DISCUSSION

ON THE ARTICLE ABOUT THE COMPONENT COMPOSITION OF TERAPHTHAL PREPARATION

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A recently published article [1] was devoted to revising the composition of Teraphthal preparation (sodium salt of octa-4,5-carboxyphthalocyanine cobalt), pilot batches of which are manufactured at our institute, and also the Manufacturer's Pharmacopoeic Article (MPA) for this preparation, which was in effect until September 2006. Furthermore, the Teraphthal production technology was presented in a considerably distorted form and criticized.

First of all, it should be noted that the contents of the article agree practically completely with the draft of the new MPA and data in it that were developed by one of the authors of the article [1] at the beginning of 2007. These data were reviewed as official documents and were unanimously rejected by specially created review committees in February 2007 and also by Scientific Councils of SSC RIOID and Blokhin ROSC RAMS.

Without intending to repeat the essence of the discussion and the arguments of the parties, we will still note that the main reason for examining the preparation composition was the reduced cobalt content, which disappears with a correct calculation of the content of water of crystallization, complete removal of which requires heating at 140°C under vacuum over P_2O_5 , as shown by recent research, and not at 125°C in air, as stated in the previously existing MPA. The hypothesis that Teraphthal contains an impurity of the corresponding metal-free compound (sodium salt of metal-free octa-4,5-carboxyphthalocyanine) is easily refuted by spectral-luminescence studies.

In addition to the data already reviewed at the Scientific Councils, the authors of the article [1] present new but incorrect proof of the presence in Teraphthal of an impurity of the metal-free compound that was obtained using PMR spectra. Phthalosens (sodium salt of sulfated metal-free phthalocyanine), the PMR spectrum of which was presented in the cited article, cannot act as a model for the sodium salt of metal-free octa-4,5-carboxyphthalocyanine because Phthalosens, in contrast with the latter, exists in aqueous solutions over a broad concentration range as a stable non-fluorescent

dimer. Proof of this is the electronic absorption spectrum of Phthalosens in water, which contains a broad band with a maximum near 610 nm. However, the spectrum in water in the presence of surfactants and in aprotic bipolar solvents (DMF, DMSO) consists primarily of the spectrum of the monomer with a strong doublet at 660 and 690 nm due to two electronic transitions, in agreement with the molecular symmetry. Furthermore, Phthalosens under these conditions fluoresces beautifully.

The sodium salt of metal-free octa-4,5-carboxyphthalocyanine that is prepared by the literature method [2], the presence of which as an impurity was postulated [1], does not aggregate in aqueous solutions and exhibits absorption and fluorescence spectra corresponding to the monomer. Its PMR spectrum shows a singlet at much weaker field at 9.56 ppm than that assigned by the article authors [1], as should be considering the molecular symmetry. Furthermore, Photosens preparation, being the sodium salt of sulfated aluminum phthalocyanine and existing in aqueous solutions mainly as the monomer, in contrast with Phthalosens, also shows a resonance for protons near the macrocycle at 9.55 – 10.15 ppm like, by the way, in that of the diamagnetic form of Teraphthal with Co^{3+} (9.75 ppm) that was presented by the authors. With respect to the weak multiplet at 8 ppm, its appearance is actually explained by the presence in the sample of an impurity of starting pyromellitic acid and its derivatives.

Thus, the "new proof" has the same weight as all previous proof and it remains only to express regret that the editorial board of this reputable journal accepted the article [1] for publication without concurrence from one of the organizations (SSC RIOID) and the proper review that are typical in such situations.

REFERENCES

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