June 21, 2019

Response to the reviewers report

Dear editor,

The following are our comments on the manuscript “A re-examination of the role of friction in the original Social Force Model”.

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Referee (1)

The paper deals with the well known Helbing’s social force model for simulating pedestrian dynamics. By means of some numerical simulations and comparisons with real data, the author improves the model unveiling, at the same time, the role of the friction coefficient. The author also shows that the role of the friction coefficient is basically the same of that of the relaxation time.

The paper is fine and I think it can be published, provided some corrections are made. More precisely, I think that the paper is not well organized and the message is difficult to follow. I strongly suggest to include Appendix A in the main text. Section 3 ("Numerical simulations") should be named somethink like "Setting and parameters". Section 4 ("Results"), which should be the core of the paper, is by far too long. Subsection 4.1 ("Hypotheses") is fine but does not contain "results". Subsection 4.2 contains numerical results, but they are not new.

Other comments:

1) Section 1: the author could add the reference to two papers which introduced the SF idea much before Helbing.

   [A] S. Okazaki, A study of pedestrian movement in architectural space, Part 1: pedestrian movement by the application of magnetic model. Trans. A.I.J. 283, 111{119 (1979).

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    [B] K. Hirai, K. Tarui, A simulation of the behavior of
        a crowd in panic, Proceedings of the 1975 Interna-
        tional Conference on Cybernetics Society (1975), pp.
        409-411.

2) End of page 6: "it is well known that the seminal version..."
   A reference should be added.

3) p.10: you cannot refer to $\mathcal A$ and \mathcal K$
   like you do. The most important equations of Appendix A
   must be introduced here.

4) p.14: Fig.1 is not present.

5) Caption Fig.9: k is k_i or k_w?
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Response to Referee (1):

The following comments are outlined in the same order as appearing in the reviewer's report.

The reviewer suggests a reorganization of the manuscript. He (she) suggests to include Appendix A in the main text. We consider that this is a very helpful advice. Therefore, we included Appendix A and Appendix B in the main text (now Sections 5.4 and 5.5 respectively). We expect that this modification will strengthen the core message of the paper.

The reviewer suggests to modify the name of Section 3 ("Numerical Simulations"), to "Setting and parameters". We followed his (her) suggestion.

The reviewer points that Section 4 ("Results") is too long and Subsection 4.1 ("Hypotheses") does not contain results. We moved the "Hypotheses" to a new Section previous to Results (Section 4 in the revised version is now named "Hypotheses" and Section 5 corresponds to "Results"). We also left out Figs. 4 and 5 in order to shorten the Section "Results".

The reviewer also made other comments:

1) He (she) mentions some research introducing the SF idea much before Helbing *et al.*. We included the corresponding citations in the first paragraph of the Introduction.

2) The reviewer points out that a specific reference at the end of page 6 ("it is well known that the seminal version....") should be included. We added the corresponding cite. Note that this text is in page 7 in the revised version and that we have replaced the word "seminal" by "original".

3) In page 10: The reviewer says that we cannot refer to $\mathcal{A}$ and $\mathcal{K}$ like we did (with no previous introduction). He (she) proposes that the equations of Appendix A should be introduced there. We followed his (her) suggestion and included a new paragraph at the end of page 11 (fourth paragraph) in order to provide an introduction to the equations of the reduce-in-units equation of motion and their respective parameters ($\mathcal{A}$ and $\mathcal{K}$).

4) The reviewer indicates that Fig.1 is not present. Fig. 1 is present in page 8.

We modified the text in order to clarify the fact that the Fig. 1 has a dashed circle representing the measurement region for the fundamental diagram.

5) In Fig. 9, κ means κ_i and also κ_w. We replaced κ by $\kappa_i = \kappa_w$ to make the caption more clear.

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Referee (2)

The paper studies the impact of the friction parameter in the original SFM.

At first I thought this is a work concerned with the calibration of a parameter in an old model for a special case: corridors. This sounds very specific and not so overwhelmingly interesting, right? However, I think this paper contains some nice ideas and results that are certainly interesting for the community if written and presented adequately. In my opinion the very best part of the paper comes in the last 2/3 of the manuscript. Only here, the paper starts to show interesting analysis and results, I would describe as original.I think the authors would be better off focusing more on the content in Appendix A and B. This nice normalization, is good. from there work with Eq. (A.3) and go on with the clustering analysis. The analysis shown in Fig. 6 is also interesting. Here, I wish the authors could shed more light on the famous boundary effects, so controversially discussed in the literature.

Here are some specific comments to the text:

1) Reading the introduction, I believe the authors know very good the works done by Helbing and some of his co-authors, especially Johansson. Most of the references in the text are citing these two (Ref.2 most of all). I believe, it would be good to broaden a bit the literature review in the introduction to more recent works as well. At last, during the last 10 years some positive development in the community could be observed, right?

2) I also suggest to reduce the obvious enthusiasm of the authors and dispense with the use of superlatives like "a wonderful summary" or "a seminal work" (repeated many times). By the way, Ref. 16 is from 2007. A more recent review of empirical data can be found here doi : 10.1007/978 - 3 - 642 - 27737 - 5_706-1" Empirical Results of Pedestrian and Evacuation Dynamics"

3) The very first paragraph in the paper is not quite accurate in my opinion. The force social force model ever presented was published in K. Hirai and K.Tarui in 1975 (a simulation of the behavior of a crowd in panic, Proc. of the 1975 International Conference on Cybernetics and Society. (1975) 409-411). The model was not called SFM, but it is a force-model that "nicely bridges the socio-psychology with Newtonian dynamics".

4) It is not clear to me why the SFM explains why the faster-is-slower effect happens. It can be produces, yes, but it does not explain why. Please clarify or reformulate.

5) In general the authors write "Helbing and co-workers". I suggest to use the more formal et al. This is also more necessary in the references.There sometimes the authors use all names of the authors and sometimes only the first author followed by et al. Please also check some errors in some names (K\"oster, L\"ohner, . . .)

6) Equations are missing punctuation.

7) Section 3: Why is the length of the corridor L=28m while the width is w=40m. This sounds strange and I'm not sure if it is necessary to have some big values for w.

8) Section 3: Why are the details about the implementation (C++, LAMMPS) necessary? I think unless the authors are intending to open-source their code (which would be nice by the way) there is no need to mention these details. Also the authors mention a LAMMPS built in function calculating the clusters. What is this function?

9) Section 4: Here the authors cite a lot of other works, especially from 2 and 36, but they do not give their own opinion, in regard of the new findings. Again just a reminder Ref 2 is 10 years old. For new findings see Loehner PED 2016 in Hefei.

10) Page 10: vd is the desired speed not the "anxiety level".

11) Page 14: "In our case, pedestrians near the walls are the ones with the lower velocity".Is this a know empirical fact? Why is it so? Maybe the authors could explain more this phenomenon.

12) I think Fig. 4 and 5 can be safely removed and just replaced with Fig. 6. The normalisation here is nice.

13) The interesting phenomenon the authors show in page 23 was not well analysed and explained. Why is it for high k pedestrians stick together more? Intuitively I would think that high k means high repulsive forces which means that pedestrians stay away from each other not other way around.

14) In Appendix A the two Parameters in Eq.A.3 are not discussed. Instead the focus is still on tau and k. How is this good? Why do you normalize the model, come up with two parameters only then to continue discussing the parameters in the un-normalized model?

To summarize, I think this paper can be published. However, some heavy restructuring and deeper analysis on the points of interests may be necessary.

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Response to referee (2):

As a general impression, the reviewer appears to be concerned on the discussion of our original results. We stress that the revised manuscript now includes a more detailed explanation on these issues. He (she) can find novel comments in parts...

The following comments are outlined in the same order as appearing in the reviewer's report.

1) The reviewer suggests to include more recent works in the introduction. We thank the reviewer for his (her) advice and thus we included a paragraph discussing the findings in Löhner PED 2016 Hefei. We also added another

paragraph that includes 6 references related to the latest developments in the field.

2) The reviewer suggests to dispense the use of sentences like "a wonderful summary" or a "seminal work". In order to avoid these expressions, we replaced them by more appropriate words throughout the manuscript.

On the other side, we acknowledged the citation mentioned by the reviewer and it appears in the revised manuscript.

3) The reviewer points out that the first paragraph of the manuscript is not quite accurate. He (she) also, indicates that the Social Force model was introduced much before Helbing *et al.*

Section 1 (Introduction) has been improved taking into account his (her) comments. We further improved the first two paragraphs of the introduction in order to make them more accurate (we included the suggested reference).

4) The reviewer points out that the SFM reproduces the faster-is-slower effect, but the model does not explain why the faster-is-slower occurs. We agree with the reviewer and reformulated the text in order to fulfill this statement.

5) The reviewer suggests to change expressions like "co-workers" by the more formal *et al.*.

We acknowledge this issue and made the corresponding changes in the revised manuscript.

We also corrected the names of the following authors: Köster and Löhner. We extended the same format to all other references.

6) The reviewer noticed that equations are missing punctuation. We added the appropriate punctuation when needed.

7) The reviewer wants to know the reasons for the analyzed corridor dimensions.

The length (28 m) was chosen according to the dimensions of the area analyzed in Ref. doi.org/10.1103/PhysRevE.75.046109, corresponding to a specific part of the entrance to the Jamaraat Bridge. The width was varied upto 40 m just to check out if the velocity profile had a qualitative difference with the velocity profile corresponding to narrower corridors (say 12 m). We verified that regardless the width of the corridor, the behavior of the scaled velocity profile remains the same.

8) The reviewer asks why we mentioned the details of the implementation despite we do not "open-source" our code.

In order to allow a better understanding of the simulation process to the readers, we considered important to mention the details about the implementation. The code is not open-source yet. But currently we are working in the documentation and cleaning up for an easy understanding before we open-source it.

The reviewer is interested on further details of the LAMMPS built-in function for computing the clusters. We included in Section 3.1 (Simulation software) the name of the LAMMPS built-in function and we provided a brief explanation on how this function works.

9) The reviewer points out that we do not give our own opinion with respect to new findings. We included two new paragraphs in the Section Introduction, mentioning new findings from other authors. The revised manuscript also contains new references from the last 10 years.

10) The reviewer points that v_d is the desired speed, not the "anxiety level". We made the corresponding change in the manuscript.

11) Pedestrians near the walls are those with lower velocities because they dissipate more energy due to the friction force (exerted by the wall). Notice that our velocity profile resembles the experimental one exhibited in Ref. doi.org/10.1016/j.physa.2013.02.019. These authors also measured the velocity profile for pedestrians walking along a straight corridor. We decided to include this reference in the manuscript to provide empirical support to the simulated results.

12) The reviewer suggests to leave out Fig. 4 and Fig. 5, by replacing them with Fig. 6. We appreciate his (her) suggestion so removed Fig. 4 and Fig. 5 and kept Fig. 6 since it contains all the relevant information of the phenomena.

13) The reviewer asks why more pedestrians stick together as κ-value increases.

He (she) appears to be concerned on the true meaning of κ. Recall that κ is the coefficient of the tangential friction force and therefore, is not actually related to the repulsive force $\mathbf{f}_s$ (actuating in the normal direction). Thus, increasing κ avoids a smooth (tangential) sliding between pedestrians regardless of $\mathbf{f}_s$ (normal direction).

14) The reviewer claims that the two parameters in Eq. A.3 are not properly discussed (at this stage of the manuscript). He (she) is right, so we added a brief explanation of the two parameters when introduced.

The reviewer asks why we normalize the model and then continue discussing the parameters in the un-normalized model.

We prefer to continue discussing the parameters in the un-normalized model to make it more understandable for the readers that are used to the un-normalized parameters of the SFM. We are planning to further investigate this normalized parameter and write a new paper focusing on the normalized equation of motion and the role of the different parameters of the SFM. This manuscript is a first attempt to study the model in the framework of the reduced-in-units equation of motion. We pretend to encourage the community to study the SFM (and other models) following this approach.

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Referee (3)

The authors discussed the role of friction in SFM. The topic is interesting and worthy studying. However, there are issues that should be addressed before processing to a possible publication of this work.

1. Nonstandard reference issues:

   (1) Numerical order. For instance, \[10, 12, 8]" (Introduction, page 5, paragraph 1).
   (2) The format of References is inconsistent.

2. The format of unit is nonstandard. For instance, \Pm- 2 " (Fig.2) and \p/m 2" (Fig. 3).

3. Numerical simulations, page 8, paragraph 3: The desired velocity for each pedestrian was 1 m/s. Whether the desired velocity has an effect on the result?

4. Clusters, page 22, paragraph 2: In an enhanced friction scenario the individuals find it harder to detach from each other. Then why are possibilities of pedestrian belong to a small cluster and giant cluster almost equal? Why does a bimodal distribution occur in Figs. 9(d)-(f)?

5. Why does not the velocity profile report any relevant difference as the corridor widens, while the fundamental diagram does?

6. Numerical simulations, page 7, last paragraph: Initially, the individuals were randomly distributed along the corridor, and the corridor with periodic boundary conditions. Whether the initial distribute and boundary conditions may have an effect on simulations?

7. English writing should be improved.

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Response to referee (3):

The following comments are outlined in the same order as appearing in the reviewer's report.

1-2) The reviewer noticed that the format of references and the format of units is nonstandard. We acknowledge these issues and made the corresponding changes in the manuscript.

3) The reviewer asks whether the desired velocity has an effect on the result.

The answer to that question is yes. If the desired velocity is much greater than 1 m/s, the flow vs density relation is a monotonic increasing function. If the desired velocity is less than 1 m/s, the results are very similar to 1 m/s. We decided to use 1 m/s because is roughly the average velocity of a moving pedestrian under normal conditions.

4) The reviewer asks why the possibilities of the pedestrians to belong to a small cluster or giant cluster are almost equal.

This interesting phenomena occurs because while most of the pedestrians belong to the giant component, some pedestrians remain isolated ("caged" inside the giant component without being permanently in contact with other pedestrians). This produces the bimodal distribution in Figs. 9 (d)-(f).

We added a brief comment on this matter at the end of paragraph 2 (Section 5.7) in order to make this fact more clear for the readers.

5) The reviewer asks why does not the velocity profile report any relevant difference as the corridor widens, while the fundamental diagram does.

This happens because the fundamental diagram is strongly dependent on the velocity attained by the pedestrians. If the corridor widens, the pedestrians in the middle of the corridor (where the fundamental diagram is measured) reach higher velocities.

On the other hand, the qualitative behavior of the velocity profile is the same regardless of the size of the corridor (a roughly parabolic shape). This does not contradict the fact that the wider the corridor, the higher the maximum attainable velocity. The shape of the velocity profile only depends on the boundary conditions in the y-coordinate (fixed walls) that leads to a parabolic-like velocity profile.

We modified the section that shows the velocity profiles. We only show the scaled velocity profile in the revised version.

6) The reviewer asks whether the initial distribution of pedestrians and boundary conditions may have an effect on the simulations.

We checked different initial conditions and saw no significant results. We also tested a situation with a corridor 10 times larger ($L = 280$ m instead of $L = 28$ m) to reduce the effect of the boundary conditions. We did not notice any significant change in the results.

7) The reviewer suggests to improve the English writing. We improved the writing to satisfy this important requirement.

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Your consideration of this revised manuscript for publication in Safety Science is greatly appreciated. Please find attached the new version.

Sincerely

Ignacio Sticco, Guillermo Frank,
Fernando Cornes and Claudio Dorso