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Rebuttal

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Dr. Riddell and Dr. Franc-Guimond present a strong, thorough and thoughtful argument in favour of surgical intervention for the management of vesicoureteral reflux.[1] It is particularly reassuring that we see eye-to-eye on important points, most notably the role of patient selection. Nevertheless, the main question that remains to be answered is not so much if we *can* surgically correct vesicoureteral reflux, but *in whom* should it be corrected. After all, let us consider that the elegantly portrayed historical evidence reflects overall improvements in medical care, with increased awareness, better diagnostic tools and advances in medical therapy, along with the surgical innovations described. Moreover, the idea that surgical intervention has played a major role in decreasing the incidence of end-stage renal disease secondary to reflux is debatable; published data (adjusting for changes in diagnostic practices) refute the contention that our treatment efforts have had a strong impact on this outcome.[2]

Adding to the debate is the somewhat heterogeneous group of interventions that "anti-reflux surgery" includes. This has to be well-defined, considering that the popular dextranomer/hyaluronic acid endoscopic injection has been reported to have unexpectedly high recurrence rates on longer-term follow-up.[3-6] Recently published data from the Swedish Reflux Trial in Children have not shown a difference in infection rates comparing antibiotic prophylaxis and endoscopic treatment groups;[7] there was no evidence of new renal damage reported in the medical therapy group.[8] Thus, confirmation that surgery is significantly better is hard to find; this unfortunately weakens our colleagues' stand and ultimate conclusion that surgery is the "gold standard" (particularly in terms of preventing long-term problems such as reflux nephropathy). Furthermore, statements challenging the potential benefits of medical therapy can also be called into question. For example, consider the studies listed in the provided table.[1] Missing from this list of seminal publications is perhaps one of the best trials recently conducted on the topic,[9] a randomized-controlled trial that showed benefit for those patients who received prophylaxis over placebo. Indeed, important criticisms of many of the studies that have shown lack of benefit from medical therapy include the problem with inadequate power of the trials.[10] In addition, some of the arguments about the lack of benefit in early treatment of pyelonephritis are based on data that may have significant shortcomings (such as the reliance on sub-group analyses[11]).

What can we make out of this debate? As indicated in the point/counterpoint article, my personal impression is that the controversy is sometimes erroneously approached.[12] I firmly believe that one of the main problems with our management originates in the idea that there is an overall "superior approach," disregarding the principles of individualization. Patients are different in many underlying factors, some of which may be far more important than the mere presence of reflux or time-honoured descriptive characteristics (such as grade). At the end of the day, we should at least agree that better data are needed, that individualized patient care will play an increasingly important role in management and that long-term endpoints will trump the potentially meaningless early outcomes that we have often focused on until now.

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