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ACETIC ACID WASH AND CERVICAL CANCER

To the Editor:

I was pleased to see the article by Frisch et al¹ on using a naked-eye inspection of the cervix (NIC) after application of acetic acid to augment screening for cervical cancer. The authors report that the combination of cytology and the NIC increased the screening yield for cervical disease but with some loss of the positive predictive value. Their high false-positive rate for the NIC with a correspondingly lower positive predictive value may be related to at least three factors specific to their study design.

First, nearly two thirds of their subjects were taking oral contraceptives, which would result in an enlarged cervical ectropion and area of acetowhitening. If these areas were read as abnormal by the observer, this could account for the high positive rate (75%) detected in the study population. Other reports evaluating the acetic acid wash in cervical disease screening found much lower positive rates (5% to 13%).^{2,3}

Second, HARNET (The Harrisburg Area Research Network) found that waiting 4 to 6 months to repeat the NIC improved their positive predictive value from 55% to 63%.³ Presumably, this effect is due to allowing resolution of mildly abnormal changes that are not at risk of progressing to more significant disease.

Third, HARNET found that the "learning-curve effect," ie, reevaluating the predictive value of the test at least 6 months after beginning the study, led to a further increased positive predictive value of 67%. Presumably, it takes time for observers to learn to correctly identify areas of true abnormalities.

The authors report that the NIC, in combination with cytology, did not miss any cases of high-grade lesions. Of the three high-grade lesions detected in the study population, two were missed by cytologic tests alone, and one was missed by cervicography alone. Only the NIC detected all three high-grade lesions. This further supports similar findings from HARNET regarding the importance of using the acetic acid wash as an adjunct to avoid missing high-grade lesions.⁴ Continued efforts to evaluate the use of an

acetic acid wash for augmenting cervical disease detection are clearly warranted.

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The preceding letter was referred to Drs Frisch and Ferris and Ms Milner, who respond as follows:

Dr Slawson and the HARNET group have made useful contributions to our understanding of the potential role that naked-eye inspection of the cervix (NIC) might play in augmenting screening procedures for cervical cancer.^{1,2} Whereas Slawson's group has emphasized the contribution of a positive NIC, our study looked primarily at the predictive value of a negative test.

A screening test should ideally perform well in predicting both true positives and true negatives. Our data suggest that the combination of cytology and NIC has a high negative predictive value (PVN), but that the positive predictive value (PVP) of cytology plus NIC was only 57%. As we acknowledged in the paper, our inability to perform colposcopy on all study participants seriously compromised our ability to determine the PVP for NIC: only six missed cases of CIN in unbiopsied NIC-positive women would have raised PVP to near 100%. While our population differs from that studied by HARNET, our 95% confidence limits (45% to 69%) do in-

clude their best PVP determination of 67%.

In his letter, Dr Slawson has made several points that are indeed relevant to the results we reported. Oral contraceptive use was common in our largely nulliparous population, and both hormone use and nulliparity could increase the risk of false-positive NIC if the temporary acetowhite blanching of villi or areas of immature squamous metaplasia were incorrectly considered abnormal. The HARNET observation that waiting 4 to 6 months allows regression of minor changes may ultimately prove important for defining postsmeared triage options, but neither our study design nor our transient population allowed us the opportunity to perform such a leisurely follow-up.

The "learning-curve effect" is undoubtedly important for practitioners just beginning to perform NIC; however, all our NIC evaluations were done by a single observer with many years' experience in interpreting this test. We agree with Slawson that continued study of NIC may be warranted. While our study³ assessed the combination of cytology, NIC, and cervicography, new screening modalities are on the horizon: flow cytometry and automated HPV DNA screening, for example. As screening effectiveness improves, the potential role of NIC may change substantially.

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1. Slawson DC, Bennett JH, Herman JM. Are Papanicolaou smears enough? Acetic acid washes of the cervix as adjunctive therapy: a HARNET study. *J Fam Pract* 1992; 35: 271-7.
2. Slawson DC, Bennett JH, Simon LJ, Herman JM. Should all women with cervical atypia be referred for colposcopy? A HARNET study. *J Fam Pract* 1994; 38:387-92.
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AEROSOLIZED ALBUTEROL TO TREAT ACUTE BRONCHITIS

To the Editor:

The report by Hueston (*Hueston WJ. Albuterol delivered by metered-dose inhaler to treat acute bronchitis. J Fam Pract 1994; 39:437-44*) on the use of aerosolized albuterol in the treatment of acute bronchitis points out the utility of considering nonantibiotic treatments for this clinical condition. Nonetheless, even with this alternative therapy, a significant number of patients were still symptomatic after a week's treatment in both the control and the experimental group. Considering that a number of the subjects had audible wheezing, a study looking at the effect of inhaled steroids might be a logical next step. Anecdotally, my experience has been that in similar types of patients, as well as those who "failed" their first prescription of antibiotics, patients showed impressive resolution of cough and sputum production soon after being started on an inhaled steroid.

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The preceding letter was referred to Dr Hueston, who responds as follows:

In light of the effectiveness of albuterol for symptoms of acute bronchitis, Dr Gaspar's hypothesis that inhaled steroids might be useful seems logical. However, because acute bronchitis is a self-limited illness with variable severity of symptoms, I would caution Dr Gaspar and others in their interpretation of anecdotal experiences. Randomized double-blinded controlled studies of steroids would be useful before we all start prescribing this drug for our patients with bronchitis.

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ACRONYMANIA

To the Editor:

I always enjoyed reading Howard Bennett's *Humor in Medicine* articles in your journal. His latest advice on "What to Do When Specialists Use Abbreviations You Don't Understand" (*Bennett HJ. Primary Care Update 1999. J Fam*

Pract 1994; 39:523) deserves some comment.

Physicians are notorious for using abbreviations and inventing acronyms. Specialists, particularly cardiologists, often take for granted that certain "trade terms" are self-explanatory and therefore do not bother to define or explain them. However, unless they are explained when used first time, they lead to confusion, frustration and aggravation.¹⁻⁸ Because of my personal frustration with unexplained acronyms in the journals, I have compiled a table of acronyms of all major cardiologic trials and updated the list several times.^{1,7,9} Whereas the 1992 list¹ contained 245 entries, the 1994 updating⁹ had 946 entries. The list has been growing exponentially.⁸

Because of these publications, your readers will not need to wait until 1999 to find out the answer to Dr Bennett's question "What To Do When Specialists Use Abbreviations You Don't Understand?"

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8. Cheng TO. Acronymophilia. The exponential growth of the use of acronyms should be resisted [editorial]. *BMJ* 1994; 309:683-4.
9. Cheng TO. Acronyms should be explained. *Atherosclerosis* 1994; 111:143-60.

The preceding letter was referred to Dr Bennett, who responds as follows:

As I read Dr Cheng's letter, I found myself wondering when doctors first began using initials to communicate with each other. Did Hippocrates speak with initials as he walked the streets of Cos? Did Galen come up with the first medical acronym when he coined the expression *phlegmatic personality disorder* (PPD)?

Should I stop writing this letter and attend to my wife, who says we have an overflowing toilet (OT)?

Dr Cheng's point is well taken, of course, and his compilation of acronyms will be useful for anyone who reads the cardiology literature. What he does not address, however, is the truth that underlies the humor in my article. Namely, the primary care docs can feel pretty dumb when talking to a specialist who is rattling off initials. When this occurs, there is that moment of doubt when we have to decide whether to ask the specialist what he means (and risk being thought of as ignorant) or stay quiet and hope to be able to find out what the initials stand for after the conversation is over. It is an ongoing battle.

Finally, during the 4 years that I spent researching *The Best of Medical Humor*,¹ I found a number of authors who poked fun at medicine's obsession with initials. Although most of these articles were serious, a few were amusing.^{2,3}

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VASECTOMY TECHNIQUE

To the Editor:

I enjoyed Dr Reynolds' well-written article on vasal occlusion techniques in no-scalpel vasectomy (NSV).¹ I agree with his management of the prostatic end, with intraluminal cautery and closing the vas sheath over the transected end, as per Schmidt.² I appreciated his detailed anatomical description of his technique, particularly the emphasis on ensuring that only the vas sheath, and not other fascial layers, is incorporated in this interposition.

I disagree, however, with his suggested management of the testicular end, also by intraluminal electrocautery. While reference was made to Denniston's earlier

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series and review,³ his more recent article had just been published when Dr Reynolds submitted his manuscript. That study suggested superb results with an open-ended technique for the testicular end.⁴ Two earlier, much larger studies likewise reported fewer complications by leaving the testicular end completely open after transection.^{5,6} Specifically, they found less epididymal congestion^{5,6} and fewer sperm granulomas,⁵ with no greater failure rate. The decrease in congestive epididymitis makes intuitive sense, but, traditionally, sperm granulomas have been thought to be caused by leakage of sperm. These studies suggest that, in fact, sperm granulomas may form when sperm burst through a dilated epididymis or vas.³ Theoretically, vasovasotomy may be more successful after this technique, as well, since there is less vasal and epididymal congestion.

As suggested by Denniston,⁴ the ideal vasectomy technique may be the NSV, with intraluminal cautery and sheath closure over the prostatic end combined with leaving the testicular end open. There is ample evidence to suggest that this combination of techniques may provide the highest success rate with the lowest rate of complications.

Charles O. Frazier, MD
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2. Schmidt SS. Techniques and complications of elective vasectomy: the role of spermatic granuloma in spontaneous recanalization. *Fertil Steril* 1966; 17:467-82.
3. Denniston GC. Vasectomy by electrocautery: outcomes in a series of 2500 patients. *J Fam Pract* 1985; 21:35-40.
4. Denniston GC, Kuehl L. Open-ended vasectomy: approaching the ideal technique. *J Am Board Fam Pract* 1994; 7:285-7.
5. Errey BB, Edwards IS. Open-ended vasectomy: an assessment. *Fertil Steril* 1986; 45:843-6.
6. Moss WM. A comparison of open-ended versus closed-end vasectomies: a report on 6220 cases. *Contraception* 1992; 46:521-5.

The preceding letter was referred to Dr Reynolds, who responds as follows:

I appreciate Dr Frazier's kind words about my technique of occluding the prostatic end of the vas deferens during

no-scalpel vasectomy. He is correct in assuming that I was unaware of Dr Denniston's most recent paper¹ at the time I wrote my article.

The notion of leaving the testicular vas open was developed by Errey and Edwards in Australia in 1979.² I have been aware of this technique for many years. However, feeling that sperm cause an inflammatory reaction when extravasated, I have been reluctant to invite problems at the operative site. Like many vasectomists, I have been waiting to see how open-ended vasectomy (OEV) works in other physicians' hands.

I have until now subscribed to Schmidt's view of the need to securely occlude the testicular vas. Theoretically, this prevents sperm granuloma formation that may lead to subsequent recanalization.³ Errey and Edward's original OEV work, and the subsequent substantial series of Moss⁴ and Denniston,¹ which show no increase in failure rate and a lower rate of congestive epididymitis, are persuading me to change to this new technique.

However, if practitioners are to switch to the OEV technique, they must take note of the prostatic vas occlusion techniques used by Errey and Edwards, Moss, and Denniston. All three articles describe the use of intraluminal cautery and fascial closure over the prostatic end.^{1,2,4} The prostatic vas closure technique must ensure that sheath continuity is interrupted, a critical step in successful vasectomy.⁵ The commonly used technique of vasal ligation without interruption of sheath continuity cannot be used with OEV. Ligation is thought to cause necrosis of the vas, ultimately leading to an open prostatic end.⁵ Prostatic vas ligation without sheath interruption during OEV would likely lead to an unacceptably high failure rate.

I believe that the underlying pathophysiology of congestive epididymitis and orchitis after vasectomy is immunologic. Most of my affected patients have had some degree of trauma a day or two preceding their pain and swelling. If it has been more than a month since their vasectomy, as is usually the case, I routinely treat them with oral prednisone rather than the usual antibiotics and NSAIDs. Most episodes resolve within a few days with this treatment.

I suspect that testicular and epididymal microtrauma leads to spermatic extravasation, with subsequent immunologically mediated inflammation. Elevated pressure within an occluded vasal system

would lead to more extravasation and, therefore, more inflammation. OEV should lower the intraluminal pressure within the epididymis and testicle, resulting in less sperm extravasation after trauma. However, there is apparently little reaction from sperm extravasation at the scrotal vent site. If sperm extravasation causes inflammation and granuloma formation, why does this not happen at the scrotal vent site? It does not seem logical that elevated intravasal pressure itself has anything to do with the development of inflammation.

Even without full understanding of the mechanism, we must often be guided by what clearly works.

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PHYSICIAN INQUIRY INTO SPIRITUAL MATTERS

To the Editor:

I read with interest the study by Drs Daaleman and Nease in the December 1994 issue of the *Journal (Daaleman TP, Nease DE. Patient attitudes regarding physician inquiry into spiritual and religious issues. J Fam Pract 1994; 39:564-8)*. I agree that there is much healing to be accomplished by incorporating the spiritual aspect of our patients into treatment. Unfortunately, I find that the clinical usefulness of the information from this study is limited.

First, the sample is skewed toward more highly educated patients (60% have education beyond high school). I therefore question the "generalizability" of these findings.

Next, a sample of convenience was used. Since the demographics, religious

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preference, or even the number of those refusing to participate was not evaluated, a significant selection bias may have occurred.

Finally, the method of reporting is a limitation. One advantage of the Likert scale is that the responses can be weighted, so that one can evaluate the "strength" of agreement or disagreement. Daaleman and Nease eliminate this advantage by changing a 5-point scale into a "Yes" or "No" answer. Furthermore, the results are skewed to "Yes" (agreement) by combining responses for the 3, 4, and 5 answers. This devalues the result since a "3" response is usually taken to mean a "neutral" response.

Another problem I find with this research is that the concurrence of the faith of the physician and the faith of the patient is not evaluated. As a Jewish physician, I find it more difficult to bring up spiritual issues with a Christian patient. This is not to say that it does not occur, for I believe strongly in the healing power of spirituality, but this occurs in an ongoing relationship with a patient (as opposed to a question on an intake questionnaire).

Perhaps the main reason we have such difficulty in evaluating if, how, and when to discuss spirituality in the doctor-patient relationship is due to the innumerable variables involved. Each relationship between doctor and patient is unique, and each individual's spirituality is equally unique. How can we quantify such variability? It seems to me that it is valuable to remember the spiritual side of our patients, and to allow this aspect of healing to blossom and grow, "prn," in the milieu of a nurturing relationship between physician and patient.

Wayne S. Strouse, MD
Gray, Tennessee

The preceding letter was referred to Dr Daaleman, who responds as follows:

Dr Strouse addresses many points and they are well taken. The study was composed of a convenience sample and subject to selection bias. This limitation, in addition to a caveat about the applicability of the results to other populations, was stated in the discussion section. The large number of highly educated patients in the study was not surprising, since the family practice center serves as a primary care site for many employees and staff of the medical center.

The methods section describes the se-

lection of the independent variables (age, education level, prayer frequency, religious denomination, frequency of attendance at religious services) used in the study. We included religious denomination because we felt, as Dr Strouse does, that this would have a significant influence on the patient-physician relationship in matters concerning spiritual issues. With the exceptions of prayer frequency and frequency of religious service attendance, there were no other significant differences between the other variables and the Spiritual and Religious Inquiry (SRI) questionnaire, based on the ANOVAs performed.

The Likert scale chosen was designed in an attempt to limit neutral responses to the questionnaire. Although this could have skewed participants' answers into a more affirmative response, the data do not support this. Patients were unequivocal in their disagreement with most statements of the SRI. For example, respondents strongly *disagreed* with the statement that physicians were either qualified (mean score 1.92, on a scale from 1 to 5, where 1 = strongly disagreed) or trained (mean score 1.76) to discuss religious issues with patients.

Dr Strouse's position regarding the research of religion and the role it plays in the physician-patient relationship is representative of many physicians: recognition of the importance and role of spiritual and religious issues in the patient care encounter and skepticism about objective inquiry into this domain. The recently released DSM-IV,¹ which cites religious or spiritual problems as a category separate from psychiatric or emotional diagnoses, serves to highlight what many primary care physicians have long acknowledged. The guidelines adopted by the American Psychiatric Association,² which encourage physicians to be respectful of patient beliefs, serve as a useful model for exploring religious or spiritual issues with patients.

The distinction between religion and spirituality, although used interchangeably in this study, is significant, and the lack of this recognition may explain much of what is problematic in the study of religious variables with health and disease. Religion is the expression of one's beliefs, through the reverence of either sacred objects or places.³ It integrates many, if not all aspects of a person's spirituality; it is the phenotype to spirituality's genotype. Spirituality is the incorporation of the existential with the

transcendental in one's life. It is the animating manifestation of the universal and perpetual need for transcendence, community, and love. The perceived lack of adequate research instruments or methodologies is a second barrier to inquiry in this area. Several disciplines, particularly qualitative research, pastoral theology and social psychology, have examined questions regarding spirituality and interpersonal relationships⁴ and provide a framework to examine these issues.

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PHYSICIAN INVOLVEMENT WITH PATIENTS AND FAMILIES

To the Editor:

I have just finished reading the article by Marvel et al regarding physician involvement with patients and their families in the December 1994 issue of your journal (*Marvel MK, Schilling R, Doherty WJ, Baird MA. Levels of physician involvement with patients and their families: a model for teaching and research. J Fam Pract* 1994; 39:535-41).

I am troubled by the approximate 50% rate of patients consenting to participate in the videotaping. I am wondering why the authors had such difficulty in getting patients to participate. My understanding is that patients were informed about videotaping before seeing their health care provider but exactly what was said and by whom is not revealed.

I think the authors should have

made some effort to address the low response rate in the paper. They recommended this information be gathered in future studies, but this is not enough! I believe the low response rate structures are fairly profound limitations to the study.

Without revealing specific patient data, basic demographic information on these patients who declined to participate in the study could easily have been obtained and subsequently analyzed. It would have been *very* interesting to compare the demographics of this group with those of the people who agreed to participate.

Were there any selective criteria in dealing with the decision to utilize faculty only? What was the faculty's agenda in all this? The authors indicate that the faculty were told that the intent was to better understand their interview methods, but the model was not described to them. I guess I have some problems with this also. Because they were dealing with faculty only, the age-old question of concurrence between faculty responses and the responses of the physicians in the community must be raised. Perhaps the family medicine in community practice relates to the models in residency in only academic and tangential ways.

My last comment has to do with the statement made by the authors that "patients who declined to be videotaped may have intended to discuss individual or family problems requiring higher levels of physician involvement." I cannot find anything in the article to substantiate that statement and believe it to be speculation. There is literature supporting the opposite: many patients and families do not wish to discuss personal or psychosocial issues with their physician, regardless of physician competence. The words "may have intended" do not release the authors from responsibility for authenticity.

Failure to deal with these issues, I believe, affects the validity of this project. I invite the authors' response.

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The preceding letter was referred to Drs Marvel, Schilling, Doherty, and Baird, who respond as follows:

We agree that the 50% rate of participation limits the validity of the results. It is important to note, however, that the

primary sampling unit was the physicians, where 100% agreed to participate. The only sampling issue with the patients was whether there was an adequate range of problems and types of patients for the physicians to demonstrate their skills. We believe this was accomplished.

Efforts were made to increase patient willingness to participate in the videotaping. Patients were given a written and verbal description of the nature of the project, ensured confidentiality, and provided the option to have the visit audiotaped rather than videotaped (such as for patients receiving pelvic examinations). Despite these efforts, many patients declined to be videotaped.

In one of the two research sites (one half of the total sample), the presenting problem for nonconsenting patients was obtained and analyzed. A comparison of five categories (preventive/educational, minor acute illness, chronic illness, serious acute illness, and psychosocial concerns) showed no significant differences in the presenting problems between consenting and nonconsenting patients. While this information shows some similarity between the two groups, insufficient information was collected to make a more definitive conclusion. Therefore, while the rate of patient participation is a limiting factor it is not a critical flaw in our demonstration that the levels model provides a useful and reliable approach for understanding the doctor-patient relationship.

Mr Wolkenstein's second concern is that our decision to use faculty physicians limits the generalizability of the results to community physicians. We agree wholeheartedly and address this issue explicitly in the article. This study is an extension of a previous investigation (Marvel MK, Morphew PK. *Levels of family involvement by resident and attending physicians. Fam Med 1993; 25:26-30*) involving resident physicians only; we plan to observe community physicians in the next phase. Additional studies with more varied physician samples may reveal differences between physicians practicing in academic vs community settings.

Finally, Mr Wolkenstein expresses concern that the LPI model was not described to the faculty. It is clear to us that such a description, especially the focus on family context, would likely influence physician interviewing behavior and invalidate the results.

Some of the concerns raised by Mr Wolkenstein highlight the difficulty of studying doctor-patient communication

through direct observation rather than simulation, self-report, or questionnaires. We hope the concerns over methodological issues, however valid, do not detract from the conceptual model, and that they lead to improved procedures in future studies.

M. Kim Marvel, PhD
Rae Schilling, PsyD
Bill Doherty, PhD
Mac Baird, MD, MS
Fort Collins, Colorado

ENCOURAGING DISCUSSION OF RESEARCHABLE QUESTIONS

To the Editor:

I would like to tell you a story which I believe ties together three of the articles in the January 1995 issue of the Journal.

After residency, I became surprised at the types of malignancies I found in my practice. The second cancer found was a primary vaginal cancer in a patient whose hysterectomy was done for pelvic inflammatory disease 18 years earlier. She had been told she needed no further pelvic examinations. Detecting such lesions at the stage of vaginal intraepithelial neoplasia would be a help. Like Ferris et al,¹ I began to have questions about cancer detection and screening.

This led to the study from MIRNET, also reported in the same issue.² I have since seen a second patient with primary vaginal cancer following previous hysterectomy, equaling the numbers of patients with cervical cancer in my own practice.

I think that the best way to encourage primary care physicians to participate in research is to encourage discussion of researchable questions. As noted by Mainous and Hueston,³ having a research network can be a help in this interaction.

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3. Mainous AG, Hueston WJ. Characteristics of community-based primary care physicians participating in research. *J Fam Pract* 1995; 40:51-6.

TREKKER CLASSIFICATION SYSTEM

To the Editor:

I enjoyed reading Dr Chauche's and Dr Bennett's *Star Trek* dialogue in the December 1994 issue. I would suggest, however, that they be more specific in identifying the genus of trekkie researchers when publishing their work. After much evaluation, I have found there is a high correlation between the quality of research, its interpretation, and the genus of the researcher ($r^2 = 0.8099976$, but as Spock would say, "difficult to be precise"). Genus can be defined as follows:

1. First-Generation Trekkie (Trekkus Primaris): Anyone who has seen an original episode, ie, prior to syndication.
2. Second-Generation Trekkie (Trekkus Revisitus): Anyone who became a trekkie while watching *Star Trek* reruns.
3. Third-Generation Trekkie (Trekkus Nextus): Anyone who became a trekkie while watching *Star Trek, The Next Generation*.
4. Fourth-Generation Trekkie (Trekkus Comus Latelius): Anyone who became a trekkie while watching *Deep Space Nine*.

Since research performed by a Trekkus Primaris is light years ahead of the slipshod work done by the "patacks" who are Trekkus Comus Latelius, I feel that including this designation is imperative. The debate over further refinement of the classification based on the number of episodes watched is still unsettled.

In answer to some of Dr Bennett's questions, I think the difficulty in identifying a Klingon fetus is magnified by several thousand parsecs given that adult Klingons continue to mutate (reference Worf's changes over the past several years and compare him to the Klingons from the original series). I hope the work done by the Klingon Genome Project will be of some assistance in this area. Nevertheless, prior to the completion of that study, it is

premature to say that a level-2 sonogram will correctly identify all Klingon fetuses.

Concerning Dr Bennett's query about surgeons having a disproportionate number of Klingons in their family tree, I find the same is true for gastroenterologists. This may explain some of the exclusionary practices we have experienced by this group in the past.

Dr Bennett's other questions are equally intriguing. Unfortunately, because of financial constraints on Star Fleet Medical imposed by ongoing investigations into the Borg, we will have to wait before they are elucidated.

I am pleased that *JFP* is branching out into this bold new arena where no journal has gone before. "Live long and prosper."

Wayne S. Strouse, MD
Trekkus Primaris
 Gray, Tennessee

The preceding letter was referred to Dr Bennett, who responds as follows:

I enjoyed Dr Strouse's letter and agree with many of the issues he raised. Therefore, while proceeding at warp speed, all medical editors and grant review boards should adopt his classification. That said, I would like to make the following points. First, many diehard fans will take issue with his use of the word trekkie (the politically correct term is *trekker*). Second, Dr Strouse should be more careful with his mudslinging. The term "patack" is a little vague for people unfamiliar with 24th-century dialects. He should have

considered a more descriptive insult like Denebian slime devil or Regular blood worm.* Third, now that *Star Trek Voyager* has premiered, Fifth-Generation Trekkers will need to be added to the list.

Dr Strouse should also realize that his classification discriminates against our younger colleagues since individuals born after 1968 never had a chance to see an original episode. (One of my residents recently informed me that he watched reruns as an infant and that it had a profound effect on his decision to go to medical school.) Therefore, since a number of medical trekkers will feel they deserve a Primaris designation, we must come up with an alternative pathway to classify doctors. Perhaps we could create a Star Trek Board with an annual qualifying exam. Although any material could appear on the exam, questions with a medical twist would have the highest point value. For example, name four physicians who served as Chief Medical Officer on the *USS Enterprise*.†

I hope other medical trekkers will continue the debate. "Peace and long life."

Howard J. Bennett, MD
Trekkus Primaris
 The George Washington University
 Medical Center
 Washington, DC

*A Klingon used these expressions when insulting Kirk and several members of the crew in the episode "The Trouble with Tribbles."

†Dr McCoy (original series), Drs Pulaski and Crusher (The Next Generation), and Dr Boyce (original pilot episode called "The Cage").

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