Reply to "Comment on 'Critical behavior of a two-species reaction-diffusion problem'"

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Recent Monte Carlo results [Phys. Rev. E **61**, 6330 (2000)] on the one-dimensional reaction-diffusion process $A+B\rightarrow 2B$ and $B\rightarrow A$ lead us to estimate $\nu=2.21\pm 0.05$ for the correlation length exponent. The preceding Comment [Phys. Rev. E **64**, 058101 (2001)] advocates the exact value $\nu=2$. We show that Janssen's arguments leave enough doubts to justify an independent Monte Carlo determination of ν.

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In our work [1] referred to by the preceding Comment [2], we treated the exponent ν as an unknown, with the result $\nu=2.21\pm 0.05$; any other value of ν deteriorates the fit to the order parameter curve, based on our Fig. 3 and the discussion of Sec. III A 2. We should have mentioned the relation $\nu=2/d$ due to Kree, Schaub, and Schmittmann (KSS) [3], and commented on it not being satisfied in our $d=1$ simulation.

Now the Comment asserts (although with a *proviso*: "as long as one assumes . . . ") that $\nu=2$ has to be inserted in the analysis of $d=1$ Monte Carlo data as if it were an established fact. [Anyone convinced of that only has to solve our equation $\beta/\nu=0.197\pm 0.002$ [1] with $\nu=2$ (instead of $\nu=2.21\pm 0.05$) to find $\beta=0.394\pm 0.004$ (instead of $\beta=0.435\pm 0.010$); thereby sacrificing the fit to the order-parameter curve.]

We show below that the Comment is misguided in attempting to impose $\nu=2$ as an *a priori* truth, not in need of independent verification. The author's criticism of our exponent values on this basis is therefore inadmissible.

Our value of ν was stated [1] not to take into account any possible systematic corrections. One is free to hope that if and when such corrections can be handled, they will reduce the Monte Carlo value of ν to 2. Alternatively, this may be a case where Janssen's arguments [2] do not apply and where $\nu\neq 2$.

A good reason for their potential failure, and hence for circumspection, is of a well-known kind. The relation $\nu=2/d$ depends on the existence of a fixed point having all the symmetries listed in the Comment. The existence of such a fixed point was demonstrated in an ϵ expansion (the "KSS fixed point" [3]), but is increasingly subject to doubt as d is lowered: The quartic terms in the action (see [4]), which are irrelevant in the ϵ expansion, may become relevant for d as far down as $d=1$. A warning signal (not a proof) is that at the Gaussian fixed point these terms are "naively relevant" for $d<2$, and that at the KSS fixed point, due to η being negative, their scaling dimensions *increase* with ϵ.

If the quartic terms are relevant in $d=1$, then, because they break the continuous symmetry on which $\nu=2$ is based, these terms destroy this relation. The discrete symmetries (IV and V in the Comment's notation), however, continue to be respected by the quartic terms [4], and it is hard to see how, even if they are relevant, these terms could affect the exponent relations that we used later on in our work [1]. In particular, $\eta=\tilde{\eta}$ is guaranteed by the time-reversal symmetry of the action [4], quartic terms included.

KSS define their model originally as a system of two coupled Langevin equations [[3], Eqs. (2.1b) and (2.4)–(2.6)], which leave the quartic and higher-order terms of the action unspecified. It is very difficult in a heuristic Langevin-type approach to guess the correct higher-order terms. The actions of various different microscopic models, constructed as prescribed by the work of Cardy and collaborators (see, e.g., [5]), all coincide with the KSS action to third order *and* provide, in addition, explicit expressions for the higher-order terms. One of these is the model ($A+B\rightarrow 2B$, $B\rightarrow A$) of our work [1], and another one the model ($B\rightarrow 2B$, $2B\rightarrow B$, $B+C\rightarrow C$) brought up in the Comment, and which the author names the "KSS model." This other model is not directly related to the question of what should be our value of ν—its ν is conceivably different from ours in $d=1$—and might be left out of the discussion. Commenting upon it nevertheless, let us point out that, interestingly, its quartic terms just like ours break the continuous, but respect the discrete symmetries. Hence, in this other model similar caution about the value of ν is required.

Finally, and for completeness, we use this occasion to evoke a possibility not advanced in the Comment, even if we consider it remote ourselves. One might attempt to explain the discrepancy between $\nu=2$ and $\nu=2.21\pm 0.05$ by the difference between, on the one hand, the process ($A+B\rightarrow 2B$, $B\rightarrow A$) supposed to take place in continuous time and with finite reaction rates, and, on the other hand, the simulation algorithm that uses discrete time steps and an infinite bare on-site contamination rate [[1], Sec. II A, rules (1)–(3)]. This difference could be accounted for in the action, in principle, by a whole host of complicated higher-order terms that would certainly break all symmetries. The preceding discussion implicitly assumes the irrelevance of these "difference terms."

In conclusion, our Monte Carlo value $\nu=2.21\pm 0.05$ and the arguments in favor of $\nu=2$ will have to coexist awaiting further investigation. The simulation data, in the absence of further theoretical guidance, at best provide some rough idea of how large the corrections to scaling would have to be if ν were equal to 2. On the analytical side, a careful analysis of the higher-order terms in the action is required before any assertions as strong as the Comment's are justified.

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