LETTER TO THE EDITOR
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# Letter to the Editor: "The Role of the Incontinence Severity Index in the Treatment of Stress Urinary Incontinence"

**İbrahim Hacıbey**

*University of Health Sciences Türkiye, Başakşehir Çam and Sakura City Hospital, Department of Urology, İstanbul, Türkiye*

**Dear Editor,**

I read with great interest the article by Öztürk and Atlıhan titled *"The Role of the Incontinence Severity Index in the Treatment of Stress Urinary Incontinence"* [J Urol Surg. 2025;12(1):34-39]. The authors provide valuable insight into the comparative efficacy of medical and surgical interventions for stress urinary incontinence (SUI) using the incontinence severity index (ISI) as a primary outcome. However, I aim to raise several critical methodological points that we believe merit further clarification or acknowledgment.

Firstly, although the authors acknowledge the retrospective design and short-term nature of the study, a crucial limitation remains unaddressed: the significant imbalance in baseline ISI severity among treatment groups. As seen in Table 2, patients in the medical treatment group had significantly milder disease at baseline, while those undergoing surgical procedures had more severe presentations (p-values for slight, moderate, and severe categories: 0.018, 0.044, and 0.032, respectively). This baseline heterogeneity introduces a confounding factor in interpreting treatment efficacy, particularly when ΔISI is compared between groups. Although designing and implementing a study based on a milder disease group may be more practical, a more robust analysis -such as adjusted comparisons or subgroup stratification- would help minimize this bias.

Secondly, the authors state that patients in the medical group received 20 mg/day of duloxetine. Although the recommended therapeutic dose of duloxetine for SUI is 80 mg/day (typically administered as 40 mg twice daily), it is common practice to initiate treatment at 40 mg/day and titrate the dose based on tolerability (1). In this study, the use of 20 mg/day remains below both the guideline-recommended and commonly initiated doses, potentially underestimating the drug's full therapeutic effect. This dosing deviation may affect the observed efficacy of medical treatment, and it could have been more explicitly acknowledged as a methodological limitation or justified with clinical rationale (e.g., tolerability concerns or prescribing practices specific to the study population).

Third, one underexplored implication of the short follow-up duration of the study is its impact on understanding the progression of patients in the medical treatment group who may ultimately require surgery. The transition from conservative to surgical treatment is common in real-world settings, particularly when pharmacologic management becomes insufficient (2). Without long-term data, the study cannot inform how many patients might have eventually opted for surgery, which weakens conclusions about the sustained value of medical therapy.

Beyond these critiques, I commend the authors for their pragmatic focus on the ISI as an accessible, patient-friendly tool. Unlike urodynamic tests, which are often costly and operator-dependent, ISI offers a low-burden method for monitoring treatment response, especially in resource-limited settings. The authors appropriately emphasize this point.

In conclusion, while the study offers meaningful contributions to the field, interpretation of comparative treatment outcomes should be tempered by the presence of baseline group imbalances, suboptimal medical dosing, and the absence of

**Correspondence:** İbrahim Hacıbey MD, University of Health Sciences Türkiye, Başakşehir Çam and Sakura City Hospital, Department of Urology, İstanbul, Türkiye
**E-mail:** drihacibey@gmail.com **ORCID-ID:** orcid.org/0000-0002-2212-5504
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long-term follow-up, needed to assess treatment sustainability and escalation. I hope these reflections will be useful for guiding future prospective and randomized research on this topic.

Sincerely,

### Ethics

**Informed Consent:** Retrospective study.

### Footnotes

**Conflict of Interest:** No conflict of interest was declared by the author.

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