Das Sarma and Hwang Reply: It has been known for a long time [1] that a semiconductor bilayer structure can support two kinds of electronic collective modes ("plasmons")—one, the so-called optical plasmon (OP), has the usual $\sqrt{q}$ two dimensional (2D) plasma dispersion in the long wavelength limit (2D wave vector $q \to 0$) and the other, the acoustic plasmon (AP), has a linear ("acoustic") wave vector dispersion at long wavelengths *in the absence of any interlayer tunneling.* These predicted collective modes have been experimentally observed via the inelastic light scattering spectroscopy [2]. In our original paper [3], which the accompanying Comment [4] by Bolcatto and Proetto (BP) criticizes, we asked the simple question of how the bilayer OP and AP would be modified as the interlayer electron tunneling t is turned on (i.e., $t \neq 0$)—the original theoretical work of Ref. [1] being strictly valid in the $t = 0$ limit. The interlayer tunneling strength in semiconductor bilayer structures is traditionally characterized by a tunneling induced symmetric-antisymmetric single particle gap, $\Delta_{SAS} = 2t$, or splitting in the lowest quantum level of the system. Among the *many* analytical and numerical results obtained by us in Ref. [3], the preceding Comment of BP concentrates entirely on one result [Eq. (7) in Ref. [3]], namely, our analytical finding that the tunneling induced single particle gap Δ_{SAS} induces a collective long wavelength gap (Δ) in the AP which goes as $\Delta \sim \Delta_{SAS}$ for $\Delta_{SAS}/E_F \gg q_{TF}d$ and $\Delta \sim \sqrt{\Delta_{SAS}}$ for $\Delta_{SAS}/E_F \ll q_{TF}d$. This analytic result follows straightforwardly from our Eq. (6) which we derived in Ref. [3]. Our Eq. (6), which is equivalent to Eqs. (1) and (2) in the Comment [4], is not questioned by BP.

The Comment of BP is trivial and is based on the misconception that the single particle gap Δ_{SAS} cannot be taken as an independent variable. This is untrue since theoretically Δ_{SAS} is a perfectly well-defined independent variable which does not, in principle, depend on subband densities, layer separation, or any other variable. Even in practice it is possible to vary Δ_{SAS} independently of layer separation d by changing, for example, the interlayer barrier height or by applying an external magnetic field parallel to the layer which affects tunneling. The Comment of BP makes the misleading claim that Δ has a logarithmic, $\ln\Delta_{SAS}$, correction by writing $\Delta_{SAS} \sim \exp(-q_\perp d)$ and then inverting this relation to write the interlayer separation d as a dependent variable which is a logarithmic function of Δ_{SAS}. In the theoretical literature on bilayer systems it is traditional to consider Δ_{SAS} and d as *independent* variables for obvious reasons that d determines the interlayer interaction strength and Δ_{SAS} determines the interlayer single particle tunneling strength. The fact that Δ_{SAS} happens to depend on d in a particular sample is a matter of profound inconsequence for the theory. It seems that the authors of the preceding Comment [4] missed the very simple point that our Eq. (7), but *not* our Eq. (6), is obtained on the basis of Δ_{SAS} being taken as an independent variable and that our results hold for *single subband occupancy,* which is completely allowed since Δ_{SAS} and E_F are independent variables. The claim by BP that E_F must be larger than Δ_{SAS} whenever $\Delta_{SAS}/E_F \ll q_{TF}d$ is not true since Δ_{SAS}, E_F, and d are independent variables in our theory. In this context we mention that conclusions similar to ours regarding bilayer tunneling plasmons were also obtained by Gumbs and Aizin [5].

Finally, the claims of BP [4] of their calculation being exact and rigorous are mistaken. We used random phase approximation (RPA) in our theory [3] whereas BP used the so-called time dependent local density approximation (TDLDA) which was first applied to double quantum well structures by Das Sarma and Tamborenea [6]. Our Eq. (6) and all our numerical results (Figs. 1–3) in Ref. [3] treat tunneling exactly as the BP calculation does. We emphasize that while RPA is a perfectly well-defined theory (which becomes exact at very high densities) for calculating the elementary excitation spectra, the TDLDA theory is in fact ill-defined and is *not* a well-controlled approximation in any situation. This is so because the single particle LDA excitation energies and wave functions are not meaningful quantities, and BP's claim of TDLDA being a rigorous theory is incorrect. All the results we derived in Ref. [3] remain valid in their regimes of applicability.

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