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SDI Review Form 1.6

Journal Name:	**Physical Review & Research International**
Manuscript Number:	**2013_PRRI_6986**
Title of the Manuscript:	**Measurements of absolute atomic oxygen density by two-photon absorption laser-induced fluorescence spectroscopy in hot air plasma generated by microwave resonant cavity**
Type of the Article	**Research Paper**

General guideline for Peer Review process:

This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty'**, provided the manuscript is scientifically robust and technically sound.
To know the complete guideline for Peer Review process, reviewers are requested to visit this link:

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PART 1: 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	**1. Line 35: Should include recent work that involves direct laser absorption spectrometry to quantify ground-state oxygen atom densities:** **a. http://dx.doi.org/10.1063/1.2408655** **2. Please refine the manuscript's English. There are several awkward sentence and phrase constructions that make the manuscript more difficult to read. For example, use "gas conditioning cell" instead of "cell of gas conditioning". This applies throughout the manuscript.** **3. Please describe the pumping system and gas flow rates.** **4. Lines 108 – 109, please provide the spectral resolution of the spectrophotometers. This is especially relevant in determining the linewide of the laser, which may be heavily convoluted with the spectrophotometer resolution.** **5. Lines 113 – 122, please provide laser power for each wavelength regime.** **6. Lines 197 – 198. The authors show the fine structure of the TALIF spectrum; however, there is no discussion about this fine structure. Please add a paragraph describing this structure and, ideally, showing that it fits model results (or matches expectations).** **7. Figure 7: The point of this figure is to show that the FWHM is independent of pressure. Thus, please list the FWHM of the 2 profiles. Also discuss the shift of the line position.**	

	8. Calculation of the ground-state oxygen density. This is the most significant issues with this paper. **a. The authors assume that the linear relationship between laser pulse energy and fluorescence signal (Figure 8) suggests that there is negligible collisional depletion and photo-ionization. While the latter is correct, the former is not. The linear relationship suggests that collisional depletion due to 2 excited state oxygen atoms colliding is negligible. However, it does NOT indicate that a ground-state oxygen atom (or other gas constituent) cannot collisionally-deactivate an excited state oxygen atom. Thus, collisional deactivation may lead to a very significant decrease is fluorescence and estimated ground-state oxygen atom population.** **b. Alternatively, the xenon calibration accounts for this deactivation issue. But, the xenon work is done at much higher pressures (1 – 10 torr versus < 1 torr), and it the xenon-xenon collisional deactivation is not comparable to the oxygen atom deactivation.** **c. Also, it seems that the collisional deactivation rate would be heavily dependent on the gas temperature, yet this is not discussed or taken into account.** **d. Thus, this reviewer does NOT understand how TALIF provides an accurate measurement of ground-state oxygen density. It is clear that TALIF provides a good relative measurement of ground-state oxygen density at a given pressure.** **9. Lines 263 – 264. Are the filter transmission percentages measured or taken from data sheets?** **10. The plasma temperature was determined by adding H2 to the gas flow and measuring the fluorescence spectra of the OH radical. There are**	

	several issues with this technique that need to be addressed: **a. Adding a small amount of hydrogen (how much was added?) changes the plasma characteristics, including the temperature. How is this taken into account or shown to be negligible?** **b. As noted in the text, the OH temperature measurement assumes ro-translational equilibrium. Is there any evidence to suggest that, at the low pressured in this plasma (< 1 torr), that ro-translational equilibrium exists?** **11. Table 1: Please add uncertainties to the listed values, using a proper convolution of measurement and listed uncertainties.**	
Minor REVISION comments	The English should be revised to make the manuscript more readable.	
Optional/General comments		

Note: Anonymous Reviewer