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LETTER TO THE EDITOR

Reply to comment on Zhou et al.: Do we really need closed-suction drainage in total hip arthroplasty? A meta-analysis

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We would like to thank Dr. Zhao and his colleagues for their thoughtful comments regarding our recent publication entitled "Do we really need closed-suction drainage in total hip arthroplasty? A meta-analysis" [1]. To answer their queries, we would like to make the following comments:

1. Your suggestion is very valuable, and we quite agree with you. Rather than undertake another meta-analysis of Chinese literature, we chose not to use Chinese literature in this study. As a result, language bias is a limitation of our meta-analysis. And we calculated the publication bias in the previous work. In our study, the wound infection study only included 18 studies, so we only drew one funnel plot of it (Fig. 1). However, neither Egger's linear regression test ($P_{Egger} = 0.607$) nor Begg's rank correlation test ($P_{Begg} = 1.0$) showed significant publication bias. For this reason, we did not add this outcome to the manuscript.
2. We revisited all of the included studies and compared the two articles which depended on the data pooled from the conference abstract by Hill et al. [2]. The results of comparison could be found in Table 1. There were reasons to believe that the prospective randomised, controlled trial (RCT) published in 2005 [3] and the conference abstract published in 2003 [2] were based on an almost identical study, though they contained a different number of included patients, and rate of wound infection. As a result, we think that removing the study of Hill et al. published in 2003 should be an acceptable proposal. However, removing the study will only influence the overall sample size of patients, not the results and conclusions of the meta-analysis, because none of the data in the meta-analysis was pooled from the study by Hill et al. published in 2003.
3. We carried out this study based on three meta-analyses [4–6] published from 2004 to 2012, and designed our study after studying the methods of the three papers. After consulting with the Cochrane handbook, we find that the DerSimonian and Laird random-effects model is a version of random-effects meta-analysis. For dichotomous data, RevMan implements two versions of the DerSimonian and Laird random-effects model—a Mantel-Haenszel method and an inverse-variance method [7]. This means that the summary odds ratio (OR) estimate with corresponding 95% CIs could also be derived by using the method of Mantel-Haenszel (MH) with the assumptions of a random-effects model.

We would like to thank Dr. Zhao and colleagues again for their constructive comments and reasonable questions concerning our article. The authors certify that there is no financial conflict of interest.

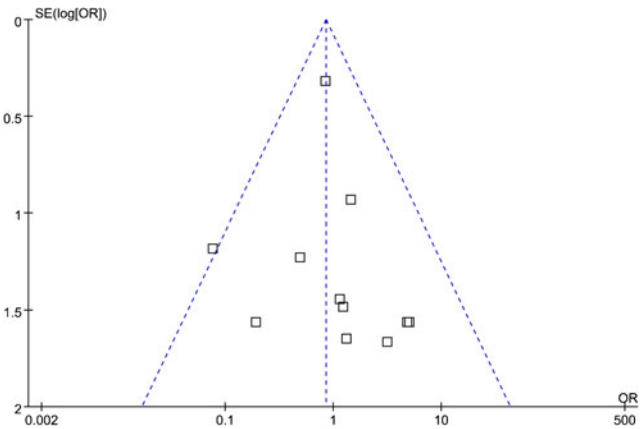

Fig. 1 Funnel plot of included studies shows that there is a low probability of publication bias for wound infection

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Table 1 Results of comparison between two studies

Description		Study	
		Hill, 2003	Walmsley, 2005
The authors		Hill RM, Brenkel I	Walmsley, P. J, Kelly, M. B, Hill, R. M, Brenkel, I.
Organisation		Queen Margaret Hospital, Scotland	Queen Margaret Hospital, Scotland
Year		Sep 1997 to Dec 2000	Sep 1997 to Dec 2000
Unilateral or bilateral		Unilateral[a]	Unilateral or Bilateral
Number of patients		577 patients	552 patients (557 hips)
Follow-up		Discharge and 6 months	Discharge, 6, 18, 36 months
Infection	Superficial	6.4 % Vs 7.1 % *(D vs ND)*	2.9 % Vs 4.8 % *(D vs ND)*
	Deep	0.4 % Vs 0.7 % *(D vs ND)*	0.8 % Vs 0.7 % *(D vs ND)*
Transfusion rate		33.0 % Vs 26.4 % *(D vs ND)*	33.0 % Vs 26.4 % *(D vs ND)*

[a] The title of the study implied that the results were only based on the patients that underwent unilateral THAs, while the methods section reported the patients underwent unilateral or bilateral THAs

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