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EDITORIAL

Retrospective studies in lower urinary tract dysfunction do matter☆,☆☆

Estudos retrospectivos sobre DTUI

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The article ''Clinical course of a cohort of children with non-neurogenic daytime urinary incontinence symptoms followed at a tertiary center''[1] highlights some important issues in the management of lower urinary tract dysfunction (LUTD) in children. The first issue that is recognized is the need for a consistent terminology in this field. The authors use the term urinary retention to describe what is defined in the International Children's Continence Society (ICCS) standardization document as voiding postponement: Children who habitually postpone micturition using holding maneuvers suffer from voiding postponement. This creates ambiguity for the reader and makes it difficult to perform an accurate literature search to do future studies in the topic. We need to have a concerted effort by all the authors to use standardized terminology to help the profession continue to advance scientifically.

I have respect for the editors of this journal being willing to accept a retrospective study, which has become a ''pariah'' in academic circles and journals. There has been a concerted bias in clinical medicine to downplay retrospective studies as poor quality medical research in lieu of other prospective studies that are of marginal value. I have seen an inordinate number of papers and abstracts being presented based on database reviews of billing or diagnostic data, with no input of patient information. The pediatric urology conferences are rife with these studies, with conference coordinators enamored by the fact that these are prospective studies and are rated higher than retrospective studies. The other form of study that has become prevalent is the prospective database study, with the authors rating subjective findings themselves. We are seeing numerous studies like this come to conferences and publication because they are prospective, so they surely must be better. How can someone reviewing his or her own outcomes, while already knowing what the hypothesis is, really produce an unbiased study? For some reason, editors and those in charge of conferences have given these studies greater relevance than the retrospective reviews. Good respective reviews, such as this one, can offer a tremendous amount of knowledge. Yes, there are limitations with retrospective studies, but as long as they are recognized and appropriately acknowledged, they should not prevent the presentation of novel data. In some cases, retrospective reviews can reduce bias. How? If the patients were treated with no prior intent to publish the data, the likelihood that the practitioners would be pushed to achieve a hypothesis outcome are lower. If the person reviewing the data is different from the caretaker, it also tends to reduce the risk of bias. We need retrospective reviews to give us pilot studies with novel concepts; without this, we would have to wait years for the perfect study. Having been involved in clinical trials for drugs attempting to achieve approval in the US and Europe, it can take a long time for these studies to come to fruition. It is even more devastating if the wrong hypothesis is established; then, the whole study could be for naught. In some cases, what could

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have been a novel concept can go unrecognized. There is a place for well-done and thorough retrospective reviews, such as this study.

The other important aspect of this paper is that the authors have concluded what I have been advocating and others have shown: that there is a limited role for the use of urodynamics in the evaluation of children with LUTD. They noted that 36/38 patients had urodynamic abnormalities and two did not have any abnormalities. Let us analyze this further and see if it really was necessary to perform these procedures. As the authors have shown that a good history is critical and very valuable in obtaining a diagnosis, the fact is that overactivity as evidenced by urgency, urge incontinence, and frequency would more than likely be evidence of detrusor instability on urodynamics. Even if the child has urodynamically proven detrusor instability, is the treatment going to be any different? The answer is an emphatic no. We would treat overactive symptoms the same way whether detrusor instability was or was not present. Why do urodynamics and put a child through an uncomfortable procedure unnecessarily. Could the dysfunctional voiding have been detected without urodynamics? Of course it could. The use of uroflowmetry with electromyography (EMG) of the perineum and concomitant EMG of the abdomen provides good proof of dysfunctional voiding, negating the need to do urodynamics. We can even diagnosis internal sphincter dyssynergia by utilizing lag times, as described by Combs and Glassberg.[2] Bladder overactivity has been found to correlate with short lag times and tower voiding patterns, making it more likely to make the diagnosis of diagnose overactive bladder (OAB) noninvasively.[3] Our recent publication[4] defining a quantitative approach to uroflowmetry helps remove much of the subjectivity in reading curves. We know that there is great subjectivity in the process of determining whether curves are bell *vs.* plateau and bell *vs.* tower. Using a flow index as we have devised removes that subjectivity and makes the interpretation of uroflowmetry more objective, rendering it a more powerful tool. The use of the flow index can lead to the ability to follow patients longitudinally, irrespective of age or volume voided; this combined with EMG of the perineum and abdomen creates a tool that is just as powerful as or more so than urodynamics. The authors show us that they have reached a high concordance of 85% by making the diagnosis of detrusor instability with history and non-invasive tests. This confirms our own beliefs and the work of Bael et al.[5] that there is a limited place in the management of children with OAB with urodynamics. There are some refractory patients in whom urodynamics are called for; these are the patients whom the practitioners suspect may have a neurogenic bladder, or in some cases, they wish to perform video-urodynamics because there is a need for a voiding cystourethrogram and the urodynamics is done concomitantly. But, even this is something that I would not look favorably upon, since to perform urodynamics a rectal balloon catheter needs to be inserted and sedation can not be given, making the procedure worse than a stand-alone voiding cystourethrogram (VCUG) which can be done using an amnestic or propofol anesthesia.

If the practitioners seriously suspect that the patient may have a neurogenic bladder, then it is probably advisable to do a magnetic resonance imaging (MRI) of the spine before performing urodynamics. If the MRI is normal, then the need to perform urodynamics is almost nil, as evidenced by a Stone et al.,[6] who showed that all patients that had normal MRIs did not have anything besides detrusor instability on urodynamics. If one is worried about non-neurogenic neurogenic bladder that diagnosis can be made with a VCUG showing evidence of severe trabeculation, Christmas tree shape, and external sphincter dyssynergia or internal sphincter dyssynergia on the VCUG. Therefore, the need to use invasive urodynamics should be severely restricted in children with routine symptoms of OAB.

Another fact that was interesting in the article is that the authors made note that they did not find an association between obesity and voiding symptoms, as has been noted in the literature. This may be explained by a recent article by Wang et al.[7] that looked at behavioral problems in early adolescence in a cohort of patients in Hong Kong. They did not find a link between obesity and emotional and behavioral problems. We know there is a link between emotional and behavioral problems and urinary incontinence in children and adults. Therefore, by extension, we can presume that the lack of obesity and LUTD in this cohort of patients may be geographic, ethnic, societal, and/or possibly due to cultural variations that could preclude the development of obesity in certain areas of the world. In this case, if the group of children in this study was drawn from a population of children with less access to food, then we could see a lower incidence of obesity in this study, and possibly negate the findings that have been observed in the United States and Europe, where obesity is an epidemic.

Conflicts of interest

Prof. Israel Franco is a consultant and clinical investigator for Astellas, Allergan, and Laborie Medical Technologies.

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