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Surgical Decision-Making: Integrating Evidence, Inference, and Experience

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Article Outline

Three surgeons were discussing a complication of a colleague: a leak following an emergency sigmoid resection and primary anastomosis for sigmoid diverticulitis with contained contamination. The patient was a 71-year-old man on antihypertensive therapy, known to have moderate chronic obstructive lung disease from a lifelong history of smoking, but otherwise active, and living independently with his spouse of close to 50 years.

“Clearly,” said the first, “this was a serious lapse in judgment by Dr. X. I can't imagine what she must have been thinking when she decided to put the colon back together again. She put her patient at risk unnecessarily.

“I don't agree,” said the second, “There is good evidence that anastomosis can be performed safely in unprepared bowel [1] and [2], even in the setting of diverticulitis and peritonitis [3]. This was a recognized complication, and simply reflects the fact that we will never eliminate complications entirely.”

“Maybe,” said the third, “but data from experimental studies show that fecal loading impairs anastomotic healing [4], especially in the setting of local inflammation [5]. The approach may be safe in ideal circumstances, but not in a man who has known comorbidities, and I would not have attempted an anastomosis here.”

“The point is,” snorted the first surgeon, “that in the real world, patients aren't experimental rats or even the carefully chosen subjects who are enrolled in clinical trials, but living, breathing humans who have plenty of associated health problems, who are operated on not by ultraspecialists in quaternary care centers, but by surgeons like me and you. You don't take chances; you do the safest thing possible, and for me, that would have been a Hartmann's procedure.”

So—did the surgeon err in her judgment? What would have been the safest course of action? What would have been best for the patient? Is there a right answer, and how can we ever know?

This article explores the basis of surgical decision-making through the integration of evidence, inference, and experience—three complementary techniques for acquiring knowledge and applying it to a clinical problem. The elements of knowledge are complex, and also include intuition, and obedience—a menagerie of inputs whose acronym (EIEIO) brings to mind a children's song. Intuition is an indefinable capacity to predict the outcome of an event or decision, and derives from the integration of prior knowledge and experience, tempered by an innate capacity to establish the appropriate connections between disparate observations, and perhaps by some less tangible element of prescience. Obedience, on the other hand, is the uncritical adoption of the counsel of one's teachers and predecessors, typically expressed as aphorism or platitude: “Never let the sun set on a bowel obstruction”; “When in doubt, cut it out”; or “Big incisions for big surgeons.” It may provide comfort for the neophyte, particularly when confronted with a difficult decision in an unusual case, but it rarely provides the insight that can help to inform a sophisticated therapeutic decision. For neither intuition nor obedience can be readily described, parsed, or taught, and both are beyond the scope of a discussion of the inputs that a surgeon has in making a clinical decision.

Evidence, inference, and experience are the methods through which observations are converted into the knowledge that informs decisions, and these three complementary approaches are our focus here.

Evidence, inference, and experience

Evidence, inference, and experience are not competing ideologies, but complementary methodologies for synthesizing empiric data (Fig. 1). Evidence is grounded in the principles of probability, whereas inference derives from those of logic; evidence is an example of inductive reasoning, and inference an example of deductive reasoning. Experience integrates these two principles, but its unique feature lies in the capacity to provide disproportionate weight to events that, although uncommon, are associated with substantial morbidity for the patient. Characteristics of evidence, inference, and experience are outlined in Box 1.

Full-size image (25K)

Fig. 1. Evidence, inference, and experience provide different, but complementary perspectives on the data that inform surgical decision-making: optimal decision-making occurs when the three are congruent.

Box 1. Evidence, inference, and experience

Evidence

Methods: frequentist

Focus: efficacy

Strengths: probabilistic, and therefore closest approximation to truth for group.

Weaknesses: probabilistic, and therefore may not represent best synthesis of information for an individual who does not meet specific characteristics of group, or who is an outlier in a population.

Inference

Methods: Bayesian

Focus: safety

Strengths: facilitates decision-making in absence of rigorous evidence. Permits refinement of evidence-based decisions.

Weaknesses: depends on assumptions of similarity; inherently subjective.

Experience

Methods: anecdotal

Focus: norms, and exceptions to the norm

Strengths: captures important, but as yet undefined elements of problem. Relates outcomes to individual surgeon's strengths and weaknesses.

Weaknesses: subservient to ego, hubris, and selective memory.

Inductive science and the methodologic basis of evidence-based medicine

Evidence-based medicine is defined as "the conscientious, explicit and judicious use of current best evidence in making decisions about the care of individual patients. The practice of evidence-based medicine means integrating individual clinical expertise with the best available external clinical evidence from systematic research" [6]. As a clinical tool, evidence-based medicine emphasizes the need to integrate the results of well-designed clinical trials with the capacity of the clinician to apply those findings to the unique circumstances of the individual patient—to integrate evidence with experience. Its particularly important contribution has been to codify the principles that underlie the generation of reliable clinical knowledge; these, in turn, derive from the application of the principles of probability and the scientific method to clinical research.

The methodologic basis of evidence-based medicine is inductive reasoning, and so the strongest evidence is that which arises from the most powerful tool of inductive science—the randomized controlled trial. Let us examine this more closely.

Inductive science is based on experimental observation and the application of principles of probability. Probability theory tells us that if one of two possible outcomes are equally likely—for example, the outcome of a coin toss being either heads or tails—then if the event is repeated numerous times, the number of each outcome should be approximately equal. If one were to flip a coin one hundred times, it would not be unexpected that the consequence might be 52 heads and 48 tails—this approximates the anticipated distribution of events if each is equally likely. But if the result were 80 heads and 20 tails, we would be suspicious that there was something amiss with the process—that the coin was loaded, or that the person tossing it was otherwise influencing the results. We can estimate the likelihood that deviations from the expected 50:50 ratio occur on the basis of random chance using a statistical test such as the χ^2 test. The probability of obtaining 52 heads as estimated by the χ^2 statistic is 0.89; or expressing it as an odds ratio, 1.08 (95% CI 0.62–1.89). On the other hand, the probability of obtaining 80 heads in 100 tosses is less than 0.0001 (odds ratio 4.00; 95% CI 2.14–7.49), or very unlikely indeed. Neither sequence of coin tosses can tell us with absolute certainty whether the process is being influenced by forces other than those of chance; it can only estimate the probability that the outcome observed reflects a purely random process.

Deviation from random chance forms the basis of scientific understanding. Indeed, biomedical research proceeds from the null hypothesis that there is no difference between two experimental

groups, and draws inferences regarding mechanism or causality by rejecting the null hypothesis; in other words, by concluding that it is unlikely that observed differences occurred on the basis of chance alone. By convention, the level at which this cutoff is made (also known as the alpha level) occurs when the chance of differences representing random variability is less than one in twenty, or when the α level for probability (*P*) is less than 0.05. This value is purely arbitrary, representing a trade-off between increasing certainty and feasibility [7]. When the *P* value is 0.1, one can conclude that the probability that the observation occurred by chance is one in ten; conversely, when a study concludes that *P* is less than 0.05, this does not mean that the conclusion is true, but rather that there is less than a 5% chance, under the circumstances in which the study was performed, that the difference represented random chance. The distinction is subtle but important: there are no truths in medicine, merely assumptions whose probability is more or less secure.

The conclusion that an observed difference between two study populations reflects the consequences of an intervention can only be drawn with confidence when the populations are otherwise similar. Individuals within the study population will vary with regard to many characteristics—age, comorbidities, gender, duration of illness, extent of disease, and genetic predisposition, along with many unmeasured factors—that may independently influence the outcome. The use of random allocation of patients to treatment groups provides the greatest likelihood that the resulting groups will be similar with respect to both known and unknown variables that might impact on the outcome of the intervention.

An evidence-based approach to the management of perforated diverticulitis

Let us apply an evidence-based approach to our patient who has perforated diverticulitis. Does the literature truly support the contention that primary anastomosis is as safe as the Hartmann's procedure?

A Medline search using the keywords “perforated diverticulitis, surgery,” and restricting the search to studies in humans reported in the English literature, yields 152 citations. Many of these are case reports; one is a systematic review [3]. Because this latter is the most comprehensive, and likely to be the most rigorous, we will review it first.

Salem and Flum [3] reviewed 98 studies that provided comparisons of the Hartmann's procedure with resection and primary anastomosis for patients who had perforated peritonitis and significant peritoneal contamination (Hinchey stages 3 and 4). They noted substantial variability in the methodologic quality of the reports, and in the definitions and reporting of study outcomes. Nonetheless, when they pooled the results of studies for which data on survival and rates of infectious complications were available, they found that adverse outcomes were less frequent following primary anastomosis, particularly when the morbidity associated with colostomy reversal was considered (Table 1), and concluded that primary anastomosis is safe in selected patients who have perforated diverticulitis.

Table 1. Primary anastomosis versus the Hartmann procedure for perforated diverticulitis: a systematic review

Results	Hartmann	Hartmann + reversal	Primary anastomosis
Deaths	18.8%	19.6%	9.9%
Anastomotic leaks	—	4.3%	13.9%
Wound infection	24.2%	29.1%	9.6%

Full-size table

From Salem L, Flum DR. Primary anastomosis or Hartmann's procedure for patients with diverticular peritonitis? A systematic review. Dis Colon Rectum 2004;47(11):1953–64; with permission.

At first glance, it appears that the issue is settled: primary anastomosis has a mortality rate that is one half, and a wound infection that is one third that of the Hartmann procedure, albeit with a higher rate of anastomotic leakage—surely it should be the operative procedure of choice for our patient. Is it simple obstinacy, or the much vaunted surgical conservatism toward adopting changes in practice that drives our reluctance to embrace the practice unreservedly, or is there something more?

Discordance between the conclusions of systematic reviews [8], or of the conclusions of systematic reviews when compared with those of large, well-performed randomized controlled trials [9], [10] and [11] is well-recognized. For example, systematic reviews of the efficacy of albumin supplementation in critical illness have variously concluded that supplementation is harmful [12], beneficial [13] and [14], or without obvious efficacy [15].

The divergent conclusions of these reviews can be attributed to multiple factors. These are outlined in Box 2.

Box 2. Sources of discordance in systematic reviews

Discordance arising from the original studies

Study design

Intervention

Target population

Exclusion criteria

Study definitions

End points and outcome measures

- Randomization of study subjects
- Concealment of allocation

Study conduct

- Cointerventions
- Variability in clinical care
- Blinding of outcome adjudication
- Completeness of follow-up

Study reporting

- Completeness of report
- Publication bias

Discordance arising as a consequence of the systematic review

Studies selected

- Search strategy
- Inclusion of unpublished reports
- Exclusion criteria

Language of publications

Evaluation of quality

Selection of subgroups for analysis

Discordance can arise because of differences in the design and conduct of the studies that compose the systematic review. The intervention may vary from one study to the next—in the present review, for example, depending on whether a proximal diverting stoma was created in those patients who underwent primary anastomosis, whether an intraluminal stent was placed, and whether a colonic washout was performed.

The conclusions of a clinical trial are further influenced by the criteria used to select patients for entry into the study, the criteria used to exclude patients, and the outcome measures used to determine therapeutic efficacy. For example, the conclusions of a study comparing sigmoid resection and primary anastomosis with resection and colostomy for diverticular disease might

be very different if the study population were all patients who had diverticular disease, or only patients who had perforated diverticulitis and established peritonitis. Even in the latter group, patients who had walled-off perforations might respond differently than patients who had diffuse purulent peritonitis. Exclusion criteria for a clinical trial—established to protect study subjects who might be at particularly high risk of adverse outcome—are typically quite variable from one study to the next, and so may disproportionately exclude those patients most likely to meet the study end points. Outcome measures are similarly variable, and even for such hard outcomes as death, conclusions may vary depending on whether mortality is measured early, at an intermediate time point such as 30 days or hospital discharge, or at a later date that might capture delayed events associated with colostomy closure.

Finally, and of particular relevance to the studies included in the present systematic review, studies in which treatment allocation is not established randomly can yield misleading conclusions. None of the studies incorporated into the systematic review was a randomized controlled trial; it is entirely possible that selection bias occurred when patients were being considered for primary anastomosis or Hartmann's procedure, and that the higher-risk patients were more likely to undergo the latter intervention. Similarly, when historical controls are used, it is uncertain whether observed differences are attributable to the intervention, or to other unmeasured advances in the process of care.

The conclusions of a trial are also influenced by factors reflecting the conduct of the study, including blinding of outcome adjudication and the completeness of follow-up, as well as by regional differences in the process of care, or variability in cointerventions.

The conclusions of a systematic review are heavily dependent on the methods of the review itself. The study question may vary subtly between reviews, and the search strategies or criteria for including or excluding studies may differ. A decision to include or exclude unpublished studies, or to restrict the review to studies in the English language may also influence the conclusions. Further, the methods used to evaluate study quality, and to pool studies for subgroup analysis can influence the outcome of the review. Finally, pooling of aggregate data commonly requires a degree of subjective interpretation, and this too may impact on the inferences that are made.

So it would appear that the decision to perform a primary anastomosis was reasonable, based on the available evidence; however it is also apparent that, in the absence of solid data from adequately powered, randomized controlled trials, the evidentiary basis for making a decision in this particular case is weak, and dependent on inferences drawn from case series, and studies using historical controls.

Deductive science and inferential decision-making

Inferential approaches are probably the most commonly used methods for making surgical decisions in individual patients. Their need reflects the fact that for many of the most complex decisions that the surgeon must make, rigorous data from randomized controlled trials are simply unavailable, and when they are, the specific circumstances suggest the need to tailor the decision to the particular circumstances of an individual patient. Unlike evidence-based medicine,

inference-based medicine lacks an articulated methodology; it may be presumptuous to suggest that such a methodology can be derived, but bear with me.

If the raw material that informs the practice of evidence-based medicine is inductive reasoning and the conclusions of randomized controlled trials, inference-based medicine depends on deductive reasoning and an understanding of the biology of health and disease [16]. These two approaches are complementary and interdependent. Inference provides a method for generating hypotheses that can be tested through randomized controlled trials, but it also provides a mechanism for making an informed decision in the absence of strong clinical evidence. Evidence-based approaches use the specific insights garnered from clinical trials and generalizes them to all similar patients. Inference-based approaches, on the other hand, integrate general principles derived from multiple data sources, and apply them to the specific problem at hand.

Inference-based thinking relies on principles of logic, but because it derives from biologic knowledge, is dependent on inductive, rather than deductive inference. Deductive inference provides conclusions that are necessarily true if the premises are true:

All men are mortal. Socrates is a man. Therefore Socrates is mortal

The truth of the first premise is accepted, as is that of the second, and therefore because the subject of the second—Socrates—is a subset of the first, the conclusion must be true.

With biologic inference, however, the reliability of the premises—drawn from experiment and observation—is less certain. The clinician creates an argument based on multiple premises that are believed to be accurate, and so reaches a conclusion that is likely to be true.

Our current question on the safety of primary anastomosis in perforated diverticulitis might be reformulated as a series of premises drawn from both experiment and clinical observation:

• Primary anastomosis can be performed safely in the unprepared bowel
[2]; therefore fecal loading alone does not preclude primary anastomosis.

• Primary anastomosis can be performed safely in patients who have penetrating abdominal trauma

[17] and
[18]; therefore fecal spillage does not preclude anastomosis.

• Risk factors for anastomotic leakage include mechanical obstruction, chronic lung disease, alcohol abuse, transfusion, hypertension, ischemia, microvascular disease, and the use of drains

[19],
[20],
[21] and
[22].

• In experimental models, infection or inflammation have a modest inhibitory effect on wound healing

[23] and
[24].

We reach a conclusion similar to that which we had reached on the basis of a systematic review of clinical studies of the management of perforated diverticulitis—that primary anastomosis appears to be an acceptable option unless clinical factors such as mechanical obstruction, chronic lung disease, vascular disease, or large volume transfusion are prominent. None of the data we have used to reach this conclusion were derived from studies of patients who had diverticulitis; yet in aggregate, they create a convincing case that a decision to undertake a primary anastomosis is a reasonable one.

The focus of an inference-based approach to decision-making is patient safety, rather than therapeutic efficacy. Its primary use is in suggesting that a decision is unlikely to expose the patient to significant harm, rather than that it may improve clinical outcome. The randomized controlled clinical trial is the only reliable tool for determining the superiority of one treatment mode over another, because of the multiple potential sources of bias inherent in other experimental designs. With these principles in mind, we can formulate a series of questions to be considered in developing an inference-based approach to a particular problem. These are listed in Box 3.

Box 3. Inference-based analysis

Are there consistent mechanistic data from studies in animal models

• that characterize the biology of the process of interest?

• that evaluate the impact of physiologic perturbations or models of disease on the underlying biologic processes?

Are there human data to support the use of the approach of interest in a different disease process

• in a single disease with features of the problem to be addressed?

• or in a variety of disorders that reproduce discrete features of the problem?

What are the potential complications

• of the planned procedure?

• of the disease which is to be treated that might be made worse by the planned intervention?

What are the known risk factors for these complications

• from cohort and natural history studies?

• by analogy to other similar processes?

Are there consistent mechanistic data from studies in animal models?

Animal models can provide insight into in vivo biology and its alteration by interventions that mimic disease; they do not replicate human disease, and they cannot reliably predict therapeutic efficacy [25]. Although specific biologic abnormalities can be reproduced in animals, disease is a complex process whose evolution reflects not only the biologic derangement, but also the genetic background and comorbidities of the host, and the response of the doctor in treating the clinical syndrome. The onset of perforated diverticulitis in humans, for example, may be gradual or abrupt, and the disease may be more or less advanced at the time of initial presentation. The extent of contamination is variable, as is the degree of host response to that contamination. The patient commonly has concomitant medical problems that may independently alter the host response. The patient is treated, with varying degrees of expertise, with intravenous fluids, antibiotics, and surgery or percutaneous drainage. Each of these factors modifies the clinical expression of the illness and therefore its potential to respond to specific therapy, and is extraordinarily difficult to replicate in an animal model.

Cecal ligation and puncture (CLP)—peritonitis induced by devascularization of the cecum by a ligature, and expression of feces by puncturing the ligated cecum with an 18 gauge needle—shares many features with perforated sigmoid diverticulitis [26]; however, its experimental rodent subjects are genetically similar, and undergo a single, standard insult. Experimental therapies are typically provided either before, or at a common time after the insult, and resuscitation, antibiotics, or surgical excision are often omitted. Indeed, animals that survive the initial insult begin to eat and gain weight by 4 days after the procedure, despite the presence of a gangrenous cecum within the peritoneal cavity. Thus inferences drawn from studies using CLP may provide insight into an in vivo response to a well-defined intervention, but the gulf between this insight and evidence of therapeutic relevance is wide.

Recognizing these important limitations, however, a preclinical model can provide important insights into how biologic processes evolve in vivo, and how defined perturbations affect that evolution. We can learn, for example, whether a colonic anastomosis will heal in the presence of peritonitis, and what specific elements of the healing process might be impacted.

Are there data supporting the use of the approach of interest in patients who have a different disease process?

The presence of underlying disease may modify the response to clinical intervention, but in general, evidence that an approach is safe in one group of patients will increase our confidence that it is safe in a different population. For example, the remarkable progress that has been made in the field of minimally invasive surgery over the past 2 decades reflects the fact that the safety and feasibility of laparoscopic abdominal surgery for cholecystectomy suggested that a similar approach could be safely applied to most intraperitoneal viscera.

Our confidence will be increased by evidence that the approach is safe in a variety of complementary clinical situations that reflect differing sources of potential risk to the patient. In our current discussion, for example, we wish to know not only that an intestinal anastomosis can be performed safely, but that the risk of leakage is acceptable despite potential risk factors such as fecal loading, emergency surgery, and active inflammation.

What are the potential complications of our plan of action?

Cohort studies and case series can provide valuable insight into the potential morbidity and mortality associated with a particular disease and its treatment. Understanding not only the short term risks and benefits, but also the longer-term impact on the patient's quality of life is important to planning an optimal approach.

The potential complications facing our patient who has perforated diverticulitis are many, but the one that drives the current debate is the risk of anastomotic leakage, and more precisely, the consequences of a leak, should it occur. We also consider risks such as that of wound infection or dehiscence or postoperative complications such as myocardial infarction or pulmonary embolism, and if percutaneous drainage of a localized perforation is technically feasible, we may infer that it is the optimal approach. It is important in the process of surgical decision-making to remember that the complications of the disease are not simply those faced at the initial operation, but also those resulting during subsequent procedures to restore intestinal continuity or to treat complications of our original management plan [27].

What are the known risk factors for these complications in other clinical settings?

Finally, inferential decision-making requires an appreciation of the factors that might increase the risk of adverse outcome in an individual patient. This information typically derives from cohort studies, and is strengthened by considering risk factors for adverse outcome from both the disorder of interest, and from related conditions in which similar therapies were applied.

Our evaluation of the risks of anastomotic failure revealed that patient factors such as hypertension, alcoholism, or vascular disease; clinical factors such as obstruction and ischemia; and therapeutic factors such as transfusion or the use of drains were all associated with an increased risk of anastomotic leakage. Importantly, inflammatory bowel disease or intraoperative spillage of gastrointestinal contents do not appear to increase the risk of anastomotic failure.

Experience: integrating subjectivity into surgical decision-making

Both evidence-based and inference-based approaches integrate insights derived from the structured and published work of other investigators; however, an individual surgeon's own experience figures centrally in the decisions that he or she makes, and influences the interpretation and application of surgical knowledge. Experience guides decision-making in three broad areas.

First, experience serves as an imperfect arbiter of published wisdom. It is this role that has created the greatest tension between the proponents and detractors of evidence-based medicine,

for the counsel of experience is frequently at odds with the norms of evidence. Experience is shaped by consistency, but it is disproportionately influenced by the unanticipated and the exceptional. A surgeon will routinely perform a stapled colonic anastomosis because this is the technique she learned during her training, and because she has not experienced significant problems with the approach. But the occurrence of an anastomotic breakdown following a stapled low anterior resection will typically cause her to re-evaluate this approach, and perhaps even to perform a hand-sewn anastomosis or proximal diverting ileostomy for the next such patient. This decision does not reflect the acquisition of new knowledge that an alternate approach is superior, but rather the implicit assumption of a causal relationship between the decision (to undertake a stapled anastomosis) and the consequence (an anastomotic dehiscence). The important issue, however, is not whether there is a risk of dehiscence when a stapled anastomosis is performed, but whether that risk can be reduced with an alternate technique, a question that can only be reliably answered by a randomized controlled trial.

Second, experience serves to integrate published knowledge with the particular strengths, limitations, and values of the surgical practitioner. For example, although case series suggest that laparoscopic colectomy is as effective as an open procedure for patients who have diverticulitis [28] and [29], a surgeon may preferentially opt for one or other approach based on his or her level of comfort with the procedure.

Finally, experience provides a mechanism to tailor a therapeutic approach to the particular needs and values of the patient. Surgeons do not treat diseases; we treat patients who have diseases, and so the optimal approach will vary with the particular priorities of the patient. The decision to create a colostomy in an elderly patient whose dexterity is limited by arthritis, or whose eyesight is failing, may result in loss of independence or the need for institutionalization, and for some, this may be a fate worse than death.

Integrating evidence, inference, and experience

Evidence, inference, and experience provide complementary perspectives on the raw material that goes into a surgical decision, and the most secure decision is one for which all three support the choice that is made. The challenge arises when they do not.

Well-designed, adequately-powered, randomized controlled clinical trials provide the strongest evidence of therapeutic efficacy, but even where information from such trials is available, it is not necessarily definitive. Fully one sixth of influential randomized controlled trials published in the early 1990s have been contradicted by the results of subsequent investigation, whereas for a further one sixth, the magnitude of the effect has proven to be exaggerated [30]. Conversely, the absence of evidence from randomized controlled trials does not invalidate an otherwise supportable conclusion: the absence of such evidence for parachutes is unlikely to convince even the most ardent enthusiast of evidence-based medicine to jump from a plane without one [31]! Because of the assumptions inherent in their design, randomized trials are best suited to answering questions applicable to populations, rather than to individual patients. Thus they provide particularly strong support for interventions that are prophylactic in nature, or therapeutic in more homogeneous patient populations, and must be interpreted more carefully when the results are used to make treatment decisions in the individual patient [32].

The results of clinical trials are but one piece of a body of information that supports a particular conclusion, and must be interpreted within that totality. Indeed this concept is inherent in the Bayesian approach to statistical inference, in which the strength of the conclusions are evaluated in the context of the prior probability of the truth of the experimental hypothesis using a Bayes factor or likelihood ratio [33]. For a single data set such as the results of a trial, Bayesian analysis affords greater confidence in the conclusions when these are consistent with what is known than when they are divergent, thus marrying evidence and inference, and mirroring the well-recognized conservatism of clinicians in adopting new approaches based on trial results that are at odds with what is expected. On the other hand, prior understanding—whether based on inference or experience—may be flawed, selective, and unduly influenced by obedience to preconceptions, and so uncertainty and controversy is an inescapable element in the evaluation of medical information.

The integration of evidence, inference, and experience does not provide the truth, or even point to a preferred approach to the management of an individual patient—what it does is to shift the fulcrum around which clinical decisions are made. Our review of the experimental and clinical literature on primary anastomosis for perforated diverticulitis does not tell us what we should do in this case, and in particular, provides no guarantee that one option will avoid complications. But it does tell us that the option is reasonable, and frees us to make the decision that will provide the best solution for this particular patient.

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