HHS Public Access

Author manuscript

Ann Surg. Author manuscript; available in PMC 2023 August 01.

Published in final edited form as:

Ann Surg. 2022 August 01; 276(2): e138–e139. doi:10.1097/SLA.0000000000005269.

Response to "Why gastric bypass might not be a good choice for type-2 diabetes treatment"

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Thank you for the opportunity to respond to Dr. Gagner's critique of our study examining type 2 diabetes (T2D) remission following sleeve gastrectomy (SG) and Roux en Y gastric bypass (RYGB).[1]

We appreciate Dr. Gagner's deep expertise in this area and agree with him on several points. For example, as we acknowledge in our paper, 2 years of follow-up is not sufficient for drawing conclusions about the long-term durability of T2D remission after surgery. Follow-up duration in our study was determined by several factors, including the relatively recent introduction of SG as a standalone bariatric procedure, and the typical enrollment spans of commercially insured health plan members.

Dr. Gagner cites a study by Mingrone *et al.*[2] to suggest that, had we followed our cohorts longer, our conclusions would have been reversed. That is, he predicts that the SG cohort would have had higher long-term T2D remission rates than the RYGB cohort. On this point, we disagree. In the Mingrone trial, RYGB was compared to a highly-malabsorptive procedure, biliopancreatic diversion, therefore its results are uninformative for predicting RYGB outcomes relative to SG. In fact, there remain relatively few studies directly comparing these surgical approaches for diabetes treatment. Those that exist do not suggest superior outcomes for SG. In a recent pooled analysis from 2 randomized trials, partial or complete T2D remission rates did not differ statistically between SG (49% remission) and RYGB (56% remission) after 5 years.[3] Larger observational studies have found greater and more durable T2D improvement among RYGB patients.[4 5] The PCORnet bariatric study, for example, showed lower 5-year risk of T2D relapse after RYGB than SG (hazard ratio 0.75).[5]

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Importantly, Dr. Gagner's assertion that SG may produce superior long-term T2D outcomes depends upon its use within a 2-stage surgical approach, where it is paired with a subsequent malabsorptive procedure such as a duodenal switch (DS). The population of patients receiving such 2-stage treatment are not comparable to the SG cohort in our study, nor are their outcomes likely to be similar to most contemporary SG cohorts. Sleeve gastrectomy as a standalone procedure is now the most common bariatric surgical approach worldwide[6], with 2-stage procedures often reserved for high-risk patients with more severe obesity[7]. It would not be accurate to extrapolate the durability of T2D remission for standalone SG based on outcomes following 2-stage procedures. A DS following SG is likely superior to SG alone for weight loss and T2D outcomes, but comes with the tradeoff of adding a second, highly-malabsorptive operation, with potential additional risks and cost.

Related to this issue, Dr. Gagner raises the important point that procedure choice should take into account overall safety, not just durability of T2D remission. We agree, as stated in the concluding paragraph of our paper. These choices should involve shared decision-making with patients, including a discussion of the risks inherent to the procedures. Even after accounting for differential safety data, some patients may still opt for the higher-risk RYGB, DS or even a 2-stage procedure over the standalone SG, based on the potential for greater comorbidity resolution and more durable weight loss. Studies such as ours, and hopefully future long-term comparisons of standalone SG, RYGB, and 2-stage SG-DS will continue to inform these complex decisions.

Funding Information:

This research was supported through a grant from NIH / NIDDK (R01DK112750; Lewis PI).

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