ЖЭТФ, 2012, том 142, вып. 6 (12), стр. 1230–1233

REPLY TO THE COMMENT BY P. MARKOŠ

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Received May 18, 2012

We present another interpretation of the data by P. Markoš and give numerous new illustrations of our conception. All the existing numerical data look perfectly compatible with predictions of the self-consistent theory of localization.

My paper [1] presents detailed predictions of the self-consistent theory of localization for the quantities that are immediately measured in numerical experiments; it allows making a comparison on the level of the raw data, avoiding the ambiguous treatment procedure. Such an approach is motivated by the different status of numerical results. The raw data are obtained independently by different groups and there is a certain consensus in this respect; it is not reasonable to question these data. However, it is possible to doubt numerical algorithms themselves, which are not based on a firm theoretical ground. Such an approach is in the interest of numerical researches as long as their present-day results contradict both experiment and the general theoretical principles. The self-consistent theory by Vollhardt and Wölfle (for the first time) allows justifying one of the popular variants of finite-size scaling based on the consideration of auxiliary quasi-1D systems [2, 3] with a finite transverse size L. This theory also predicts essential scaling corrections, such that the scaling parameter behaves as $C(L + L_0)$ with $L_0 > 0$ in the vicinity of the transition, which can be practically interpreted as $CL^{1/\nu}$ with $\nu > 1$. Analysis of the existing numerical data shows that there are no serious contradictions between the self-consistent theory and the raw numerical data.

Of course, this does not prove the validity of the self-consistent theory: deviations can be small but significant, and a serious analysis is necessary. The analysis of this kind is expected from an expert in numerical research such as P. Markoš. In fact, in his comment [4], he makes no effort to follow my suggestions but is fully satisfied with the use of the "standard scaling formulas". First of all, there are no "standard scaling formulas", since corrections to scaling certainly exist and no reliable procedure to deal with them is available. Further, the conventional scaling is certainly invalid for dimensions $d > 4$; this is a theorem [1]. Finally, in [1], I did not deny the possibility to fit the data by a simple power law dependence but I stressed the ambiguity of such procedure. From this point of view, Figs. 2–5 in [4] have no relation to the criticism of my paper.

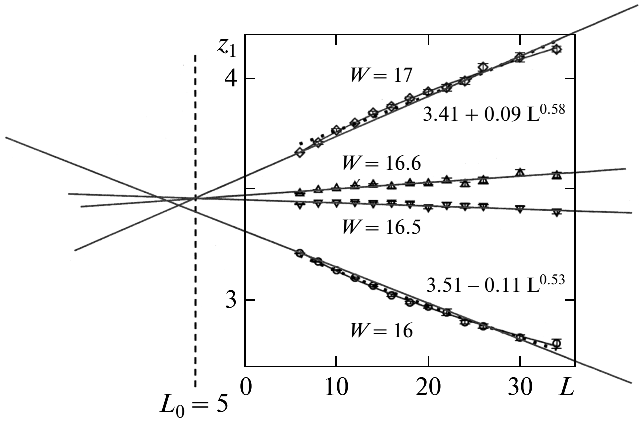

Fig. 1. Our interpretation of the 3D data in [4]

The 3D system. In this case, P. Markoš provides not much progress: he extends his results to $L = 34$, while data up to $L = 50$ were discussed in Ref. [1]. Our interpretation of 3D data is presented in Fig. 1. The following points should be noted.

a) The most interesting question is: does L_0 have essential drift when the range of L is extended? If we try to retain the estimate $L_0 = 5$ obtained in [1] for $L \le 24$, then the data for $W = 16.5$ and 16.6 are fitted well with such a restriction.

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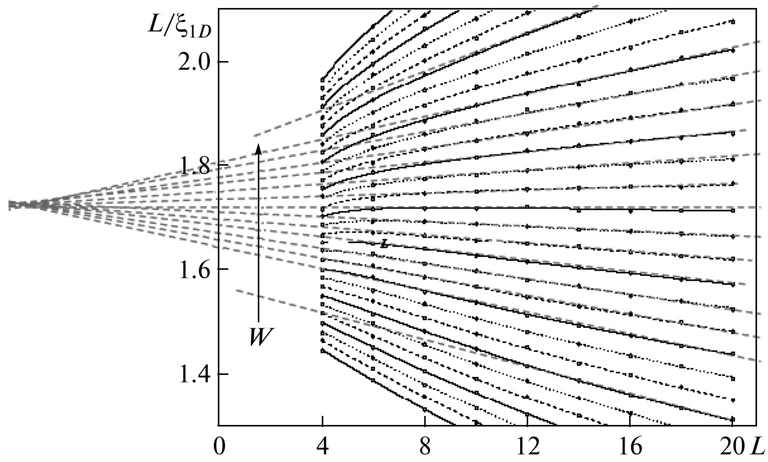

Fig. 2. The same as in Fig. 1, but with smaller L and higher accuracy (from paper [5]). For large deviations from the critical point, the dependences are seen to acquire an essential curvature, while L_0 changes significantly. To be compared with Fig. 1

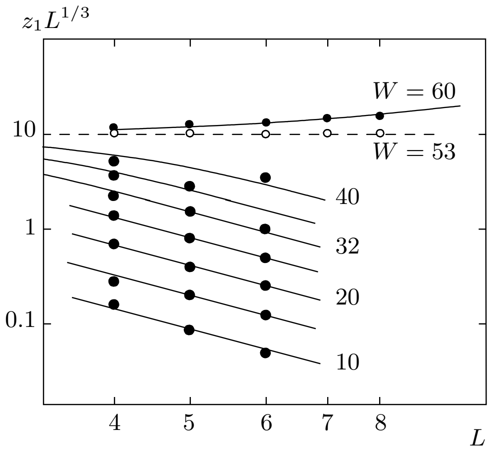

Fig. 3. The 5D data in [4] and its comparison with scaling relation (2)

b) The data for $W = 16$ and $W = 17$ show certain deviations from the linear behavior but they are not very impressive because the scattering of points is rather large.

c) In fact, the data for $W = 16$ and $W = 17$ contain the effect of the W nonlinearity. If we suppose $\nu = 1$, then $\xi \approx 30$ for $|W - W_c| = 0.5$ and nonlinear effects are essential for $L \sim 30$. Figure 1 confirms this conclusion, since the data for $W = 16$ and $W = 17$ are not symmetric with respect to the curve $W = 16.5$ [1)]. Deviations from the linear behavior are on the same level as symmetry violation. It looks rather probable that for the a narrower interval (like $W = 16.25$–16.75), fitting by a linear dependence will be satisfactory [2)]. This argument is supported by other numerical data (Fig. 2).

Markoš has an illusion that a more complicated procedure allows obtaining a higher accuracy. In particular, in the treatment of the W dependence, he relies on the quadratic expansion in W–W_c. In fact, one cannot exclude possibility that the coefficient of the quadratic term is small and higher-order corrections are essential. If different nonlinear functions are allowed, the uncertainty will be the same as for a simple linear fit in a narrower interval. In the latter case, it is impossible to obtain a nonlinear behavior for the derivative $s(L) = [z_1(L)]'_\tau$ from the apparently linear dependencies $z_1(L)$ (Fig. 2). With a nonlinear treatment, Markoš was able to do it (see Fig. 3 in [4]).

The comparison in Fig. 3 in [4] is not honest, because the dashed line does not correspond to predictions of Ref. [1]. The predicted dependence is $C(L+L_0)$ and not CL, and hence the straight line with the unit slope is irrelevant. In fact, our concept works excellently in the range $L \leq 20$ (Fig. 2), where Markoš shows disastrous deviations.

The 5D model. In this section we read:

"Our data in Fig. 4 do not indicate any discontinuity in the L dependence. On the contrary, z_1 is a smooth analytic function of both parameters, W and L".

[1)] In fact, Fig. 1 roughly confirms that $\xi \approx 30$ because deviations of z_1 from its critical value are of the order of unity (if $\nu = 1.5$, then ξ should be something like 150).

[2)] It is clear from Fig. 2 in [4] that the author has the intermediate data for Fig. 1. Why does he not show them?

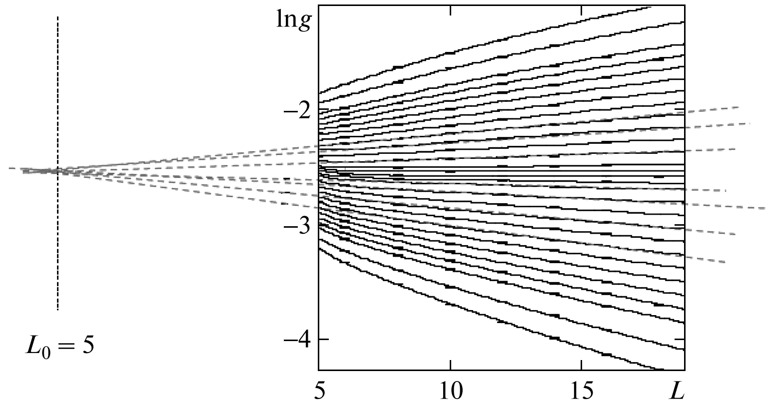

Fig. 4. Data for the conductance distribution [7] and their fitting by the dependence $C(L + L_0)$. The scaling parameter is the 0.17 percentile of the distribution

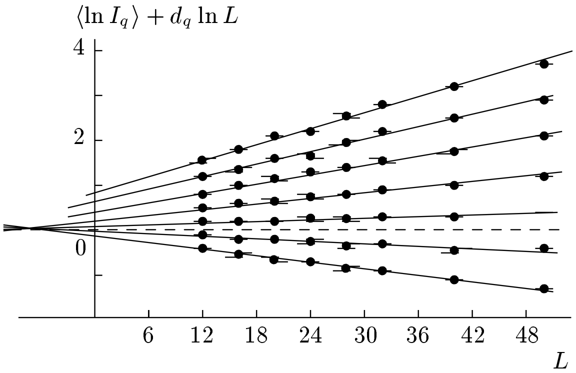

Fig. 5. Data for the inverse participation ratio I_q with $q = 5$ [8] and their fitting by the dependence $C(L+L_0)$. We can see an essential change in L_0 for large deviations from the critical point

I do not predict any discontinuity, it is a fantasy of Markoš. It is easy to see from Eq. (45) in [1],

$$\tau \Lambda^{d-2} = \frac{1}{L^{d-2}} \frac{1}{2mL} - cm^2 \Lambda^{d-4},$$

that $mL \equiv z_1$ is a regular function of L and $\tau = W - W_c$. A singularity is developed only in the thermodynamic limit $L \to \infty$, as in all scaling theories. Modifications suggested for $d > 4$ correspond to the usual scaling constructions, but in different variables

$$y = \frac{\xi_{1D}}{L} \left(\frac{a}{L}\right)^{(d-4)/3}, \quad x = \frac{\xi}{L} \left(\frac{a}{L}\right)^{(d-4)/3}. \qquad (1)$$

The scaling relation is found in the analytic form

$$\pm \frac{1}{x^2} = y - \frac{1}{y^2}, \qquad (2)$$

where the proper scales for ξ_{1D} and ξ are chosen. Figure 3 shows the quantity $z_1 L^{1/3} \equiv 1/y$ as a function of L. Its dependence on $1/x \propto L^{4/3}$ has the same form but the logarithmic scale should be changed by the factor $4/3$. The solid lines correspond to the scaling relation (2).

Conclusion. After repeating the legend on discontinuities, the author provides additional argumentation:

"We also note that the same value of the critical exponent was obtained from numerical analysis of other physical quantities: mean conductance, conductance distribution, inverse participation ratio … "

In fact, two variants of scaling, (a) quasi-1D systems and (b) level statistics, were discussed in Ref. [1]. The third variant, (c) mean conductance, is discussed in recent paper [6]. The next two variants, (d) conductance distribution [7] and (e) inverse participation ratio [8] are illustrated in Figs. 4 and 5.

The final arguments are also not serious:

"This value of the critical exponent was recently verified experimentally [11] and calculated analytically [12]".

Papers [11] deal with a quasiperiodic kicked rotor, whose equivalence to the 3D Anderson model is only a hypothesis essentially based on questionable numerical data [3]. The real experiments on disordered systems [10–12] support the results of the self-consistent theory.

The "analytic" result is the relation $s = \nu(d-2)$, which is accepted by all serious theoreticians. Its viola-

[3] In fact, localization in quasiperiodical systems has an essential specificity in comparison with random systems [9].

tion means incorrectness of the one-parameter scaling hypothesis [13], which is a basis for practically all numerical studies.

In conclusion, Markoš does not see the central idea of my paper [1] and continues to use sophisticated treatment instead of direct comparison on the level of raw data. If such a comparison is made, all existing numerical data look perfectly compatible with predictions of the self-consistent theory of localization.

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