Dear Dr. Rutkowska

We would like to respectfully resubmit our manuscript entitled “Concentration Dependent Studies on the Laser-Induced Mid-Infrared Emission from KCl-NaCl Tablets”, which was submitted for publication in Photonics Letters of Poland.

We are grateful for the comments by the reviewer and have made changes accordingly. Below please find our responses to the reviewer comments:

Reviewer Comment #1:

I would guess that at 32 mJ per pulse, there was a certain amount

of ablation that occurred due to repeated shots on the same tablet.

That would add to the noise, making the signal to noise worse. Did

the authors observe ablation? Was there one shot per site or were

there many? What were the focal lengths of the lenses used, what was

the material? Others may want to work in this region

and that would be helpful. Fig. 2 is not very distinct on the .doc

version that I was sent.

Our Response:

Additional information was added to the experimental section. We also tried to make Figure 2 clearer. More details on the experiment can also be found in references 4 and 5.

Yes, we observed a certain amount of ablation on the sample. Therefore, the sample was translated continuously to hit a fresh spot for every shot. Details of the experimental setup were previously reported in references [4] and [5].

Reviewer Comment #2:

2. They should identify the potassium 2.71 and 2.73 micron

transitions as belonging to 5P 3/2,1/2 to 5S 1/2, as was done in Ref.5.

Our Response:

We made the change in the text.

Reviewer Comment #3:

A typical formula for estimating the detection limit is : LOD=

3\*RMS noise\* slope of the calibration curve. If I put their numbers in

Table I into an excel spread sheet going through 0,0, using the ?Peak

intensity? column, I get a slope of ~0.4 a.u./wt% with an R2 of 0.97.

It looks to me that the RMS noise might be 0.1 a.u. at worst. That

would lead to an LOD of 0.12 wt%, not 0.26 wt%. So their LOD may be better than they think.

In any case they should identify the method by which they obtained 0.26%, because nothing is said about the method in the paper. They should also note in the conclusion that even 0.12 is not as good as the LOD?s obtained for some near IR lines cited in Ref. 3. Nevertheless it is not a bad number, it is what it is.

Our Response:

We made the change in the text. The IUPAC definition for LOD (see reference 8) was employed and yielded a value of 0.19wt%/wt.

Reviewer Comment #4:

They should clarify what the ?standard deviation of 0.58? means.

In what way is it the error in the potassium gluconate measurement?

Our Response:

We made the change in the text and indicated the error in the potassium measurement for potassium gluconate.

Reviewer Comment #5:

Ref. 3, 4, and 5 do not have page numbers, please add them.

Our Response:

We made the change in the reference list.

Reviewer Comment #6

Their comments in the conclusion about matrix effects are well

taken, and good observations

Our Response:

We thank the reviewer for his comment.