centers with several different ERCP physicians would be ideal.

The authors reported an incidence rate of 0.67% for PEP in the primary sphincterotomy arm. No preventive measures such as nonsteroidal anti-inflammatory agents or pancreatic stents were used. This rate of PEP is extremely low and is highly likely due to bias with such a very experienced operator.

For the sample size calculation, the authors' estimates of the rates of pancreatitis in both groups were higher than observed in the study. Lower-than-expected event rates in a clinical trial result in a lower power of the study. A planned pilot study can produce right estimates of the events for each arm and lead to a better powered study.[2] On the basis of these results, the authors would have needed >215 patients in either arm to achieve 80% power. In the current format, the study did not attain the planned 80% power.

The study was powered to look for 84% relative reduction in PEP. This is a very high effect size to look for in a clinical trial. Most benefits of interventions are modest in size.[3] Trials investigating modest effect sizes are generally much larger. We need to move toward designing clinical trials evaluating modest effect sizes.

We congratulate the authors for addressing an important clinical question in ERCP. However, future trials avoiding some of the limitations of the current trial are needed.

DISCLOSURE

Both authors disclosed no financial relationships.

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Response:

We are thankful to Roy and Kanth[1] for congratulating us on our article[2] and for raising their concern related to it. Post-ERCP pancreatitis (PEP) is a matter of great concern after ERCP, and precut sphincterotomy has been described as a precipitating factor for PEP in previous studies.[3-8] In previous research studies, precut was attempted after failure of repeated cannulation attempts, so blaming precut for PEP appears to be wrong. Our study compared primary precut with very early precut for the first time in literature and showed that primary precut by an experienced endoscopist results in a low risk of PEP.[7] We agree that the study results are not generalizable, given that all the procedures in our study were done by an experienced endoscopist with an experience of >25,000 ERCPs to date. This was a limitation of our study, and we mentioned it in the discussion. A multicenter study involving different endoscopists is required for generalizability of the data. We also discussed that the lower risk of PEP in the primary precut group was likely because of no manipulation of the ampulla and because all of the ERCPs were done by an experienced endoscopist with a large experience in ERCP with a high precut rate. The calculated sample size in our study was 150 patients in each group, based on earlier studies as described in the original article.[2,9,10] However, in our study, the low risk of PEP was likely due to minimal manipulation of ampulla and very early precut in group A while no manipulation and primary precut in group B, and all the ERCPs were performed by a highly experienced endoscopist. We agree with Roy and Kanth[1] that more studies including multicenter trials are required to further establish our study results and their generalizability.

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Both authors disclosed no financial relationships.

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No efficacy without comparison

To the Editor:

We read with interest the article by Dolan et al[1] in which endoscopic full-thickness resection (EFTR) for colorectal lesions was evaluated. The authors reported that EFTR for colorectal lesions is an effective modality with high technical success and a favorable R0 resection rate. Because their findings are important to current practice, several questions deserve attention.

First, the authors attempted to conduct the most comprehensive and largest meta-analysis involving EFTR for colorectal lesions to date; however, even though many new studies were added to the first meta-analysis that focused on this issue by Li et al,[2] there was still 1 study that was overlooked based on the inclusion criteria.[3]

Second, all the pooled estimates in the article were based on single-arm data. Therefore, the exclusion criterion for "non-comparator study" presented in the flow diagram (Figure 1 of the article) is curious. Because there were no comparators, the efficacy of EFTR was not assessed; the optimal expression should have been "effectiveness." Furthermore, the authors may have misunderstood the meaning of publication bias. Publication bias exists when studies with positive findings are more likely to be published, and they tend to be published faster than studies with negative findings. Because "positive/negative" does not occur in noncomparative and observational studies, it is meaningless to mention publication bias for a single proportion meta-analysis.[4]

Third, all of the included studies were single-arm studies, and no comparison was made; thus, randomized controlled trials are still needed to compare EFTR with other approaches.

DISCLOSURE

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Response:

We thank Wang and Li[1] for their valuable feedback and perspective regarding our recent endoscopic full-thickness resection (EFTR) systematic review and meta-analysis.[2] In that systematic review and meta-analysis, we included a total of 14 studies and 1936 patients specifically evaluating the use of EFTR for colorectal lesions. The letter-writers are correct that this article expanded on the number of included studies and patients compared with the previous work by Li et al.[3] Similarly to this prior meta-analysis, our results found EFTR to be highly effective. Importantly, our work included a meta-regression and demonstrated that EFTR may be less effective for lesions >20 mm, with a decreased rate of R0 resection (OR 0.3; 95% CI, 0.2-0.6) and a higher overall rate of procedure-associated adverse events (OR 3.5; 95% CI, 1.8-7.2).

With regard to study selection and inclusion, this systematic review and meta-analysis was performed in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) and the Meta-Analysis of Observational Studies in Epidemiology (MOOSE) reporting guidelines.[4,5] Studies evaluating non-EFTR techniques (including noncomparator studies) were excluded from this analysis. In reference to the Valli et al[6] study suggested by Wang and Li,[1] that study was specifically excluded because colorectal-specific outcomes could not be extrapolated from the study (ie, the inclusion of upper GI tract lesions as well). Furthermore, patients in this study were included (ie, overlapping or duplication of patient data) in the study