LETTERS TO THE EDITOR

NEONATAL CIRCUMCISION

To the Editor:

Dr Wolkomir's "Tips from Practice" on neonatal circumcision (*Wolkomir MS. Technique for freehand newborn circumcision. J Fam Pract 1996; 42:447-8*) exposes both a tendency to value personal taste over medical objectivity and some apparently strong conjecture. What criteria he uses to arrive at the judgment of "a very nice result" he fails to say, but surely the esthetics of elective surgical alteration of anatomy has not been subjected to sufficient rigorous analysis to permit such value-laden terms to go unchallenged.

More importantly, however, he begins the praise of his technique by saying that its purported benefits of decreased pain and diminished edema come in part from "not crushing tissue." But the first step in his procedure involves "crushing the foreskin in the anterior midline." Several subsequent hemostat and forceps applications are described, as well as several incising maneuvers, and the final picture accompanying the article demonstrates significant edema. Exactly what variables were measured to support the contention that this procedure reduces pain and edema is not explained. Nor do we learn about the speed of this procedure in experienced hands, compared with the speed of procedures using more widely practiced techniques.

Perhaps the most serious problem with Dr Wolkomir's article is that, despite his contention that use of the freehand method teaches the anatomy of the prepuce, he is apparently confused about what constitutes a mucous membrane, which we must all hope is not exposed or incised in a circumcision procedure. Preputial skin is continuous with the skin of the glans, which is continuous with the urethral mucous membrane, but no mucous membrane is involved in the ritual of male circumcision practice in this country.

One wonders if the various techniques are explained as options to the parents before the procedure is undertaken at St Michael Hospital in Milwaukee. And one wonders why the disappearance from citation indices of an antiquated technique for performing an elective procedure of uncertain benefit should be surprising or lamented. The polarized debate about the value of neonatal circumcision will not be enlightened by such subjective anecdotal comments being dressed in technical language, pretending to be scientifically based.

Thomas W. Filardo, MD
Evendale, Ohio

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

Thank you for allowing me to respond to Dr Filardo's critique. He raises issues of both style and substance, and provides an opportunity to clarify some points.

Dr Filardo describes my article as "anecdotal comments dressed in technical language." The intention was to present only the *technique* of freehand circumcision, with a brief rationale for considering its use. This technique has been in widespread use at several tertiary and primary care hospitals in our area for more than 50 years. It owes much to the time-tested methods of the Jewish Moel'em (ritual circumcisers) but has not appeared in print until now. "Technical language" is the way we describe surgical procedures.

Dr Filardo rightly notes that we do not present objective evidence that this technique reduces pain and swelling. It is beyond the scope of this article to present a scientific argument as to the method's superiority, and we do not claim to present any quantitative measures of pain response. Our observation is that when examined at 24 hours, there is noticeably less edema than with Gomco clamp circumcision. We suggest that a freehand technique may reduce *postoperative* pain and edema as a result of not leaving behind crushed and traumatized tissue. The hemostatic techniques in the initial step of the procedure, creating a dorsal slit, do not differ from other standard methods. However, excision of the foreskin itself, by cutting on the proximal side of the hemostat, effectively removes tissue that has been crushed, leaving behind a fresh, relatively untraumatized surface as shown in the bottom left photograph on page 448 of the article. The only remaining tissue that has been clamped may be one or two small ligated bleeding points. With a clamp technique, however, significant crushed tissue remains in situ. The image that Dr Filardo claims (bottom right photograph on page 448 of the article) shows "edema" in fact demonstrates the rolled inner epithelium before excision.

The correspondent challenges our knowledge of the anatomy of the prepuce. He is correct that no

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"mucous membrane" is excised in this procedure. We referred to the "partially adherent inner epithelium." The vernacular term "mucous membrane" is a common, if incorrect, synonym for this structure, and should have appeared in quotes. I apologize for the confusion of terminology.

As to the length of the procedure, in experienced hands it takes between 1 and 3 minutes to complete, roughly equivalent to the time required for a Gomco or bell procedure.

The obvious passion with which Dr Filardo writes reflects the deeply felt controversy over the appropriateness of newborn circumcision. We only wish to present an alternative method when the decision to circumcise has been taken. I have no intention of judging the "esthetics of elective surgical alteration of anatomy." This article intentionally avoids the polarized debate of which he speaks.

Finally, in response to Dr Filardo's speculation that we fail to inform patients about this option for circumcision, I will simply say that any failure to explain a procedure, its relative risks and benefits, and alternative treatments constitutes malpractice. We adhere to standards of law and ethics as well as good medical practice.

Michael S. Wokomir, MD, MA
St Michael Hospital Family Practice Residency
Milwaukee, Wisconsin

PARTIAL CAPITATION OF MEDICAID SERVICES

To the Editor:

I am very interested by the findings of Rosenthal and colleagues[1] concerning a partial capitation model for Medicaid in New York State. This model, in which primary care physicians were capitated for their services but specialty care, inpatient care, and emergency care were not, appeared to result in substantial cost savings, even more so than with full capitation. This certainly runs counter to intuition and to my experience with a partial capitation system.

Under full capitation, there is an incentive to keep overall costs down, and the literature on health maintenance organizations suggests that most savings come from a reduction in patient care.[2,3] "The philosophy of generalist care" as probable causation for the reported savings seems unlikely, as presumably such a philosophy would have been present in the fully captivated systems, along with a much stronger financial incentive to reduce all costs. In the partial capitation system described, the financial incentive favors substitution of nonprimary care services for primary care services—a perverse incentive.

A more likely explanation for the findings reported is that the attempt to find valid control groups failed. The geographic differences between the partially and fully capitated plans and wide variations in per member per month costs would support such a supposition. Problems with ascertainment of costs, given the decreased incentives for capitated providers to accurately fill out claims information, could also bias results toward a finding favoring capitation, though this might be expected to favor fully capitated systems even more than partially capitated ones. It would be very helpful to have a breakdown of charges (inpatient, outpatient, ancillary, and emergency services) to see if any patterns could be discerned that might support or refute the authors' conclusions, given their counterintuitive findings.

Barry G. Saver, MD, MPH
Department of Family Medicine
University of Washington
Seattle, Washington

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

Dr Saver's comments offer the opportunity to clarify and expand upon our discussion about the partial capitation model. We, too, are very concerned about the degree to which the control group was comparable to the group enrolled in the partial capitation program. I will not repeat the multiple facts that were incorporated into the methodology; however, because factors such as expectation of care are difficult to assess, there is still no guarantee that the control group is representative. We carefully avoided concluding that the savings (38% vs 9.3%) were greater than a full capitation model. We do suggest that there is evidence that generalist physicians employing case management concepts and accustomed to thinking of patients in a managed care environment can achieve comparable savings under partial as well as full capitation. The results actually are more likely to suggest that the physicians were the critical (or noncompara-

tive) component.

Our real intent was to demonstrate that when a managed partial capitation program is initiated and imbued with proper incentives, it can effect savings over fee-for-service. The importance of this finding is that there are populations (ie, small rural communities, chronically disabled, chemically dependent, etc) that do not fit well into full capitation programs. The full risk is unpredictable. We are at present conducting another partial capitation demonstration caring for chronically disabled adults that appears to meet the goals of New York State, special services providers, and patients. Under this arrangement, existing special care agencies can continue to care for patients who require special, nonmedical caregiving.

As managed care matures, it is likely that the full capitation models will prove better able to constrain costs in most situations. During the transition period, many patients and special care nonphysician providers may accept the concept of sharing control with physicians in partial capitation arrrangements more readily than surrendering control to physicians or insurers. Appropriate feedback and incentives can encourage significant cost control when full capitation is not practical.

Thomas C. Rosenthal, MD
State University of New York
School of Medicine and
Biomedical Sciences
Buffalo, New York

RDW TO DETECT IRON DEFICIENCY

To the Editor:

Kazal has corectly pointed out that the hematocrit is a poor screening test for early iron deficiency in young children.[1] This is an important contribution and confirms the works of other investigators.[2] He does not, however, include a discussion of the red cell distributive width (RDW) as an indicator of iron deficiency. It has been shown to be more sensitive than the serum iron level, transferrin saturation, or serum ferritin concentration for the diagnosis of iron deficiency.[3] The bone marrow stores are depleted of iron before the serum is, which is reflected by an increased RDW with a still normal hemoglobin or hematocrit. This is a subtle degree of anisocytosis.[4] The RDW is part of a complete red blood cell count (CBC) with indices. The test is readily available, economical, and requires just a little more blood than a simple hematocrit or hemoglobin test. As a cost-effective approach, Oski[4] recommends a 1-month therapeutic trial of iron in young children having an increased RDW and marginally low hemoglobin. A hemoglobin increase of 1 g/dL or more confirms the diagnosis of iron deficiency, and iron therapy is continued for 3 to 4 months.

In light of several studies confirming a decrease in cognitive ability in the presence of iron deficiency in young children,[5,6] screening for iron deficiency would still seem worthwile despite a declining incidence of iron deficiency and iron deficient anemia. Certainly, additional studies confirming the recommendation of Oski,[4] the role of RDW, and iron therapy would be helpful.

B. Claire Eliason, MD
Department of Family and
Community Medicine
Medical College of Wisconsin
Milwaukee, Wisconsin

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3. Van Zeben D, Bieger R, van Wermeskerken RKA, Castel A, Hermans J. Evaluation of microcytosis using serum ferritin and real blood cell distribution width. Eur J Haematol 1990; 44:106-9.
4. Oski FA. Iron deficiency in infancy and childhood. N Engl J Med 1993; 329:190-3.
5. Lozoff B, Brittenham GM, Wolf AW, et al. Iron deficiency and iron therapy effects on infant developmental test performances. Pediatrics 1987; 79:931-95. [Erratum, Pediatrics 1988; 81:683].
6. Idajadinata P, Pollitt E. Reversal of developmental delays in iron-deficient anemic infants treated with iron. Lancet 1993; 341:1-4.

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

Eliason is correct in stating that the role of RDW in screening infants for iron deficiency needs to be studied further. The article cited about superior sensitivity of RDW by Van Zeben et al[1] was a European study of adults, not infants; the RDW was not tested as a screening tool but used in conjunction with serum ferritin to evaluate patients with microcytosis. In a scientific overview of laboratory diagnosis of iron deficiency anemia, other investigators found that in adults with anemia serum ferritin had the greatest predictive value; RDW was the least useful.[2]

Also, Eliason seems to imply that all infants should be screened for iron deficiency because of its link to decreased cognitive ability. Cognitive and other psychomotor deficits do not occur with iron deficiency alone but with iron deficiency with *anemia*.[3,4] This is an important distinction that has obvious implications for screening. Fortunately, only iron deficiency with *anemia* carries such risk, and therefore in a practice or population with little or no iron deficiency anemia, not every infant needs to be screened—only those at risk for iron deficiency.[5]

Louis A. Kazal, Jr, MD
Department of Family Medicine
Baylor College of Medicine
Houston, Texas

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NEW AGENT FOR OBESITY MANAGEMENT

To the Editor:

I wish to take this opportunity to congratulate you on publishing the clinical review on appetite suppressants (*Elks ML. Appetite suppressants as an adjunct in the treatment of obesity. J Fam Pract 1996; 42:287-92*). Since the article's publication, the Food and Drug Administration (FDA) has approved a new agent for the management of obesity, dexfenfluramine.

The mechanism of action of this agent is unique in that it was found that the S-isomer dextro/isomer of fenfluramine is responsible for the anorectic effects. Dexfenfluramine blocks the reuptake of serotonin, increasing this brain neurotransmitter.

Dexfenfluramine does not possess catecholamine-agonist activity. Its usefulness appears to be the reduction of carbohydrate ingestion, which has been postulated in obese patients who are excessive carbohydrate consumers to be mediated on low serotonin levels.[1,2] The drugs have been described as being well absorbed orally. Approximately 20% of the oral dose undergoes hepatic first-pass metabolism.[3]

Predictably, transient side effects appear within the initial few weeks' treatment period, including gastrointestinal symptoms, diarrhea, xerostomia, asthenia, dizziness, drowsiness, and thirst. Side effects appear to be less bothersome and less frequent when the dose is administered on an every 12-hour basis. A very rare risk of primary pulmonary hypertension has been noted. Drug interactions include those in patients receiving monoamine oxidase (MAO) inhibitors. A 15-day washout period is recommended following the discontinuation of an MAO inhibitor and initiation of dexfenfluramine.

Other studied applications for dexfenfluramine include seasonal affective disorder,[5] symptoms of depression, premenstrual syndrome,[5] and bulimia.[6] This agent is indicated by the FDA for use up to 1 year. Dexfenfluramine in combination with dietary restriction, exercise, and other appropriate weight loss treatments is a welcome addition to the armamentarium for the management of obesity.

Robert L. Barkin, PharmD, MBA
Rush-Presbyterian-St Luke's Medical Center
Chicago, Illinois

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To the Editor:

Obesity is clearly a contributor to morbidity and mortality in many of our patients, so the review of appetite suppressants by Dr Elks[1] was welcome. However, I must take issue with her conclusions. She states that "new research supports long-term use of these agents in selected patients as both safe and effective," with her major support for this contention apparently being the work of Weintraub et al[2] and review articles citing this work. Dr Weintraub's study shows weight loss with dexfenfluramine and phentermine of 14.2 kg vs 4.6 kg with placebo at 34 weeks, but by 190 weeks, both groups had regained weight and the overall loss was only 5.0 kg in the drug group vs 2.1 kg in the control group.

I question the clinical relevance of a net loss of 2.9 kg attributable to drug use over $3\frac{1}{2}$ years. This amount would likely be less if dropouts were included in the analysis. Cost must also be considered. At the doses used by Weintraub et al, the wholesale cost of the drugs is about $2 per day. Is the loss of 2.0 kg worth $2500 for the drugs alone?

Obesity is an extremely frustrating chronic problem without highly effective treatments. Exercise and restriction of calories, especially those from fats, certainly makes sense, but I feel there is little evidence that currently available drugs add much benefit over the long term. New types of drugs are on the horizon, but at this point, I will remain among those described by Dr Elks as "well-trained physicians...uncomfortable with the use of appetite suppressants."

Neal D. Clemenson, MD
Great Plains Family Practice Residency Program
Oklahoma City, Oklahoma

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To the Editor:

I am writing with respect to Martha Elks's article about appetite suppressants (*Elks ML. Appetite suppressants as adjuncts in the treatment of obesity. J Fam Pract 1996; 42:287-92*). Many patients have brought me copies of articles in the lay press including the *Reader's Digest*. I received and dissected Dr Weintraub's studies. This was not a 2-year placebo vs active study, information which would have been very enlightening. We all know these drugs work in many people for 6 to 9 months. If you carefully dissect the numbers, the people using active drugs slowly over time drifted back to within a few pounds of their baseline. We need to get people to pay attention to their food intake (not diet), to eat healthy low-fat foods, and to get off their behinds and onto a treadmill, a bike, or a walking path.

I live in a community where hundreds of people, mostly women, drive more than 100 miles once a month to plunk down their $40 (cash, no checks) for a "group interview" with 24 others to get their stash of phentermine from a doctor who drives a Jaguar. These women pay others to go in their place or buy drugs from those who go. There is good reason that "many well-trained physicians are uncomfortable with the use of appetite suppressants."

Susan A. Schmitt, MD
St George Medical Clinic
St George, West Virginia

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

I agree with Dr Clemenson's concerns that "obesity is an extremely frustrating chronic problem without highly effective treatments." Indeed, as noted in my article, the efficacy of anorectic agents is modest, and not all patients benefit. This, however, should not deter the astute physician from *selective* and *adjunctive* use of some of these agents. Modest weight loss can greatly ameliorate diabetes.[1,2] Is it better to control diabetes with intermittent use of phentermine or constant use of glyburide? Is it better to control hyperlipidemia with weight loss assisted with anorectic agents or with constant use of an HMG Co-A reductase inhibitor?

I agree with Dr Schmitt that a one-size-fits-all handing out of anorectic agents is inappropriate. Not all patients who ask for anorectic treatment should receive it. For those who are cosmetically overweight, ie, unhappy with appearance but not significantly obese and without associated medical complications, appetite suppressants are rarely appropriate and may lead to "yo-yo" effects. This should not deter one, however, from using the agents judiciously to support patients struggling with hunger in their efforts to comply with recommended dietary and exercise regimens. Current technology does not make dieting easy. Anorectic agents improve the compliance and success of some patients and can be useful if applied carefully and thoughtfully. We fail to treat our patients humanely if we willfully withhold from them agents of low risk that may assist them in improving their health status. We also fail to treat our patients humanely if we give these agents to all who ask for them without an appropriate evaluation and without following appropriate criteria for treatment.

M. L. Elks, MD, PhD
Texas Tech University
Health Sciences Center
Lubbock, Texas

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