

Hand Surgery's Research Dilemma: A Lesson From Philip Hench

Editor's note: Traditionally, the Journal of Hand Surgery publishes the address given by the President of the American Society for Surgery of the Hand, which is presented at the annual meeting. For those not attending the meeting and for those interested in a historical record of concerns and aspirations surrounding hand surgery, these addresses prove valuable. Richard Gelberman was President from 2006 to 2007. His address did not get published at the time, but perhaps now it has even greater resonance with the added perspective of the ensuing years.

—RM.

It is truly an honor to have served as President of this Society for the past year. I owe the privilege to the many outstanding leaders who preceded me—those who helped make this organization what it is. I owe it to those who were influential in my career—to the teachers, scientists, and physicians who inspired me and prepared me to contribute to the specialty of hand surgery. I owe it, also, to my colleagues at Washington University, who have taught me and supported me—in particular, Drs. Manske, Boyer, and Goldfarb—and to my wife, Sarah, who has supported and encouraged me for the past 25 years. The ultimate reward is the opportunity to stand before you today at this, the 62nd meeting of this Society.

One of the duties that one takes on as President is the perusal of prior presidential addresses. I enjoyed reading them. Some focused on clinical issues, others on education, on research, on history, and on leadership and collegiality. I did notice a common feature of prior addresses—an almost uniform lack of brevity. I'm going to try to do better by following Franklin Delano Roosevelt's advice when giving a speech of this sort: "Be grateful, be brief, and be seated."

The focus of this annual meeting is the center of a target at which we've taken dead aim over the past year: the future of hand surgery, providing the scientific evidence for patient care. The theme came from a series of planning sessions, the goals of which were to identify the major challenges confronting us and to develop options for dealing with them. From our research we discovered a major fault-line—a very significant problem. We have fallen behind other specialty organizations in one of the most critical areas of importance to our success: the carrying out and the reporting of relevant scientific research.

My premise this afternoon is that, while there has been an increase in the number of presentations and of articles, in print and online, too much of the information provided in recent years has been unsubstantiated. Too many of the abstracts and presentations at this meeting have been preliminary reports—reports that have lacked the detail necessary to determine if the conclusions were valid. In some cases, it has taken years to discover that the recommendations made were not beneficial to patients, even at short- or midterm follow-up.

In his book *Blink*, Malcolm Gladwell discusses how experienced people can often make better decisions by relying more on their initial reactions to ideas than by overanalyzing them. Perhaps this is true. However, I think it

is dangerous to rely too much on one's "informed gut" when it applies to the evidence we need to practice medicine; and in my opinion, we've had more than our share of hand surgery research "blinks" in recent years. There are two potential consequences of acting too much on early impressions and on preliminary information. One is called first pass bias, which is the tendency to become anchored to the conclusions one forms on initial exposure to an idea. This impairs objectivity and prevents the research process from running its course. The second effect is that a great idea may be dismissed. Some of our best ideas have come from challenging ourselves to go beyond our "informed gut" impressions. Often, on digging deeper, we arrive at more profound conclusions, ones that may reverse our initial impressions.

If some of our data have been preliminary, lacking rigorous support, other data suffer from not having any real clinical relevance. Many of our podium presentations, symposia, and publications in recent years have focused largely on operative procedures—on how to use implants and on the short-term results of their use in selected patients. Little time has been devoted to truly novel research ideas, to the natural history of injury or disease, to objective comparisons of longer-term results, of complications, or of the costs associated with treatment.

There is another challenge. For a combination of cultural and socioeconomic reasons, there has been a decline in the number of serious hand surgeon scientists and, as a result, a reduction in the number of those fundamental discoveries required to substantially improve care. The downstream effects are substantial in terms of the quality of our annual meeting, the relevance of our journal, and the appropriateness of the treatment that we provide.

We're not alone. There has been a steady drop in the number of physician scientists in the surgical disciplines for the past two decades. As research has become more molecular, research techniques have moved farther and farther away from the skills that we typically maintain. At the same time, hand surgery clinical care has become more complex. If increasing complexity represents the natural evolution of both basic science and clinical medicine, and if, for the clinician, these developments are magnified by the burdens of increasing clinical demands and decreasing reimbursement, a very real dilemma results for the hand surgeon scientist.

These challenges are not new. Bear with me for a moment while I try to put this in perspective by describing one particularly relevant example of how an advance in medicine took place in another time.

It was the early 1930s, and a young physician by the name of Philip Hench made the clinical observation that several of his patients with rheumatoid arthritis experienced a dramatic improvement in symptoms when they were either pregnant or were jaundiced. Hench considered that a common anti-inflammatory factor could have provided the symptomatic improvement, and called it "anti-rheumatic factor x." To test his hypothesis, he began administering bile orally (not too attractive a thought) and bilirubin or blood from pregnant or jaundiced patients, intravenously. Clearly, this was done before the existence of the institutional review board. But none of these efforts worked. Then, he considered that the common factor for the two conditions, pregnancy and jaundice, might be an elevated cholesterol level, most likely from the adrenal gland. To test this idea, Hench began treating patients with an extract of bovine adrenal glands. This, too, was unsuccessful.

In the late 1930s, Hench met the chemist Edward Kendall. Kendall had synthesized some substances produced by the adrenal cortex. Working together, the two investigators gave one of the mixtures, which Kendall called cortin, to several rheumatoid arthritis patients. Once again, there were no positive effects.

To make a long story short, it cost Hench and Kendall 10 years and 150 tons of beef adrenal glands to purify a small group of closely related steroids. In the end, the two were successful in producing just 9 g of a new substance—enough to treat 1 rheumatoid arthritis patient for 9 days. This time, the result was remarkable and the immobilized patient rose from bed and walked. They called the new substance cortisone.

Hench and Kendall presented their findings at a distinguished and very important scientific meeting like this one in 1949. They received an unprecedented standing ovation and, the next year, were awarded the Nobel Prize in Physiology or Medicine.

The point to this story is that it could be told over and over again in medicine: significant discoveries made, for the most part, because of the observations of a talented clinician and because of two human qualities: passion and persistence—the willingness to stick to it despite failure. In addition, success in the long run so often has been dependent on the close collaboration of a clinician and a basic scientist, with both bringing something to the table.

If translational research of the kind done by Hench and Kendall is difficult and time-consuming, rigorous clinical research is just as hard. In 2007, a prospective, randomized trial requires training—and data managers, nurse clinicians, biostatisticians, and a great deal of money. The average trial costs over \$250,000.

Have there been any organizations that have been more innovative and more successful than us in supporting research? Based on our recent findings, the answer is yes. There are some good examples where surgical specialty societies, through their commitment and creativity, have made significant progress. For example, one society similar to ours committed to improving its financial support of young investigators by increasing its research fund to \$10 million by the year 2010—and they're already approaching that goal. Our research fund has been growing slowly. At the beginning of this fiscal year, the American Foundation for Surgery of the Hand had just \$3 million—and was still giving out \$15,000 to \$20,000 grants. This is well below the level required to initiate meaningful research in 2007.

Another organization, a sports medicine society, has assisted in the development of a multicenter orthopedic outcomes network, the goal of which was to better understand the results of knee reconstruction. Under the leadership of a talented and persistent clinician scientist, they've already completed 2 prospective, randomized trials involving 10 medical centers. In one early large patient series, they achieved an over 90% patient return rate with multi-year follow-up. As a result, they were able to demonstrate that meniscal repair, carried out at the time of anterior cruciate ligament reconstruction, was highly beneficial to patients, and 96% successful. Further, they found that patients with ligament reconstruction were just as likely to tear the opposite anterior cruciate ligament at 2 years as to rupture their grafts. This is remarkable new knowledge, and it would not have been

discoverable for many years, if at all, had more conventional clinical research methods been used.

Does the American Society for Surgery of the Hand (ASSH) have the resources, the talent, and the commitment to be successful in promoting research at this level? I know that it does. Our reputation has been one of outstanding accomplishment in research and education. We worked hard to earn that reputation—we'll work even harder to maintain it.

I'd like to spend my last few minutes describing some early steps we've taken to accomplish two things: first, to begin to improve our research productivity; and second, to improve the ways we report our findings. The purpose is not so much to tell you what we're doing, but to show how we're trying to get our house in order so that you'll consider joining us in the next phase of our development.

Early last year, we charged a task force to evaluate our current research program and to make recommendations for improvement. One of our members led a task force that was charged with conducting a 10-year audit of basic science and clinical research grants, studied a large body of data and then presented a series of immediate actions designed to reconstruct the ASSH research enterprise. Those actions were to dramatically increase our financial support of research and to leverage a modest financial investment in the training of future scientists by partnering with the National Institutes of Health and the Robert Wood Johnson Foundation. The plan, developed with National Institute of Arthritis and Musculoskeletal and Skin Diseases Director Jim Panagis, himself a hand surgeon, could train several talented young hand surgeon scientists over just a few years—an outcome that could lead to many years of research productivity.

In addition, we planned to create a multicenter outcomes network under the direction of a new Clinical Research Committee, similar to the one that was created by the American Orthopaedic Society for Sports Medicine, and to restructure the Research Planning Committee to improve grant scoring criteria based on principal investigator credentials, on innovation, and on research design—to make certain that we get the highest return on our investment.

Finally, our goal was to improve accountability by performing scheduled audits of ASSH research grants through detailed updates and through conference calls and face-to-face meetings with investigators.

Council enthusiastically approved these proposals, as well as a series of equally innovative longer-term measures recommended by the task force.

While the research task force was at work, other groups were charged to look for ways to improve the methods that we use to disseminate high-quality data through presentations at this meeting and through publications in our Journal.

The program committee was asked to reassess the abstract submission process for this meeting and to explore ways to present more relevant science. The committee studied other organizations' annual meeting programs and implemented changes to the abstract submission process and the scientific program with expanded podium and poster sessions, along with highly selective review symposia.

Another task force was asked to carry out a complex evaluation: a critical look at virtually all aspects of the *Journal of Hand Surgery*. The Council work group literally took apart the *Journal* and reconstructed it. Their recommendations, which were based on current state of the art publishing

guidelines, were to improve the *Journal's* functionality, including its appearance and paper quality, to streamline and improve the submission and review process, to increase financial stability, and to enhance oversight. The effects, which include a striking new brand, *JHS*, and a bold new look, will become apparent to you over the next 12 to 18 months.

A subgroup of the journal task force carried out a focused assessment of the review component of the Journal. The group examined the best models they could find, including the extremely successful market leader, *Pediatrics in Review*, which has been refining the approach to scientific review for over 25 years. Then, after some market research and considerable thought, it recommended a design that, in my opinion, constitutes the review journal of the future. The result will be a publication that effectively achieves the goal of helping our members regularly update their knowledge and, in the process, successfully fulfill the newly devised maintenance of certification requirements. This project, which will take 18 months to unfold, will provide a group of succinct, highly focused columns entitled “In Brief” and “Index of Suspicion,” and a comprehensive series of review articles based on a 3- to 5-year rolling curriculum written by leading hand surgeons. In addition, there are plans for a new online continuing medical education component.

To make certain that our self-assessment exam was on the same level as our annual meeting and our journal—that it provided questions and answers based on the best available evidence, the self-assessment examination committee began a 2- to 3-year reconstruction process. Among other actions, the group reintroduced face-to-face meetings of question writers in the presence of a psychometrician as they undertook the rigorous process of producing a valid examination. If you took the exam this year, you’ve already seen some of the benefits of that effort.

In another initiative, several Council members spent months re-engineering the governance structure of this society so that we could better manage these new initiatives and others to come. The result, accepted by Council in December and in May, is a unique streamlined model designed to empower the most energetic members of this society. The plan called for the elimination of some positions and the addition of others, in creating a structure that enhances communication and improves responsiveness. To give you the flavor of the governing body, consider the new research division. This new entity has the authority and responsibility to make the decisions necessary for us to reach our goals. Through its newly integrated Research Management committee, the highest quality grants and the most talented investigators will be supported, irrespective of focus, basic or clinical. And through a new Clinical Trials committee, the organization of multicenter clinical trials will be facilitated.

A newly devised Committee Advisory Group, composed of key division and committee chairs, was charged with assisting the President-elect in finding the best talent for these committees—for getting the right people, in the right seats, on the bus. It was considered a positive addition this year, enabling far more informed decisions in the process of member selection.

As for our scientific journal of the future, it will be managed by a new Journal Advisory Group, composed of members of—and reporting directly to—Council. This method of oversight provides more direct communication between the editors and leadership, a model highly recommended by a

highly experienced journal task force consultant, Lance Primis, former President and Chief Operating Officer of *The New York Times*.

Finally, I'd like to thank the many talented members of Council whom you see here, and my partner in crime over the past year, Steve Glickel. Steve is thoughtful and hard-working—a terrific colleague. The Society will be in good hands next year.

From these early efforts, we've learned something about our Society: We've learned what we cannot do, and what we must do, in going forward.

What we cannot do is to retreat from our agenda to be one of the country's foremost surgical societies. What we cannot do is to retreat from our plan to develop a much improved research base—to help young surgeon scientists reinvigorate our specialty. And what we must not do is to retreat from our commitment to the next generation of hand surgeons who choose to join with us in a common vision for research and clinical excellence.

The American Society for Surgery of the Hand has started down a path. The direction we've taken is based on the concept that our future is inextricably linked to a broader commitment to advancing the science and practice of hand surgery.

What we must do from this point on is to rise to meet the challenges that confront us. In doing so, we will realize the goals to which each of us in this room is committed, and because of our commitment and our laser-like focus, we will become one of the most accomplished and effective organizations for research and education in the world.

I thank you for the enormous privilege of having served you as the 62nd President of the American Society for Surgery of the Hand, and I pledge to do all I can to assist future leaders in the achievement of our collective vision.

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