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ALTERNATIVE THERAPIES IN WOMEN'S HEALTH

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Is Estriol Safe?

By Anthony R. Scialli, MD, and Adriane Fugh-Berman, MD

IN THE WAKE OF THE WOMEN'S HEALTH INITIATIVE (WHI), PRACTITIONERS can expect an upsurge in questions about safer alternatives to conjugated estrogens with progestin. Estriol, either on its own or as part of a mixed estrogen formulation, has long been promoted by some alternative medicine practitioners as a natural, safe form of estrogen therapy. Is there any evidence that estriol is safer than conjugated estrogens?

Estriol is an endogenous ovarian hormone available in pharmaceutical preparations to treat hot flashes or vaginal dryness.[1] A weak, short-acting estrogen, more popular in Europe, estriol is not commonly used by conventional medical practitioners in the United States.

Tri-estrogen preparations (also called Tri-est) are mixtures of estrone, estradiol, and estriol; typically, the proportions are 80% estriol, 10% estradiol, and 10% estrone. Tri-est usually is administered in doses of 2.5-5 mg/d, either continuously or 25 days a month. No clinical trials have been performed on this preparation. Natural progesterone often is prescribed with this regimen (topical forms, however, do not achieve serum levels high enough to ensure endometrial protection, and should not be used for that purpose *(see* Alternative Therapies in Women's Health, *June 1999)*.

Estrogenic Effects on Endometrium

Estriol and Tri-est proponents claim that estriol does not cause endometrial stimulation, but evidence does not support this claim. A recent study claiming that estriol does not cause endometrial stimulation distorts its own data. A cross-sectional multicenter study of 241 postmenopausal women in Sweden compared 125 women who had taken oral estriol (1-2 mg) with 116 women who had not taken any hormone replacement therapy (HRT) for at least one year.[2] All women underwent transvaginal ultrasound and endometrial biopsy. Mean treatment duration was 4.3 years (median 3 years). Fourteen women in the control group also had used estriol in the past for a period ranging from 0.5-15 years, but all had stopped at least a year prior to study entry.

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INSIDE

Although the authors concluded that "No clinically relevant difference was found between the endometrium status [assessed by histology and TVS] and untreated controls. This trial supports the endometrial safety of maintenance treatment with oral estriol … ," we disagree, based on the data presented in the study. The authors admit that mean endometrial thickness was significantly higher in the estriol group, but neglect to mention that the number of women with clinically important thickening (defined by them as > 4 mm by transvaginal ultrasound) also was increased in the estriol group. Histological diagnosis was obtained for 201 women. One endometrial cancer was found in the control group. Mean endometrial thickness was 3.7 mm in the estriol group (SD 2.83, range 1-19 mm), for controls 2.5 mm (SD 1.75, range 1-11 mm). Eighty-nine percent in the estriol group had an endometrium measuring ≤ 4 mm, compared to 97% of controls. Using the Fisher exact test, we got a P value of 0.0273, a significant difference between groups. Fourteen of 99 (14.1%) estriol users and three of 102 (2.9%) controls had polyps. Again, statistical comparison does not appear in the paper, but using the Fisher exact test results in a P value of 0.0048, providing additional support for our conclusion that endometrial stimulation is associated with estriol. The authors dismissed their own findings with the statement, "the histopathological diagnosis between the groups are not statistically significant."

As the risk of endometrial hyperplasia and cancer increase with duration of estrogen use, data should have been broken out by how long women had taken estriol. Providing only mean and median duration of use is not acceptable. Additionally, the cross-sectional design of the study would be expected to underestimate risk because it would not identify women who had discontinued estriol due to postmenopausal bleeding, endometrial hyperplasia, or other estrogen-related problems.

Oddly, a relevant previous study done by the same author is not mentioned in this recent paper. This study, of 1,110 women with irregular bleeding on HRT or postmenopausal bleeding while not on HRT, found that endometrial hyperplasia was significantly more common in women taking estriol than in women taking sequential estrogen and progestin therapy or women not receiving HRT.[3]

Unopposed estrogen stimulation of the endometrium is a risk factor for endometrial cancer, with which estriol also has been associated. A case-control study in Sweden compared 789 postmenopausal women with endometrial cancer with 3,368 controls. Five years of oral estriol 1-2 mg/d was associated with tripling the risk of endometrial cancer; the risk of atypical endometrial hyperplasia was increased 8.3-fold, compared to never-users.[4] Ever-use of oral estriol was associated with a doubling of endometrial cancer risk (vaginal estriol was not associated with significantly increased in risk). An earlier prospective cohort study in Sweden found no increase in endometrial cancer rates among estriol users, but data on duration and recency of use were not available.[5]

Breast Cancer

Since the WHI found an increase in breast cancer risk with conjugated estrogens and medroxyprogesterone acetate, natural hormone proponents can be expected to resurrect the claim that estriol and Tri-est decrease breast cancer risk. These claims are based on papers published by Henry M. Lemon during the 1960s and 1970s that claimed that estriol had potential in preventing and treating breast cancer. A 1978 commentary by Follingstad[6] cites an unpublished study by Lemon in which an unspecified number of postmenopausal breast cancer patients received between 2.5 and 15 mg of estriol for an unspecified amount of time; 37% were said to have had remission or arrest of metastasis. Lemon never published a paper with those figures in it, but in 1980 published a review on estriol in which he described

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giving oral estriol 5-15 mg/d to 24 subjects with breast cancer. No therapeutic effect was claimed. Despite the fact that six subjects (25%) experienced increased growth of metastases and two developed endometrial hyperplasia, the author's enthusiasm for estriol appears to have remained undimmed.

Conclusion

There is no reasonable scientific evidence that estriol has anticancer effects or that it is safer than estradiol or conjugated estrogens. The most recent study adds to the growing body of data that show that unopposed oral estriol significantly increases the risk of endometrial pathology. Physicians should discourage the use of unopposed estriol or Tri-est. ❖

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The Calcium-Phosphate Connection

By Robert P. Heaney, MD

BONE MINERAL CONSISTS OF CALCIUM PHOSPHATE. NOT surprisingly, both calcium and phosphorus must be ingested in sufficient quantity to build bone. Although the importance of calcium intake has received the most attention in recent years, vitamin D and even phosphorus intakes also may be marginal. Hence, supplying only calcium may do little more than uncover other nutritional shortfalls.

The one study that used all three of the key nutrients (calcium, phosphorus, and vitamin D) involved no anti-osteoporosis drugs at all; yet it produced a 43% reduction in fracture risk within 18 months of beginning the nutritional repletion.[1] The calcium intake required to maintain bone mass in healthy adults has been reasonably well defined,[2] but precisely how much calcium, phosphorus, and vitamin D may be needed to optimize bone mass, with or without bone-active drugs, is not known with certainty.

A little-recognized potential for interference between minerals arises with high-dose calcium-only supplementation. Calcium is known to bind phosphorus in the gut and to block its absorption; this property is used therapeutically to manage phosphorus absorption in patients with end-stage renal disease. Although this effect is generally known, it has not been seriously considered with respect to the use of calcium supplements. B.E.C. Nordin and I examined this issue by analyzing 636 individual calcium and phosphorus balance studies performed mostly in middle-aged and elderly women.[3] We observed that, even at levels found in typical diets, calcium does interfere with phosphorus absorption.

Quantitatively, 500 mg of calcium prevented the absorption of about 166 mg of phosphorus.

Typical American and British diets contain more phosphorus than calcium;[4] therefore, this calcium-phosphorus interaction has no appreciable effect on food sources of both minerals (i.e., dietary calcium will not block enough dietary phosphorus to make a difference). However, when extra calcium is given as the carbonate or citrate salts (especially when given with meals, as is usual to optimize calcium absorption), substantial interference with absorption of food phosphorus can occur. Additionally, women with restricted meat and dairy intakes usually have low total phosphorus intakes;[4] in such individuals, supplementation with 1,500 mg calcium could effectively prevent absorption of most or all dietary phosphorus. As both calcium and phosphorus are necessary to mineralize new bone, high doses of calcium-only supplements could counter the very purpose for which they are being used.

Against this background of potentially destructive interference, it is helpful to examine the issue quantitatively. Although most U.S. diets are relatively phosphorus-rich, about 10% of women older than age 60 ingest less than 490 mg of phosphorus per day (i.e., less than 70% of the RDA), and 15% of women older than age 80 fall below this level.[4] These individuals, who are at the greatest risk for phosphorus insufficiency, also are the ones likely to be taking calcium supplements, and likely to be taking anti-osteoporosis drugs.

The bisphosphonates and raloxifene are mainly resorption suppressors and produce a slow reversal of bone loss by virtue of inhibiting bone resorption more than bone formation. After a 3-5% increase in bone density in the first year of treatment, antiresorptive drugs produce a steady-state gain of 0.5-1.0% per year.[5] Teriparatide, an injectable fragment of parathyroid hormone (PTH) soon to be on the market, directly stimulates osteoblastic new bone formation by osteoblasts, and is capable of increasing axial bone mass by as much as 10-15% per year.

All clinical trials of antiresorptives have tested the drugs with supplemental calcium, most commonly 500 mg of calcium carbonate per day. In some cases, vitamin D also was given. Will patients treated with antiresorptive agents have enough total mineral to sustain steady-state bone gain?

An increase of 0.5-1.0% bone mass per year, typically produced by antiresorptive drugs, translates to a positive balance of 10-20 mg calcium/d and 4.6-9.3 mg phosphorus/d. Given the ability of the kidneys to reduce phosphorus loss, it is likely that enough phosphorus will be available to meet this modest need, even in the face of typical doses of calcium supplements. However, with teriparatide, the mineral demand is an order of magnitude greater; a bone gain of 10-15% translates to retention of 93-140 mg phosphorus/d.[6,7] For women with low food phosphorus intakes who are receiving high-dose calcium-only supplements, this becomes much more problematic.

Two potential solutions to the problem of interference can be suggested. Calcium supplements can be given between meals or at bedtime, so as to minimize interactions of the supplemental calcium with food phosphorus. The other option is to use a calcium-phosphate supplement, instead of a carbonate or citrate product. Between-meal dosing is the less attractive option because absorption is most efficient when calcium is taken with food,[8] and elderly individuals often absorb calcium very poorly on an empty stomach. Protecting phosphorus absorption while reducing calcium absorption defeats the purpose of adding calcium. The use of a calcium-phosphate salt, on the other hand, provides both minerals needed for bone mineralization and seems a more natural solution.

Knowing that calcium binds with phosphorus, one might wonder how calcium phosphate could possibly work as a calcium supplement. The answer lies in the quantitative aspects of the binding. In tricalcium phosphate, there is a surplus of about 100 mg phosphorus for each 500 mg calcium, over and above what is bound; and in dicalcium phosphate, the surplus is even greater. So both currently used calcium phosphates contain more phosphorus than their calcium can bind. Moreover, the supplemental calcium, by complexing with phosphate in the supplement itself, is not available to block absorption of food phosphorus. So a calcium phosphate supplement, taken with food, ensures ample phosphorus as well as calcium to support bone building.

There is even more direct evidence that calcium-phosphate sources are efficacious in supporting bone mineralization. The most dramatic of the calcium supplementation trials reported to date used tricalcium phosphate as the calcium source.[1] In this randomized, placebo-controlled trial in 3,270 ambulatory elderly French women in nursing homes (1,765 completed), supplementation of tricalcium phosphate 1,200 mg and vitamin D3 800 IU for 18 months reduced hip fracture incidence by 43% among those who completed the trial. All non-vertebral fractures were reduced by 26% in an intention-to-treat analysis.

Moreover, milk, the primary source of minerals for bone building in all young mammals, contains calcium and phosphorus in about the same ratio as dicalcium phosphate. Clearly, therefore, phosphorus-rich calcium sources work at least as well as calcium-only sources. Finally, studies in my laboratory at Creighton University have shown that calcium as a phosphate salt is absorbed nearly as well as the carbonate or citrate salts.[9]

A final question is whether the total amount of mineral usually given with current anti-osteoporosis therapies is sufficient to allow the available drugs to exert their maximal effect. Data directly addressing this question are sparse. However, an abundance of relevant calcium physiological data suggests that this question tentatively should be answered in the negative.

Net calcium absorption from a 500 mg supplement averages only about 10% (i.e., 50 mg, and half of that typically is spilled into the urine, leaving only 25 mg to offset sweat losses and to support net bone mineralization).[3,10,11] Resorptive interference from the bisphosphonates and raloxifene leads to increased PTH secretion, which in turn would result in somewhat better utilization of ingested calcium than the above estimates.[1] Nevertheless, 25 mg is very close to the minimum amount needed for a bone gain of 1% per year. Given the fact that most antiresorptives have been tested with only 500 mg of supplemental calcium, and the bone gain reported with their use is about the maximum that the available calcium could support, it is plausible that more calcium could have resulted in greater bone gain. Bones are made out of minerals, after all, not hormones or drugs, and the amount of bone that can be made must ultimately be limited by availability of the bulk raw materials.

This limiting feature of inadequate mineral support is brought into bold relief in the case of teriparatide. Using 1,000 mg of supplemental calcium, Neer et al reported teriparatide-induced bone gain at the spine of 9% per year,[6] while Arnaud et al, using 50% more calcium (1,500 mg), reported 15% per year,[7] or bone gain that was 67% greater. An often-ignored fact is that rats, which respond exuberantly to intermittent PTH, normally are fed diets with calcium and phosphorus densities several times those of the human diet.

For the moment these issues cannot be settled definitively. Trials need to be done to establish: 1) the absolute quantity of supplemental mineral required to support the full potential of today's anti-osteoporosis drugs; and 2) whether a phosphate salt of calcium better supports bone building than a carbonate or citrate salt. While waiting for answers it would seem that the prudent course would be to ensure a total calcium intake (food plus supplement) of at least 1,500 mg/d, and to use a phosphate salt—at least for those with low dairy and meat intakes and those receiving teriparatide. *[Editor's Note: Calcium phosphate supplements are widely available, most commonly listed as calcium hydroxyapatite or ossein hydroxyapatite compound.]* ❖

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High-Meat Intake During Pregnancy

Source: Shiell AW, et al. High-meat, low-carbohydrate diet in pregnancy. Relation to adult blood pressure in the offspring. *Hypertension* 2001;38:1282-1288.

A STUDY OF 626 MEN AND WOMEN IN MOTHERWELL, Scotland, evaluated maternal dietary characteristics during pregnancy on blood pressure of progeny at age 27-30 years. Mothers had participated in a dietary intervention program in which they had been advised to eat 1 lb of red meat per day and to avoid carbohydrate-rich foods.

The offspring of women reporting greater consumption of meat and fish during the second half of pregnancy had higher systolic blood pressures as adults. The regression coefficient was 0.19 mm Hg per portion per week. Diastolic blood pressure in adults was associated with higher maternal pregnancy consumption of fish but not meat; the regression coefficient was 1 mm Hg per serving per week.

The associations were independent of maternal blood pressure, body size, and tobacco use. The associations may reflect metabolic stress associated with high essential amino acid intake without adequate nutrients to utilize those amino acids.

COMMENTS BY ANTHONY R. SCIALLI, MD

It would not be surprising to learn that maternal nutrition is important in pregnancy outcome; however, the

proposal that meat intake during pregnancy plays an important role in the health of adult offspring is not intuitive. Certainly, low birth weight has been shown to be associated with hypertension in later life, but low birth weight might be due to severe maternal undernutrition or to a number of other pregnancy disorders. Fetal starvation might be imagined to produce compensatory changes that could result in long-lasting alterations in an individual's cardiovascular system. But the proposition that normally grown babies might be affected three decades later by imbalances in maternal diet appears farfetched given the important postnatal factors that influence the health of adults, such as diet, exercise, and the use of tobacco and ethanol.

The test of the influence of a high meat diet during pregnancy was made possible by advice given to women in parts of Scotland that preeclampsia could be prevented by a high intake of animal protein, specifically red meat. The idea appears foolish in the 21st century, particularly in view of the current notion that animal products are highly toxic to all human life forms, but between 1952 and 1976, it was one of the many ridiculous things that obstetricians told pregnant women.

Compliance with the dietary advice was assessed during prenatal visits using specially designed forms for recording diet. Numbers of servings per week were recorded for 10 food groups: meat, fish, eggs, cheese, green vegetables, potatoes, bread, cakes (including scones and biscuits), sweets, and milk. Women were discouraged from eating potatoes, rolls, scones, cakes, or biscuits, under the assumption that carbohydrate-rich foods were bad. Fish, eggs, and cheese were encouraged and 1 lb of red meat per day was prescribed. The evaluation of offspring ages 27-30 years included information on ethanol intake. Height, weight, and blood pressure were measured by field workers. Three blood pressure readings were taken using an automated cuff, and the average of the three was used. Cuff size, sex, alcohol intake, and body mass index were adjusted with linear regression analysis.

The relationship between maternal intake of meat and fish and blood pressure was evaluated using multiple regression techniques. Thus, there was no control group in a traditional sense; rather, the relationship between dietary intake and blood pressure was evaluated as a continuous correlation. Other correlations also were evaluated. For example, number of servings of carbohydrate was inversely proportional to pregnancy weight gain.

The increase in systolic blood pressure with meat and fish, and the increase in diastolic blood pressure with fish were modest but statistically significant. The effect was particularly noteworthy if the mother had a low intake of green vegetables. Mean systolic blood pressure in offspring of women who consumed 11 or fewer portions of meat and fish per week and at least seven servings of green vegetables per week was 118, compared to a mean blood pressure of 124 in women consuming more than 21 servings of meat and fish per week and fewer than seven servings of green vegetables.

Is a 6 mm Hg increase in systolic blood pressure important? It could be, if it represented a shift in blood pressure distribution upwards. Even though 124 is a normal blood pressure, an equivalent increase across the blood pressure distribution would result in an increase in the number of people diagnosed as hypertensive, with consequent morbidity either from the effects of the high blood pressure or from the effects of the medications used to treat high blood pressure.

How reliable are the results of this study? Could women have made up their dietary intakes at the time of their prenatal visits, perhaps to please the staff or to avoid being scolded?

This possibility is unlikely to explain the results, because such an explanation would require a made-up dietary intake to correlate with offspring blood pressure 30 years later. Also, the correlation between meat intake and blood pressure held only for the second half of pregnancy, and not for the first half, suggesting a physiologic event unrelated to maternal lying. In addition, urinary protein excretion was measured in 10 women and showed the expected correlation with reported meat intake. Finally, this study was undertaken after publication of a study from Aberdeen, in which reported maternal meat consumption showed a similar relation to offspring blood pressure. In the Aberdeen study, dietary intakes were recorded without the women having received any particular dietary instruction, and represented freedom of food choice.

The authors' proposal that high animal protein intake during pregnancy can result in long-lasting cardiovascular changes in the fetus is based on the premise that excess essential amino acids require oxidation, a folate-dependent process. In women with low intake of potatoes and green vegetables, inadequate folate is postulated to have resulted in nutritional "stress" (their term) and consequent maternal cortisol secretion. How exactly maternal cortisol would act on the fetal cardiovascular system in a long-lasting manner is not stated; the image of a fetus on steroids is as far as the authors take us mechanistically.

There are other explanations that have not been adequately considered. The first is the salt content of the maternal diets. The specific instruction to the pregnant

women in this clinic was to use canned corned beef in place of fruit or biscuits for between meal snacks. The amount of corned beef or other canned meat that was used by these women was not evaluated, and salt seems as attractive a villain as essential amino acids. To be fair, the authors considered this possibility, but could go nowhere with it given the diet records available to them.

A more compelling concern is that of maternal wealth. Eating 1 lb/d of red meat costs more than eating green vegetables. It is likely that women in this clinic were equally brow-beaten about their diets, and perhaps only the wealthier of clients could afford to comply. If so, the study might be more a correlation of family wealth with blood pressure. Family wealth could certainly have effects on health that extended decades after birth.

Finally, there is a possibility that maternal dietary habits during pregnancy were taught to offspring. Kids born to women who used canned corned beef instead of fruit may be more likely themselves to adopt such dietary preferences. Although offspring body mass index was included in the regression, dietary fat and salt intake in the offspring may still have played a role.

In spite of these alternative explanations, this paper represents a particularly competent study in which statistical analyses were used as tools and aids rather than as poorly understood window-dressing. The most important negative comment goes to forcing this sentence on an otherwise flawless presentation: "An expanded Methods section can be found in an online data supplement available at http://www.hypertensionaha.org." Call me old-fashioned, but I still read paper. Given the importance of the methods section in the evaluation of a scientific paper, I expect editors to give me everything. ❖

CME Questions

15. Estriol causes endometrial stimulation.
a. True
b. False

16. Tri-estrogen preparations have been shown to be:
a. safe.
b. effective.
c. both safe and effective.
d. neither safe nor effective.

17. Estriol reduces breast cancer risk.
a. True
b. False

18. Calcium enhances the absorption of phosphate.
a. True
b. False

19. The effectiveness of antiresorptive drugs given without supplemental calcium is known.
a. True
b. False

Clinical Abstracts

With Comments by Anthony R. Scialli, MD, and Adriane Fugh-Berman, MD

Red Clover and Hot Flashes

Source: Van de Weijer PHM, Barentsen R. Isoflavones from red clover (Promensil®) significantly reduce menopausal hot flush symptoms compared with placebo. *Maturitas* 2002;42:187-193.

Design: Randomized placebo controlled trial.

Subjects: Thirty menopausal women with more than 12 months of amenorrhea who were experiencing more than five hot flushes per day.

Treatment: Four-week placebo run-in followed by two tablets each morning of placebo or 40 mg Promensil for 12 weeks. Promensil is advertised as being manufactured from three varieties of red clover, using a standardized extraction and blending procedure to yield a proprietary ratio of daidzein, genistein, biochanin, and formononetin.

Outcome Measures: Hot flash counts and urinary isoflavones, not otherwise specified. "Overall menopausal symptoms," were measured by the Green score, a menopause symptom score.

Results: During the four-week placebo run-in, median hot flash number decreased from six per day to five per day. The median number of hot flashes continued to decrease among women randomized to Promensil by as much as 56%, but not among women randomized to remain on placebo. The proportion of women with hot flashes less than the median for the entire group was significantly lower in the Promensil group than in the placebo group. There was no difference between groups in the Greene score.

Funding: Novogen Ltd, Australia (the manufacturer of Promensil).

Comments by Anthony R. Scialli, MD: There is an old saying in the clinical trial biz: Statistics are like political prisoners; if you torture them long enough, they will tell you anything you want to hear. Nowhere is this adage more demonstrated than this effort on the part of these authors to show that Promensil works for hot flashes.

This randomized placebo-controlled double-blind study used an excellent design, including a four-week placebo run-in (important because menopausal hot flushes are believed to be placebo-responsive). This trial, then, had the potential to show whether Promensil is any good for hot flashes.

While the authors get an A for study design, they get an F for analysis. Make that an F-. The results of this study are completely obscured by statistical bamboozling and a refusal to give real numbers so the reader can tell what actually happened. The use of medians for number of hot flashes is appropriate because the number of hot flashes among women in both groups was not distributed normally. The median, however, is only an indication of central tendency of a distribution, and gives no information about the range of values that was obtained. When the authors report that the median number of hot flashes decreased in the Promensil group by 56% at week 10, they are carefully avoiding telling us how many women got better and how many women got worse. In fact, we cannot even tell what they mean by a 56% decrease: The median hot flash number at week 0 was said to be 6. A 56% in a median of 6 would be a median of 2.64. The median is the middle value in a distribution. As there are 15 subjects in this study, the median is the eighth number when you put all the numbers in ascending or descending order. In other words, the median in this sample must be a whole number and can't be 2.64!

More subterfuge: The evaluation of improvement was based on the proportion of each group above and below the median for the total group. This method of analysis is tortured indeed. Why not simply use a nonparametric test on the number of hot flashes in each group? There are only two reasons why they did not use a straightforward statistical test: They know less about statistics than my dogs, or when the appropriate analysis was performed, it showed no statistical difference.

Although they withheld data that might have permitted us to understand what actually happened, one of the tables in the paper hints at the problems they must have had creating the appearance of effectiveness: The placebo group at the end of the study had a mean daily hot flush count of 6.04 ± 5.5. We understand that the mean is not very … er, meaningful with a skewed distribution, but we can guess from this standard deviation that there must have been women on placebo without any hot flushes at week 12, as well as women with quite a few hot flushes. These pesky placebo-responders probably made it harder to show that Promensil is worthwhile, thus making trickery the only viable statistical technique. ❖

Acupuncture During Labor

Source: Ramnero A, et al. Acupuncture treatment during labor—a randomized controlled trial. *Br J Obstet Gynecol* 2002;100: 637-644.

Design/Setting/Subjects: Randomized controlled trial of 90 women delivering in a tertiary care hospital in Sweden. Forty-six received an individualized acupuncture treatment during labor by their attending midwives, who had undergone a four-day course in acupuncture for labor pain as a complementary or alternative treatment to conventional analgesia during labor.

Main Outcome Measures: Hourly assessment of pain intensity and degree of relaxation during labor.

Results: Acupuncture treatment during labor reduced the need for epidural analgesia (12% vs. 22%, relative risk [RR] 0.52, 95% confidence interval [CI] 0.30-0.92). Women receiving acupuncture noted better relaxation compared to controls, but there was no difference between groups in assessments of pain intensity or in labor outcomes (including cesarean sections, duration of labor, oxytocin augmentation, or infant outcomes). No adverse effects were seen.

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Comments by Adriane Fugh-Berman, MD: In Sweden, almost all women receive prenatal care from and are delivered by midwives. In this study, acupuncture appeared to be a beneficial adjunct to midwivery care. However, no effort was made to sham-control this study, a limitation noted by the authors. An additional limitation is that mean pain scores were used to compare groups (a statistically inappropriate but popular analytic technique). Of note, the midwives who administered acupuncture had undergone a very short course in the technique, and had learned a limited set of points commonly used for labor pain.

Acupuncture during labor is unlikely to become a trend in U.S. hospitals. Relatively few doctors and nurses are trained in acupuncture, and hospitals have been understandably leery of allowing unaffiliated practitioners to perform invasive procedures within the hospital. In this study, it was attending midwives who were performing acupuncture. In the United States, there are training courses in ear acupuncture for substance abuse treatment, but that is the only treatment-limited form of acupuncture training available. It is quite doubtful that quickie acupuncture courses for labor would be accepted in North America. ❖

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