Letter to the Editor

Reply letter to: "A commentary on "minimally invasive percutaneous nephrolithotomy versus retrograde intrarenal surgery in surgical management of upper urinary stones - A systematic review with meta-analysis" (Int J Surg 2019;71:1–11)"

Dear Editor,

We would like to thank the authors for their critical comments on our paper and for posing many interesting questions. In this reply letter, we shall attempt to answer all these questions. We have studied each comment carefully and our responses are as following: .

We re-analyzed the data for each of the included studies and performed subgroup analysis for the duration of follow-up based on the actual follow-up times for all the included studies. The outcome of SFR in one month was reported in two trials comparing MPCNL with RIRS. Our analysis showed a low heterogeneity among the trials, and thus, a fixed-effects model was selected. When pooled, the results suggested that the stone free rates of MPCNL were significantly greater compared to the RIRS group [RR = 1.22, 95% CI (1.05–1.42), p = 0.009]. For the SFR in three months, six trials involving 431 participants were included in our analysis. Our pooled analysis confirmed that MPCNL led to higher stone clearance [RR = 1.08, 95% CI (1.01–1.14), p = 0.02]

(Fig. 1). Thus, with our follow-up time subgroup analysis, we still found that the stone free rates of MPCNL were significantly greater compared to the RIRS group [RR = 1.11, 95% CI (1.05–1.17), p = 0.0005].

In addition, the absence of meta-regression analyses in our study was questioned. However, we do not feel that the meta-regression is necessary. According to The Cochrane Collaboration Handbook (section 9.6.4), the authors suggest that a minimum of 10 studies is required to justify meta-regression [1]; as we only identified 8 eligible studies, we opted not to conduct the meta-regression analysis which we feel is the correct approach. However, as you rightly pointed out and as we have indicated in our conclusions, the findings should be further confirmed through well-designed prospective RCTs with large series of patients.

Second, although some researchers have commented that clinical significance should be more important than statistical significance, most mainstream academics still hold that the *p* value threshold is the most important indicator of efficacy. We also acknowledge the effect was comparatively large (RR = 5) and MPCNL might require more
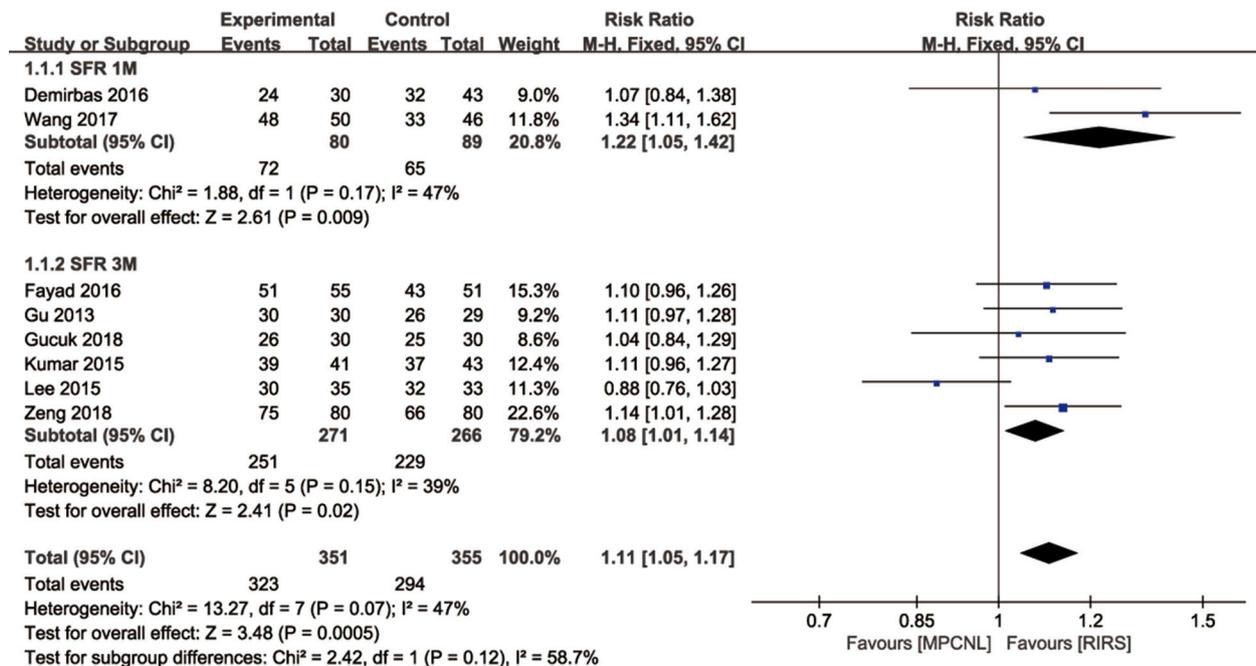

Fig. 1. Forest plot of the SFR for the duration of follow-up. [95% CI: 95% confidence intervals, df: degrees of freedom, Fixed: fixed effects model, IV: inverse variance, SD: standard deviation].

blood transfusion in the clinical settings. However, due to some of the limitations in our study, such as the small sample sizes, we have provided only tentative recommendations based on the limited quality of evidence. We were only able to analyze the results of 5 trials, and a positive sample was only identified in two of the five trials. In addition, the relatively small total sample, due to the small number of pertinent studies, contains many potential confounding factors, which undoubtedly contributed to the relative risk values. Again, we acknowledge the shortcomings in the report and further recommended that well-designed prospective RCTs with pre-calculated sample sizes should be carried out. We feel this step is necessary to confirm these findings and we remain tentative in our discussion and in providing recommendations.

We hope we have addressed the issues raised, and we are happy to discuss any other concerns.

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Writing: Binbin Jiao, Zhenkai Luo.
Statistical analysis: Shicong Lai.

Guarantor

Guan Zhang, Shicong Lai.

Declaration of competing interest

The authors declared that no conflicts of interest exit.

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Binbin Jiao[1]
Peking University China-Japan Friendship School of Clinical Medicine, Yinghuadong Road, Chaoyang District, Beijing, 100029, China
Department of Urology, China-Japan Friendship Hospital, Yinghuadong Road, Chaoyang District, Beijing, 100029, China
Graduate School of Peking Union Medical College and Chinese Academy of Medical Sciences, 9 Dongdan Santiao, Beijing, 100730, China
E-mail address: herodabin@126.com.

Zhenkai Luo[2]
Peking University China-Japan Friendship School of Clinical Medicine, Yinghuadong Road, Chaoyang District, Beijing, 100029, China
Department of Urology, China-Japan Friendship Hospital, Yinghuadong Road, Chaoyang District, Beijing, 100029, China
E-mail address: lzhenkai95@163.com.

Guan Zhang[*]
Peking University China-Japan Friendship School of Clinical Medicine, Yinghuadong Road, Chaoyang District, Beijing, 100029, China
Department of Urology, China-Japan Friendship Hospital, Yinghuadong Road, Chaoyang District, Beijing, 100029, China
Graduate School of Peking Union Medical College and Chinese Academy of Medical Sciences, 9 Dongdan Santiao, Beijing, 100730, China
E-mail address: gzhang2016@sina.com.

Shicong Lai[**]
Graduate School of Peking Union Medical College and Chinese Academy of Medical Sciences, 9 Dongdan Santiao, Beijing, 100730, China
Department of Urology, Beijing Hospital, National Center of Gerontology, Dahua Road, Dongcheng District, Beijing, 100730, China
E-mail address: sclai2016@163.com.

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[*] Corresponding author. Peking University China-Japan Friendship School of Clinical Medicine, Yinghuadong Road, Chaoyang District, Beijing, 100029, China.
[**] Corresponding author.
[1] These authors contributed to the work equally and should be regarded as co-first authors.
[2] These authors contributed to the work equally and should be regarded as co-first authors.