Reply to the 'Comment on 'Transverse fluctuations in the driven lattice gas''

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REPLY TO COMMENT

Reply to the 'Comment on 'Transverse fluctuations in the driven lattice gas''

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The comment by Albano, although aimed at showing that the conclusions of our letter [1] 'are flawed and have to be disregarded', contains neither a real criticism to the approach we used there nor any significant evidence of its possible faults. Only our conclusions are questioned, mainly because they are not in agreement with Albano's expectations and (seemingly) with his numerical results. Possibly, part of the discussion in the comment has been inspired by a misunderstanding of the method briefly outlined in the letter [1] and discussed in detail in [2]. This is confirmed by the type of numerical results that are presented in the comment as evidences against our approach and which are summarized in figures 1(a) and (b) therein. Let us critically review them.

Figure 1(a) is a scaling plot. The lowermost curve refers to the RKLS (we use the notation introduced in the comment), an interesting model different from the one we considered in the letter (KLS with $E = \infty$). The intermediate and uppermost curves refer to the latter and are obtained by using two different sets of critical indices γ and ν and, presumably, as this is not specified in the comment, different values of the critical temperature. Albano points out that the uppermost curve, obtained by using the exponents predicted by Janssen *et al* (JSLC) [3], does not display a satisfactory data collapse and therefore these predictions cannot be trusted, at variance with the conclusion we draw in our letter. (Incidentally, in the intermediate and lowermost curve we do not see the empty circles, squares and triangles that do not show a good data collapse in the uppermost one. Presumably they have $(q(T - T_c)^{-\nu})^2$ larger than 80 when $\nu = 0.63$, and thus they do not appear in the figure. A fair comparison requires the use of a horizontal range including all data in both cases. Experience with these kind of scaling plots for less controversial problems shows that they could lead to wrong conclusions for two reasons:

(1) Are finite-size corrections and corrections to scaling really negligible? This question has not been addressed at all in the comment. Given that this is really difficult to achieve numerically, critical behaviour is nowadays better studied by means of finite-size scaling (FSS) methods and, indeed, this has been the main motivation of our work. Note that in our letter we took care to identify corrections and their dependence on the aspect ratio S_Δ (see [1]).

(2) There are correlations between the estimated critical exponents and the critical temperature. This has to be avoided. To do that, we used a particular form of FSS in which only physical observables, immediately accessible to numerical simulations, enter [4, 5]. This required the introduction of a suitable correlation length [6].

Even assuming that Albano has been able to disentangle the systematic errors (particularly dangerous in controversial circumstances), he does not report on any fitting procedure used to provide statistically meaningful estimates of the critical parameters and of their errors. As far as we understand the author has simply fed into his ansatz the values of the exponents and of the critical temperature and looked at the resulting graph, instead of trying to get the *best* fit from some statistical analysis. In [7], by using an exactly soluble model, we showed an explicit example in which a scaling plot can be misleading in understanding the true critical behaviour. In the comment, Albano points out that his 'data collapse is obtained using arbitrary lattice shapes, in contrast to CGGP's scaling assumptions and their biases selection of a shape'. Albano does not seem to appreciate the difference between FSS and infinite-volume studies. Indeed, the identification of the correct shape factor is fundamental for FSS, while it is completely irrelevant in the case of infinite-volume scaling plots such as those presented in the comment. For instance, in our work [2] we verified that the infinite-volume limit of the correlation length is independent of the choice of the shape factor S_Δ.

Finally, let us discuss the relevance of figure 1(b). It shows that the two-point correlation function $G(q)$ of KLS and RKLS is well approximated by an Ornstein–Zernike (OZ) behaviour. Albano concludes that $G(q)$ is *exactly* given by the OZ expression. This however implies that $\eta = 0$ for *both* models and thus contradicts the prediction of the theoretical model (GSM) that, according to Albano, is supported by the data in figure 1(a).

In our opinion the correct interpretation of figure 1(b) is very simple and the plot has essentially no relevance for the discussion. Indeed, it is well known that, at fixed $T \neq T_c$, $G(q)$ is very well approximated by the OZ behaviour for small momenta in any model and for any size. It is a simple consequence of the fact that $G(q)$, being analytic for $q = 0$, has a regular expansion in powers of q^2. As an example see figures 3 and 4 in [8], where numerical results for the 3D Ising model are presented. Note that the deviations from OZ are better detected by plotting the function $h(Q)$, cf equation (28) therein. In other words, the analysis of $G(q)$ alone does not help in discriminating the issue, unless one is able to determine its infinite-volume behaviour in the limit of small q and large $q\xi$. But this is not easy, since finite-volume corrections are large as soon as q increases and the deviations from OZ behaviour are small (once more cf 3D Ising model).

Of course, a careful study of the scaling behaviour of $G(q)$ would be worthwhile in order to check the prediction that in the KLS model the *scaling* two-point function shows OZ behaviour for *all* momenta. But the graph presented in the comment does not address the subtle problems that are present in this type of studies. Thus, we do not really think that Albano's data confirm this prediction: a much more careful study is needed. We are fully aware of the problems that appear in studying the scaling behaviour of $G(q)$, and thus in our numerical work we only considered $G(q)$ for the smallest possible values of q, and determined the behaviour of the correlation length and of the susceptibility, with the only purpose of determining the exponent values. On the basis of figure 1(b) Albano claims that our method for checking the theoretical scaling function F_ξ of the correlation length ξ_L, as outlined in the letter, cannot be effective given that, anyhow, in the KLS and RKLS (and we might add the Ising model) $G(q)$ is well approximated by the OZ form. We see no direct connection between this statement about $G(q)$ (that is correct and on which we fully agree) and Albano's conclusions. Indeed, our FSS analysis of ξ_L does not simply boil down to study, at fixed $T \neq T_c$, the correlation function $G(q)$ (what Albano tried to do). Moreover, our prediction for F_ξ is based on the much stronger statement of the JSLC theory that the effective theory for transverse fluctuations is Gaussian. In any case, as we mention in the letter and discuss at length in our longer paper [2], not only do our data support the theoretical form of F_ξ but they also confirm the Gaussian nature of transverse fluctuations, giving $\nu \approx 1/2$, a finite value of χ/ξ^2 at criticality, and, remarkably, a vanishing Binder parameter. All these results are in sharp contrast with the GSM field-theoretical model that Albano supports.

For all these reasons we believe that the evidence given by Albano on the equivalence between the critical behaviour of KLS with $E = \infty$ and RDLG is really feeble. Numerical data look very similar but in order to extract the proper information a much more detailed analysis, along the lines we have already followed in [1, 2], is necessary also for the RKLS. Moreover, the numerical results presented in the comment are not directly related to our numerical work and do not provide any insight into the problem of the universality class of the KLS model.

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