Jeff Severinghaus,

Ishidoya et al. have produced a stunning and groundbreaking extension of the well-known millennial-scale variations in atmospheric oxygen isotopes (namely ^{18}O of O_2), sometimes known as the Morita-Dole Effect, that are recorded in ice cores. Their extension brings to the table totally new and fascinating information - namely the first high-quality observations of diurnal and seasonal cycles in ^{18}O of O_2. Their tour-de-force treatment of extremely difficult analytical techniques makes it possible now to ask totally new questions about the role of the terrestrial biosphere in the last 5 decades of (unplanned) anthropogenic CO_2 fertilization due to fossil fuel burning, as just one example among many.

The quality of their measurements is superb, and unparalleled. Their deep attention to details, and exploration of potential pitfalls, makes their conclusions robust and convincing.

One very minor comment I would make is that their box model estimate of ~1500 years for the turnover time of atmospheric O_2 may be a little too long. My ice core work shows that ^{18}O of atmospheric O_2 relaxes with a characteristic asymptotic decay curve after abrupt climate change events on a timescale of about ~1000 years, implying that the turnover time of O_2 in the atmosphere is about ~1000 years.

The authors are to be congratulated for a true breakthrough that will no doubt open many doors for future study of the interlinked carbon, oxygen, and argon cycles in Earth's atmosphere. These studies will no doubt shed light on the ongoing anthropogenic perturbations to the cycles of these gases.

Thank you very much for your significant and useful comments on the paper "Diurnal, seasonal, and interannual variations in $\delta(^{18}O)$ of atmospheric O_2 and its application to evaluate natural/anthropogenic changes in oxygen, carbon, and water cycles" by Ishidoya et al. We are so happy to hear that you are pleased with the work that was done. We have revised the manuscript, considering your comments and suggestions, and those from other two reviewers. We have added following sentences, considering your comments regarding the turnover time.

"(lines 214-221) The biospheric turnover time of O_2 in the steady state was 1398 years, which is longer than the 1200 years estimated by B94. This may be a little too long, since the $\delta_{atm}(^{18}O)$ variations reported by Severinghaus et al. (2009) based on the ice core measurements that showed a characteristic asymptotic decay curve after abrupt climate change events on a timescale of about ~1000 years, implying that the turnover time of O_2 in the atmosphere is about 1000 years. The biospheric turnover time is inversely proportional to the sum of the terrestrial and oceanic productions of O_2 incorporated into the box model, which is 26.5 (16.7 + 9.8) Pmol a^{-1} in this study (Table 1). This implies that total production of O_2 for the initial value in our model is underestimated. In this regard, turnover time decreases to about 1000 years when we simulate a case in which the GPP is increased, as will be discussed later.”