LETTERS TO THE EDITOR

The Journal welcomes Letters to the Editor; if found suitable, they will be published as space allows. Letters should be typed double-spaced, should not exceed 400 words, and are subject to abridgment and other editorial changes in accordance with journal style.

TREATMENT OF URINARY TRACT INFECTIONS

To the Editor:

I read with interest the article from Duke University, "Treatment of Lower Urinary Tract Infections with Single-Dose Trimethoprim-Sulfamethoxazole,"[1] which concluded that single-dose therapy is as effective as ten-day therapy. A recently published study from the Mayo Clinic[2] contradicts this finding. In the Mayo Clinic study relapses (persistent bacteriuria or recurrent infection with the same organism) occurred in 10 of 68 women treated with a single dose of trimethoprim-sulfamethoxazole (TMP-SMX), while only 2 of 68 who were treated with a traditional ten-day course of the same drug relapsed. In fact, close examination of the data from the Duke study also shows this increased rate of relapse; 5 of 111 patients treated with single-dose therapy relapsed while only 1 of 92 patients treated with a ten-day course did so. This higher rate of relapse in patients who receive a single dose of medication remains one of the major issues in the treatment of urinary tract infections.

In addition to this difference in relapse rates, we do not yet know the natural history of relapses after single-dose therapy. In another recent study[3] 1 of 13 patients treated with a single dose of TMP-SMX developed signs of pyelonephritis and a positive test for antibody-coated bacteria, thought to be indicative of upper urinary tract infection. Such an occurrence is especially worrisome because many practicing physicians, including myself, do not routinely perform follow-up cultures on patients with uncomplicated urinary tract infections.

Physicians must balance the advantages of single-dose therapy, such as fewer pills and a lower incidence of side-effects, against the disadvantages including a higher incidence of recurrences and the possible need for closer follow-up. Single-dose therapy may be preferable for selected patients. Until a much larger body of clinical experience is accumulated, however, it cannot be recommended as the treatment of choice for the general population.

James Dunlay, MD
Houston, Texas

References

1. Prentice RD, Wu LR, Gehlbach SH, et al: Treatment of lower urinary tract infections with single-dose trimethoprim-sulfamethoxazole. J Fam Pract 1985; 20:551-557
2. Schultz HJ, McCaffrey LA, Keys TF, et al: Acute cystitis: A prospective study of laboratory tests and duration of therapy. Mayo Clin Proc 1984; 59:391-397
3. Hooton TM, Running K, Stamm WE: Single-dose therapy for cystitis in women. A comparison of trimethoprim-sulfamentoxazole, amoxicillin and cyclacillin. JAMA 1985; 253:387-390

The preceding letter was referred to Mr. Hanlon, who responds as follows:

We appreciate the opportunity to further comment on our study results. We recognize that it is difficult for practitioners to determine the significance of clinical study findings, especially when there has been an abundance of published material in a particular topic area. It is to be expected that one will find conflicting study results. This requires that practitioners weigh the balance of the evidence that is available and decide whether isolated or conflicting results are clinically important.

Dr. Dunlay raises some interesting points regarding potential disadvantages to treating uncomplicated cystitis in women with single-dose trimethoprim-sulfamethoxazole (TMP-SMX). First, our study found neither statistically nor clinically significant differences in the relapse or reinfection rates between the single-dose group and the ten-day group.[1] The Mayo Clinic report is the only study that found significantly higher relapse rates with single-dose TMP-SMX.[2] One might speculate that this conflicting finding may be due to differences in their study population and methodology. A body of literature, however, suggests that relapse rates with single-dose TMP-SMX are not significantly higher.[3-7]

Second, none of the patients in our study who had relapses went on to develop pyelonephritis. Only one patient has been reported to develop pyelonephritis after single-dose TMP-SMX, and this was 19 days after therapy with two negative urine cultures at three and 14 days after treatment.[8] Most experts believe that the probability of pyelonephritis occurring after failure with single-dose therapy is extremely low.[9,10]

We believe that the balance of evidence[1,3-7,9-12] supports our find-

Continued on page 324

Continued from page 322

ings that the single-dose treatment with TMP-SMX in women with uncomplicated cystitis is effective and offers a number of advantages.

Joseph T. Hanlon, MS Pharm
Lawrence R. Wu, MD
Nancy E. Clapp-Channing, RN, MPH
R. Derek Prentice, MB, ChB, MRCGP
Department of Community and Family Medicine
Duke University Medical Center
Durham, North Carolina

References

1. Prentice RD, Wu LR, Gehlbach SH, et al: Treatment of lower urinary tract infections with single-dose trimethoprim-sulfamethoxazole. J Fam Pract 1985; 20:551-557
2. Schultz HJ, McCaffrey LA, Keys TF, et al: Acute cystitis: A prospective study of laboratory tests and duration of therapy. Mayo Clin Prac 1984; 59:391-397
3. Bailey RR, Abott GD: Treatment of urinary tract infection with single-dose of trimethoprim/sulfamethoxazole. Can Med Assoc J 1978; 118:551-552
4. Buckwold FJ, Ludwig P, Harding GK, et al: Therapy for cystitis in adult women. Randomized comparison of single-dose sulfamethoxazole vs trimethoprim-sulfamethoxazole. JAMA 1982; 249:1839-1842
5. Harbord RB, Gruneberg RN: Treatment of urinary tract infection with single-dose amoxycillin, co-trimoxazole or trimethoprim. Br Med J Clin Res 1983; 283:1301-1302
6. Jones RH: Single-dose and seven-day trimethoprim and co-trimoxazole in the treatment of urinary tract infection. J R Coll Gen Pract 1983; 28:827-832
7. Tolkoff-Rubin NE, Weber D, Fang LS, et al: Single-dose therapy with trimethoprim-sulfamethoxazole. Rev Infect Dis 1982; 4:444-448
8. Hooten TM, Running K, Stamm WE: Single-dose therapy for cystitis in women. A comparison of trimethoprim-sulfamethoxazole, amoxicillin and cyclacillin. JAMA 1985; 253:387-390
9. Stamey TA: Urinary tract infections in women. In Pathogenesis and Treatment of Urinary Tract Infections. Baltimore, Md, Williams & Wilkins, 1980, pp 122-209
10. Komaroff AL: Acute dysuria in women. N Engl J Med 1984; 310:368-375
11. Kunin CM: Duration of treatment of urinary tract infections. Am J Med 1981; 71:849-854
12. Carlson KJ, Mulley AG: Management of acute dysuria. A decision-analysis model of alternative strategies. Ann Intern Med 1985; 102:244-249

BIG BUSINESS AND THE PHYSICIAN-PATIENT RELATIONSHIP*

To the Editor:

The forces of change have placed the future of American medicine in the hands of politicians and big business. The "pop" phrase today is "cost containment." The noble scramble for cost containment may leave two vital components of the physician-patient relationship in the dust: The physician and the patient.

In the deepest sense, the physician-patient relationship is a mutually dependent relationship. Physicians and nobody else are entrusted with the patient's well-being—the care of the patient. For the most part, we do care. No third party computer profile of our patterns of care can ever demonstrate the care and compassion we give our patients while reasoning with updated knowledge through their health problems.

Let us communicate this simple message to our patients by continued good example and by further public education that we and nobody else can do it better. No government agency, no health care coalition, no for-profit health care giant can safeguard the patient's well-being as we can.

The physician-patient relationship is too important to be left in the hands of big business. We must insist that medicine is not and never will be exclusively big business.

Richard J. Tushla, MD
Santa Paula, California

**Excerpts from the president's address to Ventura County Medical Society, November 1984*

Continued on page 326

Continued from page 324

MECONIUM ASPIRATION

To the Editor:

As other perinatal complications have become amenable to prevention, meconium aspiration syndrome (MAS) has come to represent a significant problem in perinatal medicine. Present techniques in the delivery room can also relegate MAS to a complication of the past.

Meconium-stained amnionic fluid occurs in anywhere from 8 to 22 percent of vertex deliveries. Thick stained fluid has a worse prognosis for the infant than thin and indicates hypoxia.[1]

A study at the University of California in 1974 revealed that in 80 infants with meconium-stained fluid who had their airways aspirated, 46 had anywhere from 0.5 to 7.5 cc of fluid in their airways. Fifteen of these had thick fluid in the airway, and 62 had meconium in the stomach. Of the 80, 23 had abnormal findings on chest x-ray examination, and 16 required some sort of treatment. Of the 16, seven had either a pneumothorax or pneumomediastinum.[2]

In 1975 at the University of Colorado a combined obstetrics-pediatrics approach was begun. During the period between 1970 and 1974, with bulb suctioning, five deaths were due to MAS in 19 patients. After using the combined approach and DeLee suctioning with a DeLee suction apparatus, MAS occurred in seven patients, but no deaths. Deep suctioning and direct visualization of the cords resulted in one patient with MAS and no deaths. The recommendation of this study was deep suctioning before the infant breathed and direct visualization of the vocal cords. If no meconium was found on the cords, do no further suctioning; if meconium, do tracheal lavage.[3]

Failure of deep suctioning below the cords to prevent MAS is probably due to aspiration of fluid in utero. In most cases MAS is an acute predetermined event. In others, MAS may develop in a more chronic form with actual intrauterine pneumonitis, a condition documented by a study of 200 stillborns in New York.[4] Twenty-five infants had amnionic fluid in their alveoli and eight had evidence of meconium contamination. Four cases of significant intrauterine pneumonitis also occurred.

The bulb suction cannot give the deep suctioning required to prevent MAS. One must use a DeLee suction apparatus. The infant should be suctioned once the head has been delivered and before delivery of the shoulders. The proper technique is to suction the mouth and then the nares. After delivery of the infant, further suctioning of the oropharynx and stomach should be done. Further suctioning in the meconium-stained infant should be done under direct vision of the larynx. All delivery tables should have a DeLee suction apparatus on them and should be used at each delivery. The bulb suction should be reserved only for suction of thick mucous. This approach should prevent the majority of the meconium aspiration syndromes.

Daniel E. McGunegle, MD
Department of Community and Family Medicine
Truman Medical Center/East
Kansas City, Missouri

References

1. Starks GC: Correlation of meconium-stained amnionic fluid, early intrapartum fetal pH, and Apgar scores as predictors of perinatal outcome. Obstet Gynecol 1980; 56:604-609
2. Gregory GA, Gooding CA, Phibbs RH, Tooley WH: Meconium aspiration in infants—A prospective study. J Pediatr 1974; 85:848-851
3. Carson BS, Losey RW, Bowes WA, Simmons MA: Combined obstetric and pediatric approach to prevent meconium aspiration syndrome. Am J Obstet Gynecol 1976; 126:712-715
4. Brown BL, Gleicher N: Intrauterine meconium aspiration. Obstet Gynecol 1981; 57:26-29