Report on “Integrable fishnet circuits and Brownian solitons”

This article presents a very interesting construction, based on the notion of (entwining) Yang-Baxter maps, of integrable maps dubbed fishnet circuits that allow for randomised time steps, leading to stochastic integrable dynamics. The general framework and results are introduced in Section 2 and 3. Many of the tools and statements there will be familiar to the reader versed in (discrete) integrable systems but the authors develop these ideas in the very interesting new direction of stochastic integrable dynamics. Two paradigmatic examples are used in Section 4 to illustrate some aspects of the framework. The Toda example is very useful as its simplicity allows the reader to see how the method works in detail. The other model is technically much more challenging but the authors show that a lot can still be done in the way of deriving the analytical results needed for the implementation of the method. This is an important aspect as the Toda example alone would have weakened the impact of the work. Section 5 is the crucial feature of this work where the implementation of stochastic dynamics is introduced and some (numerical) results are provided, with a very interesting aspect related to Brownian solitons. The approach, ideas and results are exciting and certainly deserve publication in SciPost, after the authors take into account the comments below.

The overarching comment is that this article can bring closer together various communities (e.g. discrete integrable systems and integrable statistical physics). In order to do so, I think a better exposition of what is known in discrete integrable systems would help that community appreciate and digest the important results of this work. This is not a criticism as the paper is very well written overall. I will try to suggest areas for improvement with the following comments/questions.

1. p2: there's a typo in the third paragraph of the introduction “an daunting” $\rightarrow$ “a daunting”.

2. p2: fifth paragraph. It is true that conventional discretisations break integrability in general. However, this is precisely one of the origins of discrete (semi and fully) integrable systems whereby one designed discretisation schemes that would automatically preserve integrability. This is a well-established machinery now. To my knowledge the earliest example of this is the Ablowitz-Ladik model that was built as a space discretisation of the nonlinear Schrödinger equation so as to preserve integrability. Standard methods involve for instance using Darboux/Bäcklund transformations of continuous equations to construct the integrable discretisations (see e.g. [1]) or the so-called direct linearisation method (see e.g. [2]). Of course the literature on this is huge so perhaps the book 'Discrete Systems and Integrability' by J. Hietarinta, N. Joshi and F.W. Nijhoff is a good source to mention for this.

3. p3: the refactorisation (1.1) is the basis of the entire construction and the beginning of various points that need clarifying throughout the paper. Basically, it has to do with whether one needs to consider (1.1) with shifted spectral parameters or not. Traditionally, one would consider (1.1) with the same λ everywhere, although the Lax matrices would be allowed to depend on extra (lattice) parameters. The

use of $\lambda^{\pm}$ is a nice idea which induces the τ dependence on the map as desired. However, there are points in the rest of the text where it is not clear whether the shifted, non shifted or other version of (1.1) is required to make the statements. One such instance is when the authors deal with inhomogeneities. I will try to mention where confusion might arise in chronological order below.

Finally, in general the authors allows $\mathcal{L}^a$ to be different from $\mathcal{L}$ so I think they fall under the general idea of entwining Yang-Baxter maps as discussed e.g. in [3].

4. p4 just before (1.5) the integer n appears on the monodromy and transfer function and at this stage it is not clear what it means. Later, it appears that it is simply the n-fold product of the monodromy matrix but it would help to say it already here.

5. p7 it would be helpful if the authors clarified what they mean by "mutual compatibility". In addition to requiring (2.1) for $\mathcal{L}$ and also for $\mathcal{L}^a$ with the same r-matrix, do they also require $\{\mathcal{L}, \mathcal{L}^a\} = 0$?

6. p8: the authors introduce inhomogeneity parameters. As they explain with (2.8), to resolve (2.7) they need the successive maps $\psi_{\tau-\xi_\ell}$, not ψ_τ. Thus, it is slightly misleading for the reader to say that one uses repeated applications of (2.2) since, if I understand correctly, the authors need the refactorisations

$$\mathcal{L}^a_{\lambda-\tau-\xi_\ell}(y_\ell)\mathcal{L}_{\lambda+\tau-\xi_\ell}(x'_\ell) = \mathcal{L}_{\lambda+\tau-\xi_\ell}(x_\ell)\mathcal{L}^a_{\lambda-\tau-\xi_\ell}(y_{\ell-1})\,, \quad \ell = 1, \ldots, L$$

In turn, this means that the Ψ_τ in fact also depends on all the inhomogeneities, although they are not shown explicitly. My main question is what is the point of dealing with inhomegeneities since, as far as I can see, they are not used in the examples and they do not seem to play a role in the general discussion either? In the examples, the authors specify several times that $\Xi = 0$, which would not be needed if the inhomogeneities were not introduced in the first place.

7. p9: a small typo in the sentence after (2.17) which contains two 'the'.

8. p9: it is true that the ultralocality property of (2.1) implies (2.21) for $n = m = 1$ but for larger n,m, (2.21) does not hold. However, (2.22) is still true thanks to the cyclicity property of the trace.

9. p9: just before (2.23), the expansion is not necessarily in $1/\lambda$ as the Toda example shows.

10. p9: the comment after (2.25) on the τ independence of the conserved quantities is puzzling. (2.7) involves the monodromy matrix with λ^+ so that it should lead to (2.19) with λ^+ not λ i.e. I expect $\mathcal{T}^{(n)}_{\lambda^+}$ to be preserved by the time evolution under the chiral propagators. Thus I do not understand how one could also immediately see that $\mathcal{T}^{(n)}_{\lambda}$ is also preserved by the chiral propagators. Then if one expands $\mathcal{T}^{(n)}_{\lambda^+}$ in powers (positive or negative) or λ, the charges will in general be τ dependent. I checked this for the simplest charge, say $Q^{(1)}_1(\tau)$ in the Toda example. In that case

however, the charge had a very simple dependence on τ $Q_1^{(1)}(\tau) = \tau + Q_1^{(1)}$ where $Q_1^{(1)}$ happened indeed to be conserved under the evolution map. This encouraged me to believe that (2.25) does hold with τ independent charges extracted from $\mathcal{T}_\lambda^{(n)}$ instead of $\mathcal{T}_{\lambda^+}^{(n)}$ but I could not see how to obtain a general statement from (2.7). Hopefully my comment makes sense and I hope the authors can clarify this point.

11. p10: As I already mentioned, strictly speaking what is exposed here falls within the more general notion of entwining Yang-Baxter maps since the authors allows for two distinct Lax matrices in general. They have two distinct maps in (2.27) as a special case of the more general situation of 3 maps presented for instance in [3]. It also a bit misleading to say at the beginning that relation (2.2) is enough to reverse the order of the factors since in fact (2.31) is also needed and hence has to be assumed. I would suggest to introduce (2.31) before presenting (2.26) and then (2.27). Finally, there is one more condition that should be mentioned to ensure that (2.27) is indeed a consequence of (2.26), see Proposition 3.1 in [3].

12. Could the authors clarify what they mean by the comment in footnote 5 about the intertwiner not mixing the fixed points? First, the intertwiner outputs $y_j^{*'}$ from the input y_j^* so it seems that is has an action on the fixed points and it is not clear why $y_j^{*'}$ should also be fixed points for the map $X \to X'$ if y_j^* are, and vice versa. Second, the comment in the footnote seems to be a key aspect in deducing (2.34) from (2.33). Very superficially, this seems analogous to the argument based on the quantum Yang-Baxter equation to obtain the commutativity of the quantum transfer matrix trT from the relation $R_{12}T_1T_2 = T_2T_1R_{12}$, the intertwiner playing the role of R_{12}. However, here one has to project on the "spaces" (copies of $\mathcal{Y}^L$) 1 and 2 according to (2.11) and it is not clear how the intertwiner goes away when this is done and how the comment in the footnote plays a role.

13. p14: in the last line, I don't think the convention of including the identity matrix as the "Pauli matrix" σ_0 is widespread (strictly speaking the identity matrix is not even part of the Lie algebra of 2×2 traceless matrices) and a superficial reading could mislead the reader when trying to compute the permutation Π.

14. p15: it would help the reader to explain how the map (4.8)-(4.11) is used to deduce (4.17) by relating it to Figure 2 more explicitly (it took me some time to figure it out in order to check (4.17)). By this I mean that, the dashes in (4.8)-(4.11) are not the same as the dashes (and absence thereof) in Figure 2. I would suggest to include the equations as they are used to derive (4.17). If I'm not mistaken, this

should be

$$\begin{aligned}
x'_\ell &= y_{\ell-1}, \\
p'_\ell &= \frac{x_\ell}{y_{\ell-1}} + y_{\ell-1}k_{\ell-1} - \tau, \\
y_\ell &= x_\ell \left(p_\ell + \tau - \frac{x_\ell}{y_{\ell-1}} \right), \\
k_\ell &= \frac{1}{x_\ell}.
\end{aligned}$$

With this, I think there is a typo in (4.17) and $\mathcal{F}_\ell$ should be

$$\begin{pmatrix} p_\ell + \tau & -\frac{x_\ell}{x_{\ell-1}} \\ 1 & 0 \end{pmatrix}.$$

Then, a different notation should be used in (4.20) since here, it is indeed the matrix given in (4.17) that appears but it is then no longer the same $\mathcal{F}_\ell$ as in the corrected (4.17).

15. p16: the derivation of the continuous limit is interesting but I could not see what role it plays in the structure of the paper. Could the authors explain why they thought this should be included? Perhaps it is related to the continuous-time limit considerations in Section 5 but, if so, it would be helpful to explain how?

16. p18: a very minor point. The same notation H_k for the homology groups and for the conserved charges around (3.11) is used.

17. p20: a similar remark to above about helping the reader relate the setup of Figure 2 with the notations in (4.43)-(4.44) in order to derive (4.68)-(4.69) (then giving a_ℓ would also help). Also, what does the subscript ^{a} mean on ζ_ℓ^a? Finally, ζ_ℓ is used in (4.68), while X is used in (4.70). I completely understand that the authors switch between the generic notations x, X and the particular spin/stereographic notations in the example but it does makes things a bit harder to follow on a first read.

18. p22-23: to derive (4.80), again I would suggest to help the reader relate the notation y_ℓ etc. with the spin notation in (4.46). Also, it would be useful to recall that x_ℓ for the set of variables y_ℓ are the same x_ℓ used for the set of variables y'_ℓ so that $\Delta_\ell^{'2}$ is the same as Δ_ℓ^2 with $y_\ell \to y'_\ell$. It took me a while to reproduce (4.80). I think in (4.82), L^{-1} should be on both side or not at all. The argument to prove the existence of a fixed is very nice and based on (4.83)-(4.84). However, it would be very helpful to give more detail on how to derive (4.84). I suspect $d\rho_{S^2}$ is a measure on the unit sphere. If so, how does it arise and why does the discrete s_ℓ still appear under the integral? Presumably, in the continuous limit, there is a transition to a continuous interpolating function s and a Jacobian producing the measure?

19. p23: small typo in the last paragraph: “Despite that, *the* two...”

I have a few additional questions/comments that are more for my personal education than to suggest changes in the text. However, the authors might feel that it would be helpful to also say a few words about these in the text as I suspect I would not be the only reader interested in those aspects.

There is a well-known method called periodic reductions (see e.g. the Discrete Systems and Integrability book mentioned above) that has been applied to many lattice equations to produce integrable maps. The present construction is very reminiscent of this since the authors set up periodic boundary conditions on the circuit. I thought at first that this was exactly the same thing but I could not pinpoint the exact correspondence. In addition, in the periodic reductions approach one does not usually worry about the existence of a fixed point in order to be able to impose periodicity. Periodicity is imposed and then the maps are deduced. I wonder if the authors could comment on this.

The numerical identification of what the authors call Brownian solitons is very intriguing and exciting. Unfortunately, they are only briefly mentioned at the end. Can the authors say more? For instance, are there references on this topic? What would their analytical expressions look like (if known in some model)? I am not a specialist of stochastic processes but I was under the impression that a noise term for instance in a stochastic differential equation would break the conservation of energy. How could then one hope to have an integrable dynamics if even the energy is no longer conserved? I might be completely missing the point but could the authors comment?

References

[1] D. Levi, Nonlinear differential difference equations as B¨acklund transformations, J.Phys. A: Math. Gen. 14 (1981) 1083–1098.

[2] F. W. Nijhoff, G. R. Quispel, H. Capel, Direct linearization of nonlinear difference-difference equations, Phys. Lett. A 94 (1983) 125–128.

[3] Kouloukas T, Papageorgiou V. 2011 Entwining Yang-Baxter maps and integrable lattices. Banach Center Publ. 9, 163–175.