Acute Dysuria in Women

multiplied by 50 g per meal multiplied by 4.5 kcal per gram of protein yields a product of 1125 kcal. Is the error theirs or mine?

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To the Editor: We had results similar to those reported by Andersen et al. when we compared 40 patients undergoing dietary therapy and gastroplasty with 40 patients given the protein-sparing modified-fast diet of Blackburn et al.[1] At two years into the study absolute individual weight loss in the two groups was nearly identical, but late weight gain was more pronounced in the group treated with the diet alone.

However, we found a high incidence of postoperative ructus, regurgitation, and heartburn in patients treated with gastroplasty. Ten of 40 patients had severe, intractable pyrosis. In fact, symptoms in four patients were completely refractory to aggressive medical management and necessitated antireflux procedures. Not a single patient treated with the protein-sparing modified fast had gastrointestinal symptoms other than mild constipation (25 per cent).

Consequently, we believe that all morbidly obese patients scheduled for gastric restrictive procedures, regardless of symptoms, must be evaluated for lower-esophageal-sphincter incompetence before operation. Preoperative assessment should include barium esophagography, esophageal manometry, standard acid-reflux testing, acid perfusion and clearance testing, and esophageal endoscopy with biopsy.[2,3]

With this approach, a substantial number of morbidly obese patients with asymptomatic reflux can be identified, eliminated from the gastroplasty group, and treated with restricted diet alone or at least be made aware of the potential need for an antireflux procedure, should the operation be performed.

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1. Blackburn GL, Bistrian BK, Flatt JP, et al. Role of a protein-sparing modified fast in a comprehensive weight reduction program. In: Howard AN, ed. Recent advances in obesity research. London: Newman Publishing, 1975.
2. Sapala JA, Sapala MA, Hurtado MH. Treatment of reflux esophagitis with stricture using the Angelchik™ anti-reflux prosthesis; a preliminary report. Am J Gastroenterol 1983; 78:695. abstract.
3. Skinner DB, Booth DJ. Assessment of distal esophageal function in patients with hiatal hernia and-or gastroesophageal reflux. Ann Surg 1970; 172:627-37.

To the Editor: Andersen et al. note that the Gomez gastroplasty did not result in a significantly greater weight loss than a physician-supervised diet, when data on regained weight were excluded. However, significantly more weight was regained after maximal weight loss with dieting as compared with the Gomez gastroplasty, so that the overall net weight change at 18 months was significantly greater with the gastroplasty procedure. Unfortunately, the Gomez gastroplasty has been found to be an inadequate surgical procedure for weight reduction in morbid obesity. Two randomized trials have found that gastric bypass resulted in a significantly greater loss of excess weight than the Gomez gastroplasty, after a follow-up interval of either one year[1,2] or two to four years.[3] We have found a 40 per cent failure of weight reduction with the Gomez gastroplasty.[4] Gomez himself has abandoned his procedure (personal communication). The newer vertical banded gastroplasty of Mason[5] has not yet been subjected to a randomized comparison with gastric bypass. It will be important to undertake a new randomized trial comparing an optimal gastric procedure with a supervised dietary program for the treatment of morbid obesity. One would expect that if an inadequate operation (i.e., Gomez gastroplasty) compared favorably with diet alone, then a gastric-bypass procedure would have much better results.

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1. Linner JH. Comparative effectiveness of gastric bypass and gastroplasty: a clinical study. Arch Surg 1982; 117:695-700.
2. Lechner GW, Callender AK. Subtotal gastric exclusion and gastric partitioning: a randomized prospective comparison of 100 patients. Surgery 1981; 90:637-44.
3. Lechner GW, Elliott AW. Comparison of weight loss after gastric exclusion and partitioning. Arch Surg 1983; 19:685-91.
4. Sugerman HJ, Wolper JL. Failed gastroplasty for morbid obesity: revised gastroplasty versus Roux-en-Y gastric bypass. Am J Surg (in press).
5. Mason EE. Vertical banded gastroplasty for obesity. Arch Surg 1982; 117:701-6.

The above letters were referred to the authors of the article in question, who offer the following reply:

To the Editor: The above letters, which are mutually conflicting, reflect the prevailing confusion regarding cures for morbid obesity — a confusion that can be diminished only if clinical research is organized in randomized clinical trials such as ours. We are glad to see that Sugerman has been inspired to propose similar trials.

Since obesity is a common condition, results of clinical trials will be relevant only if the regimens selected for investigation are more down-to-earth than the approaches described by Nersesian and Miller.

A detailed listing of patients excluded from a study by the criteria for entry is an established requirement, too often neglected. Unlike Kral, we consider it unethical to perform an operation for gastric obesity in patients not selected for their willingness to follow the essential diet. Furthermore, if obviously uncooperative patients are not excluded, the dropout rate will make it impossible to reach conclusions. The Helsinki declaration 2 would not have been observed if patients' rejection of the randomization principle had not been accepted. Random assignment is a prerequisite for comparable study groups; without it no comparison of treatment effects is meaningful.

The determinants of weight loss after gastroplasty are largely unknown. Unlike Drane, we are not convinced of the importance of pouch and stoma size and refer readers to our data published elsewhere.* We consider the dietary advice important. Fortunately, the calculation by Green is not relevant, since it does not take into account the water content of foods.

Irrespective of the procedure, gastric surgery for morbid obesity still has several severe problems: the perioperative risk is too high, weight loss is too small in a large fraction of patients, and there are difficulties in excluding patients not prepared to live with dietary restrictions for the rest of their lives. We have taken the natural consequence of our results, combining the best elements of both treatments. We are currently studying an approach in which a very-low-calorie diet is used to achieve a large and safe preoperative weight loss, whereupon gastroplasty is performed, primarily to keep weight down but also in the hope of inducing further weight loss.

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*Andersen T, Pedersen BH. Pouch volume, stoma diameter and clinical outcome after gastroplasty for morbid obesity: a prospective study. Scand J Gastroenterol (in press).

ACUTE DYSURIA IN WOMEN

To the Editor: In the February 9 issue of the *Journal*, Komaroff makes the point that the laboratory assessment of pyuria based on a centrifuged urine sediment is subject to several sources of error.[1] He cites Stamm et al.[2] as suggesting that the number of leukocytes to be considered as evidence of clinically important pyuria is 8 or more per milliliter of uncentrifuged urine, using the leukocyte counting chamber. This number corresponds to 2 to 5 leukocytes per high-power field in centrifuged sediment, according to Stansfeld.[3] The actual critical number in the article by Stamm et al. is 8 or more leukocytes per cubic millimeter of uncentrifuged urine, which is

1000 times the number given by Komaroff. Understandably, the conversion of units in the metric system can be confusing.

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1. Komaroff AL. Acute dysuria in women. N Engl J Med 1984; 310:368-75.
2. Stamm WE, Wagner KF, Amsel R, et al. Causes of the acute urethral syndrome in women. N Engl J Med 1980; 303:409-15.
3. Stansfeld JM. The measurement and meaning of pyuria. Arch Dis Child 1962; 37:257-62.

To the Editor: Komaroff rightly emphasizes the possible venereal causes of acute dysuria in women. Other simple tests may also be useful. First, vaginitis should be considered in all sexually active women with acute dysuria. Vaginitis or cervicitis may be asymptomatic, but the etiologic agent may be causing the dysuria. Appropriate therapy for nongonococcal mucopurulent cervicitis, usually a 7- to 10-day course of a tetracycline, will also be effective against urethral *Chlamydia trachomatis* infection. In men, a diagnosis of nongonococcal urethritis is based on a finding of more than 4 leukocytes per field in five oil-immersion fields.[1] My colleagues and I have found a similar minimal concentration in conjunctival smears from neonates with purulent conjunctivitis (unpublished data), as have Brunham et al. in studies of mucopurulent cervicitis.[2] I am unaware of such data concerning the normal concentration of leukocytes in urethral smears from women with urethritis of various causes, but I would be surprised if it were not the same as for these other conditions. Thus, the clinician evaluating a patient with acute dysuria has two options for diagnosing a probable sexually acquired infection: the urethral and the endocervical smear with Gram's stain. Direct staining of elementary bodies of *C. trachomatis* with a fluorescein-labeled monoclonal antibody is now available,[3] which in our hospital costs less than a urine culture and colony count ($12.50 vs. $23.00). Sulfonamides, long a mainstay of treatment in women with acute dysuria, are probably as effective as tetracylines for treatment of *C. trachomatis* infections in women.[4,5]

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1. Swartz SL, Kraus SJ, Herrmann KL, Sargel MD, Brown WJ, Allen SD. Diagnosis and etiology of nongonococcal urethritis. J Infect Dis 1978; 138:445-54.
2. Brunham RC, Paavonen J, Stevens D, Kuo CC, Stamm W, Holmes KK. Simplied [sic] objective criteria for the diagnosis of chlamydial cervicitis: abstract 546. Presented at the 22nd Interscience Conference on Antimicrobial Agents and Chemotherapy, Miami Beach, Florida, October 4-6, 1982.
3. Wingerson L. Two new tests for chlamydia get quick results without culture. JAMA 1983; 250:2257-9.
4. Bowie WR, Manzon LM, Borrie-Hume CJ, Fawcett A, Jones HD. Efficacy of treatment regimens for lower urogenital *Chlamydia trachomatis* infection in women. Am J Obstet Gynecol 1982; 142:125-9.
5. Paavonen J, Kousa M, Saikku P, Vartiainen E, Kanerva L, Lassus A. Treatment of nongonococcal urethritis with trimethoprim-sulphadiazine and with placebo: a double-blind partner-controlled study. Br J Vener Dis 1981; 56:101-4.

To the Editor: In his article on management of the "dysuria–pyuria syndrome," Komaroff suggests that pyuria is a better predictor of treatable infection than the urine colony count. After vaginitis, subclinical pyelonephritis, and chlamydial urethritis have been ruled out, Komaroff would treat all patients who have dysuria and pyuria with a single-dose antibiotic regimen.

We recently treated a group of 52 women who had dysuria with a single dose of 3 g of amoxicillin. Exclusion criteria included signs or symptoms of vaginitis, allergy to penicillin, symptoms lasting more than seven days, signs of pyelonephritis (fever, flank tenderness, and vomiting), antibiotic use within one month, a concomitant diagnosis of renal disease or diabetes mellitus, pregnancy, and age less than 12 years. Urine specimens were obtained before treatment, but analysis of the sediment and urine colony counts were not performed until after the initial office visit. Pyuria was determined by examination of a centrifuged sediment at a magnification of 400. Urine was also treated for blood and protein with the use of Hema Combistix. (Ames Division of Miles Laboratories, Elkhart, Ind.). Colony counts were determined by a dip-slide method (Culturia EMB/TSA; Clinical Convenience Products, Madison, Wis.). A positive culture was defined as a dip-slide appearance consistent with 10,000 or more organisms per milliliter. Follow-up studies included telephone contact 3 and 21 days after therapy and an office visit 10 days after therapy. A cure was defined as eradication of the presenting symptoms and no recurrence within 21 days.

The results of our study confirm that the urine culture is a poor predictor of response to single-dose antibiotic treatment. Of the 33 patients with positive cultures, only 17 (52 per cent) were cured. Unfortunately, in our study, pyuria was also a poor predictor of response. Of the 23 patients with pyuria, only 11 (48 per cent) were cured. Similarly, the presence of proteinuria had a low predictive value for cure (25 per cent), as did the presence of hematuria (48 per cent). There appears to be a need for further studies to determine the best treatment of women with the dysuria–pyuria syndrome.

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The above letters were referred to Dr. Komaroff, who offers the following reply:

To the Editor: I am in agreement with Mr. Seery and Dr. Lasker. I attempted to translate the counting-chamber measure of pyuria used by Stamm et al. (8 or more leukocytes per cubic millimeter of uncentrifuged urine) into the measure of pyuria more commonly used by clinicians: the number of leukocytes in centrifuged sediment. On the basis of the data of Stansfeld, I stated that Stamm and colleagues' definition of pyuria was the equivalent of 2 to 5 leukocytes per high-power field in centrifuged sediment. That is the point I was making, and it is correct. I did state that a counting chamber uses a milliliter as its measure of volume, whereas in fact it uses a cubic millimeter. Fortunately, this misstatement has no bearing on the point I was making.

I generally agree with Dr. Bell's remarks. As I emphasized in the article, vaginitis and cervicitis should always be considered in a woman with dysuria. Although not every woman with acute dysuria should be subjected to a pelvic examination, the clinician should strongly consider performing a pelvic examination if the dysuria does not resolve with therapy or if it is recurrent. The leukocyte count in the discharge from men with urethritis, and perhaps in neonates with purulent conjunctivitis, is useful diagnostically. However, I doubt that the leukocyte count would be as useful in women with urethritis, because they do not often have a discharge from the urethral os. Rapid diagnostic assays for chlamydial infection, such as the one mentioned by Dr. Bell, are likely to have great utility in clinical medicine. At this point, I believe there is insufficient published evidence to recommend any such assay. I hope that will change in the near future.

As Drs. Little and Hoshino state, I do believe that in the woman who has acute dysuria without clinical evidence of acute pyelonephritis or vaginitis, pyuria is a better predictor of potentially treatable infection than is the urine colony count: women with pyuria are very likely to have some form of treatable infection, whereas women without pyuria are likely not to have a genitourinary infection. However, the presence of pyuria does not help to distinguish which type of infection is present. In particular, it does not distinguish lower-tract bacterial infection (which is susceptible to single-dose therapy) from subclinical pyelonephritis or chlamydial or other urethritis (which is not susceptible to single-dose therapy). That distinction can be made with reasonable accuracy on the basis of clinical findings specified in the article.

The study by Drs. Little and Hoshino does not seem to challenge either the value of pyuria or the benefits of single-dose therapy. The exclusion criteria that they used do not match those I proposed for making a presumptive diagnosis of lower-tract bacterial infection. Indeed, many of the 52 patients whom they treated with single-dose therapy may have had subclinical pyelonephritis or chlamydial or

gonococcal urethritis and would therefore not have been expected to respond to single-dose therapy.

In closing, I would like to take the opportunity to highlight two excellent recent reports that could not be cited at the galley-proof stage of my article. Berg and his colleagues have presented further information on clinical and laboratory findings in women with acute dysuria,[1] and Latham and his colleagues have reported a large study of urinary infections with *Staphylococcus saprophyticus*.[2] Both articles are recommended to interested readers.

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1. Berg AO, Heidrich FE, Fihn SD, et al. Establishing the cause of genitourinary symptoms in women in a family practice: comparison of clinical examination and comprehensive microbiology. JAMA 1984; 251:620-5.
2. Latham RH, Running K, Stamm WE. Urinary tract infections in young adult women caused by *Staphylococcus saprophyticus*. JAMA 1983; 250:3063-6.

CLINICAL VALUE OF THE GLYCOSYLATED HEMOGLOBIN ASSAY

To the Editor: The account by Nathan and collaborators (Feb. 9 issue)[1] on the information value of the glycosylated hemoglobin assay in the care of patients with diabetes hardly qualifies this method as the "revolutionary advance" praised by Goldstein.[2]

As I read it, the information value of the assay is its superiority over sheer guesswork in the estimation of diabetic control in a large group of patients, and on that standard the laboratory method is obviously appealing. However, since it owes its very validity to comparison with capillary blood glucose measurements done at home by patients on routine and timely schedules, why not just have the patients do the latter measurements? I think it is important for persons with a chronic disease to be responsible for their health so far as possible; putting them in charge of the assessment of their progress seems far superior to furthering dependence on remote doctors and laboratories. Patients accept this opportunity and also learn considerably more about euglycemia through recognition of their glycemic patterns than they do when such information is simply bestowed on them. Strategically timed home blood glucose measurements have also given patients valuable insights into the workings of their insulin mixtures and the timing of their dietary and exercise habits. The alterations that result from this information are simply not available from a method that varies from month to month rather than from hour to hour. This is equally true for patients seen at a public clinic and for those seen in private practice.

Although almost all studies that have defined the basis of glycosylated hemoglobin values have shown them to depend on the mean blood glucose level or related measurements, it is not yet clear just how persons vary in their glycosylated hemoglobin values from day to day — that is, whether such variation will be reflected in mean blood glucose values — and Nathan et al. give no information on this point. We can interpret and sometimes act on daily variation in the blood glucose level. Since the glycosylated hemoglobin measurement seems to be no better than the values from which it is derived and, in fact, may be less useful, there is little reason to supplant the blood glucose value with its offshoot. It might actually be misleading in the advice we must give to patients with diabetes who have periodic hypoglycemia. In addition, reliable results require expensive techniques such as high-performance liquid chromatography, probably further lessening the routine usefulness of the glycosylated hemoglobin assay.

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1. Nathan DM, Singer DE, Hurxthal K, Goodson JD. The clinical information value of the glycosylated hemoglobin assay. N Engl J Med 1984; 310:341-6.
2. Goldstein DE. Is glycosylated hemoglobin clinically useful? N Engl J Med 1984; 310:384-5.

To the Editor: The article by Nathan et al. should provide a lesson in humility for physicians who treat patients with diabetes: not only were estimates of glycemic control inaccurate, but the physicians were 80 per cent sure that they had been right.

A possible confounding issue in the study was the inclusion of both patients with Type I disease (16 per cent) and those with Type II (84 per cent). One would expect that patients with Type II diabetes (particularly those not receiving insulin) would have less glycemic excursion than Type I patients. In fact, the studies of Holman and Turner* have shown the fasting plasma glucose level to be a reasonable estimate of the mean blood glucose concentration in maturity-onset diabetes. Did inclusion of the small percentage of patients with Type I disease bias the study by Nathan et al. toward swings in blood glucose levels?

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*Holman RR, Turner RC. The basal plasma glucose: a simple relevant index of maturity-onset diabetes. Clin Endocrinol 1980; 14:279-86.

The above letters were referred to the authors of the article in question, who offer the following reply:

To the Editor: The importance of the glycosylated hemoglobin assay lies in its ability to measure objectively the degree of glucose control over time. Its role as an index of glucose control over time should not be confused with the role of measurements of short-term glucose control, as Marglin seems to do. We validated the assay as an accurate measurement of the mean plasma glucose level — and others have provided similar documentation[1] — by comparing the result of a single hemoglobin A_{1c} measurement with the arithmetic mean of numerous preprandial and postprandial blood glucose measurements obtained over a two-month period by a group of highly selected, motivated volunteers. Many patients with diabetes are not willing to perform the large number of blood glucose self-tests necessary to provide this kind of complete blood glucose profile. Moreover, it is not appropriate for all such patients to perform this time-consuming and expensive testing, especially if insulin doses are not being adjusted regularly. Hemoglobin A_{1c} measurements represent a simple and effortless objective measurement of the degree of long-term glucose control. They can be obtained independently of the patient's cooperation and allow the health-care provider and patient to determine objectively whether an acceptable degree of glucose control is being attained and whether changes in the therapeutic regimen are effective. Although we used an extremely precise high-performance liquid chromatography assay in our study, commercially available assays have been shown to be acceptably accurate and precise for clinical management.[2]

The hemoglobin A_{1c} assay cannot replace measurements of short-term blood glucose control in the management of diabetes. Home urine testing and blood glucose self-monitoring provide the information necessary to make day-to-day adjustments of insulin therapy. These measurements complement the hemoglobin A_{1c} assay.

Regarding Dr. May's letter, we also questioned whether physicians were more accurate in assessing the degree of glucose control over time in patients with Type II diabetes than in those with Type I. There was no meaningful difference between the two groups of patients with respect to physician error in guessing the mean blood glucose level (Type II, 59 mg per deciliter; Type I, 53 mg per deciliter), nor was there much of a difference when patients treated with diet or oral agents were compared with those receiving insulin (48 vs. 58 mg per deciliter).

Although studies by Holman and Turner indicate that in a controlled research setting patients with "mild" Type II diabetes have relatively stable fasting glucose values,[3] when patients with Type II disease treated with diet, oral agents, or insulin are studied in a more realistic outpatient setting, this may not be the case. In our study, recent fasting glucose values were available for 27 patients with Type II diabetes. The correlation between the fasting glucose level and the hemoglobin A_{1c} measurement was only 0.53 — far from an optimal value.

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