

Developing and Exercising the Language of Airway Management

IN this issue of ANESTHESIOLOGY, investigators from different parts of the globe, Kitamura *et al.*¹ from Japan and LeGrand *et al.*² from the United States, assess the biomechanics of direct laryngoscopy in anesthetized patients. Regardless of their results, the mere presence of these articles is welcomed, in large measure because, directly or tangentially, both address one of the most disturbing problems in the delivery of anesthesia: managing the difficult airway. Indeed, airway loss is the basis of some of the most tragic patient outcomes in the entire practice of anesthesiology, and lawsuits related to airway loss yield some of the greatest payouts by defendants.³ Despite the focus of modern medicine to reduce the number of mishaps and improve outcomes, and the desire of organized anesthesiology to lessen the number of airway catastrophes, one can only wonder why we anesthesiologists, in the words of Kitamura *et al.*,¹ still “lack fundamental knowledge of the mechanics of difficult laryngoscopy despite its clinical significance.”

In the past quarter of a century, we anesthesiologists have seen the introduction into clinical practice of fiberoptic scopes, laryngeal mask airways, intubating stylettes, light wands, a host of new laryngoscope designs, and percutaneous tracheotomy devices. Elsewhere, we have seen progressive efforts to improve practitioners' application of those devices. Algorithms have been introduced to guide practitioners of all levels of experience in their assessment and care of difficult airway patients.⁴ Amusingly, these algorithms share with other treatment algorithms the quality of *training* one to act properly, regardless of whether the practitioner is *educated* as to the foundation reasons for the action.

If there is a major oversight in our ability to advance the science and practice of difficult airway management today, it rests largely with our failure to develop an

appropriate, anesthesia-relevant language for the problem, amass the knowledge behind that language, and exercise both daily. Highly developed language allows us to more effectively describe, interpret, and store information, and develop solutions to problems. The more knowledge we gain, the more the language becomes nuanced and informative. Further, the relation between language development and knowledge is bidirectional: New language helps to describe new observations, but also new knowledge is sought when existing language proves inadequate. One need only examine the history of modern physics or the study of human immunodeficiency virus/acquired immunodeficiency syndrome or prion-related diseases to see that this is true.

Unfortunately, these concepts of language development are currently underused as we anesthesiologists study and clinically manage difficult airways. Instead, many of us remain locked in the crude language of “bag the patient,” “anterior larynx,” “small mandible,” and “big tongue,” and this coarse, indiscriminating language is costing us. Ask the typical laryngoscopist to discuss the effect of head positioning on Kitamura *et al.*'s “submandibular space”¹ or the meaning of LeGrand *et al.*'s “maximal segment craniocervical motion,”² and the expected response will be, “What?” But these failures of language unfortunately do not begin with the complex terminology of biomechanics. Hand a beginning or intermediate student of laryngoscopy a stack of pillows and ask her to place the supine patient in a “classic sniffing position,” and all too often one will be amazed at the results. Or read an anesthesia record describing a difficult or failed orotracheal intubation, and the description is often so lacking that the reader is left wondering whether the failure was the result of challenging patient anatomy, a less-than-ideal performance by the laryngoscopist, or both. Such deficient descriptions provide little guidance when preparing a more appropriate plan for the next intubation.

The language used to characterize and analyze problems directs us toward the language of problem resolution, and this can work to our favor or detriment. Indiscriminate, inaccurate language leads to miscommunication of core ideas from one practitioner to another, and—in medical care—miscommunication leads to inefficiencies and errors.^{5*} Despite this, we anesthesiologists have done little to develop utilitarian airway language, and, further compounding the problem, we have insufficiently borrowed from the language and knowledge of other specialties. However, we must recognize that borrowing language and concepts from others will not entirely solve our predicament, because other specialists do not place the same

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* Computers may help fix doctors' handwriting. USA Today. Available at: http://www.usatoday.com/tech/news/techinnovations/2003-09-23-dr-writing_x.htm. Accessed September 10, 2007.

demands on that knowledge (and its language) that we anesthesiologists do. Our need to manipulate the human anatomy to safely secure an airway, and to do so in a manner that achieves secondary clinical goals (e.g., achieving ingress to the trachea through a passage other than the mouth; minimizing alterations in systemic physiology while performing airway management), are somewhat unique to the anesthesiology perspective. As such, our perception and understanding of airway anatomy and function will differ from that of the surgical oncologist asked to resect a head-and-neck cancer. Hence, we must conduct our own research (as Kitamura *et al.*¹ and LeGrand *et al.*² have done) so that the results address our special needs.

Once we develop appropriate language, we anesthesiologists and anesthesia educators need to be more active in teaching it to others. Here we have been horribly remiss. One of the greatest obstacles in teaching the language and concepts of airway management to our trainees results from the fact that we do not sufficiently exercise that language in daily scholarly discussions and, when the language has its greatest utility in addressing the high-risk airway, that teaching opportunity often finds the practitioner under considerable stress and focused on immediate technical success, not long-term understanding of the root cause of a problem. This, in turn, results from the fact that the greatest concerns about failed airway management do not envision the consequences of minor injury to a large number of patients, but instead a large amount of injury to a few patients.³ Because of the rarity of the best “teaching moments,” and the clinical exigencies that preclude one from actually stopping patient care to adequately teach, we cannot solely rely on the hit-or-miss experiences of patient encounters during a training program to optimally prepare our anesthesiology trainees. Instead, we need to expose them to the latest in education techniques, such as can be provided by instructional videos (using anatomical dissections, cinefluoroscopy, dynamic graphic illustrations, and so forth) and participation in simulation center instruction. If the costs of such resources prevent their development at all medical centers that have anesthesiology training programs, training program guidelines should encourage trainees to travel to resource centers where analytic, technical, and communication skills can be improved. Indeed, this model of having trainees travel is already used to more appropriately expose them to populations of special-needs patients (e.g., pediatric patients).

The concept of using language development and verbal communication to advance our specialty is nothing new. The American Board of Anesthesiology, in scoring candidates on oral examination performance, uses five criteria deemed “qualities and attributes which are fundamental to performance as a Board certified anesthesiologist.” One of

these addresses the importance of communication: “Ability to communicate effectively about those issues of specific relevance to anesthesia care and also those topics of general medicine which are crucial to the care of patients with diverse diseases.”[†] Indeed, the American Board of Anesthesiology has recognized that professionalism is not attained simply by being able to perform a task (e.g., anesthetic delivery, airway management); instead, we American Board of Anesthesiology diplomates must show some mastery of the language of the specialty, so that we can communicate effectively with other anesthesia providers, physicians from other specialties, and patients and their loved ones. Such mastery of language and effective communication help to distinguish the expert from the technician and are critical to the attainment of professional respect in the broader medical community.

In the context of anesthesiologists’ approach to dealing with the complex airway, we have all too often been accepting of the successes of the skilled technician, and we have often gotten away with this approach because—with proper preparation and the development of contingency plans—a technician’s approach has sufficed more often than not. This is largely because the failure of one approach to airway management typically does not preclude trying another and another from the operator’s “bag of tricks”⁶ until a solution is secured. However, such an approach is not ideal for advancing our understanding of the problems at hand or identifying broadly applicable solutions. Taking a more scholarly approach, whereby we are able to better verbalize our observations, analysis, and treatment plans, will provide many rewards, including improving our patients’ and other physicians’ faith in our abilities and professionalism.

In closing, we anesthesiologists need to demand that we and our trainees appropriately introduce and use the language of the difficult airway and understand its meaning, and we need to approach our educational mission as if life itself depended on our success. Because, in reality, for some complex patient at some future date, that will indeed be the case.

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Acute Renal Failure in a General Surgical Population

Risk Profiles, Mortality, and Opportunities for Improvement

IT is devastating to both patients and anesthesiologists when patients, with no evidence of renal dysfunction preoperatively, develop acute renal failure (ARF) after surgery. What could have been done differently to avoid this complication? This question has been explored extensively in cardiac surgical patients, and many preoperative risk stratifications and intraoperative management protocols for this unique patient population have been proposed and tested. However, until now, no study has evaluated risk factors for ARF in a large general surgical population. In this issue of the Journal, Kheterpal *et al.*¹ attempt to identify risk profiles in the large general surgical population of the University of Michigan and have reported their analyses of perioperative data collected over a recent 3-yr period.

This interesting study is based on information retrieved from a computerized database used for all surgical patients at the university between 2003 and 2006. A total of 65,043 patient charts were screened, and 15,102 episodes of care were included in the study. Kheterpal *et al.*¹ evaluate the propensity of patients with certain risk factors or exposures to specific intraoperative variables to develop ARF. After extensive analyses, they developed propensity scores related to specific preoperative risk factors or their combinations that might help to predict the risks of unique patients to develop ARF after surgery. They also observed that the perioperative onset of ARF in patients with previous normal renal function was associated with increased postoperative mortality. The risk profiles for perioperative ARF and the association between perioperative ARF and mortality have been previously reported in cardiac surgical patients but not in a general, noncardiac surgical population.

This information is useful if it can help us to assess the preoperative risk of our patients and, hopefully, prevent their development of ARF. An appraisal of the value of this study to clinical practice requires critical scrutiny of their data and a general evaluation of the nature of observational studies. These are the important questions that need to be asked.

This Editorial View accompanies the following article: Kheterpal S, Tremper KK, Englesbe MJ, O'Reilly M, Shanks AM, Fetterman DM, Rosenberg AL, Swartz RD: Predictors of postoperative acute renal failure after noncardiac surgery in patients with previously normal renal function. *ANESTHESIOLOGY* 2007; 107: 892-902.

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First, are these results applicable beyond the University of Michigan? By the nature of their dominant referral practices, academic medical centers tend to attract surgical populations that do not match well with nonacademic practices. In this case, the authors have reported their results as observations. A commonly understood strength of observational study designs and reports is their higher likelihood of generalizability.² This aspect is particularly good in this study because preoperative renal function was assessed and found to be within normal ranges in the final selected population. There are, however, a few areas of concern. No preoperative renal function measures were made for 6,534 of the patients in the database and available for study; therefore, they were excluded from the study. If these patients had a low risk of developing ARF, their exclusion may have falsely increased the calculated risk of developing ARF in a general surgical population. Neither American Society of Anesthesiologists physical status, a simple method of assessing generalizability, nor ethnicity, hypothesized to affect a person's risk for ARF, is reported. In addition, definitions for certain comorbidities and risk factors used during the study period do not seem to have been very precise, and there is no evidence that practitioners at the university have undergone specific training in their application. For example, what constituted "coronary artery disease" in these patients? How was pulmonary hypertension defined? Validity testing of this database has not been reported; therefore, the results must be interpreted cautiously. Fortunately, the large patient population and the lack of nonrenal limitations for inclusion into the study population provide readers with a sense of reassurance that the study's results are reasonably generalizable.

Second, are the application and interpretation of statistical tests appropriate for this type of study? Propensity score methodology, an approach generally used to strengthen causal claims in observational studies, was applied to determine seven independent risk factors for developing postoperative ARF and, ultimately, a risk stratification tool. As with all procedures for statistical evaluation, the analysis of variability with propensity scores has limitations. In the case of propensity score methodology, the analysis is limited by the omission of unknown independent variables or risk factors. It is not possible to know what other variables existed that should have been included in the models. However, the observational design used in this study is a reasonable approach for this type of study, and propensity score modeling is an appropriate test for statistical evaluation. As more institutions develop and acquire the ability for computerized medical records and share their information, we can expect more opportunities for large observa-

tional studies. We will need to continue to better understand the nature and limitations of these types of studies as we attempt to determine the applicability of their results to various clinical practices.

The most useful conclusions of the article by Kheterpal *et al.* seem to center around the identification of unique risk factors, one of which is body mass index greater than 32 kg/m², with this factor being independent of diabetes or hypertension. Obese patients come with their own unique set of challenges, and this finding reinforces the overall increased risk associated with obesity. Liver diseases and chronic obstructive pulmonary disease necessitating chronic bronchodilator therapy are also identified as independent risk factors; however, neither of these factors, as they have been analyzed, is defined sufficiently well enough to help distinguish between the perioperative ARF risks of patients with varying degrees of dysfunction (*e.g.*, modest compared with severe) or types of disease (*e.g.*, cirrhosis compared with hepatitis for liver disease). Not surprisingly, risk of postoperative ARF is found to rise significantly with increasing number of risk factors. The hazard ratio for developing ARF with three or more of the seven most highly associated risk factors was found to be 16.0 (95% confidence interval, 8.9–28.8), a clinically significant ratio. Even with the noted limitations and a modest sensitivity and specificity (*i.e.*, an area under the curve of 0.73 ± 0.03; a very sensitive and specific association would have a value approaching 1), the proposed scoring system has the potential to improve our patient care. The risk identification and stratification can be applied as a guide for preoperative discussions with patients, as well as a tool to guide further quality assessment and improvements within individual practices.

Finally, if this study only identifies potential risk factors, why should any of these findings matter to busy

practitioners? The identification of potential risk factors in this observational study provides information that clinical researchers can use to design additional prospective studies that will help to elucidate the etiology of ARF in unique patient groups and generate hypotheses and clinical trials on how to avoid this complication in the future. Reducing the surprisingly high frequency of ARF in our general noncardiac surgical populations is important; the authors have shown a significantly increased frequency of postoperative death within 1 yr in their surgical patients who developed postoperative ARF. Yes, there are issues that weaken their mortality conclusions. Patients who developed ARF had significantly higher body mass indices and proportionately more chronic obstructive pulmonary disease than those who did not develop ARF. Nonetheless, their finding that all-cause 30-day, 60-day, and 1-yr mortalities are quite a bit higher in patients with perioperative ARF is compelling and worthy of additional study. Kheterpal *et al.* should be congratulated for carefully using their institution's large databases and reporting potential risk factors that we hope will lead to further investigations and, someday in the future, improvements that may decrease this major perioperative complication.

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P6 Stimulation

A New Approach to an Ancient Technique

IT is highly unlikely that any single agent will ever be 100% effective in preventing postoperative nausea and

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vomiting (PONV); however, multimodal approaches to the management of this vexing problem have shown excellent results.¹ The failure of monotherapy is not surprising given the complex nature of PONV with the myriad pathways and receptors involved in this physiologic process.² Clinicians continue to search for other techniques and strategies to add to the currently available therapeutic armamentarium. In this issue of *ANESTHESIOLOGY*, Arnberger *et al.*³ have evaluated a simple but surprisingly effective technique that will likely have widespread clinical applicability. The approach studied by Arnberger *et al.* uses a standard

neuromuscular blockade monitor as a means of stimulating the P6 or Neiguan acupuncture point. Their approach is readily adaptable to virtually any patient undergoing a general anesthetic. There is little doubt that electrostimulation of the P6 acupuncture point is effective in reducing PONV⁴; however, until now, it has been necessary to use proprietary devices,⁵⁻⁷ which add not only to the cost of providing care but also add more complexity to the already dizzying array of equipment used in administering general anesthesia. By placing the electrodes over the median nerve at the wrist (*i.e.*, the P6 point; see the text of the article for the details) the authors were able not only to assess the degree of neuromuscular blockade throughout the anesthetic, but also to provide stimulation of the P6 point for the duration of the anesthetic.

In their study, the authors have chosen to limit the study population to females undergoing gynecologic or abdominal laparoscopic surgery. Although the exclusion of males might be viewed with concern by some, a more homogeneous study population does have merit, particularly in a proof-of-concept study such as this. By its very nature, this study population does have a higher risk of developing symptoms postoperatively, which increases the likelihood of detecting a difference between groups, if in fact a difference does exist. The best data currently available indicate that interventions to limit PONV likely result in a relative risk reduction rather than an absolute risk reduction.⁸ The authors have evaluated nausea and vomiting as categorical data endpoints. Their endpoint of PONV is defined as any episodes of vomiting or retching, or a patient report of any nausea. Data collection was conducted during 24 h, with the time course subdivided into early (0-6 h), late (6-24 h), and overall (0-24 h). In addition to the PONV endpoint, the authors also evaluated both nausea and vomiting as distinct endpoints. It is the separate endpoint (*i.e.*, nausea *vs.* vomiting) by time period evaluation that provides the most meaningful information. P6 stimulation resulted in a reduction in nausea from 51% to 33% during the 0-6 h time period. This translates to a relative risk reduction of 35% and number needed to be treated of 5.6. The reduction in emesis did not reach statistical significance during any of the time periods evaluated. Although it is always suspect to ascribe "significance" to data that are not "statistically different," in all cases the "trend" was toward less vomiting in the P6 group. Nevertheless, this preponderant effect of P6 stimulation on nausea rather than vomiting is in keeping with the findings of other studies.⁴ Because the PONV endpoint chosen by the authors is actually a composite of both nausea and vomiting, it is not surprising that overall PONV reached statistical significance primarily as a result of the impact

of P6 stimulation on nausea during the 0-6 h time period. With this caveat in mind, it is still interesting to compare these findings with data for more conventional pharmacologic approaches to the prevention of PONV. Ondansetron, dexamethasone, and droperidol have all been shown to result in an approximate 25% reduction in PONV (using the same definition as in the current study).⁸ The relative risk reduction for overall PONV, as reported here, is 26%!

There are several aspects of this study that are particularly worth noting. First, the authors' study design used an active control group, not just a "sham" control as has been occasionally used in the past.^{5,6} The active treatment group had the electrodes placed over the median nerve, whereas the control group had the electrodes placed over the ulnar nerve. Both groups had single twitch stimulation at 1 Hz and a constant current of 50 mA applied throughout the duration of the anesthetic. Because electrical stimulation that was delivered was the same for both groups, the only difference was electrode placement. In fact, the point of application was separated by approximately 2 cm. This study design should provide further assurance to anyone who might still be skeptical about the efficacy of P6 stimulation for preventing PONV.

Second, the authors have again confirmed that P6 stimulation is particularly effective in decreasing the incidence of postoperative nausea,⁴ a finding that is of considerable clinical importance given that the majority of currently available pharmacologic agents seem better at reducing the incidence of vomiting, rather than preventing the extremely unpleasant sensation of nausea. This is particularly true for the 5-hydroxytryptamine type 3 antagonist class of drugs, (*e.g.*, ondansetron, dolasetron, granisetron, and so forth), which have become the mainstay for the management of PONV, both for prevention and for treatment.

Third, and perhaps of particular note, is the fact that P6 stimulation was applied only during the course of the anesthetic. Despite this, there was substantial carryover of efficacy into the postoperative recovery phase. This phenomenon of carryover of efficacy has been observed previously⁷ and has now been reconfirmed. Although it is possible that there may be added benefits in terms of prolongation of efficacy with continued application of P6 stimulation into the recovery phase after general anesthesia,⁵ this study has demonstrated a benefit even when P6 stimulation is only applied during the course of the anesthetic.

The benefit of P6 stimulation demonstrated by Arnberger *et al.* may seem modest. The impact is mostly on nausea and occurs almost entirely in the first 6 h after surgery; however, this is also the time period during which patients are most likely to experience symptoms.

Furthermore, when compared with other interventions, the relative risk reduction is indistinguishable. And, as noted above, conventional pharmacologic agents seem to be better at reducing vomiting rather than nausea. It is likely that the anti-nausea effect of P6 stimulation is additive to pharmacologic therapy for the prevention of PONV.⁹ Because the technique described by Arnberger *et al.* can be used during virtually any general anesthetic without the need for additional equipment or supplies, it may prove to be an excellent addition to the currently available strategies for limiting or even eliminating PONV.

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Forward-deployed Anesthesiologists and Pain Treatment in Combat Support Hospitals

Making Decisions about Deployment of Anesthesiologists in Support of the Global War on Terrorism

THE primary mission for an anesthesiologist deployed in the US Army is to work in a combat support hospital (CSH) supporting the operating room. One of us (Dr. Harris) is an anesthesiologist and the physician who is currently responsible for making recommendations regarding deployment of anesthesiologists on active duty within the US Army in support of the Global War on Terrorism. The composition of personnel in the CSH includes two anesthesiologists working together with one or more certified registered nurse anesthetists. Because of the availability and high deployment tempo for active duty certified registered nurse anesthetists over

the past several years, the Army anesthesiology community has filled many CSH-certified registered nurse anesthetist billets. This has resulted in a higher-than-usual proportion of anesthesiologists at several CSHs, allowing for the creation of an interventional pain clinic at one location, the Ibn Sina Hospital in Baghdad, Iraq. Major Ron L. White and Colonel Steven P. Cohen had the foresight to prospectively record the treatments rendered at this unique clinic between October 2005 and September 2006, and they detail their findings in this issue of the Journal.¹ There are many types of military missions that necessitate medical support and each of these may require specific specialty involvement. In combat operations, the concern is naturally focused on care of the acutely wounded. As an operation matures and stabilizes, more specific and tailored medical support is often required. It is tempting to conclude from White and Cohen's article that each CSH should have a pain clinic staffed by a physician with subspecialty training in pain medicine. However, no clear link can be made between the high return-to-duty rates they observed and the treatments rendered in this clinic. The

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reality of making decisions about deployment of anesthesiologists is that it will not always be possible to establish a pain clinic staffed by a practitioner with pain medicine subspecialty training within each CSH. Nonetheless, the basic pain interventions rendered in this study should always be available to our military personnel.

Less than 10% of active duty Army anesthesiologists are pain medicine fellowship trained. Although every anesthesiologist is exposed to pain management during training, some have more interest and ability in this area than others. Occasionally, a fully trained pain management specialist is deployed and will further engage the process, but this is the exception rather than the rule. It is not equitable to demand that these few individuals be repeatedly deployed to have full-service interventional pain management support. Deployments are one of the main reasons that many anesthesiologists leave the Army each year. Several years ago, a plan was introduced to make deployments more equitable. In the current operations, active duty Army anesthesiologists are deployed for 6 months at a time. The intent was and is to deploy every eligible anesthesiologist once before deploying an individual a second time. In general, this system has been well received by anesthesiologists. Thus, there is no plan to selectively deploy pain management specialists to each CSH at this time.

In an environment as vast as Iraq, movement of personnel to a level III facility, like that assembled at Ibn Sina Hospital in Baghdad, can be extremely hazardous, and only those with serious conditions should be transported. The authors report that pain management helped many patients, but it is likely that most of those treated in this clinic came from nearby areas and not from a forward operating base. As in any large population (100,000+), there will be pain issues that necessitate medical attention, and that is the value of this report: It clearly demonstrates the common painful conditions that have arisen in our military personnel on active duty in Iraq. Indeed, acute cervical and lumbar radicular pain (often associated with new disc herniations), thoracic back pain, lumbosacral pain associated with facet arthropathy, and groin pains were among the most common conditions treated in this cohort. Our understanding of the consequences of the under treatment of acute pain has grown enormously in recent decades. There seems to be a clear link between the magnitude of acute pain after surgery² or the onset acute herpes zoster³ and the subsequent incidence of chronic pain. Although the actual effectiveness of the treatments rendered the patients treated in the pain clinic at Ibn Sina Hospital cannot be judged from the current article, it is tempting to believe that early treatment of pain may well have reduced the development of chronic pain and

long-term disability in at least a small number of these individuals who received prompt treatment.

Arguably, a general anesthesiologist should have the basic skills to treat the more common ailments (*e.g.*, the majority of the workload in this report came from epidural steroid injections, trigger points, and facet injections). Indeed, the Accreditation Council for Graduate Medical Education requires that all anesthesiology residents have basic training in pain medicine, and this requirement has been expanded to 3 full months of training in pain medicine in the new the Accreditation Council for Graduate Medical Education program requirements that go in to affect in 2008. All Army anesthesiology residents receive significant exposure to pain medicine, and they are more than adequate to deal with basic pain issues. For those with more involved cases, there is a mechanism for evacuation to a large facility for complete evaluation and management services.

It is unclear from White and Cohen's report that there is any link between the treatments rendered in this clinic and the observed high return-to-duty rates. The authors state, "only soldiers who were motivated to return with their units were referred for pain treatment." This type of selectivity indicates that the patient population was highly skewed. These motivated soldiers would be likely to return to duty regardless of the results of their pain intervention. Indeed, there are no real outcomes reported on the patients who were treated, because the majority returned to their units within 48 h after treatment without any subsequent follow-up. The authors also comment that "the return-to-duty rate in this study was likely inflated by the treatment of soldiers who would not have opted for medical evaluation even in the absence of a forward-deployed pain treatment center." Therefore, these limited data alone would not alter deployment plans to establish a forward-deployed interventional pain clinic.

The authors do recognize the limitations of their work and tell us, "Whether our high return-to-duty rate is a function of forward-deployed pain treatment, a carefully selected and highly motivated patient population, or a combination thereof, is a question that needs to be determined." In reality, this type of investigation is unlikely to ever be possible in the context of an ongoing war. Military duty is fraught with dangers and is physically demanding; therefore, acute and chronic pain problems will arise. Assuming that the early and aggressive treatments provided by this innovative pain clinic in Iraq did effectively treat many common pain conditions, it is clear that basic pain management must be readily available for existing personnel and more advanced care must be promptly accessible on a case-by-case basis. The provision of early, effective pain treatment may well prove to be

the most effective strategy to reduce the incidence of chronic, disabling pain among our military personnel. Although it is unlikely that we will ever have enough pain medicine specialists to create pain clinics in every CSH, perhaps these much-needed services can be offered through the development of a pain management medical augmentation team, a group of medical personnel with the needed expertise and training to provide optimal pain care, that could be used in specific situations to provide better interventional pain management support in a mature theater.

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