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The authors report no conflict of interest.

REFERENCES

1. Shalom DF, Pillalamarri N, Xue X, et al. Sacral nerve stimulation reduces elevated urinary nerve growth factor levels in women with symptomatic detrusor overactivity. Am J Obstet Gynecol 2014;211:561.e1-5.
2. Chaldakov GN, Tonchev AB, Aloe L. NGF and BDNF: from nerves to adipose tissue, from neurokines to metabokines. Riv Psichiatr 2009;44:79-87.
3. Terry AV Jr, Kutiyanawalla A, Pillai A. Age-dependent alterations in nerve growth factor (NGF)-related proteins, sortilin, and learning and memory in rats. Physiol Behav 2011;102:149-57.
4. Lee JC, Pak SC, Lee SH, et al. The effect of herbal medicine on nerve growth factor in estradiol valerate-induced polycystic ovaries in rats. Am J Chin Med 2003;31:885-95.

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REPLY

We appreciate the insightful comments expressed by Agilli et al regarding our pilot study that proposed that sacral nerve stimulation may decrease urinary nerve growth factor (uNGF) levels in patients with detrusor overactivity. We would like to address their concerns about the study group in which we evaluated uNGF.

Patients in our study were excluded if they had experienced neurologic disease. Although cardiometabolic diseases have been shown to NGF, we chose to use exclusion criteria more specific to the urinary tract system that have been shown to alter uNGF levels in human patients.[1] For this reason, inflammatory conditions such as urinary tract infection and interstitial cystitis were used. Although 15 of our subjects had hypertension and 7 had hyperlipidemia, we believe that further research that will evaluate the effect of cardiometabolic diseases, specifically on uNGF in humans, must be done. Results will help determine precisely which medical comorbidities warrant consideration when patients are being selected for a proper study group in larger future studies.

Our study consisted of 23 cases and 22 control subjects; therefore, 1 case did not have an age-matched control subject. At 88 years old, this patient was an outlier and raised the difference in mean age between the groups. We agree that the mismatched number of cases and control subjects could lead to an unbalanced assessment; however, the patient was included for the purpose of obtaining maximum pilot data. Agilli et al cite that NGF could have been affected by older age.[2] In that study, older age was associated with increased levels of pro-NGF but decreased levels of NGF. In our study, we witnessed the opposite trend in which increased age was associated with elevated levels of NGF. It was also noted that there was a significant difference in body mass index between our cases and control groups. Although Agilli et al cite an article that shows that obesity may cause higher NGF levels, other studies have shown that uNGF is not associated with higher body mass index.[3]

Finally, we appreciate the observation that certain supplements may affect NGF levels. Three patients were receiving vitamin D therapy; 1 patient was receiving oral estrogen/progesterone therapy, and 1 patient was receiving vitamin D, B, and zinc supplementation. To our knowledge, the association of vitamin supplementation affecting uNGF in humans has not been shown. We look forward to performing a larger-powered study that will evaluate uNGF in patients with detrusor overactivity compared with control subjects, at which time comprehensive descriptive characteristics will be analyzed so that independent risk factors for differences in uNGF levels can be determined. ■

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The authors report no conflict of interest.

REFERENCES

1. Liu HT, Chen CY, Kuo HC. Urinary nerve growth factor levels in overactive bladder syndrome and lower urinary tract disorders. J Formos Med Assoc 2010;109:862-78.
2. Terry AV Jr, Kutiyanawalla A, Pillai A. Age-dependent alterations in nerve growth factor (NGF)-related proteins, sortilin, and learning and memory in rats. Physiol Behav 2011;102:149-57.
3. Liu HT, Chen CY, Kuo HC. Urinary nerve growth factor in women with overactive bladder syndrome. BJU Int 2011;107:799-803.

 http://dx.doi.org/10.1016/j.ajog.2014.10.009

Diagnosing placenta accreta

TO THE EDITORS: While we wish to underscore a statement made in the editorial by Dr Nageotte,[1] we wish to also add a relevant comment to the article by Bowman et al[2] discussed in this same issue. In our own institution, we have seen a significant rise in the incidence of placenta accreta (doubling in the past dozen years), emphasizing the obvious contribution of the recently rising rate of cesarean. The concern that this should raise for the average practicing obstetrician is considerable, with regard to the results of this commonly unanticipated occurrence.

Dr Bowman et al[2] questioned the value of its antepartum sonographic detection, yet the absence of consistent use of

advanced currently available features of ultrasound (eg, color Doppler, 3-dimensional imaging) may be the reason for this described lack of diagnostic benefit, which nonetheless has been documented elsewhere.[3] We respectfully challenge the conclusion made in this manuscript, since it does not correspond with our own experience. ■

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The authors report no conflict of interest.

REFERENCES

1. Nageotte MP. Always be vigilant for placenta accreta. Am J Obstet Gynecol 2014;211:87-8.
2. Bowman ZS, Eller AG, Kennedy AM, et al. Accuracy of ultrasound for the prediction of placenta accreta. Am J Obstet Gynecol 2014;211:177.e1-7.
3. Berkley EM, Abuhamad AZ. Prenatal diagnosis of placenta accreta: is sonography all we need? J Ultrasound Med 2013;32:1345-50.

 http://dx.doi.org/10.1016/j.ajog.2014.10.017

REPLY

We thank Drs Levine and Fernandez for their interest in our recent article regarding accuracy of ultrasound for the diagnosis of placenta accreta. We agree that ultrasound is reasonably useful for the diagnosis of accreta and that it may be improved with newer technology. However, our purpose was to illustrate that the modality is imperfect and that there might be room for improvement. Additionally, we discussed that the pretest probability for accreta is strongly driven by clinical history, and that posttest probabilities after ultrasound examination may not significantly alter clinical decision making in certain high-risk patients (eg, those with placenta previa and >2 prior cesarean deliveries). ■

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Home birth study fails to identify credentials of midwives conducting home birth

TO THE EDITORS: Design errors in the article by Grünebaum et al,[1] "Early and total neonatal mortality in relation to birth setting in the United States" raise serious concerns about the accuracy and interpretability of conclusions.

When midwives transfer care to physicians during labor, usually because of obstetrical complications, birth outcomes are attributed to physicians on birth certificates. This may happen more often among midwives practicing in hospital, therefore spuriously reducing rates of neonatal mortality in the hospital midwife group, the reference group for this study. This might explain the two-fold increase in mortality in the physician group and account for the exceptionally low rate of mortality rates among the hospital midwife group (0.3/1000).

Evaluation of birthplace alone without consideration of midwives' credentials confounds interpretation of findings. There is considerable variation in requirements for licensure of midwives between states. In some, midwives without formal training can attend homebirths, as acknowledged by Grünebaum et al.[2] It is highly probable that the association between place of birth and neonatal mortality is confounded by differences in midwives' training and practice. In contrast, sentinel studies of home birth have precise definitions of caregiver group.[3]

Another threat to the validity of conclusions from this study is the lack of adjustment for other confounders by, for example, maternal age, number of prenatal care visits and medical/obstetrical conditions but also access to primary obstetrical care and midwifery access to hospital privileges. Other leading epidemiological studies have addressed these issues previously.[4] Table 1 of Grünebaum et al's article[1] identifies important differences among exposure groups according to maternal age, race/ethnicity, birthweight and gestational age and the authors did not account for these differences.

Adjustment for confounders would still not make possible the attribution of outcomes to midwifery groups defined