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An Intellectual Agenda for Hospitalists
Lessons from Bloodletting

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The practice of bloodletting, performed using sticks, thorns, bones, or anything sharp, probably began in Egypt about 3,000 years ago.[1] The practice continued in Greece, where Hippocrates recommended bloodletting to balance the body's four humors—blood, phlegm, yellow bile, and black bile—and continued during Roman times under the influence of Galen. In the United States, perhaps the most infamous use of bloodletting was when doctors reportedly bled as much as 5 U of blood from George Washington before he died from what was probably either acute epiglottitis or streptococcal pharyngitis.[2,3] Although many infectious organisms, especially malaria parasites, require iron to proliferate—and therefore may be less virulent in iron-deficient people[4]—acute near-exsanguination undoubtedly did more harm than good in the elderly ex-president.

But the practice of bloodletting continued and even flourished. In 1833 alone, France reportedly imported more than 40 million leeches to assist in bloodletting,[5] which oftentimes was thought to be sufficiently aggressive only when the patient actually fainted. Enthusiasm for bloodletting declined in the second half of the 19th century, influenced in part by a nonrandomized study that compared mortality rates among patients who were bled early in their illness with those who were bled later.[6] Nevertheless, Sir William Osler still recommended small amounts of bloodletting for pneumonia in his last edition of his famous textbook, *The Principles and Practice of Medicine*, published in 1920.[7] By 1927, however, the first edition of the Cecil's *A Textbook of Medicine* thankfully no longer recommended venesection except to treat conditions such as pulmonary edema.[8]

Why would I start this essay with a history of bloodletting? Surely, one might argue, nothing could be less relevant to a modern discussion of the quality of in-hospital medical care. The substantial literature on quality improvement emphasizes the practical implementation of strategies to increase the appropriate adherence to processes that are known to improve outcomes. A number of common quality measures quickly come to mind: the use of aspirin, β-blockers, angiotensin-converting enzyme inhibitors, and statins in post-myocardial infarction patients without contraindications,[9,10] the rapid initiation of appropriate antibiotics to patients with community-acquired pneumonia,[11] and early endoscopy for patients with acute upper gastrointestinal hemorrhage.[12] I could go on and on, listing in-hospital interventions supported by class 1 evidence from more than one definitive randomized trial. In essentially all of these situations, the creation of quality metrics, often accompanied by measurement and feedback, have improved adherence and undoubtedly saved lives. But although adherence has improved, the explosion in evidence-based medicine means that even the best hospitals may be in perpetual catch-up mode as they try to ensure adherence with the next wave of improvement interventions.

Unfortunately, every now and then a lot of attention is paid to meeting a quality metric that turns out to be misguided. Perhaps the best recent in-hospital example was the metric of prophylactic β-blocker use before major noncardiac surgery. Although this recommendation initially appeared to be based on reasonable data,[13] the large Perioperative Ischemic Evaluation Study (POISE) trial showed that reductions in rates of myocardial infarction were more than offset by an increased risk of stroke and other complications; therefore, average-risk patients actually did worse, not better, with the β-blocker regimen used in the trial. Although some have questioned whether these results were a function of the precise β-blocker regimen that was used, the results of POISE are actually remarkably consistent with prior data on the risk of myocardial infarction and stroke.[14,15] What was really different was the relative importance of these and other end points in patients whose risk of cardiac death was lower than those of higher risk patients in prior studies. But more recently, an even more disturbing reality has emerged: a number of key reports on which the guidelines were based came from an investigator whose publications included data that could not be confirmed when his studies were reviewed by his home institution.[16] Regardless of the precise reasons, we no longer routinely recommend an intervention that at one time was a key quality indicator.

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The β-blocker fiasco brings me back to bloodletting. In the early 19th century, a hypothetical visionary physician interested in quality improvement would likely have looked for ways to improve the efficiency and reduce the cost of bloodletting. Perhaps the leeches could be bigger, hungrier, or applied in a more effective fashion? Or perhaps vacuum tubes would have been invented sooner?

Of course, I am overemphasizing to prove a point. I truly believe that more and more of what we recommend is based on solid evidence to document, at least for now, that we are doing the right thing. If we do it more often and in more people, net benefit will be realized.

What does all this mean for the future of hospital medicine and its emerging research endeavors? For me at least, the message is pretty clear. First, we must be careful not to over extrapolate from limited studies in high risk patients, or we will jump to more conclusions like we did with β-blockers. Second, most advances in medicine require new and better data.

How can new clinical data be generated most quickly and efficiently? One-off studies at individual institutions are logistically and financially challenging, whereas an enduring research infrastructure is a treasure that can study a series of questions as they arise. The Thrombolysis in Myocardial Infarction Study Group has published scores of papers looking at a series of interventions in patients with acute myocardial infarction and the acute coronary syndrome.[17,18] The Acute Respiratory Distress Syndrome Network has demonstrated the value of lower tidal volumes and less aggressive fluid strategies in patients with respiratory failure.[19,20]

The success of these large, multicenter research networks should become the paradigm for the study of common hospital problems, ranging from the conditions that result in admission on the medical service to the problems that have engendered surgical comanagement services. New data can be gleaned by studying the medical care system, by studying routinely gathered administrative and clinical data, or even better yet, by gathering prospective data on patients and their diseases. For hospitalized patients, a variety of unanswered questions remain regarding the epidemiology of common diseases, the value of diagnostic tests, the impact of various therapies and treatment protocols, and the incremental value of new technologies, ranging from self-monitoring to handheld ultrasound. High-quality research may address the genetic epidemiology of why one person is admitted with pneumococcal pneumonia, whereas family members seem perfectly healthy; which patients with a particular diagnosis might be managed for different lengths of time in different settings; what physical findings or diagnostic tests best stratify prognosis; what new technologies are truly worth their cost; and especially, what therapies really work.

I do not dispute that hospital medicine researchers should try to improve the current use of interventions that are deemed to be valuable right now. But if that is all the field does, it will be a huge disappointment. Hospitalists should not be relegated to being adherence police who spend their collective research energy finding ways to force themselves to follow recommendations based on data gathered by others.

Hospital medicine researchers are uniquely positioned to discover new information that will change what should be done and help create the quality metrics for the future. Unless both of these two goals—improving the implementation of today's knowledge and generating new and better knowledge—are part of the research agenda, we run the risk that some of the best minds in internal medicine may, when all is said and done, have spent an inordinate number of IQ hours on what, a century from now, will be reminiscent of improving the quality of bloodletting.

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