Moving Toward Evidence-Based Practice

Deborah J Cook MD

"Evidence-based practice" involves applying the best available evidence to the care of individuals. Explicit, systematic methods have developed for determining what is the best available evidence. However, often even the highest-level evidence is not thoroughly or effectively used in practice, even if it is widely known. We must rigorously and critically analyze study results to understand their strengths, limitations, and generalizability, and bear in mind that our knowledge will evolve and thereby change our practice. The clinical question is not always how to apply the evidence but whether the available evidence applies to a particular patient. We should always ask whether the right provider is doing the right thing for the right patient at the right time in the right setting with the right resources. *Key words: evidence, evidence-based, clinical medicine, clinical practice.* [Respir Care 2003;48(9):859–868. © 2003 Daedalus Enterprises]

Introduction

Thanks very much for inviting me here today to this excellent conference. The field of respiratory care has been most impressive in its contributions to improved patient outcomes. During my training as a medical student and resident at McMaster University, and when I continued as a critical care fellow at Stanford University, I learned an enormous amount from respiratory therapists (RTs). It therefore means a lot to me to be invited to speak to you today about one aspect of the way in which health care is changing, hopefully for the better. The concept of evidence-based practice, you'll notice, is quite different than *evidence-based medicine*. That's because we've made a deliberate, progressive move to acknowledge the important multidisciplinary roles of all clinicians. Remember that clinicians of all kinds can contribute to evidence-based practice.

I have 3 objectives in this presentation. The first is to examine some of the key concepts in evidence-based practice. The second is to examine the gap between our knowledge of the evidence and its application in practice. The third is to review effective strategies to improve the appropriate and timely application of evidence in practice. This last objective is the vanguard of evidence-based practice. In other words, it is not enough to just know the evidence; we have to apply it wisely to our patients.

What Is Evidence-Based Practice?

To begin with, here is one of the tried and true definitions of evidence-based medicine. Evidence-based medicine involves caring for patients based upon explicit integration of *pathophysiology*, clinician *experience*, and the *best* available evidence, adapted to each individual and the local *health care setting*. There are a few important elements of this definition. First of all, as mentioned earlier, *evidence-based medicine* is an old term and we don't use it much any more. The term *evidence-based practice* acknowledges that not just physicians but many clinicians, with different disciplinary backgrounds, care for patients. The second important element of this definition is that it is useless without understanding the relevant anatomy and pathophysiology that form the foundation of our clinical knowledge. Third, clinician experience is key to avoiding the misinterpretation and misapplication of evidence; in other words, clinical experience is tantamount to evidence-based practice. Fourth, saying that we seek the best avail-

This article is based on a transcript of the Philip Kittredge Memorial Lecture delivered by Deborah J Cook MD on October 7, 2002 at the 48th International Respiratory Congress at Tampa Convention Center, Tampa, Florida.

Deborah J Cook MD is affiliated with the Division of Critical Care Medicine, Department of Medicine, St Joseph's Hospital, Hamilton, Ontario, Canada.

Correspondence: Deborah J Cook MD, Division of Critical Care Medicine, Department of Medicine, St Joseph's Hospital, 50 Charlton Avenue East, Hamilton ON L8N 4A6, Canada. E-mail: debcook@mcmaster.ca.

able research evidence implies that evidence can change over time. Since evidence is dynamic, we have to continue to try to keep up to date with it as it evolves. Finally, our application of evidence may be modified based on the health care system in which we work. For example, universal access to health care, as in Canada, may mean that patients have a greater probability of receiving some effective interventions, because that system does not deliver care differentially based on whether a patient can pay or whether his or her health insurance company will pay.

One of the classic conceptual frameworks for evidence-based practice involves finding the best medical information. The information base at our disposal is huge. The biomedical literature is growing exponentially and there is no sign of it stopping. More and more journals are being created, and more and more research is being published each year. There are now approximately 300,000 randomized trials, for example. What clinicians want is the most valid clinical literature relevant to our patients and our health care setting. If we are trying to keep up to date with the literature, we also are interested in what is new.

The critical appraisal exercise is the backbone of evidence-based practice, and this always begins with the clinical question based on a patient or population (Table 1).

Table 1. The Critical Appraisal Exercise

Define the clinical question
Search for relevant literature
Evaluate the validity of the reports
Understand the results
Decide whether and how the results apply to your patient(s)

The question then invites a search for the relevant literature, perhaps in a computerized bibliographic database such as MEDLINE. It might involve a search of printed journals in the library, or it might involve going to a colleague who has the relevant evidence. Therefore, seeking new information often means, but does not necessarily mean, using a computer. Another key step in evidence-based practice is evaluating the validity or the credibility of the evidence to make sure we don't use research that is potentially misleading or biased. The final step is to decide whether to apply the evidence to a given patient. One of the important distinctions between what evidence-based practice is, and what it isn't, is underscored by this: we are deciding *whether* to apply the evidence, not *how* to apply the evidence; sometimes we conclude it is not appropriate to apply certain evidence to a given patient at a given time.

There are 5 main types of research study, which I'll describe (Table 2). There is one key validity feature we use to filter the reasonable quality articles from those that are more likely to be biased. For issues of prevention and therapeutics, we tend to focus on randomized clinical trials. The key validity feature is the random allocation of patients to one or another arm of the study, such that in a 2-armed randomized trial each consecutive patient has a 50:50 chance of going into either arm.

Table 2. Types of Studies Used in Evidence-Based Practice

Randomized clinical trial
Systematic review
Diagnostic test study
Prognosis study
Causation study

A modern review article is not a narrative review of somebody's opinion. Modern review articles are systematic, and the key validity feature is the methods section, which states a specific clinical question, a clear literature search strategy, inclusion and exclusion criteria for the studies considered, and critical appraisal of the primary studies included in the review.

For issues of diagnosis, the key validity feature is a direct comparison of a new test with a reference standard test. A diagnostic test study is usually observational.

For issues of prognosis, the key validity feature is a well-defined inception cohort, ideally followed prospectively to evaluate clinical outcomes. A prognosis study is usually observational.

For issues of causation (eg, smoking causing lung cancer), the key validity feature is a carefully defined exposure (eg, smoking). A study evaluating causation is usually observational, but randomized trials can also determine causation.

One common misconception about evidence-based practice is that it requires a lot of statistics, which is not true; we use about 8th-grade level mathematics. One of the trends you may have noticed in medical studies is that they now frequently refer to *confidence intervals*, not just to p values of < 0.05. In the diagnostic test literature we focus on sensitivity, specificity, likelihood ratios, and receiver operating characteristic curves, which I'll discuss, using examples from the literature on ventilator weaning. You've read about relative risk reduction and relative risk as examples of "relative measures of benefit and harm," typically communicated in the results of a randomized trial or systematic review. You've also read about "absolute measures of benefit and harm," including the number of patients we need to treat to prevent one bad outcome (ie, "number needed to treat"). These are some of the more common metrics for communicating study results and they are tools of evidence-based practice. I'm sure you are familiar with these terms as they now regularly appear in publications and come up often when we're critically appraising articles.

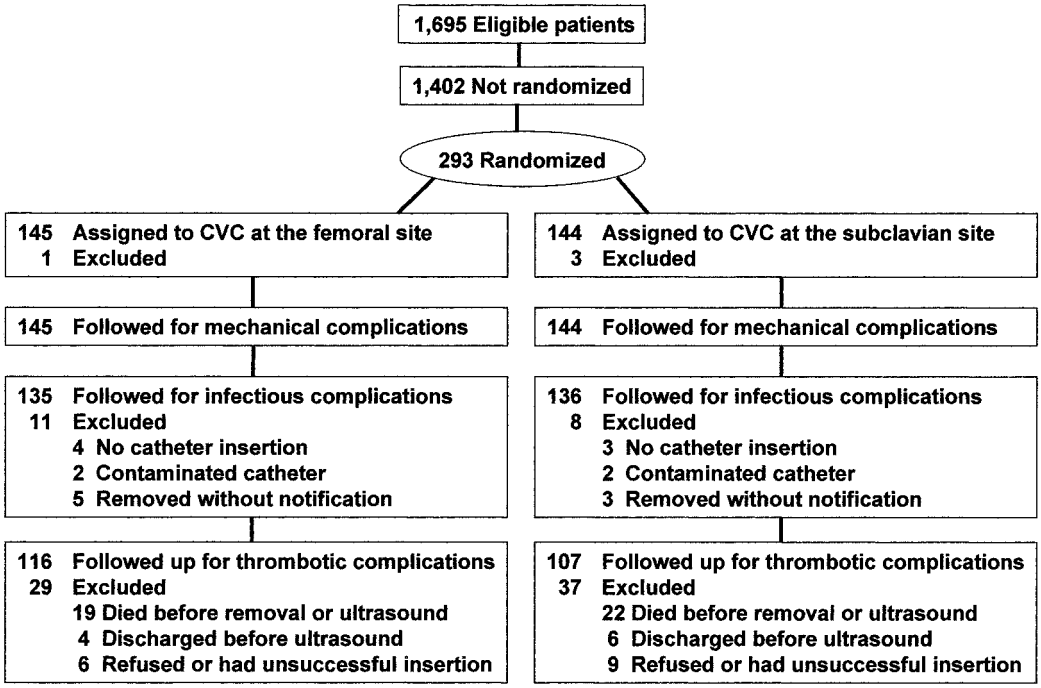


Fig. 1. A randomized clinical trial design (flow of patients through the trial). This type of flow chart facilitates critical appraisal and understanding of the study. (Adapted from Reference 2.)

Evidence-based practice involves thinking about clinical effectiveness. We can ask ourselves in our own practice setting: Is the right provider doing the right thing for the right patient at the right time in the right setting with the right resources? Let's take, as an example, noninvasive positive-pressure ventilation (NPPV) for a patient suffering an exacerbation of chronic obstructive pulmonary disease. There is a robust body of randomized trial evidence indicating that NPPV can obviate endotracheal intubation and decrease the risk of mortality for these patients. Therefore, if we ask whether it is appropriate to use it with a patient suffering an exacerbation, the answer will often be yes. We need to use it with the right patient—not profoundly obtunded or acidotic patients, or patients unable to protect the airway. We need to use NPPV at the right time—not too early, such that it may not have any effect, and not too late, when intubation is imminent. We need to use it in the right setting—with cardiopulmonary monitoring and an appropriate intensity of nursing care, such as in the emergency room, a high-dependence unit, or the intensive care unit (ICU), rather than a general ward. NPPV should be instituted and monitored by the right clinician—the primacy of the respiratory therapists cannot be overstated, along with experienced nurses and physicians. And we need the right resources—human resources (skill and experience) and monitoring capabilities—to be sure that NPPV can be used safely.

At my hospital one of our very astute critical care trainees, Tasnim Sinuff, did a careful study documenting that the clinical effectiveness of NPPV was suboptimal.[1] She found that sometimes NPPV was ordered by residents who were inexperienced, for patients who were inappropriate, in settings that were unsafe. We then developed an NPPV practice guideline for patients suffering acute exacerbations of chronic obstructive pulmonary disease; this helped us integrate the randomized trial evidence into practice, thereby turning efficacy data into clinical effectiveness.

Randomized Clinical Trials

Randomized clinical trials are not the be-all and end-all of evidence. However, they remain the reference standard and the most rigorous way of testing the effectiveness of interventions. The first thing I'd like to address about the reporting of randomized clinical trials is illustrated by a French multicenter study.[2] Eligible patients were randomized to receive central venous catheterization at either the femoral site or the subclavian site. The patients in this trial were followed for 3 main events: mechanical, infectious, and thrombotic complications of central venous catheterization. Figure 1 shows how people were identified for the trial, allocated to the interventions, and evaluated for outcomes. This type of diagram, further described in the Consolidated Standards of Reporting Trials (CONSORT)[3] statement, helps us communicate visually about the design of randomized trials.

One of the most important randomized clinical trials for RTs was the Italian trial of prone positioning for patients

with acute respiratory failure,[4] which randomized acute respiratory distress syndrome (ARDS) patients to prone positioning or supine positioning. The main outcomes were 10-day mortality and ICU mortality. The relative risk reduction of 0.84 showed a trend favoring prone position for decreasing 10-day mortality. A relative risk of 1 means that an intervention has no effect. A relative risk of less than 1 suggests benefit, and a relative risk of more than 1 suggests harm. The confidence limits give us some idea about the strength of inference from the results. In the proning study[4] the confidence limits include 1, and they are fairly wide, ranging from 0.56 to 1.27, indicating that this estimated relative risk reduction of 0.84 is only a trend; thus, prone positioning did not confer any mortality advantage for ARDS patients in that study. The investigators proceeded to consider suitable future randomized trials evaluating the potential benefit of prone positioning for ARDS patients. They generated some hypotheses about which subgroups of patients may be the most likely to benefit, if any were to benefit at all. And, in a well-acknowledged post hoc analysis, prone positioning appeared to be associated with lower mortality among patients who had a very low ratio of P_{aO_2} to fraction of inspired oxygen and who had a very high Simplified Acute Physiology Score, and among patients who were initially receiving high tidal volume (V_T). This is an example of a randomized clinical trial that was essentially negative but which explored further post-hoc subgroups to suggest which patients might benefit from prone positioning. This does not necessarily mean that we should be using prone positioning for these patients, but suggests that we should test the efficacy of prone positioning in those types of patients in future trials.

Another very important randomized trial in respiratory medicine was from the ARDS Network, which tested the effect of conducting mechanical ventilation with a V_T of 6 mL/kg, versus 12 mL/kg, with ARDS patients.[5] A strong mortality advantage was attributed to low-V_T ventilation, and this has changed practice in many centers. Let's think about the application of that randomized trial result. We'd want to apply low-V_T ventilation to patients like those in the ARDS Network study, by examining the study's inclusion and exclusion criteria. However, what about patients who are just a year older but otherwise fulfill eligibility criteria? Sometimes we generalize trial results to older patients who may be physiologically similar to younger patients, because we believe that they too may benefit from the intervention. However, we don't generally apply trial results to patients who would be ineligible for the trial, such as patients with lobar pneumonia who don't have bilateral air-space disease (Fig. 2).

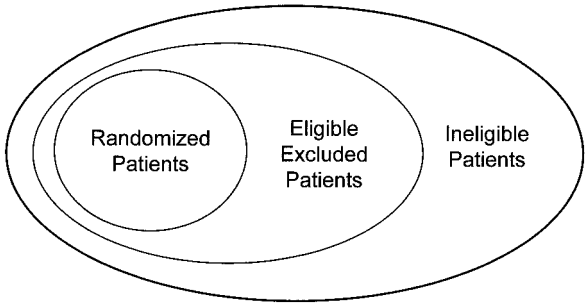


Fig. 2. Generalizing the results of randomized, clinical trials. This schematic shows the patients with whom clinicians need to consider applying randomized trial results. Patients such as those randomized in the trial are those most likely to have the trial results apply to them. Patients of the sort who were excluded from the trial and those considered ineligible for the trial are unlikely to have the trial results apply to them.

In an abstract at the American Thoracic Society conference, Gordon Rubenfeld showed that in the clinical centers in the ARDS Network study only a tiny proportion of ARDS patients were actually receiving 6 mL/kg ventilation on the first day that ARDS was identified.[6] After the publication of the ARDS Network trial, that proportion increased slightly, and following feedback to these centers, there was again a nonsignificant increase in the use of 6 mL/kg V_T ventilation. By ARDS day 3 the difference was more evident, but the application of the low-V_T strategy was still extraordinarily low. This is a good example of how good research evidence is often poorly applied in practice.

Let's consider the outcome of rare adverse events related to an intervention tested in trials, which we often witness at the bedside but do not always read about in published articles. One study compared literature from the fields of human immunodeficiency virus, acute myocardial infarction, hypertension, and a body of evidence on selective digestive decontamination (SDD),[7] which involves administration of antibiotics, orally, nasogastrically, and intravenously to minimize infectious morbidity in critically ill patients. Reporting of adverse events in randomized trials is variable, and poor for SDD (Fig. 3). We need to look hard in randomized trials for reports that the potential disadvantages of interventions are monitored and reported.

Recently Jürgen Graf, of Germany, made a rigorous appraisal of randomized trials of sepsis. He plotted the trials, ordering them by year of publication and methodologic quality.[8] Trials evaluating surrogate outcome measures, such as physiologic end points, appear to be increasing in quality, but trials evaluating the outcome of mortality are increasing in quality even more quickly (Fig. 4). This is good news for consumers of the literature. Quality does not always equate with sample size, so the data showing improved quality of publications do not reflect *larger* trials, but instead reflect *better-quality evidence*, which is good news for our patients and for us.

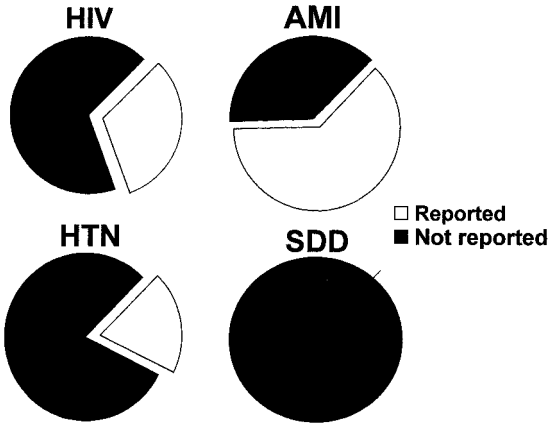


Fig. 3. Clinical adverse events. This schematic shows the proportion of studies in 4 different fields reporting adverse effects of the interventions tested in randomized trials. HIV = human immunodeficiency virus. AMI = acute myocardial infarction. HTN = hypertension. SDD = selective digestive decontamination. (Data from Reference 7.)

Systematic Reviews

Systematic reviews are the fastest growing type of publication. They differ from narrative reviews in that they address a very specific question and they involve explicit study-selection criteria, rigorous critical appraisal of the studies they summarize, and a synthesis of the study results, which may be qualitative (textual summary) or quantitative (pooled result of all studies). A qualitative summary of study results yields a *systematic review*. A quantitative summary of study results makes a systematic review a *meta-analysis*. In other words, a meta-analysis is only different from a systematic review in that the results of the studies summarized are pooled quantitatively into one estimate of effect, with confidence intervals.

With respect to systematic reviews we are now seeing a trend in the literature toward improved, more transparent reporting. Schematics for their presentation arise from the Quality of Reporting of Meta-analyses (QUOROM) statement.[9] Meta-analyses used to be badly reported, but the primary articles included in the meta-analysis can be well represented in a flow diagram, in which potentially relevant randomized trials are identified and screened, and some are excluded, with reasons why. Figure 5 illustrates a system for considering and screening studies; it shows which studies were included and why. That system is one advance in the transparent reporting of meta-analyses.

Table 3 summarizes meta-analyses that pooled the results of randomized trials of SDD.[10–16] These SDD trials have been summarized quantitatively using various metrics, such as odds ratio, risk difference, and relative risk. In this case an odds ratio of 1 indicates that SDD had no influence at all on the outcome. The main outcome that SDD is designed to avert is ventilator-associated pneumonia. The published meta-analyses indicate that SDD reduces the incidence of ventilator-associated pneumonia;

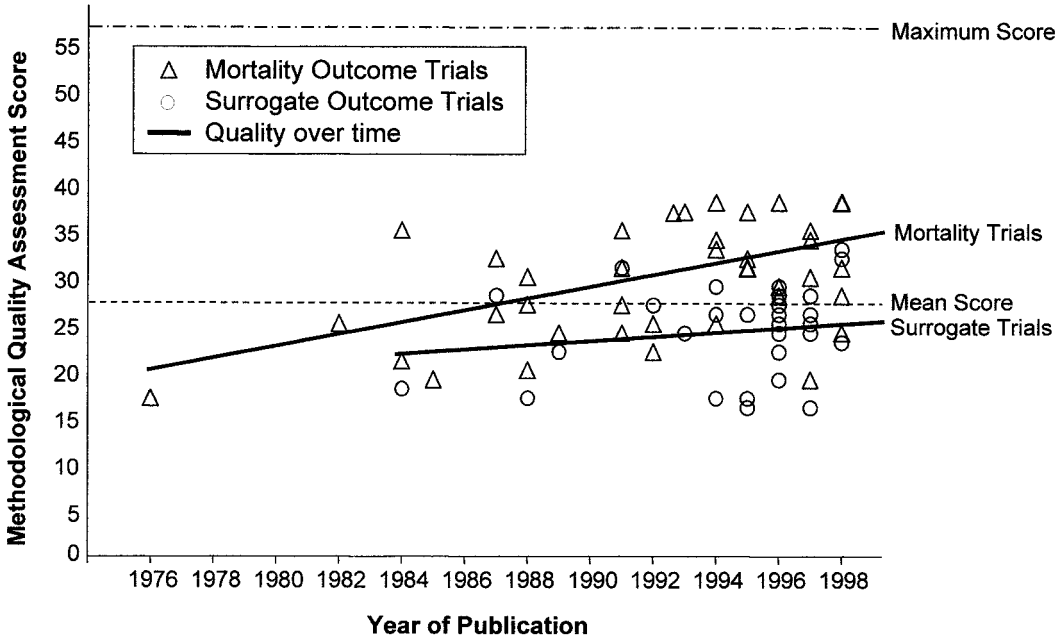


Fig. 4. Methodological quality assessment score (MQAS) of all evaluated sepsis trials, plotted according to the year of publication. The difference between the mean MQAS of mortality-based trials (triangles) and surrogate trials (circles) was statistically significant ($p = 0.0006$). The increase in MQAS was only significant for mortality-based trials (increase of 0.58 points per year, $p = 0.0011$). (From Reference 8, with permission.)

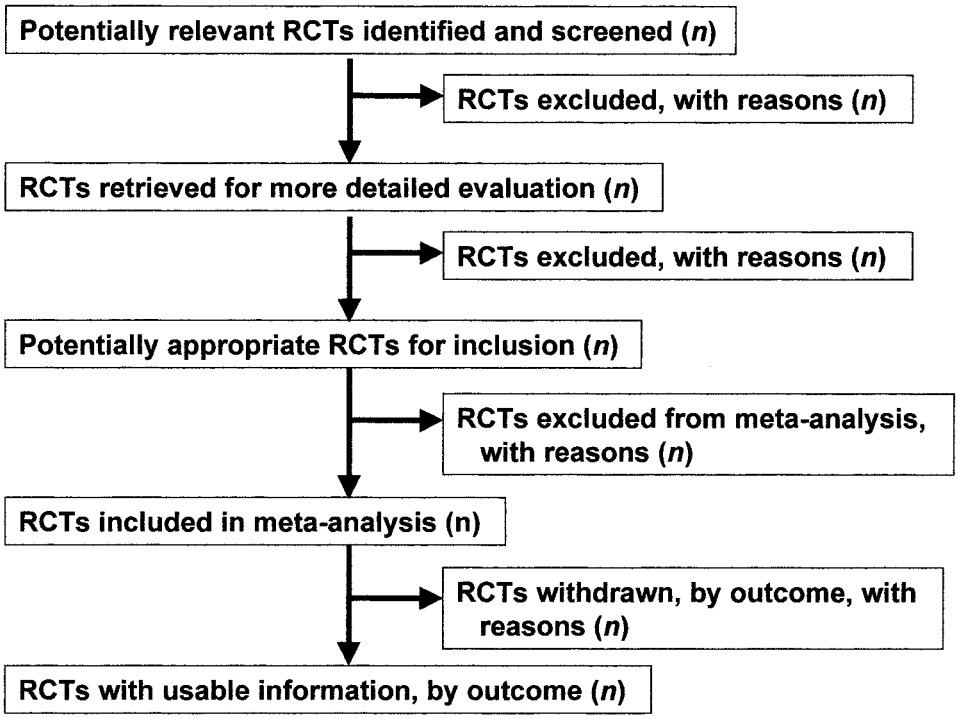


Fig. 5. Flow diagram from the Quality of Reporting of Meta-analyses (QUOROM) statement. The figure shows the flow of studies sought and considered for inclusion in a systematic review. This system of considering and screening studies for review facilitates critical appraisal and understanding of the studies. RCT = randomized controlled trial. (Adapted from Reference 9.)

Table 3. Meta-analysis Results of Studies of Selective Digestive Decontamination

	VAP	Mortality
Vanderbroucke (1991)[10]	OR 0.12*	0.70
SDD Trialists (1993)[11]	OR 0.37*	0.90
Kollef (1994)[12]	RR 0.15*	0.12
Heyland (1994)[13]	RR 0.46*	0.87*
Hurley (1995)[14]	OR 0.35*	0.86*
Cochrane (1998)[15]	OR 0.35*	0.80*
Nathens (1999)[16]	OR 0.19*	0.70*

VAP = ventilator-associated pneumonia.
OR = odds ratio.
*$p < 0.05$.
SDD = selective digestive decontamination.
RR = relative risk.

recent summaries, in which all trials were pooled, also indicate a modest mortality reduction with SDD. However, though SDD has a large body of supporting evidence from randomized clinical trials that were relatively well done, we do not use SDD in North America. Why not? It could be that we're concerned about the cost of antimicrobials in the ICU. It could be that we are concerned about the development of resistant organisms. It could be that we don't actually believe the evidence is really as good as it seems to be. However, in some places in Europe, such as in the Netherlands, SDD is used in daily practice. Therefore, another concept of evidence-based practice is that it is possible for clinicians around the world to look at the same body of evidence and come up with different inferences and, therefore, either embrace it, adapt it, or reject it.

A European group who also evaluated these randomized clinical trials found an important relationship between results and the quality of the trials.[17] For the outcome of mortality, no matter what the methodologic quality of the randomized clinical trials, there seems to be some small mortality advantage from SDD. However, for the outcome of ventilator-associated pneumonia, the lower the methodologic quality, the more enthusiastic were the results of the trials. This has been a signal in the literature for quite a while—that lower-quality randomized clinical trials may be more likely to mislead in the direction of a more enthusiastic estimate of how good the intervention is. Our defense against that is knowing how to properly critically appraise randomized clinical trials and meta-analyses.

One of the most important and contentious subjects in meta-analysis involves apparent discrepancies between meta-analysis of prior (usually smaller) randomized trials and subsequent large randomized trials. One study itemized 12 randomized clinical trials that had been published in 4 major general biomedical journals that have high im-

pact factors. The 12 studies had huge sample sizes and earlier meta-analyses of randomized trials on the same topic. The researchers examined all the outcomes and determined whether the intervention was beneficial.[18] The question was, do meta-analyses of previously conducted randomized clinical trials predict what would happen if we were to retest that intervention today in a large well-conducted trial? The investigators found that there was generally modest agreement between the randomized trials and the meta-analyses. The positive predictive value of meta-analyses was about 65%; similarly, the negative predictive value was about 65%. In 5 out of 40 cases, the meta-analysis was more positive than the randomized trial subsequently conducted. It is important to note that the randomized trials and meta-analyses never gave absolutely opposite results, but there were some differences. This work also shows that evidence is dynamic; it may change over time, so our interpretation of the evidence may change too. Therefore, though we should not discount meta-analyses, we should realize the results may be overturned in time by better research.

One interesting example comes from a study I worked on, which was a comparison of 2 drugs that have been used to prevent bleeding from stress ulceration in mechanically ventilated patients.[19] The 2 most commonly used drugs are sucralfate and histamine-2-receptor antagonists (H2RA) such as ranitidine. When we were designing our trial, we pooled the results of many randomized trials and found that both sucralfate and H2RAs effectively prevent bleeding, compared to placebo or no prevention, although there was a trend that H2RAs may be a little better. However, sucralfate does not affect gastric pH, and it therefore minimizes the emergence of Gram-negative organisms in the stomach and thus lessens the risk of aspiration pneumonia, which has been observed with H2RAs. Sucralfate may therefore be associated with a lower incidence of pneumonia. The Canadian Critical Care Trials Group, in a large randomized trial, found that H2RAs were associated with a statistically significant and clinically important 50% lower bleeding rate, but found only a trend toward a (15%) higher pneumonia rate.[19] In summary, the meta-analysis showed that the 2 drugs were equally good at preventing bleeding, but the large subsequent trial clearly showed that H2RAs were superior at preventing bleeding (thus, the meta-analysis was discordant with the trial). On the other hand, the meta-analysis suggested that sucralfate may be associated with a lower rate of pneumonia than H2RAs, and this was indeed replicated in our randomized trial (thus, the meta-analysis was concordant with the trial). In summary, some meta-analyses will accurately predict what will happen in future randomized clinical trials, and some will not.

Clinical Practice Guidelines

Clinical practice guidelines are systematically developed statements to assist practitioners (and often patients) to choose appropriate health care interventions for specific clinical circumstances. Although they have been developed with the best intentions, these have been unattractive vehicles of information, and clinicians have been worried that guidelines may be too controlling and interfere with practitioner individuality and experience. Pathways and protocols tend to organize and sequence aspects of care for typical patients. The best pathways and protocols are developed with all members of the health care team. Algorithms are more complicated; they involve instructions and decisions and consequences, using branching logic. Most clinicians are using more guidelines and protocols than algorithms. As with all kinds of evidence summaries, it is possible for guidelines, pathways, protocols, and algorithms to be poor; however, it is also possible for them to be evidence-based.

The American College of Chest Physicians, the American Association for Respiratory Care, and the American College of Critical Care Medicine sponsored a summary of the world's evidence on weaning from mechanical ventilation.[20] A large number of practitioners from various disciplines, including respiratory care, got together, and I'm sure you recognize some of these names: Neil MacIntyre, Dean Hess, and Jim Fink. At McMaster University we had 4 RTs helping us with the literature review for this project. When we summarized the literature on how best to liberate patients from mechanical ventilation, we first looked at observational studies. Evidence about what liberates a patient from the mechanical ventilator has been evaluated in observational studies using various clinical indicators, such as respiratory rate and maximum inspiratory pressure. The main outcome is usually successful sustained spontaneous breathing at 48 hours after extubation. The rapid shallow breathing index (the ratio of respiratory frequency to V_T) is among our best predictors of successful liberation from the ventilator. Figure 6 shows the receiver operating characteristic curve for the rapid shallow breathing test.[21]

Another important message for RTs comes from one of the sections of our evidence-based practice review and guideline development exercise, in which we examined the evidence about weaning protocols led by RTs and nurses. Randomized trials[22–24] indicate that these protocols can shorten the duration of ventilation and ICU stay. A Grade A recommendation based on the results of these randomized trials is that weaning and discontinuation protocols implemented by RTs and nurses ought to be better developed and implemented, since this may be one of the best ways to rapidly and safely liberate patients from mechanical ventilation. This is one of the most important

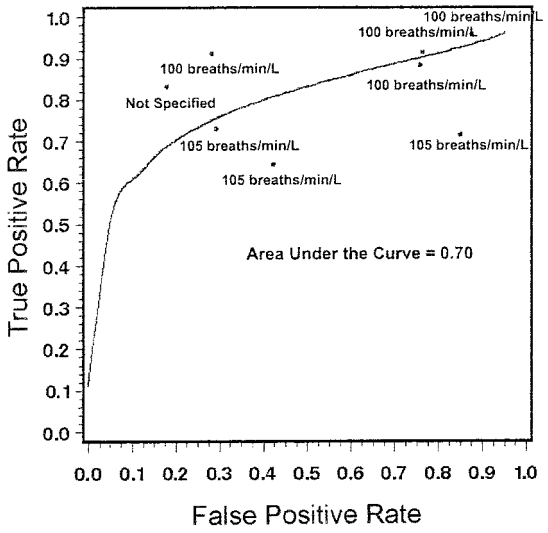


Fig. 6. Summary receiver operating characteristic curve for the rapid shallow breathing index (ratio of respiratory frequency to tidal volume) for predicting successful extubation. (From Reference 21, with permission.)

messages for attendees at this congress: RT leadership can be extraordinarily effective for weaning.

Applying Evidence in Practice

Let's now consider a concrete example about applying evidence in practice. There have been 4 randomized trials of semirecumbency, showing that when mechanically ventilated patients are positioned at 45° in bed (ie, semirecumbent, as opposed to supine), there is less gastroesophageal aspiration.[25–28] In one randomized trial conducted in Spain supine patients had a higher incidence of clinically suspected pneumonia and a higher incidence of microbiologically confirmed pneumonia, suggesting that we should position patients at 45° whenever possible.[28]

As we move from the efficacy world of randomized clinical trials to the real world in which we work, we want to know how to effectively implement semirecumbency, so we need to examine our practice. We conducted a series of studies evaluating semirecumbency, to understand the gap between the randomized trial results and our practice. First, when we studied 4 hospitals in my health care system, we found that very few patients were placed at 30–45°; most were at about 10° from horizontal.[29] We concluded that, despite being aware of the evidence, we don't really apply the strategy of semirecumbency, which could reduce the risk of pneumonia. Second, we conducted a survey of Canadian practitioners and found that semirecumbency was reported to be used by a minority of ICU practitioners. We compared self-reports of French and Canadian ICU directors in a survey that asked who is responsible for applying this evidence.[30] In Canada it was the intensivist, the RT, and the nurse who were considered responsible, but in France the directors stated that decision-making responsibility does not rest with one person—they had a policy. This underscores that sometimes when the responsibility for implementing evidence is diffuse, the evidence might not be very well applied. Third, we evaluated whether ICU clinicians were good at estimating body position (trunk angle above horizontal). Their estimates were compared to the actual flexion at the hip, measured with a goniometer. We found that the clinicians' estimates were not very accurate.[31] If clinicians are not very good at estimating body position, then it is hard to identify patients who need to be repositioned, and this is a barrier to implementing semirecumbency in practice. Finally, via qualitative research we examined reasons patients were not placed in the semirecumbent position, and of course there are many reasons, such as hypotension and pelvic trauma, and these are valid barriers to using semirecumbency.[32] In summary, we found that if we are trying to change practitioner behavior, we need to be aware of the cognitive, behavioral, and environmental influences on evidence uptake. It is only through awareness of those influences and targeting some of them for change that we can increase the use of effective interventions in practice.

I'm going to close with a few words about learning evidence-based practice "from scratch." There was a very important survey published in the British Medical Journal, in which general practitioners were asked about the route forward to evidence-based practice.[33] These clinicians were asked whether they think it is appropriate that busy practitioners learn the key skills of evidence-based practice (eg, searching the literature, learning how to critically appraise studies, and summarizing the evidence). Only about 28% actually use these skills; most respondents said that it is not appropriate to learn evidence-based practice skills from scratch. What they said that they do, and would like to do in the future, is find and use evidence-based summaries. They thought that evidence-based summaries, evidence-based guidelines, and evidence-based protocols could be extremely helpful and are the best way forward. They preferred to look for bottom lines or synopses of evidence than to search for original research. Evidence-based summaries are probably going to play an important role in improving practice.

I hope that in the future we will have better evidence and that RTs will continue to generate some of that evidence, test whether it is applied in practice, and improve the ways in which we can best look after patients. I hope we will all remember that research evidence is just one influence on how we practice. There are many other important influences to keep in mind, such as the availability and cost of interventions, patient preferences, and cultural issues (Fig. 7).

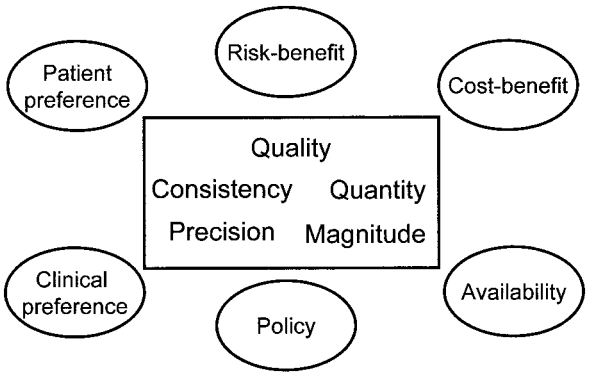


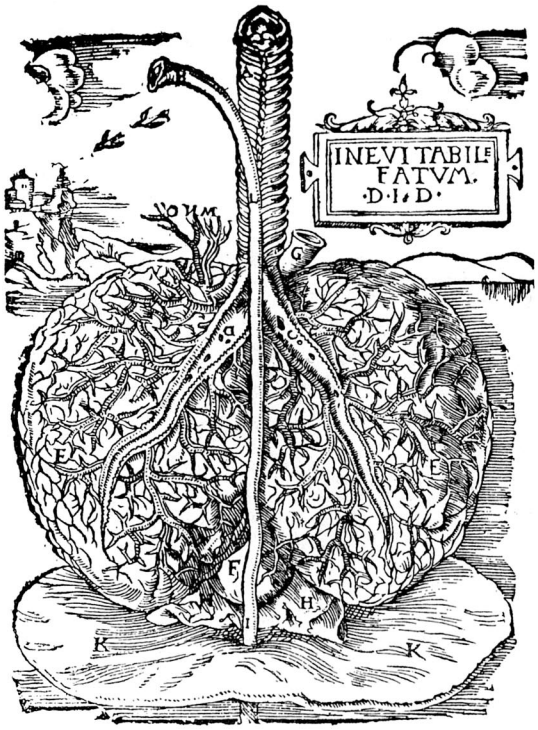
Fig. 7. Considering evidence in practice. This figure shows the influences on clinical decision-making, in addition to issues related to the research evidence.

I'd like to close by once again acknowledging the community of RTs who taught me so much. Thank you for the invitation to join you today.

REFERENCES

1. Sinuff T, Cook D, Randall J, Allen C. Noninvasive positive-pressure ventilation: a utilization review of use in a teaching hospital. CMAJ 2000;163(8):969–973.
2. Merrer J, De Jonghe B, Golliot F, Lefrant JY, Raffy B, Barre E, et al; French Catheter Study Group in Intensive Care. Complications of femoral and subclavian venous catheterization in critically ill patients: a randomized, controlled trial. JAMA 2001 286(6):700–707.
3. Altman DG, Schulz KF, Moher D, Egger M, Davidoff F, Elbourne D, et al; CONSORT GROUP (Consolidated Standards of Reporting Trials). The revised CONSORT statement for reporting randomized trials: explanation and elaboration (review). Ann Intern Med 2001; 134(8):663–694.
4. Gattinoni L, Tognoni G, Pesenti A, Taccone P, Mascheroni D, Labarta V, et al. Prone-Supine Study Group. Effect of prone positioning on the survival of patients with acute respiratory failure. N Engl J Med 2001;345(8):568–573.
5. Ventilation with lower tidal volumes as compared with traditional tidal volumes for acute lung injury and the acute respiratory distress syndrome. The Acute Respiratory Distress Syndrome Network. N Engl J Med 2000;342(18):1301–1308.
6. Rubenfeld GD, Caldwell E, Hudson L. Publication of study results does not increase use of lung-protective ventilation in patients with acute lung injury (abstract). Am J Respir Crit Care Med 2001;163(5): A295.
7. Ioannidis JP, Lau J. Completeness of safety reporting in randomized trials: an evaluation of 7 medical areas. JAMA 2001; 284(4):437–443.
8. Graf J, Doig GS, Cook DJ, Vincent JL, Sibbald WJ. Randomized, controlled clinical trials in sepsis: has methodological quality improved over time? Crit Care Med 2002;30(2):461–472.
9. Moher D, Cook DJ, Eastwood S, Olkin I, Rennie D, Stroup DF. Improving the quality of reports of meta-analyses of randomised controlled trials: the QUOROM statement. Quality of Reporting of Meta-analyses. Lancet 1999;354(9193):1896–1900.
10. Vandenbroucke-Grauls CM, Vandenbroucke JP. Effect of selective decontamination of the digestive tract on respiratory tract infections and mortality in the intensive care unit. Lancet 1991;338(8779): 1389–1390.
11. Meta-analysis of randomised controlled trials of selective decontamination of the digestive tract. Selective Decontamination of the Digestive Tract Trialists' Collaborative Group. BMJ 1993;307(6903): 525–532.
12. Kollef MH. The role of selective digestive tract decontamination on mortality and respiratory tract infections. A meta-analysis. Chest. 1994;105(4):1101–8.
13. Heyland DK, Cook DJ, Jaeschke R, Griffith L, Lee HN, Guyatt GH. Selective decontamination of the digestive tract: an overview. Chest 1994;105(4):1221–1229.
14. Hurley JC. Prophylaxis with enteral antibiotics in ventilated patients: selective decontamination or selective cross-infection? Antimicrob Agents Chemother 1995;39(4):941–947.
15. D'Amico R, Pifferi S, Leonetti C, Torri V, Tinazzi A, Liberati A. Effectiveness of antibiotic prophylaxis in critically ill adult patients: systematic review of randomised controlled trials. BMJ 1998; 316(7140):1275–1285.
16. Nathens AB, Marshall JC. Selective decontamination of the digestive tract in surgical patients: a systematic review of the evidence (review). Arch Surg 1999 Feb;134(2):170–176.
17. Van Nieuwenhoven CA, Buskens E, van Tiel FH, Bonten MJ. Relationship between methodological trial quality and the effects of selective digestive decontamination of pneumonia and mortality in critically ill patients. JAMA 2001 286(3):335–340.
18. LeLorier J, Gregoire G, Benhaddad A, Lapierre J, Derderian F. Discrepancies between meta-analyses and subsequent large randomized, controlled trials. N Engl J Med 1997;337(8):536–542.
19. Cook D, Guyatt G, Marshall J, Leasa D, Fuller H, Hall R, Peters S, et al. A comparison of sucralfate and ranitidine for the prevention of upper gastrointestinal bleeding in patients requiring mechanical ventilation. Canadian Critical Care Trials Group. N Engl J Med 1998; 338(12):791–797.
20. Evidence-based guidelines for weaning and discontinuing ventilatory support. Chest 2001;120(6 Suppl):375S–484S.
21. Meade M, Guyatt G, Cook D, Griffith L, Sinuff T, Kergl C, Mancebo J, Esteban A, Epstein S. Predicting success in weaning from mechanical ventilation. Chest 2001;120(6 Suppl):400S–424S.
22. Ely EW, Baker AM, Dunagan DP, Burke HL, Smith AC, Kelly PT, et al. Effect on the duration of mechanical ventilation of identifying patients capable of breathing spontaneously. N Engl J Med 1996; 335(25):1864–1869.
23. Kollef MH, Shapiro SD, Silver P, St John RE, Prentice D, Sauer S, et al. A randomized, controlled trial of protocol-directed versus physician-directed weaning from mechanical ventilation. Crit Care Med 1997;25(4):567–574.
24. Marelich GP, Murin S, Battistella F, Inciardi J, Vierra T, Roby M. Protocol weaning of mechanical ventilation in medical and surgical patients by respiratory care practitioners and nurses: effect on weaning time and incidence of ventilator-associated pneumonia. Chest 2000;118(2):459–467.
25. Orozco-Levi M, Torres A, Ferrer M, Piera C, el-Ebiary M, de la Bellacasa JP, Rodriguez-Roisin R. Semirecumbent position protects from pulmonary aspiration but not completely from gastroesophageal reflux in mechanically ventilated patients. Am J Respir Crit Care Med 1995;152(4 Pt 1):1387–1390.
26. Ibanez J, Penafiel A, Raurich JM, Marse P, Jorda R, Mata F. Gastroesophageal reflux in intubated patients receiving enteral nutrition: effect of supine and semirecumbent positions. JPEN J Parenter Enteral Nutr 1992;16(5):419–422.
27. Cook DJ, Meade MO, Hand LE, McMullin JP. Toward understanding evidence uptake: semirecumbency for pneumonia prevention. Crit Care Med 2002;30(7):1472–1477.

28. Drakulovic MB, Torres A, Bauer TT, Nicolas JM, Nogue S, Ferrer M. Supine body position as a risk factor for nosocomial pneumonia in mechanically ventilated patients: a randomised trial. Lancet 1999; 354(9193):1851–1858.
29. Reeve BK, Cook DJ. Semirecumbency among mechanically ventilated ICU patients: a multicenter observational study. Clin Intensive Care 1999;10:241–244.
30. Cook D, Ricard JD, Reeve B, Randall J, Wigg M, Brochard L, Dreyfuss D. Ventilator circuit and secretion management strategies: a Franco-Canadian survey. Crit Care Med 2000;28(10):3547–3554.
31. McMullin JP, Cook DJ, Meade MO, Weaver BR, Letelier LM, Kahmamoui K, et al. Clinical estimation of trunk position among mechanically ventilated patients. Intensive Care Med 2002;28(3):304–309
32. Cook DJ, Meade MO, Hand LE, McMullin JP. Toward understanding evidence uptake: semirecumbency for pneumonia prevention. Crit Care Med 2002;30(7):1472–1477.
33. McColl A, Smith H, White P, Field J. General practitioners' perceptions of the route to evidence-based medicine: a questionnaire survey. BMJ 1998;316(7128):361–365.



Lungs, trachea, bronchi, and esophagus from Johannes Dryander,
Anatomiae hoc est corpor is humani dissectionis, Marburg 1537
From: *The Fasciculo di Medicina, Venice, 1493,*
with an introduction etc by Charles Singer, Part I. Florence, Italy: R Lier & Co; 1925.
Courtesy Health Sciences Libraries, University of Washington