LETTER TO THE EDITOR

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Skin telopodes

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Dear Editor,

Regarding the paper "Infrastructure of the telocytes from tumor stroma in the skin basal and squamous cell carcinomas", which was recently published in your journal [1], I have several observations, which I will not discuss before congratulating the authors for their work, in which they used the transmission electron microscope (TEM). At this time, although there were attempts to establish a specific immune phenotype of telocytes (TCs), this goal was not yet reached and TEM remains the only tool able to indicate whether, or not, a cell is a TC [2].

Telocytes were defined as being "*cells with telopodes*" [3]. Telopodes (Tps) are moniliform prolongations of TCs, alternating thins segments named podomeres with dilations named "*podoms*" [4], and not "*podomes*" as the authors here [1] describe throughout their paper.

The authors state they got from surgery samples of cell carcinomas. Malignant epithelial and stromal lesions are documented in Figures 1–6 of the article. Then, TCs and Tps are documented in Figures 7–14 of the article.

In a previous paper, we indicated that diagnosis of TCs is difficult as hybrid morphologies can occur [5]. We commented there that the telopodial emergence should be carefully observed and diagnosed, as fibroblasts can be misdiagnosed as being TCs [5]. In this regard, to document only telopodial prolongations, without cell bodies, as in Figures 8–14 of the paper we refer here [1], could appear as speculative for a TC diagnosis in TEM. Moreover, the bipolar TC indicated in Figure 7 of the article, has a doubtful telopodial emergence. Nevertheless, the presence of caveolae, although stated as being a main trait for identifying TCs, does not firmly indicate TCs; different other cell types can present these plasmalemmal specializations [6].

Attention is pointed to the TC-mast cells vicinities [1], as was done in previous papers dealing with TCs [7, 8]. In this regard, the authors should have investigated and performed a differential diagnosis between TCs and dermal dendrocytes (DCs) that were first described by Headington [9, 10] and are referred or investigated in more than 150 papers. These dermal DCs are morphologically similar to TCs in TEM [11]. However, different subtypes of DCs are described [12, 13], making an accurate differential diagnosis a difficult task. Since their description, DCs were considered as being "*part of an immunologically competent system that is indigenous to the dermis and is either supplementary or complementary to immunologically functional cells of the epidermis*" [10]. As DCs have specific markers, such as the Factor XIIIa [14–17], an immunohistochemical study should be designed complementary to TEM skin studies. This is mandatory when it is known that DCs are involved in the dermal immune response to skin neoplastic processes [18]. The authors should have also taken into account that dermal DCs have antigen-presenting and healing functions in the basal cell carcinoma (BCC) when the peritumoral stroma demonstrates an increased microvascular bed, an increased number of mast cells and an increased number of DCs expressing CD34 and GP1b-α, a vascular adhesion molecule [19]. CD34-positive TCs were assessed in skin dermis samples [20] but they were not checked for a GP1b-α or Factor XIII phenotype.

I congratulate hereby the authors for their valiant effort, and thank the editors for publishing an interesting topic, which leads to further researches and debates.

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