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REPLY

Reply to Comment on 'Exact analytical solution for the generalized Lyapunov exponent of the two-dimensional Anderson localization'

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Abstract

We reply to comments by Markoš *et al* (2004 *J. Phys.: Condens. Matter* **16**) on our recent paper (Kuzovkov *et al* 2002 *J. Phys.: Condens. Matter* **63** 13777). We demonstrate that our quite different viewpoints stem for the different physical assumptions made prior to the choice of the mathematical formalism. The authors of the comment expect *a priori* to see a single thermodynamic phase while our approach is capable of detecting the co-existence of distinct pure phases. The limitations of the transfer matrix techniques for the multi-dimensional Anderson localization problem are discussed.

In our original publication [1] a conceptually new view of two-dimensional Anderson localization has been put forward. The main message of our work can be summarized in one sentence: the metal–insulator transition (MIT) 'should be interpreted as a first-order phase transition' [1]. We believe that a misunderstanding of this crucial point (which indeed 'contradicts standard wisdom' [2]) has led to the objections of Markoš *et al* [2]. In this reply we elaborate on this conceptual difference and then comment briefly on specific points of the criticism mentioned in [2].

One of the cornerstones of the famous scaling theory of localization [3] and subsequent numerical studies is that, for each particular energy and disorder strength, the system must be *only in one* of the two phases: with either localized or extended wavefunctions. The main conclusion of our work does not fit this phenomenological framework: we argue not for the existence of a single extended phase, but for the *co-existence* of extended and localized phases for the same energy and disorder strength. *Which* of these two pure phases is realized depends on the particular realization of the disorder.

How does this conceptual difference translate into the difference between our results and the results of conventional transfer matrix calculations [2, 4]? Both approaches start

with the formal statistical ensemble which includes all realizations of the disorder and thus describes both pure phases, extended and localized. Averaging a physical quantity over this ensemble produces an average between the properties of two pure phases which is not capable of characterizing each phase separately. This is the source of failure for the standard approach: it considers such heterogeneous averages only and views them as characterizing a single pseudo-pure phase. In such a mixture, Lyapunov exponents of the pure insulating phase always dominate [1] and this gives a false impression that only localized solutions are present. This is how Markoš *et al* pose the question: whether the average transfer matrix $T^{(2)}$ describes the extended or the insulating phase? Subsequently, the answer they find is not satisfactory: the assumed pseudo-pure phase appears to be localized in 2D (as well as in 3D) for any degree of the disorder.

Distinctive features of our mathematical formalism (signal theory) follow naturally from the requirement that *the theory must be able of describing adequately multiple pure phases* (in case more than one such phase exists). We have repeatedly emphasized this point in our original work by introducing the term 'multiplicity of solutions' (MS) [1]. The original problem is linear and the MS requirement for an exact solution puts severe restrictions on the form of the mathematical formalism. Two points are essential here:

(i) a precise definition of the phase in analytically exact theories is possible only in the thermodynamic limit [5]. That is why our approach assumes an infinite system in the lateral direction ($M = \infty$ in terms of [2]) from the very beginning.

(ii) We go beyond the description of heterogeneous averages $\langle \psi_{nm} \psi_{n'm'} \rangle$ (signals in our terminology) by considering the fundamental function—the filter $H(z)$. In the region of phase co-existence the filter shows MS, corresponding to multiple subsets of propagating signals. This is the point where one can start analysing the properties of distinct pure phases and not the mixture of them.

The transfer matrix approach [2, 4, 6] does not possess these crucial features. It starts with a quasi-1D system and approaches the thermodynamic limit only asymptotically. We have already emphasized in our work (see section 4.2 in [1]) that the quasi-1D transfer matrices lose the MS property which is the key for a correct description of multiple phases. Indeed, Markoš *et al* note [2] that the thermodynamic limit is problematic in their approach: 'the limit $M \to \infty$ of the discrete model discussed here bears various conceptual and technical difficulties'. Their suggested solution (studying this limit numerically) is hardly adequate to overcome these difficulties, while our approach treats the system as a truly multi-dimensional one from the starting point.

It is not a coincidence that we were able to solve the 1D case both by the method of the transfer matrix and signal theory, while for 2D the former method was abandoned [1]. In 1D, there is no phase transition and the MS property plays no role, while in higher dimensions the two methods are *not* equivalent.

Even within the transfer matrix approach, which should give the properties of a single pseudo-phase, we do not fully agree with the authors of the comment [2]. In the transfer matrix approach (as well as in the signal theory), the fundamental Lyapunov exponent is determined by the *maximal* eigenvalue of the transfer matrix (respectively, the *maximal* root of the equation for filter poles). Therefore the analysis of the eigenvalues close to the unit circle [2] is not sufficient to determine the phase diagram of the system.

We can also not agree with the statement [2] concerning the ambiguity of our averaging procedure. The latter has been described at length in section 3.2 of our original work [1], assuming familiarity of the reader with only basic aspects of signal theory and linear algebra.

Regarding the last critical remark in [2] we note that, since our approach does not seek to 'detect a metallic phase' but rather to find the multiplicity and the properties of pure phases, there is no *physical reason* to prefer the analysis of the $\langle \ln|\psi| \rangle$ over the analysis of the second moment $\langle |\psi|^2 \rangle$.

Finally, we note that we are not alone in challenging the prevailing view that there is no MIT transition for the 2D Anderson Hamiltonian. The support from finite-size scaling studies has come under question [7, 8]. An even more important challenge comes from experiment. A MIT has been observed experimentally in 2D samples [9], causing significant new research activity. Direct electrostatic probing [10] and photoluminescence spectroscopy [11] show a co-existence of localized and metallic regions associated with 2D MIT and new theories are put forward to address this issue [12, 13]. Spivak [13] associates the phase separation with a *first-order* phase transition between a Fermi liquid and a Wigner crystal. At the same time there is growing evidence that the transition is *disorder-driven* and does not stem from electron–electron interactions [12]. Thus there is a clear need for a revision of the canonical point of view on the localization problem. This revision should touch not only the numerical studies of a tight-binding Hamiltonian [7, 8], but mainly the scaling theory of localization which stands on phenomenological grounds. Our exact analytic results for a microscopic model show that one does not need to go beyond the original framework of the Anderson Hamiltonian in order to describe a MIT and phase co-existence in 2D.

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