Antibiotics for Children With Upper Respiratory Tract Infections

To the Editor.—Dr Nyquist and colleagues1 and Dr Gonzales and colleagues2 have addressed the prescription practices of ambulatory care physicians, including pediatricians,1 with particular reference to the degree of inappropriate antibiotic use. The authors reexamine the 1992 National Ambulatory Medical Care Survey (NAMCS) to determine how often antibiotics are prescribed for viral illnesses and conclude that overprescribing is widespread and that family practitioners were somewhat more guilty than pediatricians.

The methods used in these studies are suspect for the following reason: The authors state, “Colds, URIs [upper respiratory tract infections], and bronchitis represent a set of infections that have a viral etiology in the vast majority (>90%) of cases, so ... antibiotics have little or no clinical impact on their resolution.”2 This statement should be clarified. While it is generally agreed that the common cold is a viral condition, there is no International Classification of Diseases, Ninth Revision, Clinical Modification (ICD-9-CM) code for it, as it is an inexact lay term. Therefore, the authors arbitrarily equated “cold” with “acute nasopharyngitis” (ICD-9-CM 460), even though colds usually are not associated with pharyngitis. Thus, the triad of nasopharyngitis, bronchitis, and URI is being characterized as almost uniformly of viral etiology, but there is no credible support for this contention. The 7 studies cited by Gonzales et al2 to prove that URI and bronchitis are always viral did not isolate viruses in these conditions. Experienced practitioners know that bacterial illnesses such as pneumonia may start as URIs. By misidentifying all cases of nasopharyngitis, URI, and bronchitis as viral illnesses, the authors arrive at an improbably high frequency of antibiotic misuse.

In addition, Nyquist et al1 do not consider that antibiotics often are used prophylactically to avoid secondary bacterial infections in patients with preexisting conditions such as mitral valve disease, asthma, chronic obstructive lung disease, and immune deficiency. Because of their overly simplistic methods, Nyquist et al1 fail to provide a reliable critique of the prescribing practices of pediatricians and other ambulatory care physicians.

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To the Editor.—Two of the 3 articles that Dr Nyquist and colleagues1 cited on the ineffectiveness of antibiotics in pediatric URIs are from 1993 and 1996, ie, published 1 to 4 years after the NAMCS data they analyzed.2,3 Certainly, physicians cannot be held accountable for practicing by future standards or evidence.

A larger concern about their article is what to do with the results. There is no doubt that physicians in all specialties overuse antibiotics to treat URIs. I am a family physician and teach in a family practice residency, but I am not, as categorized by Nyquist et al1, a “nonpediatrician.” If family practitioners overprescribe antibiotics to treat URI, it is essential to document that. However, I cannot approach family physicians and report that nonpediatricians overprescribe antibiotics. The article would have been more helpful if family practitioners were analyzed as a separate group rather than grouped as nonpediatricians.

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reassessed in terms of the natural course of the illness, low complication rate, and, at best, marginal effectiveness of antibiotics.1

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To the Editor.—The study by Dr Nyquist and colleagues1 found that nonpediatricians more commonly prescribed antibiotics for children than pediatricians, while the accompanying Editorial by Dr Schwartz and colleagues2 questions the skills of family physicians and general practitioners. However, the assertions by Schwartz et al are specious for 2 reasons. First, the assumption that a specialty is to blame oversates the evidence. Nyquist and colleagues found an association, not a causal relationship, between prescription rates and nonpediatricians. Other variables affecting antibiotic use (eg, severity of illness, follow-up arrangements, patient income) that are not included in the model may be distributed unevenly by specialty. Second, the explanations that Nyquist et al offer for what is wrong with family practice have no basis in fact.

For example, Schwartz et al3 assert that family physicians are inexperienced and “see few pediatric patients.” Quite the contrary—caring for children is routine in family medicine. In 1993, children in the United States made 33 290 000 office visits to family physicians and general practitioners and 69 462 000 visits to pediatrics. One in 4 office visits by children is made to family physicians.3

The authors argue that family physicians prescribe antibiotics to children because they are “accustomed to prescribing antibiotics for adults with similar clinical findings,” implying that family physicians are unaware that etiologic organisms and natural histories differ in children. The authors provide no evidence of this astonishing ineptitude and leave readers wondering why family physicians, practicing with such ignorance, test children for rotavirus, respiratory syncytial virus, and other organisms that rarely affect adults. Perhaps Schwartz et al believe that family physicians give children adult drug dosages based on habit.

Schwartz et al also contend that family physicians are less educated about antibiotics than pediatricians, but they offer no proof (other than an irreprovable study). They allude to “a belief by many [family physicians] that antibiotic overuse has few adverse effects” but offer no documentation of this extraordinary misconception. They say that family physicians are “less confident with their diagnostic skills for children.”

Surely we can get past such parochialism; all specialties undergo intense clinical training, study current literature, and strive to provide the best care possible. Quality improvement requires more enlightened thinking. Answers lie in understanding the clinical circumstances that prompt unnecessary prescriptions, the reasons good clinicians make bad choices, and innovative solutions (eg, prescription checklists, computerized reminders) to overcome human error. If studies confirm that a specialty does have a problem with quality, let us look at facts and not irresponsible speculation to understand why.

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In Reply.—Dr Chen argues that we arbitrarily identified the lay term cold with acute nasopharyngitis. However, our use of the term acute nasopharyngitis as synonymous with the common cold was based on the research taxonomy commonly used for acute respiratory tract illnesses. The common cold is identified with acute nasopharyngitis because the predominant feature is inflammation of the nasopharynx, and proven rhinovirus infections frequently (30%-50% of the time) produce pharyngitis symptoms. In addition, we do not claim that URIs and bronchitis are “always viral,” since Mycoplasma pneumoniae and Chlamydia pneumoniae are uncommon (<10% of the time) bacterial causes of URI and bronchitis syndromes. Nonetheless, until randomized, placebo-controlled trials demonstrate unequivocal efficacy of antimicrobial treatment for colds, URIs, and bronchitis, we believe it is irresponsible to continue to prescribe antibiotics for these conditions in otherwise-healthy patients given the current epidemic in antibiotic-resistant Streptococcus pneumoniae in our communities. Chen also incorrectly states that we failed to take into account preexisting conditions, such as asthma or chronic obstructive lung disease, when in fact our exclusion of these comorbidities was mentioned in our “Methods” section. There were no cases of mitral valve disease or immune deficiency reported in our study sample.

Dr Westfall comments correctly that physicians practicing in 1992 should not be held accountable for studies published subsequently. However, ours is not a new message. Studies that date back to 1950 fail to show any substantial alleviation of symptoms or reduction of complications with the use of antimicrobials for the treatment of URIs. In 1984, Todd et al performed a randomized, placebo-controlled trial of treatment of purulent nasopharyngitis and found no significant difference in the development of complications (otitis media, more severe illness) with empirical antibiotic therapy compared with placebo treatment.

In response to the concerns of Dr Westfall and Dr Brooks, we repeated our analysis using NAMCS data from 1996. Basically the same magnitude and patterns of association persisted. With pediatricians as the referent group, family practitioners (odds ratio [OR], 1.76; 95% CI, 1.1-2.9) and other nonpediatricians (OR, 2.1; 95% CI, 0.8-5.6) were still more likely to prescribe antibiotics for children with colds, URIs, and bronchitis.

We agree with Drs Fahey and Stocks that health care professionals may prescribe antibiotics for URIs because they believe that these agents can reduce complications such as otitis media and pneumonia; however, except for their recently published review evaluating prior studies, data to support this practice are scanty.

We believe that inappropriate antibiotic prescribing is not fundamentally a problem of either an inadequate knowledge base or any particular specialty. The key lies in a better understanding of physician decision making and patient health-seeking behavior, which will enable us to design improved interventions to improve judicious antibiotic use.

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In Reply.—These letters raise several important issues: Is there a benefit of antibiotic therapy for children with cough or colds (nasopharyngitis)? Are antimicrobial use practices of family physicians really different from those of pediatricians and, if so, why? Dr Chen suggests that nasopharyngitis, bronchitis, and URIs may not be viral in etiology. Although some misclassification may occur in the diagnosis of the common cold, a recent study of 200 young adults in Finland who had rhinorrhea, congestion, or sore throat identified a viral etiology in 138 (69%) and a bacterial etiology in 7 (3.5%). Among the latter group, 4 patients had evidence of infection with C pneumoniae, and 1 each had M pneumoniae, S pneumoniae, and nontypable Haemophilus influenzae. Given the unclear benefit of antimicrobial therapy for C pneumoniae infections, the potential benefit of treating these colds would have been minimal. Clinical trials comparing outcome in patients with colds and bronchitis who received antimicrobial therapy or placebo confirm the lack of substantial treatment effect.

Drs Fahey and Stocks suggest a possible role of therapy in decreasing the risk of bacterial complications of a primary viral infection. Although available data cannot rule out a small treatment effect, there remains little rationale for routine therapy of viral infections. A better approach is to communicate with patients about the need for follow-up if illness becomes worse. Dr Brooks and Dr Westfall question whether family physicians truly use antibiotics more often than do pediatricians for children with colds. In an analysis of data from Kentucky Medicaid patients, family physicians were significantly more likely to be high prescribers (in the top quartile of antibiotic prescribers for colds) compared with pediatricians (25 of 39 [64%] vs 4 of 31 [13%], P < .01). These data indicate that, in general, family physicians prescribe antibiotics more frequently but also highlight the variability between individuals within a specialty and illustrate that no specialty is without room for improvement.

Each physician, regardless of specialty, must be encouraged to prescribe judiciously. However, in a public health campaign, it is important to understand the reasons for overprescribing among a population of clinicians and to develop and target appropriate behavior change strategies for that group. Differences between specialties in training, experience, and approach to the patient-physician relationship may affect practices. A survey of Virginia physicians found that family physicians were significantly more likely than pediatricians to prescribe antibiotics in response to a written scenario describing a child with purulent rhinitis. Why? Help us develop a more effective campaign by helping us understand what you think (fax: [410]-639-3970; include your address and we will send the Centers for Disease Control and Prevention’s patient education materials on antibiotic prescribing that physicians can use in their practice.)

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Rehabilitation After Hip and Knee Arthroplasty

To the Editor.—Dr Munin and colleagues1 demonstrated several benefits of early rehabilitation for patients undergoing hip or knee arthroplasty, including decreased hospital stay and more rapid attainment of short-term functional goals. Unfortunately, the authors failed to assess several important factors that influence the postoperative course of these patients. This study focused on early rehabilitation but neglected the perioperative care provided to their subjects. The anesthetic technique, method of postoperative pain management, and strategy for deep venous thrombosis (DVT) prophylaxis can affect recovery from surgery, thereby altering participation in rehabilitation.

Epidural anesthesia, particularly when combined with epidural postoperative analgesia, decreases blood loss, postoperative pain, and the risk of venous thrombosis in patients undergoing hip or knee arthroplasty.2,3 Epidural analgesia extending into the postoperative period can improve progress in rehabilitation and shorten hospital stay.4-6 The benefits of epidural analgesia (decreased pain, improved range of motion, improved ambulation, and less fatigue) may have the most impact on patient outcome when combined with protocols emphasizing early mobilization and early rehabilitation. Previous studies examining epidural analgesia did not examine early rehabilitation, and the study by Munin et al neglects anesthesia and analgesia options in its study design. The authors do not mention anesthetic technique or immediate postoperative pain management. During rehabilitation pain was “aggressively treated” with ice packs applied to the incision and unspeciﬁed narcotic analgesia was given before therapy sessions. Thus, the additional benefits of treatment with aggressive pain control via epidural analgesia, when combined with early rehabilitation, are not known.

Several options exist to reduce the risk of DVT, including warfarin sodium, low-molecular-weight heparinoids, and mechanical devices. Not only does DVT increase the risk of mortality, but treatment of DVT may prevent or limit participation in rehabilitation and add to the length and cost of hospitalization. The 6% incidence of DVT (and subsequent exclusion of patients with DVT from rehabilitation) in this study highlights the importance of this problem.

Numerous factors contribute to the postoperative course in patients undergoing hip and knee arthroplasty and also can promote earlier rehabilitation, shorter hospital stays, and decreased complication rates. Appropriate perioperative management may increase the beneﬁts demonstrated for early rehabilitation or allow more patients to participate in an appropriate rehabilitation program. Epidural anesthesia and analgesia can make important contributions and need to be considered when determining the best overall perioperative and rehabilitative management options for such patients.

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To the Editor.—I disagree with several points raised in the Editorial by Dr Zuckerman7 commenting on the article by Dr Munin and colleagues. When I was an orthopedic resident 25 years ago, it was not unusual to keep patients in the hospital for 2 to 3 weeks following total knee and hip replacement surgery. Hospitals gained from the cost-plus basis of presurgical admissions and long postoperative hospitalizations. The diagnosis related group (DRG) system did change this, but did not change the fact that patients still need time to heal. I agree with Zuckerman that the shorter lengths of stay and the financial incentives from DRGs encouraged the explosion of rehabilitation in the 1980s. But this explosion was fed by the need for the rehabilitation services, which, if not provided at the acute care hospital, was to be done elsewhere. To condemn a hospital for providing a needed service and generating revenue is absurd.

Total joint replacements are most often performed on older patients. These patients often live alone, have spouses with medical or physical problems that limit their contribution to the recovery program, and have more comorbidity (eg, heart disease, prior stroke). These same factors prolong recovery and influence the ability to live independently. Family members and home health agencies provide support, but are limited by the availability, time involved, and therapies needed.

Zuckerman commented that the limited available studies show no significant differences at 6 months with or without rehabilitation treatment following total joint replacements. Outcome measurements performed at end points overlook the quality of life during the interval. For instance, outcome studies for lumbar disk herniation demonstrate that disectomy and nonoperative treatment outcomes are the same after 2 years, but improvements in function, symptom relief, and quality of life are well known.3

The goals of therapy need to focus on how to deliver the best care for each patient in the most efficient way while recognizing that not everyone has the same needs or resources. The art lies with the decision making. Whether the patient should best be treated at home, as an outpatient, or at an inpatient care facility is the most appropriate, should be based not only on the patient and family, but also on the expertise and capabilities of the facility. These components of outcome studies need to be assessed.

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In Reply.—We agree with Drs Botney and Stacey that postoperative pain management can affect rehabilitation performance after joint replacement surgery. We previously demonstrated that patients with higher postoperative self-reported pain scores had decreased functional performance and range of motion during the acute care postoperative stay.1 Because of this finding, all patients in our study were offered patient-controlled analgesia immediately after surgery followed by hydrocodone bitartrate and acetaminophen dosed one-half hour before morning and afternoon therapy sessions.

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Communicating With Patients About Advanced Cancer

To the Editor.—The key finding of the study by Dr Weeks and colleagues suggests that patients with colon cancer and lung cancer overestimate their likelihood of survival, compared with the estimates of the physicians who were more accurate. The authors suggest that this underestimation greatly underestimates the actual risk of death among patients with advanced cancer.

We agree with the principle of the importance of telling the truth in the patient–physician relationship. However, we have data to suggest that there are differences in the patient’s autonomy in the “one-size-fits-all” approach to telling the truth. We consecutively interviewed 100 patients from Memorial Sloan-Kettering Cancer Center, New York, NY, who were referred for consultation to our Pain and Palliative Care Service and who mostly presented with advanced cancer. The purpose of our study was to investigate the effect of psychological and physical symptoms and of prognostic factors on palliative care decisions.

Our data showed that physicians estimated that about two thirds of the patients had a poor prognosis with no hope of survival, yet only 21% of the patients perceived that they had a poor prognosis. The remaining 79% of patients were evenly divided among those who believed that they had a good prognosis, those who were uncertain about their prognosis, and those who felt that they were still actively fighting their disease and had a chance of survival.

When patients were asked about their palliative care decisions (eg, the existence of a durable power of attorney or living will, preference for a treatment focused on life extension vs pain relief, desire for resuscitation) 43% of the sample did not have advanced directives, and those who did had mostly signed them with their families alone. Only 29% of the overall sample wanted the opportunity to discuss palliative care decisions with their physician. Depressive symptoms, avoidant thinking, and patients’ understanding of their prognosis significantly influenced the presence of and desire to discuss palliative care decisions.

Our data suggest that telling the truth may not always be preferred by patients with cancer. In trying to understand why so many patients did not want to discuss palliative care decisions with their physicians, the psychological state, coping style, belief system, and amount of social support of each patient must be considered. For instance, patients may wish to maintain hope and avoid the emotional impact of a full understanding of their prognosis. It may be that the right avenue in communicating prognoses to patients is not always through direct patient-physician discussions. For some patients, it may be most effective to communicate prognostic information with their family or through patient education or support groups.

Perhaps the primary issue is not as clear-cut as whether to tell the truth but to match the delivery, amount, and type of information given with the coping style and psychosocial resources of the patient receiving it.

To the Editor.—The article by Dr Weeks and colleagues on the relationship between patients’ predictions of prognosis and their treatment preferences and the Editorial by Dr Smith and Ms Swisher both use the terms terminally ill cancer patients and terminal cancer.

Twenty years ago, I urged the medical profession to eliminate reference to the terminal patient or to terminal cancer. My contention was that patients with advanced cancer are subjected to no greater cruelty than being referred to as having terminal cancer or being a terminal cancer patient. I emphasized that patients with incurable cancer reach the dying phase of the disease at which point no further definitive anticancer therapy is appropriate. The physician’s role is to demonstrate compassion and provide supportive therapies.

The use of the term terminal should be limited to morticians who make the funeral arrangements, members of the clergy when they deliver the eulogy, end-of-the-line transportation services, such as railroad or bus terminals, and the many new terminal-related illnesses of computer operators. Terminal illness has been defined as an illness that can be expected to cause the patient to die—usually a chronic disease for which there is no known cure. Yet patients with chronic congestive heart disease; advanced diabetes mellitus with retinopathy, neuropathy, and nephropathy; cirrhosis of the liver; chronic obstructive pulmonary disease; or irreversible renal failure (end-stage renal disease) rarely have their disease described as terminal.

The study by Weeks and colleagues indicated that physicians may be able to provide prognostic data with respect to survival. Patients can be told the prognosis for a group of 100 patients with cancers. However, I patient is an individual, not a statistic, and may well be included as a favorable subgroup of that prognostic data.
The caring physician has a major responsibility in educating the patient as to the realistic potential for survival. It is not appropriate to tell patients how many weeks or months they have to live. It is reasonable to stress the incurability of the disease, the lack of specific anticancer therapy or even suitable clinical trials, and the plans for supportive care to attain a desired quality of life in the process of dying.

Terminal cancer should be excluded from physician language and scientific publications, just as the American Cancer Society has urged exclusion of the terms victim and terminal from its programs and publications (Harman Eyre, MD, American Cancer Society, written communication, June 18, 1998).

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5. Stein JS. Terminal is for buses, not patients. Oncol Times. 1990;22.

In Reply.—Dr Costantini-Ferrando and colleagues confirm our findings that patients with cancer tend to overestimate their prognoses. More important, they shed light on how this failure of communication actually may be contributing to patients' suffering at the end of life. We found that patients' perceptions of their prognoses were associated with a preference for life-extending therapy, which, in turn, was associated with higher rates of adverse events in the last months of life. Costantini-Ferrando and colleagues identify a possible mediator for this effect, namely, that patients' perceptions of their prognoses may be driving their willingness and desire to even discuss palliative care. Their observation that avoidant thinking also was associated with an unwillingness to discuss palliative care decisions underscores how difficult it may be to successfully intervene in this dynamic. Our finding that patients who simply understood that there was at least a 10% probability that they might not survive 6 months had substantially different treatment preferences suggests that these avoidant tendencies need not be fully overcome, however, to help patients make treatment decisions that are consistent with their underlying values.

Dr Kennedy's insightful comments remind us of the importance of careful and compassionate choice of language in discussions with patients regarding their prognoses. However, our data do not support his contention that physicians are unable to prognosticate for individual patients, and therefore that discussions of life expectancy, even in general terms, are not appropriate.

Our receiver operating characteristic curve analysis indicated that physicians in our cohort were quite accurate in estimating individual patients' prognoses. In particular, physicians were able to discriminate between patients who ultimately did and did not survive for 6 months. Our findings that physicians are in a position to provide patients with accurate information about prognosis, and that patients who understand this information are less likely to experience adverse events, suggest that withholding such prognostic information from patients, even if it is done with the most altruistic motives, may not be the most compassionate approach to care at the end of life.

As we move toward an evidence-based approach to defining optimal end-of-life care, the need for further research on how to elicit, understand, and honor patients' preferences, regarding not only treatment choices, but also styles and modes of communication about prognosis, is clearly critical.

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In Reply.—I agree with Dr Kennedy that the term terminal seems impersonal, but it is truthful. I do not use terminal unless a patient asks me specifically, but I have always preferred to say, "You have a disease that cannot be cured with medicines." However, it is important to emphasize that data for populations do apply to individuals. If 95 of 100 patients with advanced pancreatic cancer will be dead within 1 year, then the patient should know that. If all that the patient hears is "You have pancreatic cancer" and "Some people do well, after all, not everyone is the same," they have not been given decent information.

I agree with Dr Costantini-Ferrando and colleagues that "one-size-fits-all" discussion does not fit all. But I have heard too often "They never brought it up" or "I thought it would disturb them" as reasons that a patient's incurable disease never was discussed. If physicians try to tailor everything to personality style, I suspect we will err quite frequently, and this approach might give yet another excuse to avoid the issue. The only way to know what patients want is to ask them.

The most important part of the discussion about dying is to start one, just as the biggest hurdle of going into a dying person's room is to enter the room and sit. The discussion can then start, "What would you like to know about your illness?" and lead to the finer points about prognosis, what can be done, and what cannot be done.

 Granted, none of this is pleasant for physicians or for patients. The most difficult aspects of cancer care are those transition points when active therapy can no longer help, and clinicians must help patients to switch from "I could be in that 5 in 100 group" to planning for end of life. After all, planning for end of life if the patient is in the 95 of 100 group does not diminish his or her chances of living longer. Until physicians admit how difficult it is and learn these important skills and have societal expectations that not all diseases can be cured, patients and physicians will continue to fight battles that cannot be won and are lost with too much suffering.

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CORRECTION

Error in Table.—An error occurred in the Clinical Investigation entitled “Human Cardiovascular and Metabolic Response to Acute, Severe Isovolemic Anemia” published in the January 21, 1998, issue of THE JOURNAL (1998;279:217-221). On page 218, in Table 1, the median age range under “Patients” should have read “(35-69)” [not (35-39)].