Tognolini *et al.* Reply: In the preceding Comment, Arafune *et al.* [1] expressed their concerns about the interpretation of the dichroic signal measured on the image potential state, that, in our Letter [2], we consider a measure of a Rashba spin-orbit coupling. In this Reply, we take up and address their comments step by step.

The first and the most important issue is that the adopted geometry should not allow one to measure a Rashba type dichroic signal in the image potential state (IPS). We recognize that this issue is grounded but indeed the effective geometry is not so compelling to hinder the measure of the Rashba splitting of the surface states. The angle of incidence of the laser beam with respect to the surface normal was $\theta = 30°$ at normal emission; this means that the wave vector (q) of the incident light has both a perpendicular component to the sample surface (q_z) and a parallel ($q_\parallel$) component. In addition to q_x, along the electron $k_\parallel$, there exists a not vanishing q_y due to the dimension of the viewport and to the optical lever of the laser trajectory. As a consequence the incidence plane of the light is not coincident to the emission plane and few degrees (about five degrees) exist between them. Furthermore, during the measurements with circularly polarized light, to improve the counting statistics, we have increased the angular acceptance to 1.3° corresponding to $\delta k_\parallel = 0.026$ Å^{-1} increasing the coupling between the light wave vector q_y and the Rashba spin of the surface state electrons. Conclusively, the measure of a dichroic signal can be considered the experimental proof that our geometry allows one to observe a not negligible Rashba effect.

In the second issue, Arafune *et al.* [1] do not exclude that the observed circular dichroism could be assigned to an initial state effect. Two occupied surface states lie, in fact, below the Fermi level. We notice that this hypothesis, being the two surface states as well as the IPS Rashba-type splitted, can be advanced only if it recognizes that our geometry is not so restrictive.

Further, to address the initial state effect, we have reported in our Letter [2] the experiment performed by using two different photon energies (4.64 and 3.12 eV). Using a photon energy of 4.64 eV, the IPS is populated in a quasiresonant way from the surface state SS1 (0.26 eV below the Fermi energy) that shows a significant Rashba-type splitting, 45 times larger than the one measured on the IPS. Using a photon energy of 3.12 eV, the IPS could be populated, by absorbing two photons, by a second surface state SS2 (2–3 eV below the Fermi energy) that shows, in contrast with the SS1, a negligible Rashba-type splitting [3]. We underline that, while in the first case, by tuning the pump photon energy, we measure a strong increase of the IPS intensity due to a quasiresonant population of the IPS from the SS1, in the second case, by decreasing the photon energy up to 3.12 eV, the IPS intensity does not change significantly indicating that the photon energy may be not able to populate in a resonant way the IPS from the SS2.

The dichroic signal and the spin-orbit splitting estimated by using the two different photon energies (4.64 eV and 3.12 eV) results the same ($\Delta E_{SO} = 11.5 \pm 2.0$ meV at $k_\parallel = \pm 0.16$ Å^{-1}). This result excludes the initial state effect that, on the contrary, should result in a completely different IPS dichroic signal due to the great difference between the SS1 and SS2 Rashba splitting.

In conclusion, we reject the criticisms raised by Arafune *et al.* [1] and we are firmly convinced that the measured dichroic signal is due to the Rashba splitting of the IPS, as theoretically predicted for metal surfaces with a significant spin-orbit coupling and as recently experimentally confirmed by the authors of the Comment [4].

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Received 11 October 2016; published 1 December 2016
DOI: 10.1103/PhysRevLett.117.239702

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