A Historical Perspective of Anterior Knee Pain

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Summary: Anterior knee pain, like back pain, is a common complaint in modern developed countries. It has attracted and held significant interest among investigators for generations. Yet we might ask ourselves, “Are we truly making progress in explaining anterior knee pain?” Are our discussions constructively seeking consensus or distilling observations into testable hypotheses that can be scrutinized scientifically? It can be argued that in large measure, the causes of anterior knee pain are as elusive today as they have ever been. The author examines some lessons from medical history to illustrate how strikingly similar issues in anterior knee pain are to other challenges medical scientists have faced and overcome. These recurring historical themes offer guides that are useful for approaching any arena of medical inquiry, not just knee pain. Key Words: History of medicine—Patella—Knee pain—Malalignment—Chondromalacia.

Why put a history paper in a scientific journal? Because our perception of the world around us is influenced in a very real sense by where we stand. Our predecessors delivered orthopedic science to its current state, and our teachers have indoctrinated us with certain assumptions, which we often accept as established fact. They have set us upon a path, and the path we take from here is determined to a large degree by how we have been taught to view disease and injury. By looking back and viewing how we came to our current circumstances, we can improve our judgment when it comes to deciding how we should proceed into the future and perhaps whether we should retrace a portion of our track. This is a fundamental principle of orienteering, or finding one’s way in the wilderness, and it is useful in science for that very same purpose.

In orthopedic surgery, as in other fields of medicine, we grope our way toward the future. But looking back in 20 years’ time, many of today’s students and residents will look at us as though we knew where we stood, and what we were doing, just as today many of us look back on the influential thinkers of the middle and late 20th century. It is important for us to realize that they were groping as well, that the ascendancy of one idea over another may have owed more to the charisma of its champion than to the strength of the evidence, and that even the best theoretical systems should be challenged continually by close and meticulous observation by skilled clinicians.

This article is intended to provide a historical framework, or perspective, from which to view contemporary theories of anterior knee pain. It is not meant as a review of one or another theoretical system for explaining knee pain, such as chondromalacia patellae, malalignment or excessive lateral patellar pressure syndrome, for several good reviews already are available to the interested reader. In addition, no attempt is made to justify one doctrine over the others because, in truth, each of them has its merits. The danger of applying one or another theory to a particular patient lies not so much with the doctrine as with its injudicious application, which can result in complex and sometimes dangerous surgical techniques being used where there is little chance of success. Thus, dogma and experience are central themes of this article.

MODERN DOCTRINES OF ‘PATELLOFEMORAL’ PAIN

It is hard to say when it first dawned on surgeons that there might be some value in distinguishing pain arising from the patellofemoral articulation or knee extensor mechanism from that arising from other knee pathology.
Certainly there would not be much point in such distinctions before the introduction of sterile surgery by Joseph Lister in 1867 because there was little of consequence that could be done for either malady before that time. William Hey had coined the phrase “internal derangement” in 1803, to characterize intermittent symptoms arising from the tibiofemoral joint caused by ligament laxity or “some slight derangement of the semilunar cartilages.” However, he did not strictly limit use of the term to specific etiologies, so “internal derangement” became more closely associated with symptoms of giving way and locking than with any specific knee pathology. The implication that the symptoms were attributable to internal knee pathology amounted to an indication to operate, but the vagueness of the term meant to do so without a diagnosis in many cases. This led to much unnecessary surgery. Consequently, by the end of the 19th century the term “internal derangement” had attracted criticism for its association with ill-conceived surgery and poor treatment outcomes.6

The time had come to delineate more specific causes of knee pain to develop better diagnostic tests and more specific methods of treatment. In 1908 Büdinger6 called attention to traumatic cartilage lesions of the patella, reporting 15 patients who had relief of pain after excision of the offending patellar cartilage lesion. Axhausen7 noted that the symptoms in patients with traumatic patellar cartilage lesions, such as those reported by Büdinger, often were similar to those caused by meniscal pathology.

These reports kindled increased interest in the study of patellofemoral pathology. Their efforts were facilitated by the availability of Roentgen’s new x-rays,8 which extended the clinician’s realm of observation. But knowledge still was limited, and few clinical or radiographic techniques were available for the purpose of making a specific diagnosis in cases of knee pain.

Initially, patellar cartilage lesions were referred to merely as “chondropathy.”9,10 But eventually “chondromalacia patellae” became the term used most commonly to describe patellar cartilage softening or fissuring.11,12 Although Büdinger’s and Axhausen’s reports were based on traumatic cartilage injury, Aleman11 and König12 noted chondromalacia in a variety of circumstances, most of which had no traumatic antecedent. In fact it was such a common operative finding that surgeons soon began to presume chondromalacia of the patella was the cause of anterior knee pain in the absence of trauma. Eventually the term chondromalacia became synonymous with anterior knee pain of insidious onset. Note the emergence of a recurring pattern: this was the second time in the context of knee pain that common usage had drifted toward overbroad interpretation of a term originally intended to describe a specific lesion with a specific etiology. The same misuse for which Büdinger had criticized “internal derangement” now had been committed for “chondromalacia patellae.” During the middle and late 20th century, it became more and more clear that cartilage softening was neither necessary nor sufficient to cause pain in the anterior compartment of the knee, but many ill-advised operations continued to be done for a diagnosis of chondromalacia. However, the tide was turning toward a more restricted and specific use of the term, limited to describing only the pathologic lesion, and without implications for any symptoms that might be reported.1,13,14

Now at least the patellofemoral joint was firmly established as a specific source of knee pain. The periarticular tissues still were not recognized as potential sources of pain, but it probably did not matter: clinicians lacked accurate and reliable noninvasive techniques for studying the pathology underlying that pain, so it is unlikely they could have distinguished one etiology from another anyway. To better understand the patellofemoral articulation, a great deal of literature was published on methods for roentgenographic analysis of the patellofemoral joint, which has been reviewed extensively by Minkoff and Fein.15 As the single noninvasive diagnostic modality available for the objective study of knee pain, x-ray data defined how the joint was perceived throughout much of this century. As a result, a bewildering variety of joint configurations was described during the mid-20th century,15–19 with little consideration of the potential contribution of bursal, synovial, capsular, ligamentous, or tendinous pathology to the development of knee pain.20,21

In France, Maquet18 and Ficat et al.22 began studying the role of joint and intracapsular pressures in the production of pain when limb components were aligned abnormally. Ficat et al.22 established that an arthrogram could demonstrate patellar cartilage defects. They noted a pattern in which the cartilage in the inferolateral patella wore excessively and devised a surgical treatment in which they realigned the extensor mechanism to reduce the pressure on that specific area of patellar cartilage. In other words, Ficat et al. began to address not the immediate causes but the original causes of the pathology. Among those who subscribed to this approach, the frequency with which patients with anterior knee pain exhibited evidence of patellofemoral dysplasia or malalignment was thought to represent an indication for surgical realignment as part of any treatment for anterior knee pain. This was a significant departure from previous operative strategy in dealing with patellofemoral pain, and
this departure established treatment doctrine as it is most widely practiced today.

However, today the doctrine of malalignment has begun to draw criticism for many of the same reasons chondromalacia and internal derangement fell from favor. For example, it is just as true of malalignment as it is of cartilage softening that it is neither necessary nor sufficient to cause knee pain in and of itself. In addition, despite the absence of measurable changes in a patient’s alignment, symptoms often come and go. The argument has been proposed that if the immediate cause of the pain could be treated, realignment surgery and the attendant risks could be avoided. Unfortunately, the immediate causes of many episodes of knee pain remain obscure, whether or not malalignment can be documented, and ominously, some are of the opinion that malalignment can exist in the absence of any measurable abnormality. “Malalignment” thus shows signs of becoming yet another synonym for anterior knee pain, with obvious implications for inappropriate surgery.

The perspective of most literature on anterior knee pain and indeed the view of most surgeons remains focused on the bony configuration and positioning of the patella as the primary cause of anterior knee pain. But some investigators have begun to look beyond the patella. Dye et al. mapped the sensory input from individual intraarticular knee joint components, noting essentially no input from the patellar articular surface in comparison to exquisite sensation in several locations within the peripatellar soft tissues. Fulkerson noted retinacular tenderness in patients with “patellofemoral pain” (sic), including some who demonstrated histologic changes within the perineural tissues of the lateral retinaculum on examination of surgical biopsy specimens. These findings raised the question of whether pain in maligned knees is caused by excessive joint pressures or excessive tension in the retinaculum. Recently, Biedert et al. have studied pain receptors in the periarticular tissues of the knee using modern histologic techniques. This approach to anterior knee pain offers much promise, particularly if clinical diagnostic measures can keep pace with technological developments in anatomic pathology. Modern diagnostic radiologic techniques have made it possible to view the articular cartilage with greater accuracy, so it is increasingly plausible to study local soft tissue conditions about the knee extensor mechanism.

There is much to be learned from this evolutionary process that can guide us into the next century. It is ironic that, despite much progress in our understanding of anterior knee pain, there appears to be a tendency to overinterpret data based on a limited numbers of cases. As a result, all the predominant doctrines on anterior knee pain eventually have led unwary surgeons into trouble. We need to recognize the limitations of theoretical constructs in explaining knee pain at this time because they are without exception based on a fairly restricted, not to say dated, perspective of pathology. Our limited ability to observe changes that contribute immediately to pain continues to be a major obstacle to a better understanding of knee pain by careful examination of cause and effect. With the limited resources at our disposal, we should be skeptical of doctrine and maintain a high level of alertness when evaluating these patients. Better methods of observation make for more direct experience and less theorizing, which is a desirable thing when data are limited, as they are in the case of anterior knee pain. The remainder of this article is devoted to parallel lessons from the history of medicine that can offer additional perspective on this topic.

THE ORIGIN OF DOGMATISM IN MEDICINE

It began innocently enough. As the Greek philosophers undertook to systematize and codify what they observed of the world around them, it was only natural that they should develop “schools of thought,” each founded on its own basic assumptions and working toward its own understanding of the universe. In this way, Aristotle laid the groundwork for what would later become scientific inquiry. In remote Croton, Pythagorus created a universal system to explain both physical and metaphysical phenomena. And specialty schools such as those at Cos and Cnidus, and later at Alexandria, began to codify what was known of diseases and their remedies. Dogmatists, as they came to be known, subscribed to the teachings of one or another of the medical scholars, the most prominent of whom was Hippocrates.

But it is Galen’s name that has become most closely attached to the stigma of dogmatism. He was a charismatic, ambitious Greek physician and an extremely prolific writer who built his reputation by successfully treating the gladiators of Pergamum and Rome. His writings survived the destruction of Rome and formed the basis of medical practice for nearly 2000 years. Galen is one of the most controversial figures in all of medical history, but there is good evidence that he recognized the limitations of his studies and warned against unmitigated acceptance of some of his ideas. He was distressed by the unavailability of human tissue for study, perhaps recognizing the potential for inaccuracies in his anatomic theories. He admonished his readers to follow his example with “zealous research” into natural phenomena, as opposed to blindly following his recommendations.
From the start, there were prominent skeptics. The early Empiricists opposed the Dogmatists on the grounds that too much of nature was unfathomable. Empiricism (literally “by experience”) in medicine first appeared as an outgrowth of philosophical skepticism, which was a broader philosophical movement. But over time it was continually fueled by practical experience, which exposed theoretical flaws. In contrast to the scholars learning and studying at the medical schools, the empiricists relied on their own experiences or those of their colleagues and immediate teachers. Unfortunately, because they avoided systematization, Empiricists left few writings; yet there were many great physicians of antiquity who practiced empirically. As long as neither the Empiricists nor the Dogmatists gained the upper hand, the two approaches probably could have continued to provide foils for each other and to facilitate the growth of medical knowledge. But it was not to be.

Roman civilization disintegrated under continual pressure from Barbarian hordes, and Western Europe turned inward. As the last remaining institution capable of maintaining some level of cultural integration, the Roman Church became the repository and perforce the proprietor of all higher knowledge. Among the leading scholars, the intellectual curiosity that had characterized the Greek thinkers was replaced by monastic faith and obedience. Consequently, by the end of the first millennium, Galenic doctrine had achieved unassailable status among physicians: it was recited virtually as catechism among the monks who practiced the healing arts. True, there seems to have remained a measure of empiricism among lower classes, including the surgeons and folk healers, and eventually folk remedies would provide much of the fuel to drive Paracelsus’ iconoclastic campaign against Galen’s doctrines. But generally there was little enough contact between scholars and common folk that there was no chance of debate or discussion.

In the East and South the Byzantines and later the Arabs continued to develop the sciences, so that by the time the medical school at Salerno re-kindled secular medical study in Western Europe, the works of Avicenna and Rhazes had joined those of Hippocrates, Galen, and Aristotle as worthy medical literature of the classical age. Following on the heels of the school at Salerno, the new medical faculties at the sprouting universities showed interest in the great Indian and Chinese physicians. The faculties sought to merge the intellectual capital of different cultures—a good idea and a promising start. But after a thousand years of blind obedience, old habits were hard to shake: would-be innovators paid a heavy cost for daring to question classical authority. Despite the addition of Arabic and Oriental knowledge, the study of medicine remained dogmatic, if eclectic. Its method remained dialectic, rather than experimental. As the woodcut in Figure 1 illustrates, the professor rarely touched the cadaver, but read from the ancient texts while dissections, if they accompanied the lecture at all, were done by surgeons standing below the lectern.

On one hand, the reinvigorated intellectual activity of the late Middle Ages had breathed new life into the study of medicine. It had resuscitated a keen and focused curiosity about the true origins of the wisdom being uncovered. Despite many abortive attempts, accurate, high quality translations of the original masterworks eventually were produced. However, the discourse at Salerno and later in the universities merely orbited the doctrines of the classical thinkers. The medieval universities made it their business not only to promulgate classical doctrine, but “to guard it against infraction.” This was a mistake.

![FIG. 1. Mondino lecturing. Woodcut from the title page of Anathomia. From Pachter.](image)
It may seem obvious in retrospect that the factual knowledge Greeks had of human anatomy and physiology constituted a poor foundation for the edifice medieval scholars built upon it. But who is at fault for such a construction? Pachter points out that Aristotle was neither as worthy as the scholastics thought, nor as worthless as Paracelsus’ diatribes would suggest.33 We have ample evidence that both Aristotle and Galen wished in vain for an opportunity to study human anatomy and physiology; they were aware of at least some of the potential for error. Certainly then, they cannot be blamed for the overbuilt edifice that bore their names; they can be guilty of nothing more than communicating their observations and organizing their thoughts. For his part, Hippocrates repeatedly stressed the importance of continuing research in understanding and treating disease:

So I made more or less hypothetical theories of glycogenesis; after mine came others; my theories, like other men’s, will live the allotted life of necessarily very partial and temporary theories at the opening of a new series of investigations; they will be replaced later by others, embodying a more advanced stage of the question, and so on. Theories are like a stairway; by climbing, science widens its horizon more and more, because theories embody and necessarily include proportionately more facts as they advance. Progress is achieved by exchanging our theories for new ones which go further than the old, until we find one based on a larger number of facts.35

It is worth asking why medieval scholars stressed the doctrines of the dead masters but neglected to consider their methods of observation. For in so doing, they failed to expose the gaping deficiencies in the theoretical systems. Perhaps the answer lies simply in that the task was overwhelming. Simply expressing all these lines of thought adequately was an enormous undertaking that occupied generations of scholars and produced numerous failed attempts to communicate their original meaning. Whatever the cause, the approach carved a schism that stultified progress for many centuries. By disallowing any perceived challenge to classical authority, they effectively separated theory from worldly experience. Without skepticism or practical experience to ground them, the scholars’ interpretation of classical doctrine lost any value it may have had. This was not a problem among the surgeons and folk healers, for they had little use for theory when it was easy enough to see the immediate consequences of straightforward treatments. But once men like Paracelsus, Vesalius, and Bacon began trying to reconcile their own observations with what they had been taught, they found there was little evidence to support the traditional view.

**THE RENAISSANCE OF INQUIRY**

The historian Henry Sigerist has attributed the preeminent status of contemporary Western medicine to the scientific developments of the Renaissance. Before that, European, Indian, Chinese, and Middle Eastern medical thought followed roughly similar lines, and all methods of studying disease were philosophical. The result of Renaissance thinking was no less than a fundamental revolution in man’s understanding of, and his study of, disease.33 This understanding was to undergo additional evolution from an organic to a cellular level over succeeding generations, but the dynamism of the Renaissance opened for medicine the path that allowed continual renewal of its theoretical systems.

The great surge of Western thought that occurred in the wake of the Renaissance carried on its crest a slew of...
related fields, including medicine, which would be changed forever. It is tempting in retrospect to think of these developments as inevitable, but were they? A long and difficult struggle, lasting several centuries, took place before the scholastic approach to medical study was discredited once and for all. Indeed, laboratory study and hands-on training, the very essence of modern medical and surgical training, was not commonplace in American medical schools until a century ago.

What was the key change brought about by the Renaissance, and how did it give so great an impulse to Western medicine? The key lay in the emergence of a new, descriptive approach to scientific study. Figure 2 is an illustration from Vesalius’ masterwork of descriptive anatomy. Notice the contrast between Figures 1 and 2: Vesalius is lecturing as he dissects, whereas earlier scholars rarely touched cadavers. This simple difference is responsible for the revolution that followed. Vesalius demonstrated the immense power of descriptive anatomy. As a result of this new observational approach, anatomists ceased to ponder the meaning of organs on philosophical grounds and moved on to describing their form in great detail. The descriptive anatomists of 16th century Europe grounded medical study in the study of organs, both diseased and normal, and ignited the spark of modern pathology. Most importantly, they experienced the shape, color, texture, and minute details of the organs for themselves, gaining insight and raising doubts about the accuracy of classical teachings. No one could claim to be an “expert” without having first-hand knowledge of his or her subject: that is to say, without being “experienced” with the organs themselves.

By the time William Harvey arrived in Padua to study anatomy, a distinguished tradition of descriptive anatomy had been established. Vesalius’ successors at Padua, Bologna, Rome, and Naples were continuing to solve many riddles that had hung in the air for 1500 years under Galen’s doctrines and raised many new questions. By 1600, the new approach of descriptive anatomy had largely supplanted the speculative approach that had descended almost directly from Aristotle’s time. The heart and vessels received much attention, but their function and the nature of the movements of the blood remained somewhat obscure. Colombo had described the lesser circulation. Harvey’s teacher at Padua, Fabricius, had described the valves of the veins. All that remained was for a single simple experiment to finally prove the nature and significance of the circulation of the blood. The simple elegance of Harvey’s approach was the critical step in closing the circle.

After examining the beating hearts of hundreds of animals, Harvey established that during systole, or contraction of the heart, the blood is driven out of the heart and into the arteries. He measured the capacity of an adult human heart at approximately 2 fluid ounces. He then calculated the volume of blood pumped by both sides of the heart to equal 72 x 60 x 2 = 8640 ounces per hour, or roughly three times the average weight of the adult human body. Clearly, it was impossible that this volume of blood could be produced hour after hour. He employed Fabricius’ experiments on the peripheral veins to show then that the veins return the blood to the heart. In this way, he proved that the blood circulates.

Harvey’s contribution has been disputed by some authors because much of his discovery had been anticipated. Indeed, Fabricius had discovered most of the particulars used by Harvey to demonstrate the circulation of the blood. However, others defend Harvey because it was he alone who formulated the problem, and the solution, once and for all. Indeed, as Sigerist pointed out, “The right phrasing of the question is already a great step
on the way towards its answer.\textsuperscript{30} Before Harvey, all observations were qualitative. His revolutionary innovation was to measure cardiac output to answer the question of the movements of blood.

But there is another dimension to his simple experiment with calculated volumes that brings him into the current discussion. Indeed, what is most significant about Harvey’s studies is what he did not attempt. There are many examples of experiments done by his contemporaries on the question of the movements of the blood. But the questions they asked were too broad and failed to address the salient point that would finally overturn Galen’s doctrine. Harvey knew nothing of and had no way to observe the exchange of blood in the capillaries, for example. Thus, he did not address the microscopic anatomy of the circulation. Harvey’s work on the circulation begins with a question, provides a calculation of cardiac output, and ends with a simple demonstration that arteries carry the blood away from the heart and the veins return it. In this simple experiment is the embodiment of Thoreau’s words: “What is once done well is done forever.” Many of his contemporaries and too many of us today tackle broad issues far too complex to be clearly answered by a single experiment. We have much to learn.

What Vesalius had done for anatomy, Harvey now had done for physiology. Both men followed similar lines: for Harvey, as for Vesalius, the current lines of thinking about the movements of the blood were unsatisfactory for explaining observations that were inconsistent with experience. They set out to discover more plausible explanations for what they observed. In this they exceeded Paré’s ambitions, for their methods of study exposed too many flaws in traditional doctrine for them not to confront the discrepancies. But they stopped short of constructing a second, competing theoretical system, as Paracelsus had done. Their investigations shattered Galenic doctrine, but did so by direct demonstration, rather than by logical argument. This approach created a virtual explosion of new discoveries and greatly accelerated man’s growing comprehension of natural phenomena.

\section*{SCIENCE IN PRACTICE}

It is hard to overstate how confusing these changes were for the average physician.\textsuperscript{4} Scholastic dogma was being overwhelmed by anatomic and pathologic data, a great paradigm shift had occurred, yet no system had appeared that allowed for the incorporation of the new experimentalism into everyday medical practice. According to Osler, the key to experience lies in “seeing wisely”\textsuperscript{36}, but without a framework for evaluating a patient’s symptoms and the physical manifestations of disease, practitioners had no means of synthesizing what they saw in the context of contemporary scientific evidence. Think about it for a moment: despite all the emerging scientific knowledge, medical practice still followed rituals that were laid down by Hippocrates. It is not surprising then that physicians were bewildered. Even the most prominent faculties clung to the old system, occasionally with absurd consequences. It would take another Hippocrates to devise a new method of practice. How fitting that the man who emerged, Thomas Sydenham, was a practitioner, rather than a scientist or scholar.

In a rather radical departure from tradition, Sydenham took an ontological approach to the study of disease, in which malady was an entity in its own right. Whereas Hippocrates had studied the natural history of sick persons, Sydenham studied the natural history of diseases. He established that the physician’s first task in evaluating a patient was to establish the diagnosis; in other words, to determine what disease the patient had. He inquired as to the symptoms and the course of the illness, then proceeded in gathering additional data on the physical manifestations with which to test his emerging hypothesis. Only after establishing a diagnosis did he consider treatment or prognosis.

Using Sydenham’s methods of careful observation, the medical scientists of succeeding generations proceeded to dismantle the classical systems of medical theory. The application of the descriptive method to medical practice gave rise not only to important discoveries, but also to inventions. Auenbrugger’s percussion technique, Laennec’s stethoscope, the Jannsens’ microscope, Wunderlich’s thermometer, and Roentgen’s rays all enhanced physicians’ senses as they strove more and more to observe disease processes within the living human body. The process continues today: arthroscopy, magnetic resonance imaging, and ligament arthrometry are just a few techniques that have greatly influenced the way we understand pathology of the knee. It was Sydenham who established the clinical method in which all these instruments become useful for determining the particular cause of disease, and as long as clinical methods can keep pace, more or less, with contemporary concepts of disease, theory and experience can strengthen each other, and medical progress can continue unabated.

\section*{IDEAS AND OBSERVATION}

Of the many great works one might read for historical interest or to marvel at their timeless wisdom, there is only one that should occupy the shelf of every investi-
In 1865, Claude Bernard published a book that remained in print for more than 60 years. An *Introduction to the Study of Experimental Medicine* is no longer being printed, but remains widely available today. In the introduction to the first English printing, in 1927, L.J. Henderson wrote that in the book, Bernard “laid bare, so far as that is possible, what others had concealed.” But those who read this book will understand that rather than conceal, few men of his day appreciated, and even fewer could express, the complex and difficult issues so beautifully laid out in this classic book. Bernard goes beyond Aristotle, beyond Bacon, in a fascinating dissection of human intelligence applied to the study of natural processes. Drawing on a lifetime of prodigious research in which he had helped to lay the foundations of modern physiology, Bernard used the book to examine the essence of medical research, expose its scholastic ghosts, enumerate both its proper and improper habits, and lay out with crystalline clarity its fundamental principles. Truth was not a metaphysical ideal to Bernard; it was concrete fact. Before fact, all theories must yield; and without continual refinement and improvement by new facts, all theories must sooner or later cease to be of value. His ability to express this single, immutable principle sets him apart in the history of medical science. With it, he eclipsed his teachers who, to use Magendie as an example, came to acknowledge him as their intellectual superior. He tells a vignette in which he produced results that called into question an analysis by Magendie. Magendie acknowledged his error as follows: “This difference between us comes from my having inferred more than I saw; if I had simply ... remained within the facts, I should have been unassailable.”

Bernard stood astride the great schism between theory and experience because he assigned to each its rightful place. Like the empiricists, he recognized only one truth: the pure fact that was demonstrated by direct observation. Thus, fact was distinguished from all analysis, which was just as prone to lead us away from truth as toward it. In this regard he was as wary of Baconian logic as of Aristotelian logic. But he also recognized the importance of theory in providing structure to support scientific inquiry and systematic progress, in the sense that facts can be made to stand upon one another to support a construct of a given disease that enhances our understanding and gives direction and meaning to additional researches.

Empiricism has survived chiefly among practitioners who use whatever means are available to them and treat patients according to what their experience has shown them to be effective. Even today, the working clinician continues to provide an important practical counterpoint to scholastic medicine, but only to the extent that we practice warily and make accurate observations.

We should stop to consider how little fact there is to support most arguments we might present on behalf of a particular theory on the origin of knee pain. If we can offer no fact, then let us listen and think, the better to construct an experiment that might illuminate the truth more brightly. I prefer to follow the example of Sir Thomas Browne, and keep silent “when the cause of truth might suffer in the weakness of my patronage.” Where those expressing an alternative point of view can provide a greater foundation of facts, let us, like Montaigne, acknowledge the superiority of their argument, yet seek ways to verify or refute their analysis by additional experimentation and observation. We must remain open minded in considering the possible causes of knee pain.

**SUMMARY AND CONCLUSIONS**

This is a time of great change. Information flows around the world at a bewildering rate. The doors have opened on virtually every culture, and the international orthopedic community is but one expression of the new cosmopolitanism. New diagnostic techniques are being introduced at an ever-increasing rate. At the same time, medical science is again upon the cusp of an epochal paradigm shift. All of medicine—diseases and therapeutics—are being examined now on a subcellular level. Surgical therapeutics are on a trajectory toward less invasive procedures and more advanced (bioengineered) materials for tissue replacement and repair. It remains to be established what implications these changes hold for the study and treatment of the patellofemoral joint, but these developments must certainly create new perspectives on pain and its treatment. In the fascination and excitement of our intellectual commerce, we ought to remember the lessons of Salerno. We must not forget that the exchange of ideas is merely the starting point. It is the empirical beginning of the perpetual experiment: the period of the hypothesis.

Bernard reminds us that words are not important in themselves. Neither are theories. I have seen my colleagues bristle at a study that shows the KT-1000 inferior to stress x-ray in the evaluation of certain ligament injuries. “Dale Daniel,” they say, “must be turning in his grave.” I ask them what they think is Dale’s legacy? Is it an instrument, a test for stability? I think it was the act of measurement that set him apart, the compulsion to measure and document his examination of an unstable knee. And whether the displacement is measured with an arthrometer or on a radiograph surely would not matter to
him, as long as the measurement is taken, so long as the opportunity is not lost to study the phenomenon of ligamentous instability. I have seen great orthopedists argue vainly over whether an investigator had interpreted their theories correctly when using them to study a particular clinical problem. I do not think it really matters whether the theory was applied correctly, as long as we know the conditions of the experiment, and can draw our own conclusions from the results.

Let us not get caught up in a misguided struggle to replace one doctrine with another. Let us not rail against facts but challenge weak assumptions and overdone analyses that can lead us into surgical misadventure. If we are to challenge theory, then let us do so in a manner consistent with, and worthy of, the tradition of scientific inquiry, which is our rightful heritage. Let us continue to develop new tools and instruments that can help us to observe more effectively and leave speculation to the philosophers. Let us, like Bernard and Harvey, confine our studies to that which can be demonstrated, while seeking ways to refine and enhance our capacity for observation. After all, it is the business of science to examine the immediate causes of phenomena. In considering remote or original causes, we easily outstrip our means of observation and are too easily led astray by speculation.

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