**Response to Reviewers**

Dear Sirs,

Thank you very much for your attention to our manuscript as well as for your valuable comments and recommendations. The answers to your comments are presented below.

Reviewer A

*The work of Vedyagin et al. describe synthesis of MgO-based catalysts for destructive adsorption of CF2Cl2. The work is interesting but there is a lot of repetition from some previous reports and the work seems not to be much focused. I advise against publication in the present form. Some of the issues that should be resolved are appended below.*

**Q1.** *At some points the manuscript contains too much details from the literature. For example, section “Modification of Mg(OH)2 and MgO aerogels” mainly reviews some earlier literature reports and then quickly says: “This procedure yields nanocrystalline AP-MgO samples with the particle sizes of 1-5 nm and surface areas of 400-700 m2/g. Figure 1 presents TEM images of the AP-MgO sample”. On the other hand, the caption of Fig. 1 states “Fig. 1. TEM images of AP-MgO aerogel samples.” (are the images for two different samples?). Moreover, TEM is not mentioned in the experimental section.*

**A1.** In this paper we have overviewed our previously published results and summarized it with new findings (corresponding changes were made in introduction section). TEM images with different magnification were provided for the same sample (caption for Fig. 1 was corrected). Description of TEM and EDX methods was added in the experimental section.

**Q2.** *Please explain the meaning of the sentence “Although the induction period phenomenon was always reproduced, its duration in our experiments was reproduced with ca. 10% accuracy.”*

**A2.** The sentence was replaced by “Note that the IP phenomenon was reproducible with ca. 10% accuracy.” It means that, for example, for induction period of 20 min the reproducibility of its duration was ± 2 min.

**Q3.** *Page 7. “The induction period phenomenon must be related to gradual formation of some active sites or defects on the MgO surface. Intense bulk substitution of oxygen … ” (lines 185-197) almost half of the page is actually repeating of the findings from ref. 3, no new results are provided.*

**A3.** We agree, this part is repeating the findings from ref. 3, and we consciously used it to describe the essence of the phenomenon.

**Q4.** *Fig. 2 – obviously, not only the induction period but also the extent of the reaction. Please address. It would be interesting to provide some XRD data of the samples obtained after the experiments presented in Fig. 2 (with appropriate analysis).*

**A4.** We agree. It was changed to “According to the data shown in Figures 2 and 3, both the duration of the induction period and degree of MgO conversion were found to be a function on the reaction temperature.” Additional XRD data were provided.

**Q5.** *In lines 190-191 the authors state “Probably, chlorine exists in the form of adsorbed species and defective oxyhalide structures with variable amounts of halogen atoms partially substituting oxygen atoms of MgO.” but MgCl2 participates in the equations 1-3.*

**A5.** The sentence was changed to “It was proposed there that chlorine exists in the form of adsorbed species and defective oxyhalide structures with variable amounts of halogen atoms partially substituting oxygen atoms of MgO.” Initially, we had no evidence to propose the chlorination of MgO. Later, we have found some indirect confirmation of it by analyzing the gas phase products with mass-spectrometer.

**Q6.** *Section “Synthesis of V-Mg(OH)x and VOx/MgO aerogels” I suggest authors to tabulate prepared samples, experimental conditions and the resulting SSAs. In the present version the text is rather difficult to read.*

**A6.** SSA values were summarized in an additional table.

**Q7.** *Lines “The acceleration is apparently due to the fact that vanadium acts as an intermediate in the solid-state exchange of the oxygen atoms for halogens.” Is this new results/assumption, or taken from the literature. If latter is applicable then the reference is missing.*

**A7.** Yes, this is new assumption, which is in good agreement with literature data. The following sentence and corresponding references were added “This assumption is in good agreement with mechanism proposed in the literature.47,48”

**Q8.** *Table I. Define τind.*

**A8.** τind was defined.

**Q9.** *In Experimental, the authors stated that the VOx\*MO samples with V loading of 1, 10, 15 and 25 wt.% (normalized to V2O5) were prepared. In table I only the data for 1 and 10 wt.% are provided and for the reaction temperature of 350oC. For the samples with highe V loading the authors only say “The induction period was 2 min or shorter”. Please provide more data. Also, are there experiments at different temperatures (I guess yes because Fig. 3 reports the results for 400oC).*

**A9.** We agree. In this paper we are mostly talking about the V loading range of 1-10 wt.%. So, the sentence was replaced by “Samples with vanadium loading in a range of 1-10 wt.%...” The studied temperature range was 300-450 °C. Unfortunately, we cannot provide all data in this article, so, we have chosen the most illustrative figure to show the effects.

**Q10.** *As it is obvious that addition of V reduces the induction period, is it possible that it can also reduce the reaction temperature (at the expense of induction period)? Are there any information about that?*

**A10.** Shortening of induction period is connected with the change of reaction mechanism. Activation energy for chlorination of VOx is significantly lower comparing with MgO. Moreover, VOCl2 can exchange chlorine with oxygen of MgO. It means that similar induction period can be observed in the case of VOx-MgO sample at reduced temperature. No such comparison was done.

**Q11.** *Lines 304-308 are more appropriate for the introduction. Also, lines 309-316.*

**A11.** We agree that these lines look like introduction. From the other side, it relates to the subsection concerning electron-donor and electron-acceptor sites. We do not think that it should be replaced to introduction section.

**Q12.** *The sentence “Our quantum chemical simulations that will be reported later suggest that only positively charged surface species can have the required electron affinity about 7 eV” is not useful at all. It seems like the authors have some new data that might be used to explain experiments but they keep them for another publication. So, why mentioning something that does not contribute the paper?*

**A12.** We agree. The sentences were rewritten as “Note that the electron-acceptor properties of conventional surface acid sites are not even close to the values required for ionization of the used aromatic probes. We think that only positively charged surface species can have the required electron affinity about 7 eV.”

**Q13.** *Schemes provided in Fig. 6 – as there are some uncharged halogen atoms and these are not likely to adsorb at Mg2+ sites of MgO surface. Both F and Cl like mixed O-Mg sites with sertain preference to O2- centers, see. Surface Science 632 (2015) 39-49.*

**A13.** Fig. 6 shows simplified scheme for electron-acceptor sites, where all halogen atoms were considered as charged. The scheme is just visualization of our assumptions with no confirmation by quantum chemical calculations.

Reviewer C

*The authors have published a number of papers concerning the sol-gel synthesis of nanocrystalline MgO aerogels and its modified form with the transition metals, as well as the reactivity of these systems in the reaction with halocarbons (chlorofluorocarbons-freons). Therefore, this work, in my opinion, has the character of the review work. Confirmation of this assertion lies in a large number of references listed in this article, from which a considerable number belongs to the authors of this paper. However, the paper also presents the new results that relate to this matter, such as the new method for characterization of electron-acceptor sites by electron spin resonance spectroscopy using perylene as the spin probe molecule, correlation between the rate of the difluorodichloromethane (CF2Cl2) destructive sorption and the concentration of weak electron acceptor sites, as well as simplified models for these centers. The authors concluded that the electron-acceptor sites on the modified MgO surface are acid sites originating from separation of the charged fragments resulting in the surface polarization. The authors also point out that further experimental and theoretical studies are necessary to understand the properties of electron-acceptor sites on the surface of halogenated MgO and the mechanism of their involvement in the reaction with CF2Cl2. My main objection is related to the methods used for the characterization and their comment in the results and discussion. For example, in the experimental section it is mentioned that XRD method was used to characterize the present phases, but later in the commentary of these results there are no corresponding diffraction patterns. However, despite the aforementioned shortcoming, the manuscript is written correctly and contains enough significant material to warrant publication in Journal of the Serbian Chemical Society. Therefore, I recommend publication with minor revision.*

**Q1.** *1. Page 1, in INTRODUCTION: (line 32) it would be desirable to replace particular with particularly*

**A1.** We agree. It was replaced.

**Q2.** *Page 1, in INTRODUCTION: (line 32) put comma after the word materials*

**A2.** Comma was added.

**Q3.** *Page 7, in RESULTS AND DISCUSSION: (line 192) put comma after the word studies – studies*

**A3.** Comma was added.

**Q4.** *Page 7, in RESULTS AND DISCUSSION: (line 211) replace increased with increase*

**A4.** It was replaced.

**Q5.** *Page 8, in RESULTS AND DISCUSSION: (line 223) put comma after the word sites – sites*

**A5.** Comma was added.

**Q6.** *Page 8, in RESULTS AND DISCUSSION: (line 224) put comma after the word sites – sites*

**A6.** Comma was added.

On behalf of the authors,

Dr. Aleksey A. Vedyagin