To the Editor.—I have read with interest the report about the use of proliferation rate in the differential diagnosis of dermatofibroma (DF) versus certain malignant fibrohistiocytic tumors, such as atypical fibroxanthoma, malignant fibrous histiocytoma, and dermatofibrosarcoma protubersans.1

The title of the report states that “high proliferative activity excludes dermatofibroma.” Nevertheless, in the authors’ series, they admit to have intentionally included “only classic examples of the entities under consideration” and to have in particular excluded “atypical variants of DF.” However, 1 of the variants of DF that is more difficult to diagnose is the cellular DF. It is easily misdiagnosed as a dermatofibrosarcoma protubersans if one is not aware of the entity.2 Moreover, the mitotic count in a cellular DF can be up to 10 mitoses per 10 high-power fields.

The authors use the immunohistochemical stain of MIB-1 to categorize their cases of fibrohistiocytic tumor as having a high proliferative index if there was expression of the marker by more than 10% of the cells. However, in areas of pathology in which a correlation between MIB-1 and mitotic rate is typically not seen at a distance from areas of necrosis. Consequently, there exists the potential for high mitotic counts to be reported if counts are performed only in areas of high mitotic activity related to necrosis and the full spectrum of mitotic activity within a lesion is not studied. This is more apt to occur when mitotic activity is reported rather than staining with MIB-1, as regional variation within tumors is far more easily recognized when MIB-1 staining is used. Although an article on CBFH does indicate that mitotic activity relates to cellularity, it fails to indicate whether the cases with high mitotic activity and high cellularity were related to tumors with necrosis. Furthermore, it is not stated how mitotic counts were calculated.1 If the tumors with central necrosis or infarction had high mitotic activity only within the vicinity of areas of central necrosis, and not in areas distant from the necrosis,
then there remains the possibility that the higher levels of mitotic activity reported in this article may have been regionally related to necrosis or infarction and not reflective of a uniform high proliferative rate for this subset of CBFH.\textsuperscript{1} As this information is not presented, we cannot further evaluate this line of reasoning. I raise this issue as I believe that expecting a large series of DFs without necrosis to show mitotic counts of 10 per 10 high-power fields is somewhat suspect and therefore the whole presumption of Dr Fernandez-Flores’ discussion becomes moot.

The author also raises an issue with our study based on our selection of typical storiform dermatofibromas and not CBFHs.\textsuperscript{2} The DFs in our study demonstrated a storiform pattern and were otherwise typical. Whether some of our cases would be considered CBFH or not by independent observers is difficult to predict as acceptable scientific criteria are neither established nor uniformly accepted to allow for separation of storiform DF from CBFH. In their article on CBFH, Calonje et al\textsuperscript{1} state, “Although the cutoff from ordinary dermatofibroma is not sharp, we regard high cellularity and numerous mitoses as the minimum diagnostic criteria for the cellular variant.” The cutoff is not sharp, what denotes high cellularity is not clarified, and what numerical value equates with “numerous mitoses” is not stated. We can only assume that separation of CBFH from storiform DF is completely subjective and consequently not ideal for the basis of a scientific study.

Dr Fernandez-Flores also reaches the conclusion and states the following with regard to our study: “Nevertheless, they do not perform any studies about the proliferation rate on any cases of dermatofibrosarcoma protuberans.” I can only assume that he failed to read large sections of our article with regard to the scientific methods used and that he completely ignored Table 2, where the immunohistochemical results of our study are presented, including a summary of MIB-1 staining in dermatofibrosarcoma protuberans.

In summary, we feel Dr Fernandez-Flores fails to establish sufficient argument to support his stated assumptions to a satisfactory level of certainty or to warrant further comment.

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