Response to letter Re: Jain A, Dunlop R, Hems T, Tang JB. Outcomes of surgical repair of a single digital nerve in adults. J Hand Surg Eur Vol. 2019: 1753193419846761

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Dear Sir,

We thank McArthur el al. for their letter. The methodology they critique was informed by the Cochrane Handbook of Systematic Reviews of Interventions and undertaken by authors with formal training in systematic review conduct. The search strategy was designed and conducted with medical librarian support by individuals trained in literature searching. We note that the databases Medline, EMBASE, AMED and Cochrane Database of Systematic Reviews are MeSH indexed, therefore we can only assume that McArthur et al were referring to clinicaltrials.gov. We felt it was clear that clinicaltrials.gov was different but we are very happy to clarify that we did not use MeSH terms when searching this database. We therefore refute the suggestion that the search strategy was flawed.

We do not agree that there were substantial differences between the protocol and review, which affect the overall finding of the review. The PROSPERO record states the main outcome will be 'recovery of nerve function, as evidenced by the 2-point discrimination test'. We defined a positive outcome as Highet Grade 4 as it was most frequently reported and best approximates return of normal sensation. Highet Grades S1 to S3+, where s2PD can be as high as 7mm, potentially represents only minimal benefit. However, we accept such assessment have limitations irrespective of the definition applied.

The distinction between case series and cohort studies is often not clear in the medical literature; this is perhaps particularly so in the surgical literature where a "surgical case series" has additional connotations. Dekker et al. (2012) and more recently by Mathes

and Pieper (2017) describe conceptualisations of the two study designs. Mathes and Pieper (2017) note the challenges in doing so. Applying their definition, Chow and Ng (1993) could be considered a cohort study. When the corresponding tool for assessing the quality of cohort studies (https://www.nhlbi.nih.gov/health-topics/study-quality-assessment-tools) is applied to the study by Chow and Ng (1993), it is still classified as being of poor quality and therefore would not alter the review's overall finding.

McArthur et al. have selected one paper from the 30 included studies of our review and based their opinion on this alone, rather than the whole body of literature as we did. This creates inherent bias in their conclusions and disregards the value of the evidence synthesis process. With regards to the relevance of the hypothetical "Parachute trial" concept, this has often, and in our view in this specific case, been misinterpreted by those who argue against evidence-based medicine. It should not be used to protect from the lack of high-quality evidence supporting common hand surgery interventions for which there is no compelling evidence of benefit. In the case of digital nerve injury, there is only one comparative observational study on 82 patients that is at high risk of bias (Chow and Ng, 1993). No randomised trials were identified. This is not sufficient to support either a repair or non-repair approach to digital nerve injury. Even if this were considered a reliable study, most researchers would accept the need for replication of findings from a single study. Furthermore, we note that having a digital nerve injury and falling from a plane are not remotely similar situations. McArthur et al. state "having re-extracted the primary study data in the review ourselves, we do not believe that the systematic review presents an impartial and balanced representation of the evidence". It is our view that McArthur et al. are not

presenting an impartial view of the evidence but are seeking to defend an aspect of surgical practice they personally are attached to.

McArthur et al. also state that the question of digital nerve repair is neither important nor safe which is in contrast to the BSSH James Lind Alliance Priority Setting Partnership that contradicts this opinion; both management of common trauma and nerve conditions are among the top ten priorities of patients and healthcare professionals. Furthermore, the NIHR Health Technology Assessment programme commissioned a call on this topic prioritising it as an important area in need of further evidence. With regards to the perceived 'safety' of the proposed trial of digital nerve repair, we infer that this pertains to neuroma rates. In our systematic review, neuroma rates as far as can be assessed given the limitations of the studies were not clearly different for repaired and unrepaired group (Dunlop et al., 2019). Furthermore, with regards to safety in hand surgery trials, we should look to other surgical specialties that have pushed the frontiers of evidence-based medicine in areas of much higher risk. For example, vascular surgeons compared open versus endovascular repair of abdominal aortic aneurysms (Lederle et al, 2019) and neurosurgeons evaluated the use of corticosteroids in severe head injury in prospective randomised controlled trials (CRASH, 2004).

At no point do we suggest that we aim to "stop trying to repair the nerve altogether", and we hope that McArthur et al. will reconsider their stated position regarding contributing to a study, which aims to improve the evidence base for this injury. It is not in our view, an acceptable situation to continue a practice for which there is a lack

of reliable evidence to demonstrate its benefit, when there is an associated risk to the patient, even if a small one compared to other areas of medicine. A large comparative study (such as a trial) in this area is likely to demonstrate a spectrum of outcomes, and could identify which patient groups benefit from surgery. Such a study would allow surgeons to better inform patients of their likely outcome.

References

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