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Interactive comment on "NO_2 and HCHO photolysis frequencies from irradiance measurements in Thessaloniki, Greece" *by* C. Topaloglou et al.

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General Comments

This short paper describes the application of an empirical method, which had been introduced previously by the authors, to derive photolysis frequencies from spectral irradiance data. The determination of accurate photolysis rates from such data is a worthwhile aim because the historical and geographic coverage of irradiance data far exceeds that for actinic fluxes or direct measurements of photolysis rates.

The two photolysis rates investigated are J(NO2), which is independent of absorptions

by ozone; and J(HCHO), which, like the more-studied rate J(O3), is dependent on ozone absorptions. Because J(HCHO) is less studied, the paper would been improved if there had been a better balance between the treatment of J(NO2) and J(HCHO). Currently, the paper is too heavily weighted to discussions on J(NO2). For example, Figures 1 and 2 would have been improved if the authors had included information on J(HCHO) - for both branching paths -as well as for J(NO2).

Although the method is empirical, the authors demonstrate that, at least for the case of J(NO2), the conversion algorithm can be used without change at two different sites. However, the value of this comparison would be greatly improved if more information about differences in aerosol optical parameters between the two sites had been presented. For example, if the aerosol optical properties were very similar, then much less significance would be attached to that result. It would be very interesting to see whether the same relationship applies in pristine conditions, or under heavy pollution such as seen in mega-cities.

The paper would also have been improved if the transportability had been demonstrated for J(HCHO) as well as for J(NO2). The authors should make it clear why this was not done.

Was there any other reason why J(HCHO) was discussed instead of J(O3)? Perhaps all three rates could have been inlcuded.

The paper is clearly written. The methodology used is sound, and the results are plausible.

Minor points.

The paper would be improved if the same notation had been used for both species. For example, the term “pseudo” J-value is used only for J(CHCO).

It would be helpful if data from the two Brewer instruments were distinguishable in Figure 6.

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1619, 2005.