Agam *et al.* Reply: In the preceding Comment [1] the authors express concerns surrounding the validity of the field theoretic approach outlined in Ref. [2]. Their criticism is based largely on two issues: First, the seeming arbitrariness in the choice of the parametrization of the σ model, and, second, claiming that phase information is discarded in an "uncontrolled" way. We believe that this criticism is based on incorrect premises, and take this opportunity to elucidate these points which have not been discussed at length in Ref. [2] due to lack of space.

The general aim of the field theory approach is to express statistical properties of chaotic quantum systems in terms of the low lying modes of density relaxation. The guiding principle is to exploit *all* the symmetries of the problem, those in Hilbert space, such as time reversal or reflection symmetries, as well as those in the internal space of the retarded and advanced sectors. The massless modes of the resulting effective theory are described by *slowly varying functions* in the phase space of the system. They are generated by the symmetry transformations that preserve the interaction term. Each discrete symmetry of the problem leads to an additional sector of the saddle manifold. To illustrate this point consider a time reversal invariant (TRI) system. As the authors point out, the analysis of a TRI system with non-TRI parameterization would lead to wrong results. The point where the mistake occurs is the assumption that all massless modes of the problem were correctly identified. For a TRI system it is a wrong assumption, since additional sector of the saddle manifold can be generated by the TRI symmetry of the problem. Taking this symmetry into account one may proceed using four component fields as in the Gaussian unitary ensemble (GUE) case. However, from a technical point of view, it if more convenient to extend the internal space of the fields to include conjugate field components explicitly.

We are not aware of a way of obtaining a σ model corresponding to two GUE's starting from one Gaussian orthogonal ensemble (GOE) Hamiltonian. In our opinion, the authors make this claim based simply on the identity of the tangent spaces in these two cases, which is not sufficient for the construction of the σ model.

Similar to the TRI case, systems which possess a discrete symmetry show additional low lying modes that in the semiclassical approximation correspond to the interference of paths related by the discrete symmetry. Such modes are associated with new saddle manifolds related to the first by the symmetry operations of the group. As for the case of time invariant systems, these modes are more conveniently accommodated within the general framework of the supersymmetry approach by an extension of the internal space for each discrete symmetry.

Arithmetic billiards on surfaces of negative curvature have no simple geometric symmetry, but rather a hidden symmetry of number theoretic origin. It is unclear how the approach of Ref. [2] should be generalized, i.e., how to construct and parametrize all the massless modes.

To summarize the first point, we stress that there is no arbitrariness in the choice of the σ model. It is dictated by the symmetries of the problem as much as the construction of the diagonal approximation in periodic orbit theory. The investigation of the role of T invariance as well as discrete symmetries was discussed at length in Refs. [3,4].

Turning to the second issue, we would like to reiterate that quantum interference effects within the nonlinear σ model are encoded in two ways: The first is through the structure of the nonlinear manifold $Q^2 = 1$, while the second is through the local dynamics which in our case is given by the commutator with the Hamiltonian of the system, $[H, \cdot]$. A relaxation of the nonlinear constraint generates a perturbative expansion of the field theory [5]. Quantum (or weak localization) corrections absent in the conventional diagonal approximation are obtained from the higher orders of the perturbation expansion [6].

We believe, therefore, that the line of reasoning that led the authors to conclude that "Taking the classical time evolution .... involves discarding phase information in an uncontrolled way" is incorrect. Indeed, the virtue of the present theory over previous semiclassical approaches lies in the controlled nature of the approximation. The substitution of $[H, \cdot]$ by $i\hbar\{H, \cdot\}$ is consistent with the aim of constructing an effective semiclassical theory accounting for *low* lying modes of density relaxation (i.e., slowly varying Q fields).

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