

Interactive comment on “Biogenic isoprene and implications for oxidant levels in Beijing during the 2008 Olympic Games” by C.-C. Chang et al.

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Interactive comment on “Biogenic isoprene and implications for oxidant levels in Beijing during the 2008 Olympic Games” et al. Anonymous Referee #1

I recommend this paper to be rejected for two reasons: 1. Severe self-plagiarism. I just get a chance to read the Wang et al. [2013] paper. This paper appears to largely copy the text from the Wang et al. paper, without any modification. I don't think this can be allowed on ACP, or any other journals. I will just give a few examples here. I am sure there are other examples but I would rather not to go any further. I am also surprised that this level of plagiarism was not detected by the iThenticate.

Example 1: Page 25945 Line 18: “2.2 Assessment of biogenic and anthropogenic con-
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tributions to isoprene In urban areas, isoprene has both biogenic and anthropogenic sources (Borbon et al., 2001; Wang et al., 2013). Several studies have quantified isoprene in vehicle exhaust (Duffy et al., 1999; Borbon et al., 2001), and measurements in temperate urban winter periods have revealed a strong correlation between isoprene and common vehicle exhaust tracers, such as 1,3-butadiene, alkenes and carbon monoxide (Reimann et al., 2000; Borbon et al., 2001; McLaren et al., 1996). The results indicate that anthropogenic sources of isoprene in the investigated cities were mainly a result of vehicle exhaust. Considering that anthropogenic isoprene has a strong correlation with vehicle.” This is the exact same as Section 2.4 in Wang et al. paper.

Example 2: Page 25946 Line 21 “The MIR method can be used to estimate the potentials of individual VOCs for O₃ formation. Although the initial reactivity of a VOC with OH in the kOH reactivity method does not directly reflect OFP, it is involved in the production rate of organic peroxy radicals and implies the potential for formation of subsequent products. In the study, both the MIR method and the kOH reactivity method were utilized. The OFPs and OH reactivities of individual VOCs were calculated by multiplying individual VOC concentrations measured at PKU by their corresponding MIR and kOH (reaction rate constants of VOCs with OH radical), as shown in Table 1.” Please see the section 2.3 in Wang et al.

Example 3: Page 25952 Line 4: “Although the emission strength and reactivity of a precursor are important for the secondary pollutant formation potential, the timing of release is also critical from the perspective of photochemistry. The midday surge of biogenic isoprene could produce a much larger loss and more efficient production of midday ozone and/or secondary pollutants because the midday peak of isoprene is almost concurrent with the peak of OH, a condition that could maximize photochemical reactions (Lee and Wang, 2006).” This is exactly the same as a paragraph in Section 3.3.2 in Wang et al. 2. Scientific content. The authors simply replace the observation dataset in Wang et al. with their measurements in Beijing. However, I don't see any

insightful analysis, novel concept or ideas. The whole paper is rather weak. The only difference between this paper and Wang et al., is the discussion on the possible causes that lead to high ozone in Beijing. But I don't see any in-depth discussion on that. Overall I don't think this paper is suitable for publication in ACP.

Reference Wang, J.-L., C. Chew, C.-Y. Chang, W.-C. Liao, S.-C. C. Lung, W.-N. Chen, P.-J. Lee, P.-H. Lin, and C.-C. Chang (2013), Biogenic isoprene in subtropical urban settings and implications for air quality, *Atmos. Environ.*, 79(0), 369-379, doi:<http://dx.doi.org/10.1016/j.atmosenv.2013.06.055>.

Reply:

We are very regretful to receive comments that are unfounded, hostile and unprofessional. Never before have we encountered a review outcome like this one, which in our opinion is a disgrace to the science community by any standard. We are deeply saddened as a result. We are willing to accept comments as critical as they can be from any scientist so long as the comments are solely based on the soundness and merits of a paper, but clearly these comments are not.

In this manuscript, we used two methods commonly utilized by many atmospheric scientists to calculate ozone forming potential (method 1) and to separate the biogenic fraction from the vehicular one (method 2). Since we are not the original authors for the two methods, we gave citations when using them to discuss isoprene. We indeed used these two approaches in two separate studies of ours including the current one, as the referee accused us of doing, but that is what any decent author would and should do: to cite the original literature from which the concept, approach, method, results, etc. is being adopted in one's work. Does that make it "self-plagiarism"? We strongly and solemnly object the referee's accusation.

Method 1: Reactivity scales of VOCs (MIR and KOH reactivity method, Carter, 1994; Chameides et al., 1992; Dimitriadis, 1996; Atkinson and Arey, 2003)

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Method 2: Simple regression analyses for the concentrations of isoprene and exhaust tracers to estimate the contributions of anthropogenic and biogenic sources to ambient isoprene levels (Borbon et al., 2001; Reimann et al., 2000; Wang et al., 2013).

The referee also wrote: "I will just give "a few examples here. I am sure there are other examples but I would rather not to go any further..". It is a shame that the referee did not give those examples, while we hope he/she would in order to support the accusation. The referee attempted to misguide viewers that we are at fault repeatedly, but in fact the referee was being prejudice and hostile for reasons that only he/she would know. This is a serious manipulation of the reviewing system, and the anonymous referee's conduct is totally irresponsible and unacceptable. No doubt that this referee failed his/her duty as a righteous and objective role to critic a scientific article.

We do admit that when we describe the two methods mentioned above, we used similar wording as in our previous work on isoprene. As a non-English native speaker, we think that is a mindless negligence. We agree that we were not being careful with our wording when referring to these two methods in the process of discussing our viewpoints, but we cannot agree with the referee's comment of being self-plagiarizing, and the referee seemed to misunderstand the meaning of it.

The main theme of this current paper is to discuss the role of biogenic isoprene in Beijing during the Olympic period while the anthropogenic contribution was tightly controlled. To separate the biogenic portion from the anthropogenic one, we resorted to the two methods mentioned above. Very interesting findings, discussion and conclusions were produced as a result which are fundamentally different from the ones published before. These two studies of ours involved the same analytical techniques (GC/MS) and two approaches mentioned above with proper citations as the basic tools to decipher ambient isoprene for entirely different purposes. Can the referee say that we are guilty of being self-plagiarizing when we used the same GC/MS technique in both works to analyze air samples? If the answer is negative, then how can it not be applied to the notion of using the two approaches for interpreting the data?

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Again, the mistake we made was an unintentional negligence, since we used similar wording when describing the tools we used to analyze our data. Normally, when reminded out of the good conscious of a referee, we would certainly make corrections with full appreciation. However, the referee chose a different approach by tarnishing not only the work itself, but the reputation of the authors all together. The essence of science that should have been subject to fair critic in the first place was completely lost. We feel deeply saddened because of that.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25939, 2013.

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