(Johnson and Hummelshoj, 2013). It was a timely article unique in its methodology, especially its process of global networking and the inclusion of the views of women who suffer from endometriosis. The recommendations on the management of endometriosis in low resource settings and the development of centres (or networks) of expertise are commendable. However, we feel that the non-inclusion of transvaginal ultrasound scan as a non-invasive diagnostic tool of choice in the primary evaluation of women suspected of endometriosis is a significant omission.

Firstly, evidence has evolved on the role of the transvaginal ultrasound scan in the preoperative management of women with extra-ovarian endometriosis (Dessole et al., 2003; Abrao et al., 2007; Hudelist et al., 2011; Benacerraf and Groszmann, 2012). In particular, pattern recognition of the sonographic characteristics of deep pelvis infiltrating endometriosis (DIE) of the posterior pelvic compartment has been well described (Bazot et al., 2007). Furthermore, ultrasound techniques that assess the status of the pouch of Douglas (POD) have also been described (Reid et al., 2013). This is especially important as >60% of women with an obliterated POD will have evidence of bowel endometriosis (Khong et al., 2011). Indeed, a systematic preoperative ultrasonographic assessment of the pelvis has demonstrated a high detection rate and low false-positive rate in predicting POD obliteration and presence of midline DIE in women with high stage disease (Reid et al., 2011).

Preoperative ultrasound evaluation therefore provides a stepwise approach to the diagnosis of higher stage disease giving valuable preoperative information. It has the potential to facilitate the triaging of women to the appropriate network of expertise. And with the failure of laparoscopy to sometimes correctly estimate the extent of bowel endometriotic disease in the posterior compartment and incompletely excise ‘skip’ lesions, preoperative imaging enables the laparoscopic surgeon to plan the surgical procedure within the context of a multidisciplinary team approach.

Furthermore, we also believe that maximum cytoreduction at the first surgical procedure is best achieved with preoperative mapping of location and extent of disease using transvaginal ultrasound scan. This has the potential to avoid the need for a diagnostic laparoscopy (Menakaya et al., 2013), reduce patient exposure to anaesthesia and result in significant cost savings for the health system.

Secondly, transvaginal ultrasound scan is a low cost readily available non-invasive diagnostic tool that has been subjected to robust evaluation of its accuracy in predicting posterior compartment endometriotic disease (Abrao et al., 2007). In contrast to the serum biomarkers described in the consensus paper, transvaginal pelvic ultrasound is ready for immediate introduction into clinical practice even in low resource settings.

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Reply: Consensus on current management of endometriosis

Sirs,

We are grateful for the appreciation of our consensus statement on the current management of endometriosis (Johnson et al., 2013) from Menakaya and Condous and we believe they have raised valid points concerning imaging.

Our consensus process, and the resulting paper, was not designed to examine diagnostic techniques in detail. We are proposing a detailed consensus process to examine the diagnosis and classification of endometriosis to coincide with the 12th World Congress on Endometriosis in Sao Paulo, Brazil, in May 2014.

However, we acknowledge that there may be value in imaging techniques such as transvaginal ultrasound. Any diagnostic technique that accurately predicts the presence of a finding that would alter the type of surgery undertaken is valuable—the principle of ‘facilitation of triaging
of women to the appropriate network of expertise’ described by Mana-
kaya and Condous. We agree that transvaginal ultrasound might also be
helpful, as suggested, to minimise under-diagnosis of the extent of endo-
metriosis as can occur in some settings at laparoscopy.

However, the problem with transvaginal ultrasound is that its diagnos-
tic accuracy is notoriously operator dependent. Vaginal examination and
vaginal procedures must yield information that has the potential to
change management because, although medical practitioners might con-
sider these ‘non-invasive’ interventions, this is often not how they are
perceived by women (for whom there may be a lengthy history of
pelvic pain) who must undergo the diagnostic procedure. While the diag-
nostic accuracy of ultrasound in predicting ovarian endometriomas
cannot be challenged, external validation is required of the studies
that have assessed the accuracy of more sophisticated transvaginal ultrasound
markers in diagnosing deep endometriosis (such as the ‘sliding sign’
described by Reid et al. (2013)). Such external validation must include
the accuracy of ultrasound markers in predicting bowel or urinary tract
endometriosis, findings that would substantially alter preoperative
planning. Until these diagnostic accuracy studies have been externally
validated, there remain questions about the generalizability of the trans-
vaginal ultrasound signs as valuable routine diagnostic tools and their rela-
tive accuracy compared with other imaging techniques such as magnetic
resonance imaging (MRI). Nonetheless, with appropriate ultrasonogra-
phy expertise, we agree that there is a reasonable argument in
favour of offering ultrasound in the diagnostic work-up of women with
endometriosis.

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Sleep efficiency in patients with poly cystic ovarian syndrome

Sir,

Shreeve et al. (2013) conducted a case–control study targeting urinary
6-sulfatoxymelatonin (aMT6s), 8-hydroxy-2-deoxyguanosine (8-OHdG)
and sleep efficiency in 26 patients with polycystic ovarian syndrome
(PCOS) using wrist actigraphy. As a main result, significantly lower sleep
efficiency and higher aMT6s and 8-OHdG were observed in patients
with PCOS.

I have some concerns regarding their study outcome. First, the preva-
ience of obstructive sleep apnoea (OSA) has previously been found to be
higher in patients with PCOS (Nandalike et al., 2012; Randeva et al.,
2012), partly caused by insulin resistance or by an endocrine disorder.
I recommend that the authors check OSA as a sleep-related variable
for patients with PCOS.

Secondly, short sleep duration and elongated sleep latency are signifi-
cantly correlated with increased levels of anxiety and depression
(Argyriou et al., 2011). I also reported that subjectively reported short
sleep duration was significantly related to an increase in depressive epi-
isodes recorded by the Patient Health Questionnaire 9-item version
(Kawada, 2012). As these two reports handled healthy subjects, emo-
tional distress should be checked for patients with PCOS to speculate
the relationship between poor sleep quality and PCOS.

Shreeve et al. (2013) conducted a survey using physiological apparatus
to measure sleep efficiency and total sleep time. My third concern is the
information of validation for wrist actigraphy. Actigraphy is an accelerom-
eter and it does not reflect sleep status in the cases of insomniacs.
Shreeve et al. used actigraphy, namely Actiwatch®, and the cut-off
value of sensitivity for making a sleep/wake judgment was initially set
at 40 counts per minute. As there was no description of the cut-off
value, I suppose that this initial setting was used for their analysis. Strictly
speaking, the cut-off value should be set according to each test situation
by using sleep polysomnography as the gold standard.

As the number of samples is limited in the study of Shreeve et al. further
study is needed to make a definite conclusion with satisfactory statistical
power.

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