Review of the manuscript “Joint distribution of currents in the symmetric exclusion process” by A. Grabsch, P. Rizkallah and O. Bénichou

The present manuscript sets to study the joint distribution of the integrated current and the generalized current in single file diffusing systems exemplified by the SSEP.

When considered separately, the statistics of these two currents have been determined exactly. First by microscopic methods like the Bethe ansatz. Then, more recently, a series of works have shown that in the long time limit, these problems are completely integrable with far greater generality. This was established using two related yet different approaches. The first invokes the Macroscopic Fluctuation Theory (MFT) in conjecture with the inverse scattering method (ISM). The second uses a simple and exact closure scheme for the infinite hierarchy of correlation equations satisfied by a generalized density profile. This second approach reduces the complicated many-body problem to solving a simple integral equation for the generalized density profile. The latter can in some cases be solved exactly.

The current manuscript presents a significant advance in this line of research by extending the ISM method to solve for the *joint* statistics of the integrated and generalized currents. The derivation reduces the highly non-trivial problem of solving coupled PDE's that lie at the heart of the MFT framework into solving a much simpler trilinear integral equation that then serves as the starting point of a systematic expansion for the joint cumulants.

This integral equation presents an extension of the integral equation found before for a single current within the alternative closure scheme for the generalized density profile.

This result highlights the relation between the MFT approach, which starts at the hydrodynamic level, to the alternative approach taken by the hierarchical correlation closure scheme, which starts at the microscopic level.

A nontrivial step here necessitates the derivation of boundary conditions accompanying the integral equations. The present work shows how these are found within the MFT approach using a nontrivial time reversal mapping which also provides a clearer physical interpretation of these boundary conditions.

Overall this work presents an important advance of complete integrability in many body systems which will be of interest to specialists working in this field and on single-file motion.

I found the paper well organized. It wisely presents the main results and their discussion before their explicit derivation. The derivations that follow are detailed and clear, although relying on the familiarity of the reader with the basics of the MFT formalism and the inverse scattering transform.

I enclose here some comments and suggestions that the authors might wish to consider. These aim at increasing readability and intuition especially for the less familiar reader.

1. In the introduction the second and third paragraphs compare two methods that provide exact results. The first relies on "tools from integrable probabilities" while the second involves a closure scheme for a "generalized density profile". For the unfamiliar reader it might be unclear at this point how the two are different (both are based on microscopic calculations). The authors might consider adding some more details.

2. The introduction declares what is the approach that is undertaken in this work to solve for the joint statistics only in the very last sentences and only briefly. In fact, up to that point, it might seem that this work uses a microscopic approach based on the closure scheme for the generalized current ("Here, we show that the integral equation of [6,7] can be generalised to describe the joint correlations between the currents Q_T , J_T and ...").

   Perhaps the authors would like to consider introducing the hydrodynamic MFT approach in a bit more detail and at an earlier stage, and to state explicitly that this paper uses it as a starting point in conjecture with the ISM. Then explain that the paper highlights the relations between this hydrodynamic approach and the alternative one that appeared in refs [6,7]. In my understanding, this is in fact one of the important results of this work.

3. I believe that the manuscript would benefit if the introduction of the main objects under study, the generalized density profile [Eq.(5) and Eq.(6)], would be presented together with their clear physical meaning. Possibly, accompanied by some physical intuition as to their dynamical behavior. This is currently only partially presented at later stages of the manuscript.

   Explicitly, I think it would be useful to declare that these are precisely the mean density profile of the so called tilted or canonical path ensemble. I.e., the ensemble of paths biased towards realizing values of the integrated currents different from their mean, see e.g. [1]. This then immediately clarifies subsequent relations such as the Eq.(19), the boundary condition (15) and the conservation equation (16).

   More importantly, it provides a clear intuition behind the singularity which exits at the initial and final times as encoded in the definitions Eq.(7) and the boundary conditions. Otherwise, it seems very peculiar that Ω and $\bar{\Omega}$ are defined in this piece-wise fashion.

Some of these points are currently only briefly discussed at the end of Sec.2.2. I believe that having a more comprehensive discussion at an earlier stage would help the reader make more sense of the list of results, including the boundary conditions that serve an important part of the derivation. It might prove useful to repeat the physical picture behind the boundary conditions derived later on in Sec.5.1. Lastly, I believe it would be very helpful if Fig. 1 were advanced to an earlier stage of the manuscript as well.

Apart from these suggestions, here are some minor issues the authors might want to attend

1. P.2 second paragraph. Fix grammar (?) on the last sentence: "Furthermore, this generalisation has to advantage to provide a clear physical meaning to these relations".

2. Last paragraph of the introduction seems to skip the description of the remaining Secs 6 and 7.

3. The system size L, in Eq.(2), appears without explicit definition. Also, it is not completely clear why there is a need to introduce this regularization here, but not in Eq. (1). For instance, writing (2) as

$$J_t = -\sum_{t=1}^{x_t} \eta_r(0) + \sum_{r>x_t} [\eta_r(t) - \eta_r(0)] \tag{1}$$

4. Below Eq. (37): "This result was... later extended to arbitrary single-file system". In fact, the generality of this result goes beyond single file systems and includes a family of lattice gases, see e.g. [2].

5. In Eq.(122) the function $T(\dots,\dots)$ appears without definition (it might be also helpful to state the definition of erfc)

6. Many explicit results seem to apply for the particular case $\rho_- = \rho_+$. I found it hard to find the occasions where the manuscript declares that this is assumed, and when is it solving for the more general case $\rho_- \neq \rho_+$.

[1] H. Touchette, Phys. Rep. 478, 1 (2009).
[2] P. Krapivsky and B. Meerson, Phys. Rev. E 86, 031106 (2012).