Further Considerations on The So-Called Rowell Syndrome

The recent article by Solanki, *et al*. [1] described a further case of Rowell Syndrome (RS), that is a long debated nosological entity, historically defined as a unique clinical association between cutaneous lupus erythematosus (CLE) with erythema multiforme (EM) like lesions and characteristic immunologic pattern. Last year we reviewed all the 71 cases reported as RS up to 2011, and questioned its framing as separate entity [2].

The 13-years old female child described by Solanki, *et al*. [1] apparently fulfilled the diagnostic criteria suggested first by Rowell, *et al*. [3] in 1963 and then classified as major and minor by Zeitouni, *et al*. [4] in 2000. However, as already pointed out in our review, in the majority of cases, different entities were reported as RS, misdiagnosing association between subacute CLE (SCLE) annular polycyclic type, described for the first time in 1977 by Gilliam and better defined by Sontheimer et al. in 1979, and other specific type of CLE as discoid LE (DLE), acute CLE or chilblain lupus variant.

In our opinion, the case reported by Solanki, *et al*. [1] should be considered as SCLE, since it shows annular-polycyclic lesions on the upper chest (different from symmetrical typical raised targetoid lesions of EM) with erosive lesions of the hard palate, frequently reported as non-specific lesions of SCLE, representing a clinical marker of active disease (American College of Rheumatology Criteria). Other features, including photosensitivity and malar rash, strongly support our hypothesis, despite the negativity of anti-Ro antibodies, that are absent in about one third of the patients with SCLE.

In conclusion, we reiterate our critical opinion about RS, stressing the concept that different entities have been wrongly reported under this name. In particular, annular-polycyclic type of SCLE is often misdiagnosed as EM-like rash. Moreover, the real association between LE and EM, as happens for other associations (*i.e.* CLE and lichen planus or psoriasis), should be considered a mere coincidence, that does not justify the framing of a separate syndrome as originally suggested by Rowell, *et al*. [3]

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Effect of Infliximab 'Top-down' Therapy on Weight Gain in Pediatric Crohn's Disease

I would like to make certain comments on recent article by Kim, *et al*. [1] on growth facet of Crohn's disease [1]. Azathioprine was started at the outset of treatment itself that in a group had mild to moderate disease though it is recommended only in those with severe disease or those with frequent relapses [2]. Also, authors have not mentioned the frequency of disease flare-up in follow-up and their management. Since present study takes into account growth parameters as major outcome, inclusion of nutritional intake assessment in all study groups at 0, 2, 12 month time interval and their comparison would have added to results of study. Finally, there is a significant difference noted between increment in weight *Z* scores of steroid and azathioprine group at 2 months however in both groups steroids were used in induction phase and azathioprine effect is generally seen after 3 month of start. For such a difference no plausible explanation is given in text.

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