “a number of” DMS faculty seeking “the advice and assistance of this association.” Dickey responded a few days later, thanking Denefeld for the letter but saying that he didn’t think they needed help, although they would consider the possibility. Despite that brush-off, by June it was clear that Denefeld’s calming influence was still needed. Mudge wrote him again,



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**Carleton
Chapman**

once more agitating for “clarification of the status of Dr. Inoué’s charges.” Denenfeld told Mudge that the allegations were part of an “open file” at the AAUP (in other words, their seriousness was still being considered), but that he could not reveal their substance. Mudge, having failed to get what he wanted from the AAUP, wrote directly to Inoué and Noda in June, urging them to make public the particulars of the complaints. It does not appear that either responded to him.

Meanwhile, on a happier note (though with a reference to DMS as a “scene of unrest”), the *New York Times* announced on May 19, 1966, that Carleton Chapman, M.D., a professor at the University of Texas’s Southwestern Medical School and a former president of the American Heart Association, would take over as dean in the fall. Chapman, said the article, was “well known for his concern over problems of medical education and research.” Several members of the Policy Group expressed relief and pleasure at his appointment.

Yet the appeal to the AAUP continued to rankle. In late July, cytologist Kenneth Cooper, Ph.D. (who had taken over as spokesperson for the dissidents after Inoué’s departure), Noda, and Fuller wrote another letter to Denenfeld. They noted that “44 [Dartmouth] faculty members submitted a petition to your office appealing for an investigation by Committee T” (a step taken only in cases of major unresolved issues regarding academic governance) and begged him to take action; Dartmouth’s refusal to accept AAUP

mediation, they said, proved how serious the intra-institutional politics had become.

Numbers played an important role in the issue. The signatories to the July AAUP letter wanted to demonstrate that full-time members of the faculty were underrepresented in leadership posts. Citing the 1965-66 DMS catalogue, Cooper, Noda, and Fuller said the full-time faculty—those “who have invested their total activity in teaching and allied scholarly pursuits in the Medical School’s academic program, administrative officers included”—numbered 52. By this count, the 14 resignations to that date represented 27 percent of the faculty. By contrast, Tenney and Associate Dean Philip Nice, M.D., told the media that DMS had an “instructional staff” of 135, of which 14 is obviously a much smaller percentage. But that number included doctors at the Hitchcock Clinic who had been granted faculty status as part of the effort to create a true medical center. At least some basic scientists believed these appointments had been made by administrative fiat, to ensure that clinical faculty would outnumber research faculty.

On August 19, 1966, Masland met Denenfeld in Washington, D.C., for a discussion that lasted more than four hours. Four days later, Denenfeld wrote a seven-page confidential memo to the Committee T file and sent a copy to Masland. The provost wrote back immediately, indicating that he did not think Denenfeld had accurately represented every aspect of their conversation, though he acknowledged that “in general you have recorded the principal elements of our discussion.”

In October, Cooper went to Washington and met with Denenfeld. Again, the AAUP officer wrote a long memo to the file and sent a copy to Cooper. Like Masland before him, Cooper expressed gratitude for the opportunity to pursue the matter and responded to several of Denenfeld’s points. In particular, he contested the recurrent canard (as he saw it) that those who had left did not understand medical schools and were going to university rather than medical school posts. He noted that “8 of 16 are continuing in medical education,” naming the individuals and the institutions to which they were moving. “I don’t believe it can seriously be held that we came here with unrealistic views of medical education,” he wrote. “We came . . . because of ideals, the pleasure of participation in a new venture in medical education.”

When Chapman arrived in September 1966, Marsh Tenney once again relinquished the deanship. A few weeks later, Chapman detailed for the DMS faculty his hopes and dreams for the School. He spoke of the “singular advantages of Dartmouth” and expressed his belief that DMS was “uniquely

equipped to bring the more academic aspects of education for medicine in line with the emerging needs of the last quarter of the 20th century.” He did not dwell on the tensions except “to conclude by reminding you that the gaze of the academic world has been attracted to this school and that our academic colleagues over the country consider us on trial in a very real way. . . . The job [of medical education] requires talent, it requires courage, and, above all, generosity. We have them all here, and it’s time now to get moving again.”

On December 14, 1966, Philip Denenfeld wrote a memorandum on the Dartmouth situation to the AAUP staff, prior to a meeting to determine “whether this case should be referred to Committee T with a recommendation for formal investigation.” He apologized for the “unusually large” briefing, but his four-point summary was masterful.

In January, Denenfeld wrote Chapman to say that the AAUP had decided to table the case in order to give the new dean a chance to try resolving matters. Denenfeld expressed a desire to meet with Chapman as soon as possible, “since we do not feel we should delay much longer in determining our position.” Chapman agreed that a meeting in the near future was a good idea.

The view that morale improved with Chapman’s arrival is confirmed by a mid-February letter written by Kenneth Cooper (and also signed by Lafayette Noda) to Denenfeld. Cooper expressed controlled enthusiasm but was full of optimism. Chapman had acted with “graciousness . . . that kindles cooperative desire” and “could not . . . have improved his handling of [the first real faculty] meeting,” Cooper wrote. “What is especially important is that our dean, for the first time in my eight years here, has asked the faculty to discuss and to decide an issue very important to it. . . . The prospect of such faculty action has more of the staff talking to one another, and in reasonably good spirits, about their school’s future.”

Denenfeld called this letter “a joyous document” (even though Cooper ended it by insisting that “investigation by Committee T is still greatly to be desired”). Further correspondence between the two during March made it clear that the AAUP would continue to monitor the situation at DMS but had no intention of doing anything that, as Denenfeld put it, “might well jeopardize some promising beginnings.” In May, Cooper and Noda had a long phone conversation with Denenfeld, during which Cooper repeatedly used the word “surveillance” to describe the AAUP’s role with respect to DMS. Denenfeld acknowledged that the word was technically accurate, but said Chapman had objected to it. He suggested giving way on the point to avoid roiling the waters with the new dean.

Finally, the upheaval came to an end—at least as far as the AAUP’s involvement was concerned—not with a bang, but with something closer to a whimper. In a letter to Chapman, Denenfeld effectively signed off on the case. “In your relatively brief tenure as dean,” Denenfeld observed, “you have made remarkable progress away from a grave situation and have achieved a sound basis on which to build cooperatively.” Chapman responded a week later, saying he thought the AAUP had been “very fair indeed.” Denenfeld had engaged in a lot of hand-holding, but DMS was never formally investigated, censured, or put on probation.

When all is said and done, it is hard to know whether the sharp reactions by those who believed legitimate paths were being blocked were necessary or appropriate. The 1961 memo from Masland to Dick-



Philip Nice

When all is said and done, it is hard to know whether the sharp reactions were necessary or appropriate. The 1961 memo from Masland to Dickey about a molecular biology graduate program had been bold but made sense.

ey naming molecular biology as the first area in which DMS would establish a graduate program had been bold but made sense; molecular biology was preeminent as one of the “broadest categories” (as Masland’s memo had it) of scientific disciplines. That notwithstanding, most nonmolecular biologists had no understanding of the extent to which 1950s molecularists “looked down upon the traditional biochemists.” And “the latter were smugly ignorant of the powerful forces the devotees of the new approach were mobilizing to change the way biological problems would be thought about and tackled in the coming decades.”

Nothing in Masland’s memo suggested that the “exciting” cross-field program would be the centerpiece of what has been variously described as a “brouhaha,” a “crisis,” a “rocket that blew,” a “horrible business,” a “long and tangled story,” “an unfortunate incident,” “a nasty academic imbroglio,” and—simply—“anarchy.” But others have insisted that the situation was not as dire as it has often been cast. One erstwhile DMS faculty member—who would have played a central role in the molecular biology program had he not left—visited friends at Dartmouth during the “revolution” and came to the conclusion that there actually were no issues, “just slogans, such as ‘medicine vs. biology,’ in a struggle for resources.” Others have said the whole thing was “dumb” or “silly.”

But the debates and tensions at Dartmouth during this period were different from comparable ones elsewhere. They loomed particularly large at DMS for three reasons.

First, the excitement generated by the refounding was enormous. Students and faculty alike were eager to be recruited to DMS, and the future looked very bright. But the resulting optimism may have meant that disappointments were felt especially keenly.

Second, DMS’s parent institution was not a university but a liberal arts college. This meant that discussions about “primary purpose”

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