

In this manuscript, starting from basic equations of many body Green's function theory, the authors provide a derivation of the excitonic Bloch equations beyond that based on model Hamiltonians. In this way they provide a theoretical tool for the description of the exciton dynamics that can be interfaced with modern ab-initio calculations of electronic structure and lattice vibrations. In addition their derivation suggests that the exciton dynamics governed by model Hamiltonians is affected by an overscreening of the electron-phonon interaction. The problem of the overscreening seems to be removed in their formulation.

In my opinion the manuscript sounds physically clear, well explained and I am in favor of its publication. However I found that there are some issues in the formulation of the theory and some questions the author should answer before the paper is published.

1. I am not really convinced that the definition of the exciton-phonon coupling provided in previous works such as in Ref. [73,75] of the present manuscript is affected by overscreening. From basic equations of many body Green's function theory, the Bethe-Salpeter equation (BSE) in presence of electron-phonon interaction can be obtained once an approximation for the electron self-energy in terms of the electron-phonon coupling is given. In particular, the functional derivative of the electron self-energy respect to the electron Green's function gives the kernel of the BSE from which we can extract an effective exciton self-energy that provides also the definition of the exciton-phonon coupling as discussed in Ref. [50] of the present manuscript. Still in that work the authors have shown that a suitable approximation for the electron self-energy is that reported in Fig. 1 (b) of the present manuscript. Indeed in this expression, as mentioned by the authors, the electron-phonon coupling is not overscreened in contrast with the expression reported in Fig.1 (a) where the overscreening is clear. Thus, I totally agree with authors that the correct expression of the electron self-energy is that reported in Fig. 1 (b). At this point, it is important to note that the structure of the exciton self-energy (and hence the exciton-phonon coupling) depends from the way in which the functional derivative of the electron self-energy is performed. In Ref. [50] the functional derivative is done neglecting the explicit dependence from the electron Green's function of  $K^{(r),SEX}$  and the electron-phonon matrix elements  $g^s$ . This leads to an expression of the exciton self-energy in terms of the proper part of the tow-particle correlation function  $\tilde{L}^{SEX}$  instead of the full  $L^{HSEX}$  as in Ref. [73,75]. This is the origin of the asymmetric structure of the exciton-phonon coupling  $\tilde{G}$  in Eq. (52). However, in principle there is no reason to neglect the functional derivative of  $g^s$  which depends from the electron Green's function through the screening. This leads to the appearance of additional diagrams in the exciton self-energy. In particular, I suspect that, when the screening is evaluated using  $L^{HSEX}$  (i.e. consistently with our treatment of vertex corrections), the inclusion of the terms arising from  $\frac{\delta g^s}{\delta G}$  would lead to the natural appearance of  $L^{HSEX}$  in the exciton self-energy and thus to a symmetric exciton-phonon coupling  $G$  as in Eq. (44).

In the present manuscript, an expression of the exciton-phonon matrix element formally equivalent to that of Ref. [50] has been obtained evaluating the collision integral in Eq. (43) using the ansatz in Eq. (46) and taking only terms linear in the off-diagonal part of  $G$ . However in doing this, the authors consider only off-diagonal  $G$  appearing in the external lines. But what about the linear terms where the off-diagonal  $G$  appear inside  $g^s$ ? I suspect that neglecting these terms is in some way equivalent to neglect  $\frac{\delta g^s}{\delta G}$  in the derivation of Ref. [50].

I suggest to think about this point before resubmitting the manuscript.

2. The eigen-value problem in Eq. (2) describes excitons in absence of population. Thus it can be used only for optical absorption. In the present case the authors are treating excitons out-of-equilibrium. What about the Pauli blocking factors. Is there a reason to neglect them in Eq. (2)? Is this related to some assumption concerning the laser pulse (frequency of the laser pulse close to the exciton energy, small population etc)?

The author should clarify this point in the manuscript.

3. The Bloch equations obtained in this work should describe how photoexcited coherent excitons are converted into incoherent excitons and how the latter propagate and eventually thermalize. Thus when the quasi-equilibrium is reached, these equations predict a photoexcited system consisting of a thermalized incoherent exciton gas. However, in general we expect a configuration in which incoherent excitons coexist with an electron-hole plasma [see for example: phys. stat. sol. (b) 131, 151 (1985)]. What about the electron-hole plasma? What is the regime in which the theory developed by the authors is applicable? I think the author should discuss these aspects in the article.

4. In Eq. (2) I do not see any index associated to the spin degrees of freedom. Thus, I suppose that spin variables have been summed up in some way. This requires the introduction of two decoupled BSEs. One for the singlet channel and one for the triplet channel [see: RIVISTA DEL NUOVO CIMENTO VOL.

11, N. 12]. If this is the case Eq. (2) refer to the singlet channel [see Phys. Rev. B 62, 4927 (2000)]. As a consequence, the matrix elements of the bar Coulomb potential in Eq. (3) should be multiplied for a factor 2.

5. In the manuscript the author call  $v$  "direct electron-hole interaction" and  $W$  "exchange electron-hole interaction". In the literature, on the contrary, direct and exchange are usually used to indicate  $W$  and  $v$ , respectively.