We begin our response to the Comment by Albert et al., pointing out that a detailed investigation of the nature of the improved efficiency of the two-crystal arrangement of XPW generation in comparison with a single-crystal arrangement was never a subject of the original Letter (only \(-2\%\) of the Letter’s text is relevant to the content of the original paper [Opt. Lett. 31, 1456 (2006)], misinterprets our results and leads to a wrong conclusion: that the rotation of the second crystal in the double-crystal scheme is unimportant. On the contrary, it follows from the Comment itself as well as from our experiment that the crystal rotation is the main factor in improving the efficiency.

The authors of the Comment are also not consistent on the issue of whether the crystal rotation improves the efficiency. For example, at the beginning they state that “polarization rotation does not contribute to the XPW generation,” and later that “rotation is not the main decisive factor that influences the double-crystal efficiency” and that adjustment of the optimal distance is of prime importance. As a matter of fact, it follows even from their explanation (Fig. 2 of the Comment) that the rotation provides a 10\(^{11}\) contrast improvement of 3 orders of magnitude using microjoule input energy. Microjoule energy can be achieved by direct amplification without the complications of a double CPA system. The 10\(^{11}\) contrast is sufficient for experiments on high-damage-threshold solid targets with focused intensities up to 10\(^{22}\) W/cm\(^2\).

In conclusion, we have demonstrated that pulse cleaning based on XPW generation in two BaF\(_2\) crystals yields a 10\(^{11}\) contrast ratio for a 50 TW laser system. Dispersion compensation and direct amplification of the oscillator output are necessary for efficient XPW generation using only microjoule energy. Two BaF\(_2\) crystals placed in series increase the XPW conversion efficiency by a factor of 3 compared with a single crystal, yielding 3 orders of magnitude of contrast improvement.

Of these results, the claimed improvement of efficiency of the two-crystal configuration compared with a single crystal is the only one that has anything to do with the content of the Comment. We suggested in the Letter that a possible explanation for experimentally observed improvement of efficiency when we rotate the second crystal relative to the first one could be a compensation of experimentally observed rotation of polarization ellipse in the first crystal. The angle of ellipse rotation in the conditions of our experiment was experimentally measured to be about 10\(^\circ\). This value is higher than 4\(^\circ\), which the authors of the Comment claim the maximum value should be. (They are not consistent on this claim: in Fig. 2 of the Comment they rotate the second crystal by 7.5\(^\circ\) to maximize the efficiency!) Rotation by 10\(^\circ\) is quite enough to affect the efficiency, according to the efficiency curve published previously by the authors of the Comment themselves.\(^3\) By rotating the second crystal (by the same angle as the rotation of the ellipse in the first one), the orientation of the second crystal relative to the polarization ellipse is optimized for the highest efficiency.

The authors of the Comment “assumed” in their analysis of the Letter that the distance between two crystals in our experiment was \(-0.75\) mm, which was placed at a point (in Fig. 1 of the Comment) that matches their results for the optimal distance; however, our experimental distance was 4 mm. It follows from their own analysis (Ref. 2 of the Comment) that the distance between the crystals exceeds the optimal distance calculated by them by a factor of

\[\text{OCIS codes: 140.7090, 140.3280, 140.3590, 320.1590, 320.5540, 230.5440}\]
4/0.75–5.3 there should be no improvement of efficiency, and there could be even a reduction.

Without knowing all the details of the calculations it is hard to pinpoint the discrepancies of the proposed theory. One of the obvious problems in Ref. 2 of the Comment is a neglect of the effect of the fundamental wave on the XPW through cross-phase modulation as well as other nonlinear phenomena.

In conclusion, we do not agree with the Comment for the following reasons:

1. The rotation is the major factor affecting the efficiency—this follows not only from our results but from Fig. 2 of the Comment.

2. The value of ellipse rotation is significant: the rotation angle (10°, as we measured experimentally, or even 7.5°, used by the authors of the Comment in their calculation in Fig. 2 is significantly larger than the “maximum” value of 4° that the authors of the Comment claim it should be less than.

3. The explanation of the improved efficiency in our experiment offered by the authors of the Comment is wrong because in their analysis of our experiment they assumed a wrong value for the separation distance.

4. The whole subject is only marginally relevant to the content of the original paper.

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References