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Letter to the Editor

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Please be very careful, when describing variant muscle

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To the Editor,

I read with interest the article of Kowalczyk and Topuzov [5] on the study of a variant palmaris longus muscle (PL) — “Three in one — unusual palmaris longus muscle anatomical variation”. The authors present a biventer PL, with superior (SB) and inferior belly (IB) interrupted by a long tendon. According to authors the IB passed under the flexor retinaculum (FR) and its tendon joined to flexor digitorum superficialis tendon of the fifth finger.

However, I want to apply my modest comments to the article:

1) The authors stated that the SB is connected via tendon to the medial epicondyle of humerus. In spite of this, there is no such a tendon on the Figure 1 and on scheme.

2) Kowalczyk and Topuzov [5] reported that “furthermore short muscle fibers deriving from the belly were attached to antebrachial fascia”. However, I do not see something specific that deserve to be pointed out. This is absolutely normal finding.

3) In the case presentation is reported that the intermediate tendon traversed uninterrupted in the middle and lower third of the forearm, and finished above the FR. But, on the Figure 1 it is clearly seen that the aforementioned tendon did not terminate above the FR, and is localized in the middle part of the forearm and then prolong as IB which is considerably proximal to the FR and not located below it.

4) The authors stated that the IB tendon originate below the superior border of FR. However, this belly is localised considerably proximal to the FR.

5) Kowalczyk and Topuzov [5] described that after incision of the FR, the IB and its terminal tendon is presented. However, the IB is not localized in the carpal tunnel and is situated in the distal forearm, but not in a close connection to the FR. When opened FR only the distal tendon of the reported variation could be observed.

6) The authors stated that they present the clinical significance of the reported biventer PL. However, they discussed the significance of palmaris longus inversus and palmaris longus profundus. This do not coincide with bivented PL. What I mean, clinically, the reported IB could simulate only soft tissue mass in the forearm; the PL inversus or reversed PL and palmaris longus profundus as the authors present in the discussion, could be attributed to carpal tunnel syndrome. However, they have different morphology [1–4] from the reported case [5]. The possible median nerve compression by reversed PL and palmaris profundus is well known and clearly presented [1–4]. The pathophysiology is reduction of the carpal tunnel space by the muscle belly or additional tendon of the aforementioned muscles. In the case reported by Kowalczyk and Topuzov [5], no additional muscle belly or additional tendon in carpal tunnel exist. Why I do think so? On the Figure 2 it is well presented that the reported muscle replaced the flexor digitorum superficialis muscle for the fifth finger and is the main flexor of the proximal interphalangeal joint (PIJ). No other flexor of the PIJ of the fifth finger exist. Therefore, the reported biventer PL variation could not be involved in carpal tunnel syndrome with till now known in literature mechanisms of nerve compression by additional muscles. The biventer PL possible function is flexion of PIJ.

7) The scheme does not correspond to the presented case. On the scheme is presented that the IB start significantly above the FR, then passed through the carpal tunnel and finally the IF inserted with a small tendon.

8) The aim of my letter is not to nagging with authors, but to avoid future misdescriptions and misinterpretations of a well-known anatomical variation as palmaris longus muscle. Such an article as Kowalczyk and Topuzov [5] could provoke numerous errors in future works of PL muscle if other authors accepted the presented description.

Article information and declarations

Conflict of interest

The author declares no conflict of interest.

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