



SDI Review Form 1.6

PART 1:

Journal Name:	British Journal of Applied Science & Technology
Manuscript Number:	2013_BJAST_3803
Title of the Manuscript:	Effect of Sodium Carboxymethyl Cellulose concentration on the Photophysical Properties of Zinc Sulfide Nanoparticles

General guideline for Peer Review process is available in this link:

(<http://www.sciencedomain.org/page.php?id=sdi-general-editorial-policy#Peer-Review-Guideline>)

- This form has total 7 parts. Kindly note that you should use all the parts of this review form.



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PART 2: Review Comments

	Reviewer's comment	Author's comment (if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)
Compulsory REVISION comments	BJAST manuscript # 2013_BJAST_3803 by Ahemen, et al. This paper presents some results from surface passivation of zinc sulfide nanoparticles using carboxymethyl cellulose (CMC). The results are incomplete and contradictory. The reviewer recommends rejection of the manuscript. Specific comments are as follows. The authors use x-ray diffraction extensively to characterize the nanoparticles covered with CMC layers of different thickness. The thickness of the CMC layers were not determined; the samples are only labeled according to the concentration of the starting precursors. The reviewer does not understand how XRD patterns can be obtained from a surface layer of zinc oxide (or zinc hydroxide or zinc sulfate) with a thickness of approximately 1-3 nm, which is less than the coherence distance for x-ray diffraction. Rather than interpreting the additional peaks in figure 1, spectrum a as being due to growth of zinc oxide surface shell on a zinc sulfide core, the reviewer believes that it is more likely that particulate zinc oxide exist with dimensions greater than the coherence length or XRD. Thus the claim	1. We disagree with the reviewer that the results are incomplete and are contradictory. 2. Coherent diffraction occurs when the wavelength of X-ray is less than the path length, contrary to the reviewers' position. The reviewer is referred to other articles involving ZnS capped with ZnO. We do not agree that particulate ZnO were present, but that ZnS nanoparticles located near the surface were more readily hydrolyzed forming thin capping layer of CMC on the surface of ZnS in the same way core-shell of ZnS-ZnO are formed. Though we could not measure the thickness of these layers, we assumed proportionality between the added amounts of CMC with thickness layer. This argument looks more plausible when you consider the XRD and SEM results. 3. Looking back at Table 1, it is clear that



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	of growth of passivating layers is suspect. The authors use the Scherrer analysis to determine the crystal size from XRD, and compare those values with the particle size from SEM, and the crystal size predicted by the effective mass approximation (EMA). The crystal size from XRD are reasonably consistent with those from EMA. Neither show a systematic increase in crystal size going from 1_1 to 1_3 samples. The crystal size from XRD/EMA are both smaller than particle size from the SEM analysis. It is clear that the SEM is looking at agglomerated material. The lack of increase in crystal size is not consistent with the red shift the authors show in their PL data in figure 6. Neither does the absorption data in figure 4 show a shift due to particle size. The reviewer therefore does not believe the explanation offered for the red shift in PL data in figure 6. The authors should specify the values of electron effective mass and hole effective mass that they use to calculate the particle size from EMA. The photoluminescent emission and excitation data are very confusing. First the copy supplied does not provide a clear image of the value of the wavelength on the abscissa. Second the PLE data shown in column IV looks more like noise rather than real excitation peaks. If the excitation peaks at 285 and 330 nm are real, why are they resonant in this system as indicated by their very narrow full width half maximum. Why does the emission wavelength peak shift	the crystal sizes changed with the amount of CMC added. The changes may not be monotonic, but it clearly agrees with the results of PL measurements. 4. Please take a look again at Table 1. The absorption data have blue shifts of 0.6, 0.67 and 0.53 eV for sample S₃, S₂, and S₁, respectively. These changes are consistent with the result from XRD and do have significant effect on the PL intensity and peak shift. 5. The values of the effective masses have been included. The authors could not recall the earlier masses used, but the present result follows the pattern of the former. For the XRD results in Table 1, notice a reduction in size. This is because in earlier submission we did assume spherical nanocrystallite in which case we multiplied each result by the factor $4L/3$ (where L is the coherence length). We have removed
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	from 442 nm for 1_1 to 520 nm for 1_2 and 1_3 samples? The authors attribute the shift to changes in the shape and dimensions of the nanocrystalline, yet the XRD and EMA data in table 1 shows the crystal size remains constant. Because the data are limited, incomplete, and conflicting, the reviewer recommends rejection of the manuscript.	this factor in the present case after reviewing the SEM results. 6. The PL and PLE results were obtained from a reliable laboratory in France (look at the methodology). Noise was removed from the spectra. Please refer to Table 1 concerning particle size changes. Note that for nanoparticles, any slight change in the structure and energy could have pronounced effect on its opto-electronic properties. 7. The present data and analysis carried out are to the author's opinion sufficient to make good collusion from the work.
<u>Minor</u> REVISION comments		
<u>Optional/General</u> comments		