

Reply

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1. Introduction

We welcome the opportunity to offer further explanations of our model published previously in this journal (Tung and Orlando 2003a, hereafter TO). We thank Dr. Smith (Smith 2004, hereafter S04) for providing numerical illustration of some subtle points concerning the effect of small-scale dissipation on the shape of the energy spectrum in two-dimensional turbulence. His analytic derivation of the dissipation scales in the presence of double cascades of energy and enstrophy is, however, problematic. In any case his analytic prediction of the transition scale in two-dimensional turbulence does not apply to the case of the two-level model considered by TO. Nevertheless, some issues raised are common to both 2D and quasigeostrophic (QG) turbulence and these will be discussed first here.

As pointed out recently by Tran and Bowman (2003), and also by Tran and Shepherd (2002), the classical configuration of a dual cascade of Kraichnan (1967), Leith (1968), and Batchelor (1969), with a $-5/3$ spectral slope in the energy spectrum upscale of injection and -3 slope downscale of injection, is an idealization that is probably not achievable in any finite-domain numerical simulation or in nature. The Kraichnan–Leith–Batchelor (KLB) theory envisages an infinite domain in which a pure inverse energy cascade exists on the long-wave side of the scale of injection, transferring all of the injected energy to larger and larger scales without being damped. On the short-wave side of injection, there exists a pure direct cascade of enstrophy, transferring all of the injected energy downscale, dissipated by molecular viscosity even in the limit of vanishingly small viscosity coefficient. In finite domains of numerical simulations discussed here and in the atmosphere, Ekman damping provides the physical infrared sink of energy.

This sink is “imperfect” in the sense that not all of the injected energy can be absorbed at the large-scale end of the spectrum, and part of the injected energy is diverted downscale. In the presence of finite viscosity there is a downscale flux of energy on the short-wave side of injection, whose presence was demonstrated in the numerical simulation of TO and in atmospheric observations. Gkioulekas and Tung (2004) show that, *in general*, the dual cascade should be a dual *mixed* cascade. In a finite domain, there should be a double downscale cascade of both energy and enstrophy on the short-wave side of injection. In the limit of large separation of dissipation scales and the scale of injection, the downscale enstrophy flux is the leading cascade and the downscale energy flux is the subleading cascade. In this limit the contribution of the subleading cascade will be hidden, consistent with the prediction of S04. In the atmosphere, there is less than one decade of separation between the energy injection scale and the largest scale permitted by the domain geometry. Furthermore the Ekman damping is not concentrated on the largest scales only. This physical situation leads to a configuration that is very different from the ideal dual pure cascades in the KLB theory. Nevertheless, diagnostics of atmospheric data (see Boer and Shepherd 1983; Straus and Ditlevsen 1999) show that most of the injected enstrophy still goes downscale, and most of the injected energy goes upscale. There are, in these observational analyses, also a small fraction of enstrophy that goes upscale and a small fraction of energy that is diverted downscale. The numerical simulation of TO reproduces this observed energy and vorticity budget quite well.¹ On the short-wave side of injection, the direct enstrophy cascade is the leading cascade, and the direct energy

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¹ For some reason, in the numerical model of S04, although a large fraction of injected energy goes upscale, only about half of the injected enstrophy goes downscale. In particular, of the total 16 units of enstrophy injected, 8–9 units go downscale and 3 units go upscale (see S04’s Fig. 1a). At least four units of injected enstrophy are unaccounted for. They are probably dissipated at the forcing scale.

cascade is the subleading cascade. The presence of the subleading energy cascade can be detected from the energy flux, which would be positive. This is seen in both TO and in S04. Whether the subleading cascade will manifest itself in the energy spectrum in the inertial range depends on the treatment of the small-scale dissipation, which is the subject of the exchange here.

The numerical calculations of S04 actually provide support to some major points of our theory. We are glad to see that even in his two-dimensional model, which is quite different from that of TO, the energy flux is downscale on the short-wave side of injection, as we predicted for either quasigeostrophic (QG) or 2D turbulence in the presence of a small-scale sink of energy. This is in contrast to the prediction of the traditional KLB theory. When a downscale enstrophy flux η and a downscale² energy flux ε are simultaneously present in the same inertial range on the short-wave side of injection, TO found the energy spectrum $E(k)$ to possess a -3 spectral slope transitioning to a $-5/3$ slope, with the transition wavenumber given approximately³ by

$$k_t \sim (\eta/\varepsilon)^{1/2}. \quad (1)$$

There are at least two ways for a numerical model to determine ε : by controlling the total energy dissipation rate, as TO has done, or to hold the hyperviscosity coefficient ν constant, and let the model determine the total dissipation rate. In the second approach, ε , and hence k_t , then vary as resolution changes. S04 gives a more detailed discussion of this second approach and its sensitivity to resolution. This topic was not explicitly discussed in TO, and we welcome the comment by S04. The discussion in S04 is useful in reconciling the different numerical results obtained previously, mainly along the second approach. An important point we wish to emphasize to readers is that the second approach was *not* the one adopted by TO.

Tung and Orlando (2003a) were trying to model the situation in the upper troposphere where there is a finite sink of energy at the small-scale end of the spectrum, and there are some recent measurements of the magnitude of such a sink. In the abstract and in the text, TO made it clear that the model proposed has two important ingredients: 1) a means for energy and enstrophy injection at the intermediate (synoptic) scales, which is accomplished with the use of a two-level QG model; and 2) “a small sink of energy at the small scales due to subgrid hyperdiffusion; this attempts to model the small-scale sink not resolved by the two-level QG model.” Unlike the surface-QG model, or a more general QG model with evolving temperature on the top and bottom boundaries, a two-level model does not by itself

provide the sink in ingredient 2 as the viscous coefficient becomes vanishingly small. This absence of “anomalous energy dissipation” is a property it shares with 2D turbulence in the KLB limit (see discussions in Tung and Orlando 2003b), and has to be remedied with a finite subgrid parameterization. As is the case with most parameterization of subgrid processes in atmospheric models, the hyperdiffusion used in TO is *resolution dependent*; its coefficient needs to be adjusted as resolution is increased so as to maintain a finite sink of energy, as discussed in TO. S04 is correct in pointing this out, although this property has already been emphasized in TO. It is obvious that, after we introduced such a sink into the model, it would not make much sense to then choose the model parameters and resolution so that the model approaches the KLB limit of vanishing small-scale dissipation rate.

The numerical experiments shown in S04 are all of the type where *the magnitude of the sink of energy (the total energy dissipation rate) is not controlled* to model this observed atmospheric feature. Consequently its magnitudes vary wildly from case to case. In some of S04’s cases, the energy sink is too weak relative to its enstrophy sink, when compared to that observed in the atmosphere, for the transition wavenumber to occur in the resolved range in his numerical model.⁴ The magnitude of the sink in the model determines the magnitude of the energy flux—in fact the ensemble average of one is equal to that of the other at statistical equilibrium—as the energy transferred downscale by nonlinear wave-wave interaction is dissipated by the sink. [There are exceptions to this, of course, when dissipation occurs at the forcing scale. See Tran and Shepherd (2002).] Using model data provided to us by K. S. Smith (2003, personal communication), the downscale energy flux in his model case C is $\varepsilon \sim 0.7 \times 10^{-4}$ in his nondimensional unit, and is equal to the total energy dissipation rate of 0.7×10^{-4} for this case, as expected. The enstrophy flux for case C is $\eta \sim 9$. (It does not change much for all three runs, also to be expected.) These numbers imply a transition wavenumber of

$$k_t \sim (\eta/\varepsilon)^{1/2} \sim (9/0.7)^{1/2} 10^2 \sim 400,$$

which is beyond the inertial range in his model. It is instead in the dissipation range, where $E(k)$ does not

⁴ In the classical KLB theory, as the energy dissipation rate is taken asymptotically to zero, ε approaches zero, and k_t approaches infinity. Only the -3 slope is then seen in the energy spectrum, as the transition to the $-5/3$ slope now disappears. Of course, in numerical models, such as that in S04, one cannot have infinite wavenumbers. The next best thing is then to have k_t occurring beyond the inertial range, in the dissipation range. This is a requirement that a finite numerical model must obey in order for it to simulate the infinite-domain, infinite-resolution, nearly inviscid result of the classical KLB theory. Only then would the simulated spectrum be independent of the energy dissipation rate in the numerical model and achieve the k^{-3} shape predicted by the KLB theory for vanishing energy dissipation rate, and remain so as resolution is further increased. But this is *not* TO’s model for the atmosphere.

² The notation may be confusing: ε is actually the portion of the total energy injection rate that is diverted downscale. It is the same as ε_{net} in other publications.

³ Equation (1) is an empirical result, inferred from the numerical output of TO (their Figs. 4 and 7).

possess a scaling law. That is why the transition scale disappears in case C despite the fact that there is both a positive η and a positive ε . Although case C has the same dissipation coefficient ν as case B, the *energy dissipation rate* is different because the integral of the energy spectrum $E(k)$ in the dissipation range, which yields the rate of energy dissipation, is different. It is thus not surprising that those features that are sensitive to the dissipation rate, such as k_t , are different in the two cases.

Although S04 does not have a still higher resolution run to show that the results of case C will not be further changed with higher resolution, it is reasonable to accept his assumption that this is true. The energy spectrum has already dropped to zero exponentially at the small scales at the resolution of case C that resolving even smaller scales will not make much of a difference to the energy spectrum at larger scales. This is probably the physical interpretation of S04's "hyperviscous Kolmogorov (HVK) scale": When the resolution exceeds that implied by this scale, the energy dissipation rate will be independent of resolution even as the hyperviscosity coefficient is held fixed. In that limit, it often turns out that the resulting dissipation rate is too small as compared to the atmospheric value, although there may conceivably be a way to find a remedy to this problem. That small limit, however, is useful if one's goal is to simulate the KLB dual cascade as closely as possible in a finite domain (see footnote 4).

2. Remarks on the analytic formulas

The main result, derived in Eq. (7) of S04, is a statement that the transition from the -3 to the $-5/3$ range should occur where the dissipation range starts if the Kolmogorov scale is "resolved." There are inconsistencies in the derivation leading to that formula, and the derived formula is not useful as a prediction of the transition scale, except in the KLB limit.

Section 2 of S04 gives a derivation of an approximate analytic formula for a Kolmogorov-type "dissipation scale," modified for hyperviscosity (called HVK scale). For energy dissipation, $k_{\nu\varepsilon}$ is defined to be the wavenumber where the *downscale* energy flux is $(1 - \gamma) \sim 90\%$ of its value ε in the inertial subrange, based on the following integral [see Eq. (1) in S04]:

$$\gamma\varepsilon = \int_{k_f}^{k_{\nu\varepsilon}} 2\nu k^s E(k) dk. \quad (2)$$

Similarly, an enstrophy HVK wavenumber $k_{\nu\eta}$ is defined from

$$\gamma\eta = \int_{k_f}^{k_{\nu\eta}} 2\nu k^{s+2} E(k) dk. \quad (3)$$

To evaluate the above integrals, a form for the energy

spectrum $E(k)$ needs to be assumed. S04 appears to have used

$$E(k) \sim C_1 \varepsilon^{2/3} k^{-5/3}, \quad C_1 = 6 \quad (4)$$

in the first integral, but

$$E(k) \sim C_2 \eta^{2/3} k^{-3}, \quad C_2 = 1.3 \quad (5)$$

in the second integral, and assumed the two C s to be the same. This procedure is inconsistent since in the definition of the dissipation scales, (2) and (3), $E(k)$ should be the same. Although S04 is following the procedure introduced by Frisch (1995), this problem does not arise in the case of 3D turbulence considered by Frisch. In 3D turbulence there is only a downscale energy flux, and (4) would be the appropriate energy spectrum to use in (2). In this way S04 derives an energy dissipation wavenumber $k_{\nu\varepsilon}$, his Eq. (2), and an enstrophy dissipation number $k_{\nu\eta}$, his Eq. (3):

$$k_{\nu\varepsilon} = a(\varepsilon/\nu^3)^{1/(3s-2)},$$

where

$$a = [\gamma(3s - 2)/(6C_1)]^{3/(3s-2)}, \quad (6)$$

$$k_{\nu\eta} = b(\eta/\nu^3)^{1/(3s)},$$

where

$$b = [s\gamma/(2C_2)]^{1/s}. \quad (7)$$

If one tries to be more consistent and uses the same $E(k)$ in the integrals, the energy dissipation scale would become dependent on the enstrophy flux, as well as on the energy flux. There does not appear to be a way to disentangle the two scales if one uses S04's integral definition for the dissipation scales (2) and (3). In the following, we will proceed with the rest of S04's argument using (6) and (7). Combining (6) and (7), we get

$$\eta/\varepsilon = \sigma k_{\nu\varepsilon}^2, \quad (8)$$

where

$$\sigma = [(s - 2/3)/s]^3 (C_2/C_1)^3 (k_{\nu\eta}/k_{\nu\varepsilon})^{3s}. \quad (9)$$

S04 approximates the proportionality constant σ by unity to arrive at his conclusion that the transition wavenumber of Eq. (1) is the same as the Kolmogorov dissipation wavenumber:

$$k_t^2 = \eta/\varepsilon = k_{\nu\varepsilon}^2. \quad (10)$$

We see from (9) that this parameter depends on the order of hyperdiffusion sensitively for low s , and for high s , it depends critically on how close to unity the ratio of the two dissipation numbers is. Furthermore the ratio of the two universal constants in σ is $(C_2/C_1)^3 = 0.010$, which S04 set to one because it was raised to a very small power in a and b . Note, however, that in arriving at (8), a and b are themselves raised to a very high power.

For molecular viscosity $s = 2$, and $\sigma \approx 0.0030$. For $s = 18$,

$$\sigma = 0.0089(k_{v\eta}/k_{ve})^{54}k_{ve}^2, \quad \text{for } s = 18.$$

S04 *hypothesizes* that the two dissipation scales are exactly the same, claiming that it is “qualitatively obvious.” While qualitatively it may be true, it is the exact quantitative values that are important, because the ratio is raised to a very high power, 54. The reason S04 gives for assuming that the two dissipative scales are the same is based on a local argument: the wavenumber for which $E(k)$ or $k^2E(k)$ begins to feel the effect of viscosity should be the same. While this is true, the dissipation wavenumbers are instead defined by S04 by the integrals (2) and (3). There is no indication that these two scales, namely (6) and (7), defined this way, should be identical. As we mentioned in the introduction, the ε in Eq. (6) is the amount of energy flux diverted downscale from the imperfect infrared sink, and consequently it depends critically on the nature of the infrared energy sink and the separation of that sink from the injection scale. On the other hand, the downscale enstrophy flux η , being the dominant (leading) cascade, is much less sensitive to the conditions on the infrared end (Danilov 2004).⁵ Since the enstrophy dissipation wavenumber is independent of ε , while that for energy is dependent on ε , there is no a priori reason to expect that the two dissipation wavenumbers should be the same regardless of the condition in the infrared end. Let us for a moment suppose that they happen to be approximately the same but not identical. If $(k_{v\eta}/k_{ve}) = 0.99$, then $(k_{v\eta}/k_{ve})^{54} = 0.58$, and (8) becomes

$$\eta/\varepsilon = 0.0052k_{ve}^2. \quad (11)$$

Using the above formula instead of (10) would have led to a completely different conclusion concerning the nature of the transition wavenumber in (1).

Gkioulekas and Tung (2004) provided a different theoretical framework, which allowed them to bypass the difficulties of the S04 derivation mentioned above. They managed to show that S04’s main result, that the transition scale coincides with the dissipation scale, *is correct in the KLB limit of 2D turbulence*.

3. Resolving the Kolmogorov scale

What is the meaning of “resolving” the Kolmogorov scale? It does *not* mean that the dissipation scale is not resolved if the model’s truncation scale is larger than this Kolmogorov scale. S04 acknowledges this point, and so there is no disagreement here. Whether or not the energy dissipation scale in the model is actually resolved can be revealed more accurately with a model diagnostic of its energetics. For the results shown in TO, several diagnostics are used to ensure that the small-

scale dissipation sink is resolved by the resolution adopted. Figure 5 of TO shows the rate of energy dissipation by the hyperviscosity for each wavenumber in the 129-km-resolution run. It is clear from this figure that, for wavenumbers higher than 180, the subgrid diffusion is the dominant term—with a very rapid dissipation rate of $\sim 1/(0.6 \text{ days})$ —and it balances the energy transferred downscale by the wave–wave interaction. Therefore the energy dissipation was adequately resolved in TO to their satisfaction. (The reduced black and white figure in the printed version of the journal is admittedly difficult to read. The curve we are referring to is the light dotted one that veers downward from the horizontal axis near wavenumber 180.)

S04 asserts, without giving any justification, that “if this HVK scale is not resolved by the model, that is, if the hyperviscous coefficient is too small for the given enstrophy flux, then enstrophy will build up at the large-wavenumber end of the spectrum. While any finite hyperviscosity will ultimately dissipate the enstrophy, the large-wavenumber spectrum will be altered by the constipation of enstrophy if the HVK scale is not resolved.” It is not clear what reference state S04 is referring to: “constipated” as compared to what? “The spectrum altered” from what? In view of what is said in the introduction and in footnote 4, it is reasonable to suggest that the reference state S04 has in mind, though never acknowledged in his comment, is the KLB limit.

A further confusion arises when one examines more carefully the truncation scale adopted by TO. One finds, surprisingly, that the Kolmogorov dissipation scale *is* resolved in that model, though barely so. In his estimates, S04 uses order-of-magnitude figures in a formula with various inaccuracies [e.g., setting $a = 1$ and $b = 1$ in (6) and (7)], while comparisons are made with the truncation wavenumbers at the second significant figure. The case picked by S04 happens to have the least meridional resolution (with $1/8$ the number as in the zonal direction); all other cases shown in TO’s Figs. 3 and 4 have higher meridional resolution (with $1/4$ the number of wavenumbers in the zonal direction). Yet they all exhibit the same behavior of spectral shape, showing insensitivity to meridional resolution as long as the number of wavenumbers used in the meridional direction equals or exceeds $1/8$ of that in the zonal direction ($1/7.7$ is the ratio of the meridional width to the zonal length of the channel in TO). For this case, S04 concludes that the zonal direction is resolved while the meridional direction is not. In view of the fact that the adopted resolution, in terms of grid distance, is about the same in either direction, it is more reasonable to conclude that either both are not resolved or both are resolved. Our recalculation shows that the latter is the case. Then why does the transition scale for this case in TO not obey Eq. (10) predicted by S04? Why is the spectrum found not the same as in the KLB limit? These confusing paradoxes are probably a result of the inac-

⁵ This may not be true in the model of S04. See footnote 1.

curate prediction of Eq. (10)—it could just as well be Eq. (11).

An interesting question arises. If the case of TO picked by S04 to examine satisfies S04's criterion, though barely, of having the Kolmogorov dissipation scale resolved, does it mean that its transition scale will become independent of resolution for higher resolution? There are other higher-resolution runs presented in TO. The case highlighted in their Fig. 3 has more than double the meridional resolution and about 30% higher zonal resolution. The transition scale found is moved only very slightly to higher wavenumber. However, in that case, the shift was attributed by TO to the parameterization scheme, which lowers the hyperviscosity coefficient slightly as resolution increases. It appears that if that coefficient were held fixed as resolution was increased, the transition scale would not have changed. Although this fact cannot be ascertained without further numerical experiment, it does suggest that a useful application of the concept of the Kolmogorov dissipation scale is in predicting resolution independence for fixed hyperviscosity coefficient. The secondary prediction of the location of the transition scale with respect to the dissipation scale is not useful in its present form.

4. Remarks on modeling philosophy

In classical fluid mechanics, the viscous term is due to the physical process of molecular diffusion, and the governing partial differential equation, the Navier–Stokes equation, describes the physical situation down to all macroscales. Since the viscous coefficient is a physical quantity in the Navier–Stokes equation, it makes physical sense to hold it constant as one increases resolution. Two-dimensional turbulence simulation with the second-order viscous diffusion has been problematic, however, due to the demanding requirement on numerical resolution. A recent successful simulation by Lindborg and Avelius (2000) of the KLB limit uses high-order hyperdiffusion, as S04 is doing in his comment. Hyperdiffusion does not have the same physical basis as molecular diffusion. Whether or not this numerical device is “physical” depends on what physical process we want it to model. Holding its (artificial) coefficient constant as resolution increases is not necessarily the physical thing to do.

Let us reiterate what TO are trying to model physically.

1) There is evidence that the energy flux over the mesoscales is *downscale*. Cho et al. (2003) give a magnitude of the order of $\varepsilon \sim +10^{-6} \text{ m}^2 \text{ s}^{-3}$ for turbulence in the free troposphere. At statistical equilibrium, this downscale energy flux must be dissipated by a small-scale sink of energy, with the rate of dissipation $\varepsilon_D = \varepsilon \sim 10^{-6} \text{ m}^2 \text{ s}^{-3}$. This rate is very small (compared to the large-scale sink), but not negligible.

2) The small-scale dissipation may occur at scales not correctly described by the QG theory, so the common mathematical approach of considering dissipation at the infinite-wavenumber limit is irrelevant for our model. Tung and Orlando (2003a) list a number of mechanisms, such as frontal genesis and gravity wave radiation, which are known to occur in the atmosphere as sinks for QG energy transferred to the small scales by nonlinear wave–wave interactions, and which are known to be not describable by the QG equations.

The existence of a nonzero sink of energy at the small scales is modeled in TO by a hyperdiffusion of order s ; the order is chosen so as to leave as much of the resolved scale uncontaminated by this viscosity as feasible. The rate of the dissipation rate can be calculated diagnostically—that is, after the fact—as

$$\varepsilon_D = 2\nu \iint |\mathbf{k}|^{s+2} |\psi(\mathbf{k})|^2 d\mathbf{k} = 2\nu \iint |\mathbf{k}|^s E(\mathbf{k}) d\mathbf{k}.$$

The coefficient ν is varied and the dissipation rate calculated. The dissipation rate is generally smaller the smaller the coefficient ν , but larger ν sometimes would actually yield a smaller ε_D by having the effect of driving $E(\mathbf{k})$ down precipitously in the dissipation range. Controlling ε_D is not an easy task because $E(\mathbf{k})$ cannot be specified a priori. Tung and Orlando (2003a) developed an algorithm for controlling the dissipation rate and obtained a range of such values. The algorithm, which is explained in TO, involves adopting a ν that *adjusts with resolution*, such that the last resolved scale has a dissipation rate due to hyperdiffusion that is 10 times that of the Ekman damping rate. This algorithm is not perfect in its ability to hold the total dissipation rate constant as resolution is changed, but it is able to hold it to the same order of magnitude, which is all that is needed because the observed rate is known in order of magnitude only. The range for the runs shown in Fig. 3 in TO is, in descending order of resolution,

$$\varepsilon_D \sim 0.5\text{--}1.1 \times 10^{-6} \text{ m}^2 \text{ s}^{-3},$$

which is very close to the observed range of dissipation rates in the free troposphere. This then determines the downscale energy flux

$$\varepsilon \sim 0.5\text{--}1.1 \times 10^{-6} \text{ m}^2 \text{ s}^{-3}.$$

There perhaps may exist a better numerical filter, which removes at the truncation scale a fixed amount of energy per second, say $0.5 \times 10^{-6} \text{ m}^2 \text{ s}^{-3}$, independent of the particular truncation scale at which it is applied. Then the simulated energy spectrum would be independent of resolution at scales larger than the truncation scale. We have not found such an ideal filter, but the device with hyperdiffusion comes quite close in achieving our modeling goal.

5. Conclusions

We thank Smith (2004) for his comments, which help contrast two different approaches taken in treating small-scale dissipation of energy in numerical models. As pointed out by him, the approach taken by TO of controlling the rate of energy dissipation by “subgrid” diffusion is unconventional in 2D turbulence studies, but is motivated by an observed atmospheric feature. In traditional 2D turbulence calculations, the rate of small-scale dissipation is not controlled but is allowed to vary as resolution is changed. This gives the appearance of sensitivity of the model results to numerical resolution. There does not appear to be a right or wrong approach; which approach one should adopt depends on one’s modeling goal. To reproduce the result of TO in S04’s calculation, after one finds a correct small-scale dissipation rate at one resolution, one must adjust the magnitude of the coefficient of hyperviscosity to maintain that dissipation rate as resolution is increased. As pointed out by S04, the model proposed by TO is a finite-resolution, finite-dissipation model of atmospheric turbulence. Extending the QG model to arbitrarily small scales is probably nothing more than a mathematical exercise.

We suggest that hyperviscous Kolmogorov scale defined by S04 is probably useful in predicting resolution independence for fixed hyperviscosity coefficient. Although holding the coefficient fixed is not the approach taken by TO, it may be useful to those who do numerical simulation of 2D turbulence using the classical approach. The secondary prediction of S04 that the transition scale should be located at the dissipation scale when the Kolmogorov scale is resolved is found to be highly inaccurate—the range of uncertainty is so large that that it does not preclude the case of the transition scale located in the middle of the inertial range—and consequently the formula is not useful.

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