

COST-EFFECTIVENESS ANALYSIS

To the Editor:

I have heard and read the works of Dr Hueston for several years and have been impressed by the scope and quality of his work. Most recently, he conducted an economic evaluation of antibiotic treatment of acute bronchitis. The paper is presented as a cost-effectiveness analysis (Hueston WJ. *Antibiotics: neither cost-effective nor "cough" effective. J Fam Pract 1997; 44:261-5*). I see, however, no measures of quality of life or adverse outcomes avoided. The paper appears to be a pure cost analysis with no measures of benefit or effectiveness.

Additionally, Dr Hueston indicates that the analysis is taken from the perspective of the patient. While this is reasonable, some of his assumptions do not adequately reflect that perspective. Including the cost of an office visit overestimates the cost to the patient. Most of our patients are covered by insurance that pays for office visits, at least in part. This part of the cost is, in fact, irrelevant, since the cost of an initial office visit is the same in each strategy. Additionally, the patient's perspective is one where opportunity costs ought to be included. This would include the economic impact of the time spent in seeking care (eg, child care, transportation, lost wages). These are absent from his analysis.

One could argue that the model is overly simple. It would have been nice to include strata for smokers, patients with underlying pulmonary conditions, and so forth. The paper appears to be a model for treating acute uncomplicated bronchitis in an otherwise healthy adult. If that is the case, the conclusions are probably correct.

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To the Editor:

Dr Hueston's well-organized article in the March issue of the *Journal*¹ presents the scenario of various treatment strategies to treat bronchitis:

1. Treating only patients with persistent cough.

2. Screening patients and treating those patients with positive results with antibiotics.

3. Treating all patients.

His conclusion is to reject the third option unless the prevalence of bacterial infection is greater than 25%.

There is documentation in the medical literature that the prevalence of bacterial infection is greater than 25%. In an article published in the *Journal*,² 25% of patients with acute bronchitis tested positive for *Mycoplasma pneumoniae*. A recent clinical microbiological review documented that the prevalence of acute *Chlamydia pneumoniae* infection was 5% in patients with acute bronchitis.³ In another recent study, 32.7% of 2000 patients with acute bronchitis had positive cultures for bacterial organisms⁴: the most common four organisms were *Haemophilus influenzae*, *Streptococcus pneumoniae*, *Moraxella catarrhalis*, and *Haemophilus parainfluenzae* (tests for *M pneumoniae* and *C pneumoniae* were not done).

The combined experience of these studies suggests that the prevalence of bacterial infection is much greater than 25%, and the option of treating all patients with antibiotics would be the most cost-effective. The failure of previous antibiotic trials to show significant improvement in cough and symptoms compared with placebo may be related to the use of narrow spectrum antibiotics (usually erythromycin) for those trials. Further research is needed to evaluate whether the use of broad spectrum antibiotics is cost-effective in acute bronchitis.

I applaud Dr Hueston for his interesting and helpful analysis of the cost

strategies in this challenging syndrome.

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REFERENCES

- Hueston WJ. Antibiotics: neither cost effective nor "cough" effective. *J Fam Pract 1997; 44:261-5*.
- King DE, Williams WC, Bishop L, Schechter A. Effectiveness of erythromycin in treatment of acute bronchitis. *J Fam Pract 1996; 42:601-5*.
- Thom DH, Grayston JT, Campbell LA, Kuo CC, Diwan VK, Wang SP. Respiratory infection with *Chlamydia pneumoniae* in middle-aged or older adult outpatients. *Eur J Clin Microbiol Infect Dis 1994; 13:785-92*.
- Dere WH. Acute bronchitis: results of US and European trials of antibiotic therapy. *Am J Med 1992; 92(suppl 6A):53S-56S*.

The preceding letters were referred to Dr Hueston, who responds as follows:

In Reply:

Dr Barry raises two issues in his letter. First, cost-effectiveness analysis is a broad term that refers to "the outcome of decision options in terms of their monetary cost per unit of effectiveness."¹ Dr Barry points out that the analysis used for acute bronchitis was simplistic and focused on the eventual outcome, which was recovery from illness. Since all patients recover from acute bronchitis, the outcomes for all strategies were similar, so the decision analysis was reduced to one of cost comparisons. Opportunity costs (such as time lost from work for illness and physician visits), quality of life issues (including the reduced quality of life when individuals are ill), and side effects caused by antibiotics are legitimate other outcomes that could have been factored into the analysis had there been additional data available that estimated these costs. Unfortunately, there are few reliable data that enable us to even guess at

some of these issues.

Dr Barry's comments about stratification based on risk is also a very good point. As he noted, the model assumes a standard risk based on evidence from otherwise healthy adults. Data for patients with pulmonary problems are not available that would allow risk stratification. As far as smoking is concerned, data from my work suggested that smokers and nonsmokers had similar poor responses to antibiotics and responded no differently to bronchodilators.^{2,3}

Dr King's comment that the literature documents a high rate of bacterial infection in acute bronchitis is based on very little data. His own research in one population has shown significant rates of cultures positive for *Mycoplasma*,⁴ but conflict with serologic data that have shown much lower rates of serologic conversion.^{5,6} It is possible that the high prevalence of *Mycoplasma* in his patients with acute bronchitis could indicate colonization with this agent rather than infection, a conclusion supported by his observation that treatment with erythromycin did not improve outcomes any more in patients positive for *Mycoplasma* than in those without the organism.

Dr King's second comment about the prevalence of other bacteria in acute bronchitis refers to a single report based on two series of unpublished data from the Eli Lilly Company that were collected to justify the use of loracarbef for acute bronchitis.⁷ Patients in these trials were prescreened and only eligible for culture when they had sputum that was judged to be more likely to yield positive bacterial cultures. Any conclusions based on these biased data must be suspect.

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REFERENCES

1. Pettiti D. Meta-analysis, decision analysis, and cost-effectiveness analysis. New York, NY: Oxford University

Press, 1994:31.

2. Hueston WJ. Albuterol delivered by metered-dose inhaler to treat acute bronchitis. *J Fam Pract* 1994; 39:437-40.
3. Hueston WJ. A comparison of albuterol and erythromycin for the treatment of acute bronchitis. *J Fam Pract* 1991; 33:476-80.
4. King DE, Williams WC, Bishop L, Shechter A. Effectiveness of erythromycin in the treatment of acute bronchitis. *J Fam Pract* 1996; 42:601-5.
5. Evans AS, Brobst, M. Bronchitis, pneumonia, and pneumonia in University of Wisconsin students. *N Engl J Med* 1961; 265:401-9.
6. Evans AS, Allen C, Sveltman S. *Mycoplasma pneumoniae* infections in University of Wisconsin students. *Am Rev Respir Dis* 1967; 96:237-44.
7. Dere WH. Acute bronchitis: results of US and European trials of antibiotic therapy. *Am J Med* 1992; 92(suppl 6A):53S-56S.

PRAYER IN OFFICE PRACTICE

To the Editor:

The recent editorial on prayer by Philip Magaletta and associates¹ provides a cogent discussion regarding physician roles and responses to religion and spirituality in a clinical context. A precautionary note, one implied but not directly stated by the authors, is needed before crossing over or allowing ourselves to be carried over this "threshold."

In the patient-physician relationship, physicians possess a power that can be divided into three components: Aesculapian, charismatic, and social.² The rise in scientific, empiric research that supports the relationship between religion, spirituality, and health and well-being³ has added to the knowledge base that contributes to Aesculapian power. The positive and salutary effects of this power are substantial,⁴ yet the potential for harmful or misguided use of this power⁵ remains, if unchecked or unguarded by physicians.

Clinicians should be prudent when inquiring about patient belief systems, disclosing their own faith

tradition, or promoting a spiritual intervention, since their religious beliefs will influence patient-physician interactions.⁶ This approach is particularly salient in the twilight of the 20th century. Recent survey data reveal that most Americans are searching for meaning for their lives, and that there is a hunger to experience God.⁷ There is also, however, a growing number of "unchurched" who do not know what they believe or why. This lack of spiritual or religious discernment can manifest itself in a clinical setting, when the disclosure of disease or life-threatening illness, or death, can place patient beliefs in a transient or unstable state.

From an ethical perspective, prayer involving physician and patient, either at the physician's suggestion or the patient's request, is permissible if both parties are willing. This process, which has been called "elective affinity," is consistent with patient autonomy.⁸ In addition, physicians and other care providers should not be deterred from praying for patients privately or nonlocally.⁸

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REFERENCES

1. Magaletta PR, Duckro PN, Staten SF. Prayer in office practice: on the threshold of integration. *J Fam Pract* 1997; 44:254-6.
2. Brody H. The healer's power. New Haven, Conn: Yale University Press, 1992.
3. Larson DB, Greenwold Milano MA. Are religion and spirituality relevant in health care? *Mind/Body Med* 1995; 1:147-57.
4. Benson H, Friedman R. Harnessing the power of the placebo effect and renaming it "remembered wellness." *Annu Rev Med* 1996; 47:193-99.
5. Howe EG. Influencing a patient's religious beliefs: mandate or no-man's-land? *J Clin Ethics* 1995; 6:194-201.
6. Olive KE. Physician religious beliefs and the physician-patient relationship. *South Med J* 1995; 88:1249-55.
7. Gallup GH. The epidemiology of spirituality. Presented at Spirituality and Healing in Medicine II Conference. Los Angeles, California, March 15, 1997.
8. Post SG. Ethics of religion in health care. *Mind/Body Med* 1997; 2:44-8.

The preceding letter was referred to Drs Magaletta, Duckro, and Staten, who respond as follows:

In Reply:

The cautions expressed in Dr Daaleman's perceptive commentary on our editorial are well taken and cannot be stated boldly enough. Self-awareness and careful discernment should always precede inquiry into patients' belief systems, and physicians are encouraged to continue developing objective assessment strategies for determining when and how such inquiries should be made. It is also critical to remember that just as it is possible to unconsciously proselytize a patient to a particular religious system, it is equally possible to "proselytize" for a secular world view.

From an ethical perspective, a range of beliefs currently exists concerning the physician's private prayer for a patient. Some believe that it is appropriate regardless of patient consent, while others believe it is appropriate only with consent.

Still others experience their prayer as a particular way of being in the world. In this instance, physicians and their work become prayerful and cannot be separated from any interpersonal context of which they are a part, regardless of anyone's consent. Whatever one's orientation within this spectrum may be, we must continue to share our thoughts and experiences as Dr Daaleman has generously done. In this way we will further our ability to address religious and spiritual issues naturally and effectively within the healing relationship between doctor and patient.

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PUTTING MEDICAL STUDENTS TO WORK

To the Editor:

I could not help but smile as I read the letter by Scherger and Fowkes in a recent issue of the *Journal* (Scherger JE, Fowkes WC. *Medical student teaching and family physicians' use of time* [letter]. *J Fam Pract* 1996; 43:112). I have continued to teach medical students all the years that I have been in practice, and I have noticed that in the more recent years, they have come to me often with less than adequate skills in the basics, ie, history taking and physical examination.

I feel that to get the students back to "working" in our family practice offices or centers, it will be necessary to have appropriate role models teaching them the basics at the undergraduate level. The present academic curriculum in which students serve some 24 hours a week in our offices and clinics is not enough. Allowing them additional exposure and oppor-

tunity to witness role modeling by older physicians in these sites will make them far better clinicians and better able to attend to their patients' needs after they have graduated either from their medical schools or subsequently from their residency training programs.

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ZINC LOZENGES FOR THE COMMON COLD

To the Editor:

I enjoyed reading the JFP Journal Club review on a cure for the common cold by Drs Stevermer and Adams. (Stevermer JJ, Adams P. *A cure for the common cold?* [journal club]. *J Fam Pract* 1996; 43:346).

I have found a very good tasting zinc lozenge that provides 23 mg of zinc (derived from zinc-free form amino acid complex and zinc gluconate). It is made of whole dried peaches, apricots, honey, and fructose and manufactured by TWIN-LABS, Ronkonkoma, NY. I found these lozenges in a health food store in rural Vermont! They must not be too hard to find, and these taste fine and should not cause need for further study! Patients will most likely choose shorter symptom duration.

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CORRECTION

A JFP Journal Club reviewer's name was spelled incorrectly in the April issue (*Office management of suspected UTI*. *J Fam Pract* 1997;44:342-3). The co-author with Warren P. Newton, MD, on the review should have read Matthew S. Buss, MD. We regret the error.