

Reply to “Comment on ‘Two-loop renormalization-group analysis of the Burgers–Kardar–Parisi–Zhang equation’ ”

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We explain why we feel the objections raised in the preceding Comment do not apply. We also take the opportunity to further clarify our renormalization scheme and the physical and conceptual differences appearing in the cases $d < 2$ and $d \geq 2$, respectively, where d is the substrate dimension. Furthermore, we link our calculations to recent progress within the framework of the directed-polymer representation [M. Lässig (unpublished)], by which our results are confirmed on considerably more general grounds.

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In the preceding comment [1], T. Sun, based on his work with M. Plischke [2], raises several strong objections to both the methods we have applied and the results we have obtained in our recent two-loop dynamic renormalization-group analysis of the Burgers–Kardar–Parisi–Zhang (KPZ) equation [3]. As is properly explained in the comment [1], the qualitative differences in the results and conclusions of Refs. [2] and [3], both papers embarking to extend the previous one-loop analysis [4,5] to next order, are indeed severe and require clarification. As we shall explain in more detail, we find that Sun’s arguments to discard our renormalization scheme and scaling analysis are invalid; in fact, most of the issues have already been addressed in our paper [3]. Yet, Sun questions our main conclusion for substrate dimensions $d \geq 2$, namely the nonexistence of a finite strong-coupling fixed point within a perturbational approach in that regime (*including* the two-dimensional case), and the values $z_c = 2$ and thus $\chi_c = 0$ for the dynamic and roughness exponent at the dynamic phase transition, although these are fully in accord with the scaling analysis by Doty and Kosterlitz [6]. However, all these results have recently been confirmed to *any* order in perturbation theory by M. Lässig in a completely independent approach using the directed-polymer representation [7], and thus seem to render Sun’s criticism towards our findings obsolete.

We now turn to address his objections in detail; we shall use the notations of the preceding comment [1]. To start with, we agree that one of the main differences in the two calculations lies in the absence of (height) field renormalization in Ref. [3], while Sun and Plischke, in contrast, claim a singular contribution to the vertex function $\Gamma_{11}(\mathbf{k}, \omega)$ (Γ_{hh} in our notation [3]) which may not be absorbed into a renormalization of the diffusion coefficient ν . Our conclusion in Ref. [3] is based on very familiar arguments in dynamic renormalization-group theory [see, e.g., Ref. [8], Eqs. (8.16) and (8.17), for the, in this respect, very much related case of the critical dynamics of model B]. Namely, *all* loop contributions to $\Gamma_{11}(\mathbf{k}, \omega)$

vanish at $\mathbf{k} = \mathbf{0}$, due to the wave vector dependence of the vertex, i.e.,

$$\Gamma_{11}(\mathbf{0}, \omega) = i\omega \quad (1)$$

holds to *any* order in perturbation theory, and in this sense constitutes an exact relation [9]. Equation (1) is certainly valid for any *finite* value of the ultraviolet momentum cutoff Λ ; hence it still remains true in the limit $\Lambda \rightarrow \infty$, from which the singularities in $1/\epsilon$ are to be inferred in the dimensional regularization scheme, if the limit $\mathbf{k} \rightarrow \mathbf{0}$ is taken *first*. Thus by the conventional choice of the normalization conditions (see Ref. [8]), it follows that the height fields do not renormalize. (Note that the assignment of Z factors to physical quantities is not unique; but the number of *independent* renormalization constants stays, of course, fixed, see footnote [39] in Ref. [3].)

The above procedure does not cause any problems, as long as it is assured that the regime of the infrared singularities is carefully avoided [10,11]. In a massless field theory, such as for the Burgers–KPZ problem, one has to be careful here, as we have noted already [3]; yet the above requirement is definitely fulfilled by taking the normalization point at finite frequency, i.e., $\mathbf{k} = \mathbf{0}$, $i\omega/2\nu = \mu^2$ [3]; note that $i\omega$ effectively acquires the role of a “mass” parameter.

Sun, however, criticizes this choice and states that we employed an incorrect normalization point [1] in our calculations, elaborating on the point that, in his opinion, we should have rather used a normalization point at finite wave vector instead of finite frequency. But, whenever the normalization point lies well outside the (infrared) critical region, its specific choice cannot alter any (physical) results. This is assured by either fixing the momentum at $k^2 = \kappa^2$ (the choice in Ref. [2]), or, *equivalently*, by fixing the frequency at a finite value (our prescription). The fact that the theory is massless merely implies that wave vector and frequency at the normalization point must not be taken to be zero *simultaneously*. Furthermore, both choices must indeed lead to identi-

cal conclusions, and neither “violates the principles of renormalization-group theory” [1]. Also, as there exists no meaningful static limit of the Burgers–KPZ equation for general dimensions $d \neq 1$, and because in a scaling theory all variables are generically equivalent (there is no such concept as a basic scaling variable [1] apart from matters of convenience and physical intuition), there appears to be no reason why the choice of finite momentum should in any respect be preferential *a priori*. It should thus be clear that indeed $M_1 = M_2 = \dots = 0$ in Eq. (3) of Ref. [1]. We are not inclined to embark on any speculation as to why, despite these facts, there appear singular terms in the k -independent part of $\Gamma_{11}(\mathbf{k}, \omega)$ in the calculations of Ref. [2].

In his comment, Sun also states that the k -dependent parts [N_1 and N_2 in his Eq. (3)] did not acquire singular contributions in our paper, thus leading us to a “free-field result” of the Edwards–Wilkinson type [1]. We do not agree with this statement. In fact, our two-loop contributions to Z_ν [Eq. (3.44) in Ref. [3]] do not vanish, and even in the disputed case of two substrate dimensions we find

$$Z_\nu(d=2) = 1 - \frac{\hat{g}_0^2}{4\epsilon} + O(\hat{g}_0^3) \quad (2)$$

[using $F_\nu(d=2) = 8$ according to Eq. (A38) in Ref. [3]]. Therefore, we feel that Sun’s assertion that in our analysis $\Gamma_{11}(\mathbf{k}, \omega)$ was not renormalized [1] is wrong, and his following claims that we were merely investigating the scaling of the Edwards–Wilkinson equation [12] are misleading (the coefficient for the noise correlation D is renormalized, too, anyway). Neither does our renormalization scheme “remove the nonlinear term from the KPZ equation in $d = 2$ ” [cf. Eq. (2)], as is implicated in the comment [1]. (Sun’s criticism towards our “partial ϵ expansion” scheme shall be addressed below.)

Sun also claims to find some problems with our scaling analysis in Sec. III of Ref. [3]. We note that our scaling analysis is based on entirely standard and well-known procedures and mathematical techniques, such as the method of characteristics for solving partial differential equations (see also Refs. [10,11]). We do *not* “use three basic units to measure the canonical dimension” [1] of the vertex functions, and we do not understand why the author of the preceding comment assumes us having done so. The factors ν_0 and D_0 in Eqs. (2.15)–(2.17) of Ref. [3] could have been absorbed into other quantities and parameters, of course, and our equations may be restated in a different way [Eq. (8) in Ref. [1]]. Yet, obviously, no real progress has been achieved here. The origin of the flow-dependent expressions $\nu(l)$ and $D(l)$ appearing in our Eqs. (3.54) and (3.55) rests in the solution of the renormalization-group equation via the method of characteristics. Our Eqs. (3.56)–(3.58) are in accord with the postulated scaling behavior of the Burgers–KPZ equation [cf. Eq. (1.4) in Ref. [3]], and they are valid at *any* fixed point $g^* < \infty$ of the renormalization-group flow (not just at the Gaussian fixed point, as Sun attempts to suggest [1]), and neither do they violate the statistical symmetry of Galilean invariance. Furthermore, our scaling analysis parallels the corresponding analysis for the

nonlinear σ model [13]. Of course, the Callen–Symanzik equation may be analyzed with different mathematical techniques as well. In addition, we would like to mention that Lässig confirms our Callen–Symanzik equations in his recent work [7].

We now come to the important conceptual differences in treating the situations $d < 2$, and $d \geq 2$, respectively, which, of course, imply very distinct physical situations. In order to treat both regimes on the same footing as long as possible, we demonstrated in Sec. III A of Ref. [3] how to one-loop order the Feynman diagrams may simply be evaluated at *fixed* dimension d , without any expansion in $\epsilon = d - 2$ whatsoever, within both Wilson’s k shell and the dimensional regularization scheme. Already at this level it becomes obvious that an ϵ expansion below $d_c = 2$, which serves as the *lower* critical dimension for the dynamic roughening phase transition [4,5], is rendered at least doubtful due to the divergence of the (strong-coupling) fixed point at $d = 3/2$. But, even in the absence of a small parameter, the renormalization-group method may still remain a valid concept. Moreover, an inclusion of the physically interesting situation $d = 1$ into the renormalization scheme is highly desirable for the following reasons. First, the one-dimensional case may serve as a check to the (extensive) algebra. Second, it allows for systematic tests on the results for the scaling functions calculated within the mode-coupling (self-consistent one-loop) approximation at $d = 1$ [14]. We shall return to this topic in a forthcoming publication [15].

As we have demonstrated and discussed at length in our paper (see Sec. III A of Ref. [3]), this goal may readily be achieved once one becomes aware of the fact that there are *two very distinct origins* for the appearance of dimension-dependent factors. Namely, there are, on one hand, purely geometric factors stemming from the angular dependences of the momentum space integrals, appearing quite irrespective of the employed renormalization procedure and, on the other hand, poles in $1/(d - 2)$ stemming from the specific treatment of the ultraviolet singularities within the dimensional regularization scheme. The latter would not emerge at all if, e.g., a cutoff regularization procedure were used instead. We believe we have made it quite clear that, at least conceptually, one should indeed strictly avoid mixing these two categories of d -dependent factors.

The conceptual importance of the distinction between the different d -dependent factors can be illustrated further by using Sun’s Eq. (6) [1]. If one is interested in the value of the integral I_{11} in the limit $d \rightarrow 2$ and cutoff $\Lambda \rightarrow \infty$, one has to distinguish between the cases where the limits are performed in the order (i) $d \rightarrow 2$ first and then $\Lambda \rightarrow \infty$ or (ii) in the reverse order. At $d = 2$, indeed, there is no singular contribution to that integral, simply because it is zero at *any finite* value of the ultraviolet cutoff, i.e., the corresponding term just does not appear in the perturbation theory at two dimensions. Only when the cutoff is pushed to infinity *first*, and the dimensional regularization method is employed does one obtain Sun’s Eq. (6). One way to proceed would be to argue that due to the apparent cancellation of factors $(2 - d)$ the first

term proportional to c_1 becomes finite, and hence should not be taken into account in a minimal-subtraction procedure. This line of argument would lead to a (singular) contribution $c_2/(2-d)$ to the Z factor. Since this term is of the form $1/\epsilon$ it could, in general, contribute to the corresponding (physically observable) flow function. This, however, would contradict the fact that the integral I_{11} is identically zero at $d = 2$ for *any finite* value of the ultraviolet cutoff, i.e., there should be no contribution of I_{11} whatsoever to any observable quantity at $d = 2$. Hence, one would be in danger of *falsely* retaining the pole c_2/ϵ , in spite of the fact that the entire integral simply vanishes in two dimensions. Now, our line of argument is quite different. Since the prefactor $(d-2)$ is of purely geometric origin, one has to keep both terms, namely c_1/ϵ and c_2/ϵ^2 . The fact that all flow functions have to be nonsingular at d_c implies that all terms originating in $1/\epsilon^2$ poles, like the one $\propto c_2$, must disappear in the expressions for the ζ and β functions [11]. All those terms hence must be (and, in fact, are) cancelled by corresponding ones stemming from the one-loop contributions, when the latter are re-expressed in terms of renormalized quantities. Thus the distinction between the different origin of the d -dependent factors allows us to prove that, whenever an expansion near two dimensions is possible, both limit procedures, (i) and (ii), do indeed yield identical results. This is actually a prerequisite for the applicability of the method of dimensional regularization.

In addition, by separating factors “ d ” and “ ϵ ,” as we suggest, one does avoid severe errors in treating the situation at fixed dimensions *away* from $d = 2$, e.g., by neglecting the pole $\propto c_1$ in Eq. (6) of Ref. [1] one would in effect miss a contribution to the corresponding Z factor and for example violate the fluctuation-dissipation theorem valid in one dimension. At $d = 1$ the angular integration would yield a finite result, and the remaining one-dimensional k integral diverges in the ultraviolet for $d \rightarrow 2$; but it is exactly this singularity that needs to be taken into account. Thus upon applying sufficient care and treating the $(2-d)$ terms of different origin separately, we do not “violate the basic principle of consistency” [1]. Rather, as we have noted in Ref. [3], we hereby cure a potentially dangerous problem of the dimensional regularization scheme, which has proven to be a most valuable tool indeed, but must not be employed in too naive a manner.

Our procedure hence permits us, in principle at least, to investigate dimensions further away from $d_c = 2$. Quite generally, however, by applying the dimensional regularization scheme to *massless* field theories at fixed d is plagued by the appearance of new *infrared* singularities at each additional order in perturbation theory (see Ref. [16]). Yet we may overcome this difficulty, and still preserve the benefits described above, by expanding the one-dimensional k -space integrals occurring after the angular integrations have been performed, with respect to $\epsilon = d-2$, and only retain the (ultraviolet) singular terms, but keeping the full d dependence stemming from the angular contributions [3]. This is possible because any infrared problems can emerge solely from the integrals over

$|k|$, of course. Our prescription, which we have (perhaps misleadingly) termed the “partial ϵ expansion,” may thus be summarized as follows: (i) retain the full information of the angular integrations (i.e., keep all factors d there without modifications), and (ii) treat the remaining one-dimensional integral over $|k|$ within the dimensional regularization, ϵ expansion, and minimal-subtraction scheme.

In order to proceed towards a *controlled* series expansion, we now have to deal with the situations below and above $d_c = 2$ separately [3]. For $d < 2$, we observe that the strong-coupling fixed point g_1^* , which at $d = 1$ describes the scaling behavior of the Burgers-KPZ equation correctly [4,5] (its values to one- and two-loop order remarkably being identical [3]), approaches zero for $d \rightarrow 0$. Therefore, for small d the “naive” perturbation theory simply applies, as g is small in that regime. The situation for $d < 2$ may then be viewed as an expansion in d , which is basically equivalent to a power series in $g_1^* \propto d$; one simply has to expand the loop results to correct order [15]. Clearly, this scheme breaks down at some finite dimension $d > 0$, an upper limit to which is set by the divergence of g_1^* : to one-loop order at $d = 3/2$ and to two-loop order at $d_c = 2$. According to Lässig’s recent results (see below) [7], there are good reasons to believe that this divergence will not be shifted beyond $d_c = 2$ in higher orders of perturbation theory. The emerging physical picture for $d < 2$ is therefore the following: there is a single finite and (infrared) stable strong-coupling fixed point governing the scaling behavior.

According to these findings, specifically the divergence of g_1^* at $d_c = 2$, any kind of $(2-\epsilon)$ expansion as in Ref. [2] seems extremely doubtful. Moreover, the appearance of a finite fixed point in Sun and Plischke’s two-loop calculation at $d_c = 2$, i.e., a fixed point of order $O(\epsilon^0)$, not existing to first order in perturbation theory, appears to be hardly reconcilable with the ϵ expansion concept, according to which *any* meaningful fixed point should be of order $O(\epsilon)$, at least. To us, it is thus not at all surprising that the ensuing “strong-coupling” critical exponents do not agree with the simulations [2].

Above $d_c = 2$, on the other hand, one may utilize the fact that there appears a critical fixed point $g_c^* \propto \epsilon = d-2$. Performing a “full” $(2+\epsilon)$ expansion for $d \geq 2$ hence provides a controlled perturbation series, but *only* for this unstable fixed point, which physically describes a dynamic phase transition between the regime described by the Edwards-Wilkinson scaling exponents, [12] and the strong-coupling region, which is *not accessible* by these perturbational means. In addition, the “Gaussian” fixed point may of course be treated, as well as the weak-coupling crossover regime [3].

The application of the renormalization-group method to infer the correct scaling behavior from the ultraviolet singularities is necessarily based on the renormalizability of the field theory describing the Burgers-KPZ equation, which is questioned in Sun’s comment for the case $d \geq 1$. Yet again, we have already fully explained in the final paragraph of Sec. III A of Ref. [3] that, very similar to what happens for the nonlinear σ model (compare Ref. [13]), although the theory is apparently nonrenormalizable from a naive power-counting point of view, the

appearance of an ultraviolet-stable fixed point ensures its renormalizability above $d_c = 2$, which is thus not “an open issue” [1]. We find it somewhat surprising that Sun cites the work of Doty and Kosterlitz [6] in order to strengthen his standpoint, as their scaling arguments are, in fact, fully in accord with our findings [3].

On the basis of the directed-polymer representation and by utilizing a replica renormalization technique, Lässig [7] has recently investigated the structure of the perturbation theory for $d \geq 2$ carefully. He manages to prove that hyperscaling holds at the roughening transition, and, as a consequence, that the critical exponents at the unstable fixed point are $z_c = 2$ and $\chi_c = 0$ to any order of perturbation theory. Furthermore, he demonstrates that at $d_c = 2$ there appears a singularity in the flow of the renormalized coupling as obtained in the minimal-subtraction scheme. Therefore, the parameter space becomes divided into an infrared (strong-coupling) and ultraviolet regime, with only the latter being accessible to perturbational approaches [7]. Thus our conclusions, both for the critical values of the dynamic and roughness exponent, as well as regarding the non-existence of a perturbational strong-coupling fixed point, are confirmed on a much more general basis.

Finally, we would like to point out that the $(2 + \epsilon)$ expansion cannot be extended beyond $d = 4$. This breakdown becomes obvious from the divergence of the geometric factor

$$C_d = \Gamma(2 - d/2) / 2^{d-1} \pi^{d/2} \quad (3)$$

for $d \rightarrow 4$, which appears as an overall prefactor to each of the integrals in perturbation theory (cf. Appendix A2 in Ref. [3]). In the same manner, the regularization of the perturbation series breaks down at $d = 4$ in the directed-polymer representation [7]. One may be tempted to identify this marginal dimension with a supposed *upper* critical dimension for the roughening transition, beyond which the critical exponents assume their mean-field values, and $z = 2$, $\chi = 0$ also hold in the strong-coupling

regime [7]. A more physical argument [17] rests on our result for the (transverse) correlation length exponent [3]

$$1/\nu_{\perp} = \epsilon + O(\epsilon^3) \quad (4)$$

which we conjecture to be valid to any order in ϵ as well. This result must, however, be wrong when the mean-field value $\nu_{\perp} = 1/2$ (or, equivalently, $\nu_{\parallel} = z_c \nu_{\perp} = 1$) is reached, because this, of course, constitutes a lower bound. Thus again $d = 4$ is found as a borderline dimension. Previously, Halpin-Healy has argued in favor of an upper critical dimension $d = 4$ within his functional renormalization-group technique [18]. Also, in a recent mode-coupling analysis [19], very peculiar and interesting behavior is found above $d = 4$, namely the occurrence of a “glassy” solution with a finite Edwards-Anderson order parameter. In our opinion, a thorough study of the Burgers-KPZ problem in the vicinity of four dimensions, preferably by some kind of controlled expansion near $d = 4$, would be highly desirable.

Summarizing, we feel that the differences in the results of Refs. [2] and [3] can basically be attributed to an incorrect finite field renormalization in Sun and Plischke’s work [2]. With regard to our conclusions at physical dimensions, the scaling exponents in $d = 1$ had already been known exactly, while our findings for $d \geq 2$ have recently been confirmed quite independently and on a much broader basis [7]. We do hope that this exchange of arguments may finally help to clarify the situation, such that further attention may focus on the remaining open and indeed really intriguing issues.

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