Reply to Suslov

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In a recent submission to this archive [1], Suslov has claimed that our recent numerical estimate of the critical exponent of the Anderson transition \( \nu = 1.57 \pm .02 \) is in error and that the available numerical data are consistent with a value of \( \nu = 1 \). We contest this claim and demonstrate using Suslov's own data that the estimate of the critical exponent is inconsistent with \( \nu = 1 \).

I. OUR PUBLISHED ESTIMATE

In [2] we published an estimate of the critical exponent of the Anderson transition based on a finite size scaling analysis of the localisation length \( \lambda \) of electrons on quasi-1d bars of dimensions \( L \times L \times L_z \). In our published work \( L \) ranged from \( L = 4 \) to \( L = 14 \) and \( L_z \) was of the order of \( 10^7 \) in units of the lattice spacing of the tight binding model which was studied. In more recent numerical studies we have extended the maximum system size for which data is available to \( L = 18 \).

The critical exponent is estimated by fitting data for the localisation as a function of system size \( L \) and the magnitude of the random potential fluctuations measured by a disorder parameter \( W \) to a finite size scaling form that contains one relevant scaling variable \( \chi \) and one irrelevant scaling variable \( \psi \).

\[
\Lambda = F_0 \left( \chi L^{1/\nu} \right) + \psi L^\nu F_1 \left( \chi L^{1/\nu} \right) \quad (1)
\]

The functions \( F_0 \) and \( F_1 \) are expanded in Taylor series, as are both scaling variables. An important point to note is that the relevant variable is zero at the critical disorder \( W_c \), so that

\[
\chi = b_1 \tau + b_2 \tau^2 + \ldots \quad (2)
\]

while the irrelevant variable is not, so that

\[
\psi = c_0 + c_1 \tau + \ldots \quad (3)
\]

where

\[
\tau = \frac{W_c - W}{W_c} \quad (4)
\]

In practice, when fitting our data for the box distributed random potential for example, we truncate the expansion for \( \chi \) at second order, the expansion for \( \psi \) at zero order and the expansions for \( F_0 \) and \( F_1 \) at third order.

Using these equations to fit our numerical data we estimated \( \nu = 1.57 \pm .02 \), where the error is a 95% confidence interval, for a box distributed random potential. We obtained estimates consistent with this for Gaussian and Lorentzian distributed random potentials. This value of the exponent is also consistent with a completely independent estimate based on the scaling of the conductance of \( L \times L \times L \) three dimensional systems for \( L \leq 16 \) [3] and with the published estimates of other authors [4–8].

II. THE VALIDITY OF CONFIDENCE INTERVALS

The fitting of a set of parameters \( X \) given a set of data \( D \) and a model \( M \) is often based on finding the maximum of the probability

\[
p(D|X, M) \quad (5)
\]

This is called a maximum likelihood method since the parameters are chosen so as to make the probability of the observed data a maximum. If the probability is calculated by assuming that differences between the observed and predicted data are due to random measurement errors which are independently distributed according to a Gaussian distribution we are led to the well known \( \chi^2 \) fitting procedure.

It is crucial to note that analysis is predicated on the assumption that the model is correct. In particular, the estimate of the accuracy of the fitted parameters are all contingent on this assumption. To be sure, if the goodness of fit is low, then one can doubt the model. However, a reasonable goodness of fit is not a guarantee that the model is correct.

What then are the assumptions underlying our model? We take for granted that the Anderson transition is a continuous phase transition which can be described by the renormalisation group. In addition we assume that the deviations from scaling in our numerical data can be described in practice by a single irrelevant variable. Given these assumptions we arrive at the accuracy quoted for the exponent.

We would like to emphasise that the procedure for arriving at the accuracy of the exponent is a rational and rigorous procedure and the results have a precise scientific meaning and are not a matter of opinion. As with any mathematical procedure the validity of the results...
depends on the correctness of the assumptions made. These have been clearly stated.

It is of course always possible that if data for much larger system sizes were available some limitation of the model we use to fit the data might be exposed. If this turns out to be the case then our estimate of the exponent might have to be revised. Nevertheless, we strongly object to Suslov’s characterisation of our results as “evident disinformation.” They are nothing of the sort.

We think it worth pointing out that with the methods currently used for estimating the exponent the computational time increases as $L^7$. It seems unlikely that accurate data for substantially larger systems will be available any time soon. In this context we are forced to do the best we can with the available data by proceeding in a logical manner. In our opinion the libelous and ill-informed comments in Ref. [1] contribute nothing.

III. SPIN GLASS, WHAT SPIN GLASS?

Suslov claims that out fit is highly non-linear and that maximising the probability $p$ is somehow akin to finding the potential minimum in a spin glass. Though our model does contain many parameters many of them are linear. For example, of the 12 parameters required to fit the data for the box distributed random potential in Ref. [2] 7 are linear. Hardly a spin glass ...

IV. AN ERROR IN SUSLOV’S PAPER

Suslov proposes what he calls “a simple procedure to deal with corrections to scaling.” Unfortunately, his proposal is flawed because of an elementary error in Eq. (6) of his paper. Suslov assumes incorrectly that the irrelevant scaling variable is zero at $W = W_c$ or $\tau = 0$. In fact, only the relevant variable changes sign at $W_c$ as is clear in the correct expansions (4) and (5) given above. If we look at Eq (9) of Suslov’s paper we see that the quantity being fitted must be independent of $L$ at $W = W_c$. However, the most important correction to scaling which is present in our numerical data is precisely a size dependence of $\Lambda$ at $W_c$. Thus Suslov’s method is a non-starter as far as modelling our data over the full range of system sizes is concerned. To use Suslov’s method we are forced to discard any data for which corrections to scaling due to an irrelevant variable are statistically significant.

V. SUSLOV’S PROCEDURE WITH OUR DATA

By restricting the ranges of system sizes and disorder, we have been able to fit some of our numerical data for the box distributed random potential to the following form based on the suggestion in Suslov’s paper.

$$\Lambda = a_0 + \tau f(L) + a_2 (\tau f(L))^2$$ (6)

The fitting parameters are $W_c$, $a_0$, $a_2$ and one $f_i \equiv f(L_i)$ for each system size present in the data. In this case this is a total of 8 parameters, 6 of which are non-linear. The results are shown in Figure 1. The critical exponent is then estimated by fitting the $f_i$ versus $L_i$ to a power law which introduces a further two fitting parameters, giving 10 in total. The result is $\nu = 1.53(46,60)$ and is shown in Figure 2. The numbers in brackets give the 95% confidence interval. For comparison we also plot a line corresponding to $\nu = 1$. We leave it to the reader to speculate on whether or not he or she thinks $\nu = 1$ might be recovered for much larger systems sizes. We do not see any evidence for such a claim.

VI. SUSLOV VERSUS SLEVIN-OHTSUKI FITTING

When we compare the two fitting procedures the following points present themselves. First, Suslov’s method cannot describe the most important corrections to scaling which are present in the numerical data. Second, given that one non-linear parameter is needed for each system size, we end up with more, not fewer, non-linear parameters than the fit we used in Ref. [2].

VII. FINAL THOUGHTS

In conclusion, Suslov’s fitting scheme [1] does not correctly take into account the most important corrections to scaling in our data. If we restrict ourselves to the larger systems simulated where corrections to scaling are negligible and apply the fitting scheme suggested in Ref. [1], we find $\nu = 1.53 \pm .07$, consistent with our previous estimate in [2] but not with $\nu = 1$.

Someone once said that the only certainties in Life are Death and Taxes. Certainly current estimates of the critical exponent of the Anderson transition have not reached this level of certainty and it is important to keep an open mind about how the estimates might need to be revised when new data becomes available. Yet we feel there is no need to be overly pessimistic concerning the accuracy of our current estimates.

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FIG. 1. A fit of our numerical data for the box distributed random potential with disorder $16 \leq W \leq 17$ and $10 \leq L \leq 18$ to Eq. (6).

FIG. 2. The system size dependence of the fitting parameters $f(L)$ from which the critical exponent is estimated. The best fit $\nu = 1.53$ and for comparison a slope corresponding to $\nu = 1$ are shown.