Report on the thesis entitled *Kolmogorov's model of turbulence - mathematical analysis* by Przemyslaw Kosewski, M.Sc.

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September 28, 2023

In this thesis, Mr. Kosewski deals with the so called Kolmogorov model of turbulence. Namely,

$$\begin{cases} \partial_t v + (v \cdot \nabla)v + \nabla\pi - \nu \operatorname{div}\left(\dfrac{b}{\omega}\mathbb{D}v\right) = f \\ \partial_t \omega + v \cdot \nabla\omega - \alpha_1 \operatorname{div}\left(\dfrac{b}{\omega}\nabla\omega\right) = -\alpha_2\,\omega^2 \\ \partial_t b + v \cdot \nabla b - \alpha_3 \operatorname{div}\left(\dfrac{b}{\omega}\nabla b\right) = -b\,\omega + \alpha_4\,\dfrac{b}{\omega}\left|\mathbb{D}v\right|^2 \\ \operatorname{div} v = 0\,. \end{cases} \tag{1}$$

In the previous system, the three unknowns v, ω and b are functions of time and space variables $(t,x) \in \mathbb{R}_+ \times \Omega$, where Ω is a domain in $\mathbb{R}^d$, with $d = 2, 3$. The vector field $v \in \mathbb{R}^d$ represents the time-averaged velocity in the original paper by Kolmogorov. The velocity field u is assumed to be incompressible, whence the last equation in (1) and the presence of the lagrangian multiplier $\nabla\pi$ in the first relation of the system, where π is the mean pressure of the fluid. On the other hand, ω and b are positive scalar functions, representing respectively the mean frequency of the turbulent fluctuations and the average turbulent kinetic energy. The vector field $f \in \mathbb{R}^d$ is a known external force acting on the fluid. Finally, all the parameters $\nu, \alpha_1 \ldots \alpha_4$ are physical adimensional parameters which are strictly positive numbers. In general, their values depend on the characteristics of the flow; however, for some of them Kolmogorov is able to specify the precise numerical value.

In the thesis, the author focus on the local and global existence of regular solutions to Kolmogorov's two-equation model of turbulence. On the one hand, the author establishes the local well-posedness first in H^2 and then in $H^{d/2+}$. On the other hand, the author proved the global existence of a regular solution under certain size conditions on the initial data and the global existence of weak solutions. All these results are established in the case where the spatial domain is the torus.

These results are new and challenging. As such, they have appeared in several papers that have been already published in research journals in mathematics. Furthermore, the thesis is overall well-written and the exposition is quite clear in general.

In my opinion (although I'm not really familiar with the polish PhD system) the work, knowledge and contributions here presented are more than enough to defend a PhD thesis. However, I have a number of questions and comments that the author may need to address. In the rest of this report I'm going to list and ellaborate on the important ones, keeping the minor and obvious typos outside of this list.

Comments on "Introduction"

1. (page 12) Several other models of turbulence are mentioned. In particular, the $\kappa - \varepsilon$ and $\kappa - \omega$. In my opinion, it would be very interesting to say something on their similarities and differences when compared with the Kolmogorov model of turbulence.

2. (page 12) In the same line as my previous point. If b denotes somehow the *amount of turbulence*, then the increase of dissipation may mimic what is called the *zero-th* law of tubulence (that basically states that turbulence flows disspate energy faster). The author may want to comment on this.

3. (page 13) The time averaged formula and the averaging operator should probably be explicitly written.

4. (page 13) It would be interesting to see the derivation of the model directly from the Navier-Stokes equations.

Comments on "Chapter 1"

1. (page 19) ε may be kind of a weird notation for a function space.

2. (page 20) The right hand side of inequality (1.6) can be replaced by the term

$$3\|f\|_{L^\infty}\|\nabla^2 f\|_{L^2}.$$

   Does it change the computations? Can this improve somehow the results?

3. (page 20) The inequalities (1.7)-(1.13) depend on the dimension. I do not see it clearly stated. The author may want to comment on this.

4. (page 21) F is bad notation, in my opinion, for the Fourier transform. Compare with Lemma 1.2.3.

5. (page 22) Toroidal Fourier transform is, again in my opinion, a weird name for Fourier series.

6. (page 25) The author may want to improve the formatting to avoid large white spaces.

Comments on "Chapter 2"

1. (page 32) The author uses *a.a.* instead of the *a.e.*. I guess its a matter of the language used. This happens several times along the text.

2. (page 34) One should compare (2.19)-(2.21) with (4.19)-(4.21). In chapter 4 much more details are added. However, in my opinion is better to give those details in chapter 2. Alternatively, the details in chapter 2 can be expanded to look alike those in chapter 4.

3. (page 34) The author may want to comment on μ and its meaning.

4. (page 35) The author may want to expand the details of the Cauchy-Lipschitz theorem. In fact, in chapter 4 there is a more detailed explanation of the same argument.

5. (page 50) The author uses a classical argument based on L^2 estimates of positive and negative parts of the function to achieve the desired bounds. In my opinion, one can also use pointwise arguments directly in the same way as in

(*) Alonso-Orán, D., & Granero-Belinchón, R. (2022). Global existence and decay of the inhomogeneous Muskat problem with Lipschitz initial data. *Nonlinearity*, 35(9), 4749.

The final estimated would be the same, however, in my opinion the pointwise estimate is easier and more natural than the integral estimate.

Comments on "Chapter 3"

1. (pag 57) The author states several times that the value of κ_2 is important in the estimates. However, I think that the impartant value is not κ_2 but the value of κ_2 *when compared to* κ_1.

2. (pag 57) The author established the global well-posedness in a Sobolev space. In my opinion, it is interesting to know whether such a result hols also in other functional spaces. In particular, in functional spaces that share the same scaling as the system of PDEs under study

$$v_\lambda(x,t) = \lambda v(\lambda x, \lambda^2 t),$$
$$\omega_\lambda(x,t) = \lambda^2 u(\lambda x, \lambda^2 t),$$
$$b_\lambda(x,t) = \lambda^2 u(\lambda x, \lambda^2 t).$$

3. (pag 57) In the same line of my previous comment, the author may want to address the global well-posedness in the so called Wiener spaces

$$A^s = \{f \quad s.t. \quad \widehat{\nabla^s f} \in L^1\}.$$

   See also

   (*) Granero-Belinchón, R., & Scrobogna, S. (2020). On an asymptotic model for free boundary Darcy flow in porous media. *SIAM Journal on Mathematical Analysis*, 52(5), 4937-4970.

4. (pag 58) The author states a remark on the threshold value for κ_2. In my opinion such a remark should also mention κ_1. In particular, the author may want to address the question of why $\kappa_2 = 1/2$ is critical from a physical viewpoint.

5. (pag 71) The author may want to perform certain numerical simulations to increase the intuition on the behaviour of the solution for the different ranges of κ_2.

Comments on "Chapter 4"

1. (page 83) I think that the system has a invariant scaling:

$$v_\lambda(x,t) = \lambda v(\lambda x, \lambda^2 t),$$
$$\omega_\lambda(x,t) = \lambda^2 u(\lambda x, \lambda^2 t),$$
$$b_\lambda(x,t) = \lambda^2 u(\lambda x, \lambda^2 t).$$

   Such a scaling can be used to define several critical spaces in the sense that the scaling of the homogeneous spaces matches the invariant scaling of the system. An interesting question that the author may want to address is the local well-posedness in one these several critical spaces. I think that this question is very challenging though.

2. Similarly, the author may want to compare his techniques and result with the one in

   (*) Cuvillier, O., Fanelli, F., & Salguero, E. (2023). Well-posedness of the Kolmogorov two-equation model of turbulence in optimal Sobolev spaces. arXiv preprint arXiv:2306.16014.

3. (page 87) The author mentioned complex valued functions. That seems kind of weird given the origin of the system of PDEs under study.

4. (page 88) The author skips many details on the elliptic estimates. I think that it would be interesting to have such details explicitly stated in the thesis.

5. (page 105) I think that the author can establish the same desired bounds by pointwise methods. In my opinion, such approach is more elegant.

Comments on "Chapter 5"

1. (page 111) I think that the same holds if $b_0 \geq 0$ with approriate minor changes. The author may want to generalize in this directions.

2. (page 115) $\mathcal{L}$ can be defined using Fourier multipliers. In my opinion this approach is shorter and easier.

3. (page 122) The author may want to explain the different role of the succesive approximations.