

Letters

STORAGE OF NEWBORN STEM CELLS FOR FUTURE USE

To the Editor:

In "Storage of Newborn Stem Cells for Future Use" (OBSTET GYNECOL 1997;89:300-3), Wiley and Kuller discuss the inherent value of umbilical cord blood and its use in stem cell transplantation as an alternative to bone marrow. We disagree with the authors' portrayal of private cord blood banking. The authors assert that the role of private banking is only for autologous use, when in fact patients privately bank their newborn's cord blood primarily for its potential use within their family—related, allogeneic use. The first and many of the cord blood transplants to date have been sibling to sibling.¹

The article fails to recognize the importance of private cord blood banking when there is another child in need of a stem cell transplant or when the family has a significant health history or genetic risk that may result in a family member's future need. Private cord blood banking provides the family a readily available, lower cost alternative, free of the risk and pain of a bone marrow harvest. Moreover, insurance companies and Medicaid are providing coverage in such cases of need or risk, even when it is the parent with the known malignancy or disorder.

We agree with the authors' position supporting public (unrelated) cord blood banks to supplement the national marrow registry. It is premature, however, to assert that public cord blood banking is the panacea for stem cell transplantation. Furthermore, the authors' concern that private banking programs will limit the development of public donor banks is inaccurate. The three federally funded public banks are collecting cord blood from less than 0.1% of all births in the United States during the next 4 years. The likelihood that private banks will impede or take away from these public banks is difficult to support.

It is important that expectant families have access to bank their newborn's cord blood privately whether there is a need or risk, or if they choose to bank based on their own personal experience and assessment of their

risk profile. There is a definite need for both private and public cord blood banks, and both options should be presented factually, be balanced, and include the scientific unknowns and potential.

John T. Repke
Cynthia A. Fisher
Viacord Inc.
551 Boylston Street, Suite 40
Boston, MA 02116

Reference

1. Wagner JE, Kernan NA, Steinbuch M, Broxmeyer HE, Gluckman E. Allogeneic sibling umbilical-cord-blood transplantation in children with malignant and non-malignant disease. *Lancet* 1995;346:214-9.

In reply:

Repke and Fisher disagree with our portrayal of private cord blood banking, specifically with reference to allogeneic use within the family. We did not specifically exclude directed cord blood banking for affected family members or for families with high risk for certain genetic diseases. In addition, we did not assert that private cord blood banking was for autologous use only. In our clinical commentary we discussed the general use of cord blood for stem cell transplantation and tried to place, in perspective, the questionable benefit of directed autologous cord blood banking.¹

We agree that, if there is a child or other family member in need of an allogeneic stem cell transplant, the cord blood of a newborn sibling should be collected and typed for storage and potential use. However, in each of these particular situations, this decision and storage should be made with full consultation with a pediatric hematologist-oncologist and in conjunction with a pediatric transplant center. In situations in which transplant is a possibility, the transplant center is fully capable of arranging for collection and storage of the product and should be the choice for this procedure. This is true when the transplant is urgent or in situations in which transplant is a significant future possibility (leukemia in remission, high risk for future genetic disease in siblings). Because the transplant center in conjunction with the family will be making the determination of the type of transplant necessary, this obviates the need for private cord blood banking.

Additionally, there is still controversy as to which source of stem cells is most desirable. Graft failure, the most serious post-transplant complication, is still more frequent with cord blood transplants (15%) than when marrow is the source (1-2%). The risk of graft failure is highest in transplants for genetic disease using cord blood. For this and many other reasons, allogeneic cord blood transplant should be limited to qualified trans-

plant centers. We agree that public cord blood banking is not the panacea for stem cell transplantation. This is certainly true for private banking as well.

Joseph A. Wiley, MD
Jeffrey A. Kuller, MD
Department of Pediatrics
University of North Carolina School of Medicine
417 MacNider Building, CB# 7220
Chapel Hill, NC 27599-7220

Reference

1. Wagner JE, Kernan NA, Steinbuch M, Broxmeyer HE, Gluckman E. Allogeneic sibling umbilical-cord-blood transplantation in children with malignant and non-malignant disease. *Lancet* 1995;346:214-9.

COMPLICATIONS AND RECOVERY FROM LAPAROSCOPY-ASSISTED VAGINAL HYSTERECTOMY COMPARED WITH ABDOMINAL AND VAGINAL HYSTERECTOMY

To the Editor:

Meikle and her colleagues¹ review the outcome of laparoscopy-assisted vaginal hysterectomy compared with abdominal and vaginal hysterectomy. However, they based their investigation on a MEDLINE search using the term "laparoscopy-assisted vaginal hysterectomy," whereas the commonly accepted term for this procedure is "laparoscopically assisted vaginal hysterectomy." A current MEDLINE search revealed 96 papers using the latter term compared with only 69 papers using the term applied in their review. This may explain the fact that they identified only two randomized trials published up to September 1995, omitting from their review two additional randomized studies^{2,3} (a fifth study⁴ is not recognized in their review as randomized). The authors also failed to analyze other large series of laparoscopy-assisted vaginal hysterectomy published during the period of their review indicating a low rate of operative complications. This is a serious flaw, because laparoscopy-assisted vaginal hysterectomy is a relatively new procedure and most of the studies published so far reported very preliminary experience. Moreover, the reviewers even disregard randomized cost comparisons related to laparoscopy-assisted vaginal hysterectomy.⁵ In addition, it is unclear how they excluded all total laparoscopic hysterectomies, which they classify as "type 4" laparoscopy-assisted vaginal

hysterectomy, while stating in the "Discussion" section that "[c]lassification of the type of laparoscopy-assisted vaginal hysterectomy performed was difficult and unreliable."

The extended duration of surgery and the remarkably short convalescence characteristic of laparoscopy-assisted vaginal hysterectomy long have been recognized. But it may be too early to conclude about the safety of laparoscopy-assisted vaginal hysterectomy without resorting to large controlled trials performed by surgeons adequately trained in both procedures.

Daniel S. Seidman, MD
J. Buchmann Gynecology & Maternity Center
The Chaim Sheba Medical Center
Tel-Hashomer 52621
Israel

References

1. Meikle SF, Nugent EW, Orleans M. Complications and recovery from laparoscopy-assisted vaginal hysterectomy compared with abdominal and vaginal hysterectomy. *Obstet Gynecol* 1997;89:304-11.
2. Nezhat F, Nezhat C, Gordon S, Wilkins E. Laparoscopic versus abdominal hysterectomy. *J Reprod Med* 1992;3:247-50.
3. Phipps JH, Nayak JS. Comparison of laparoscopically assisted vaginal hysterectomy and bilateral salpingo-oophorectomy with conventional abdominal hysterectomy and bilateral salpingo-oophorectomy. *Br J Obstet Gynaecol* 1993;100:698-700.
4. Richardson RE. Is laparoscopic hysterectomy a waste of time? *Lancet* 1995;345:36-41.
5. Nezhat C, Bess O, Admon D, Nezhat CH, Nezhat F. Hospital cost comparison between abdominal, vaginal, and laparoscopy-assisted vaginal hysterectomies. *Obstet Gynecol* 1994;83:713-6.

In reply:

We identified 139 articles using the term "laparoscopic-assisted vaginal hysterectomy" in our initial MEDLINE search; "laparoscopic" was replaced by "laparoscopy" by journal editors. We did not rely solely on a MEDLINE search but also considered all of the references in the bibliographies of the reports included in our review as well as bibliographies in book chapters and other literature summaries, resulting in the evaluation of hundreds of sources.

Seidman states incorrectly that we failed to analyze other reports. We evaluated each of the reports cited by Seidman; the reasons for their exclusions can be found in the "Methods" section of our article. To compare the outcomes of a similar intervention, our case definition excluded reports of total and supracervical procedures. As documented in our report, we avoided duplication of treatments and outcomes by excluding patients who were study subjects in more than one article by the same author. Thus, with careful examination of the literature, some of the patients that Seidman stated

were excluded actually were included in this study, but only in the report with the largest series by that particular author. We are not certain that we located one of the reports Seidman cited¹ because although we found a reference with the same first author and journal dates and pages, the secondary authors in Seidman's citation do not match those in this publication. Finally, because we were evaluating complication rates, we excluded reports that did not study complications.²

Seidman was unclear how total laparoscopic hysterectomies were excluded. As stated in the "Methods" section, the application of a classification scheme to the types of laparoscopy-assisted vaginal hysterectomy was difficult in this retrospective analysis. However, total and supracervical procedures generally were classified by the authors of the reports.

We agree that a randomized study would be the definitive manner to evaluate the safety of laparoscopy-assisted vaginal hysterectomy, but as yet no study with sufficient power has been conducted. Until then, we hope that our review of the current literature has made a contribution.

Susan F. Meikle, MD, MSPH
Elizabeth Nugent, BS
Miriam Orleans, PhD
Colorado Permanente Medical Group
10350 East Dakota Avenue
Denver, CO 80232

References

1. Casey MJ, Garcia-Padial J, Johnson C, Osborne NG, Sotoongo J, Watson P. A comparison of laparoscopic assisted vaginal hysterectomies compared with vaginal hysterectomy unassisted by laparoscopy and transabdominal hysterectomies. *J Gynecol Surg* 1994;10:7-14.
2. Nezhat C, Bess O, Admon D, Nezhat CH, Nezhat F. Hospital cost comparison between abdominal, vaginal, and laparoscopy-assisted vaginal hysterectomies. *Obstet Gynecol* 1994;83:713-6.

SERUM IONIZED MAGNESIUM LEVELS IN NORMAL AND PREECLAMPTIC GESTATION

To the Editor:

Although Standley et al¹ reported that serum ionized magnesium concentrations decrease with gestational age and suggest that this provides evidence of progressive "hypomagnesemia associated with pregnancy," we found previously² that this correlation with gestational age is not independent. We also found that the 8% fall in serum concentrations of ionized magnesium was

much smaller than that of serum albumin (19%), the fall in the latter being a marker of the hypervolemia and hemodilution associated with pregnancy. Our findings suggested that the total mass of magnesium in serum actually increases during pregnancy. Thus, if there truly is a progressive magnesium deficiency in pregnancy, it is more likely to occur in areas of the body that mobilize magnesium to maintain serum concentrations within a 10% range throughout pregnancy. We wonder whether the authors also concurrently measured albumin levels, particularly in view of their finding of progressive edema in the preeclamptic group.

The authors also sampled postpartum patients to "establish non-pregnant values" of ionized magnesium, reporting a mean concentration of 1.19 ± 0.03 mg/dL, a value much lower than the concentration (1.46 ± 0.01 mg/dL) we reported³ for non-pregnant women. Their values are low despite the presumed normalization of blood volume in the postpartum period. We reported previously that ionized magnesium concentrations continue to decrease during labor,⁴ possibly as a result of stress. Is it also possible that the low ionized magnesium concentration found in the postpartum period represents a more prolonged, true hypomagnesemia?

Sara M. Handwerker, MD
Bella T. Altura, PhD
Burton M. Altura, PhD
Department of OB/GYN
North Central Bronx Hospital, Rm. 8-C-08
3424 Kossuth Avenue
Bronx, NY 10467

References

1. Standley CA, Whitty JE, Mason BA, Cotton DB. Serum ionized magnesium levels in normal and preeclamptic gestation. *Obstet Gynecol* 1997;89:24-7.
2. Handwerker SM, Altura BT, Altura BM. Serum ionized magnesium and other electrolytes in the antenatal period of human pregnancy. *J Am Coll Nutr* 1996;15:36-43.
3. Handwerker SM, Altura BT, Altura BM. Ionized serum magnesium and potassium levels in pregnant women with preeclampsia and eclampsia. *J Reprod Med* 1995;40:201-8.
4. Handwerker SM, Altura BT, Jones KY, Altura BM. Maternal-fetal transfer of ionized serum magnesium during the stress of labor and delivery: A human study. *J Am Coll Nutr* 1995;14:376-81.

In reply:

Handwerker and co-workers raised two questions regarding our work. One centers on the fact that the levels of magnesium found in our patient population in Detroit were lower than those reported by them in a different population.¹ We agree that these values are not identical and probably reflect population differences. Interestingly, this same group of scientists collected samples in Detroit, analyzed them in New York, and

found values similar to ours.² Furthermore, the relative changes seen in our study are convincing because we followed the same patients through each trimester of pregnancy and into the postpartum period longitudinally.

Their second question regards a possible dilutional effect on magnesium with advancing pregnancy. They suggest that falling serum albumin is a measure of that dilution, implying that changes in serum albumin correlate to volume of dilution, and thus, conclusions can be drawn about the electrolyte concentration from this information. Albumin is an indirect measure of the volume of dilution. Furthermore, it is the ionized component of an electrolyte that possesses biologic activity. Therefore, any relative change in an ionized electrolyte concentration observed in a group of patients longitudinally during an event such as pregnancy is of great interest. Serum albumin levels do decline in pregnancy, but the change in this hepatically-synthesized, renally-excreted protein does not solely reflect passive volume shifts. For example, serum ionized calcium levels are tightly regulated throughout pregnancy despite declining total calcium and declining albumin levels.³ We

believe there is a great deal more to the regulation of these important biologic mediators than passive dilution or protein binding.

Cynthia A. Standley, PhD
Brian A. Mason, MD
Janice E. Whitty, MD
David B. Cotton, MD
Midwestern University
19555 North 59th Avenue
Glendale, AZ 85308

References

1. Handwerker SM, Altura BT, Altura BM. Serum ionized magnesium and other electrolytes in the antenatal period of human pregnancy. *J Am Coll Nutr* 1996;15:36-43.
2. Bardicef M, Bardicef O, Sorokin Y, Altura BM, Altura BT, Cotton DB, Resnick LM. Extracellular and intracellular magnesium depletion in pregnancy and gestational diabetes. *Am J Obstet Gynecol* 1995;172:1009-13.
3. Dahlman T, Sjoberg HE, Bucht E. Calcium homeostasis in normal pregnancy and puerperium: A longitudinal study. *Acta Obstet Gynecol Scand* 1994;73:393-8.