Reply to the referee's report

V. Gritsev and A. Polkovnikov

1 General reply

We are grateful to both referees for raising interesting and relevant points. We implemented changes in the new version of the paper to reflect most of them.

2 First referee report

Report: The authors deal with Floquet systems whose Floquet Hamiltonian is integrable and therefore the driven time evolution is not chaotic. This is a very interesting and promising field of research and the conclusions they find are definitely worth a publication. However the article as it is written now has many unclear parts which need to be addressed. Here I list the main questions that a reader may have.

Requested changes

1) First of all, it is not clear how the authors define integrability of the Floquet Hamiltonian. In the introduction they say they find Floquet integrable systems in which one can define a local unfolded Floquet Hamiltonian, but then below formula (2) they say Integrability of HF in this paper will be understood as existence of enough conserved integrals of motion to be able to diagonalize it. I personally struggle to see how these two definition are compatible. From a physical point of view they claim that Floquet integrable systems are those ones that do not heat up. Therefore I would expect that the eigenstates of the Floquet integrable Hamiltonian are not indistinguishable from a generic infinite temperature state, as it is usually the case for Floquet Hamiltonians. The authors do not mention this point (and I do not see how the fact that their Floquet Hamiltonians are diagonalizable is related to this) and therefore it is hard for me to understand what they mean for they do not heat up.

Reply: *The definition of what does it mean for the model to be integrable is not a straightforward issue. Even for the traditional systems this definition is not uniquely* *defined (see the paper by Caux and Mossel, Ref.4 of our paper. Recent discovery of MBL systems makes the definition of integrability even trickier as e.g. these systems have Poisson statistics but do not satisfy the adiabatic theorem and therefore do not have differentiable smooth integrals of motion (this point is somehow carefully avoided in the literature but is really obvious by simple generalization of the paper by Khemani et. al. (Ref. 26 in the updated draft). We thus do not pretend to solve a general problem and we added a more extended discussion about that in the end of introduction. At the same time existence of local integrals of motion obviously does prevent systems from heating like e.g. in standard quenches, where existence of local Hamiltonian prevents system from infinite heating. Additional integrals of motion can only constrain the system even more. In that sense the definition of integrability we are using is the same as for the integrable lattice models (like e.g. XXZ spin chain). The Floquet dynamics is then governed by the Generalized Gibbs Ensemble (see Ref. [XXX] by Lazarides et. al.), rather than by the thermal one which means that this dynamics does not lead to the infinite temperature heating. The models from the first class are diagonalizable by the unitary transformation from a finite-dimensional group, and thus integrable.*

2) At page 3 the authors say . Finally let us note that as with any other driven systems the physics can strongly depend on initial conditions, which can be also integrable or non-integarble. Apart from the typo on the word integrable, it is not clear what do the authors mean with integrable initial conditions.

Reply: *We first thank our referee for spotting a typo in this place. In studies of quench dynamics of integrable models in many cases (especially in the field-theoretic models, like e.g. conformal field theory or sine-Gordon) one can distinguish between integrable and non-integrable boundary states (which correspond to initial conditions in quench problems). This philosophy goes back to Sklyanin's construction of integrable boundaries in the lattice models and to Ghoshal-Zamolodchikov boundary states in the sine-Gordon case. The same philosophy has been essentially used in the paper by Calabrese and Cardy on quenches in the CFT. We followed this tradition here and mention this concept without explicitly citing the above works. Moreover, in the context of connection to integrable lattice Statistical Mechanics models we mention that the six-vertex model with so-called domain wall boundary conditions is also integrable (see e.g. the book by Korepin, Bogoliubov, Izergin "Quantum Inverse Scattering Method and Correlation Functions"). This type of integrable boundary state becomes initial state in our models of the second class. Except for the general discussion in the introduction we are using the word integrability in this paper in the same sense as in standard translationally invariant systems so we believe this terminology does not create any confusion. We corrected the omission of the corresponding dis-*

cussion and explained this for better clarity of the text. We inserted a paragraph at the end of the page 3. While the issue of absence heating is probably independent of the boundary conditions, the ability to solve dynamics by the Bethe ansatz is not.

3) Figure 2 is taken from reference [43]. I am not sure if it is possible to use an already published figure without some consent.

Reply: *To avoid this problem we have generated our own figure (although in a slightly different coordinates).*

4) Equation 33: what is λ? Operator B has no dependence on the spectral parameter.

Reply: *This is an abuse of notations. Here λ is a coupling constant of the transverse field Ising Hamiltonian. We changed it to β to avoid the confusion. We have consistently changed several notations in various parts of the paper to avoid further confusions.*

5) Equation 36: The authors claim that this Hamiltonian is integrable. However, also due to the fact that their definition of integrability is unclear, I struggle to see the reason. For sure this Hamiltonian has not an extensive number of conserved operators, or at least if it does the authors should explain why.

Reply: *This is indeed an interesting and subtle point. Let us make two points: (i) there is an infinite set of periods where the coefficient b in the effective Hamiltonian vanishes and then it is explicitly integrable and (ii) In the end of this section we are discussing quench protocols to the effective Hamiltonian and show that even if $\bar{\lambda} = b$ is non-zero there is an infinite set of times T_n after the quench, where all integrals of motion are explicitly conserved (Eq. (65)). These integrals simply do not have enough time to become non-local between T_n and T_{n+1} this the dressed integrals of motion must remain local. As the corresponding discussion goes beyond the Floquet integrability we decided to avoid this discussion in the text.*

6) Still on this equation: the operator B scales as L^2, with L the system size, while we expect the series $\sum_n a_n Q_n$ to be convergent and therefore to be extensive in L. The authors should comment about this. Does this mean that the coupling constant b should scale as 1/L ?

Reply: *We perfectly understood this problem in the early stage of the project but as highlighted above decided to avoid this discussion in the text. As we comment the rotating frame picture gets rid of this problem and defines a dense set of "integrable points" with unavoidable existence of integrability in between.*

7) Section IV.B is not well written. Many different notions and ideas are exposed without a clear picture of what is done. I suggest to organize this section in a clearer way and to expose clearly the main ideas/aims.

Reply: *The logic of this section is to introduce a class of models related to the* *row transfer matrices which is in a sense dual to the corner transfer matrix. This logic is contained in Section IV.A. We can rewrite this Section if the referee could give us more precise indications what is not clear in our text. We will be happy to improve it.*

8) Formula 50: it should be a scalar product between the Pauli matrices.

Reply: *Yes, indeed.*

9) Section IV.C The authors say However, the convergence of this formal expression should be checked for every state —?0i separately. For this reasons we avoid presentation of these formal expressions. I believe this should not prevent them to at least check their expression on a simple state, for example an infinite temperature state or a simple product state. That also would partially address question 6).

Reply: *Following this suggestion we have added an Appendix B where we collected known results on expectation values of conserved operators in several product states. All of them result in convergent expressions for the reasonable time protocols.*

10) Just below the authors notice that B has diverging matrix elements. Therefore a regularization has to be provided for B. This however seem strongly dependent on the boundary conditions. The authors should comment about this.

Reply: *This is indeed the same divergence as noticed by the referee in the point 6 above, it is not a new one. Also let us point that because there is no heating in the system*

11) Just below eq 11: what is the matter of fact ?

Reply: *We added a clarification on the spectrum of B below that equation.*

12) Eq 60: It is not clear to me why the authors compute the rotating frame Hamiltonian and not directly the Floquet one. I kindly ask the authors to clarify a bit more their logic here in this section.

Reply: *The logic is precisely to get rid of the B-part in the Floquet Hamiltonian. This is explained in teh paragraph just before this subsection "Rotating frame". Also as extensively discussed in Ref. [3] going to rotating frame is very advantageous for many problems as it allows one to avoid infinite resummations required in the lab frame. For example, we do not see a simple way of deriving the results of this section, in particular finding special times, where $\bar{\lambda} = 0$, in the lab frame.*

13) Non-numbered equation above eq 61: what is the variable n on the right hand side?

Reply: *Here n is an arbitrary integer. This is clarified now in the text.*

14) Eq 62) what is $\bar{H}_{rot}$? Could the authors explain why in this case the Floquet Hamiltonian is the same as the rotation frame one?

Reply: *This is because all terms commute with each other at all times and one can remove the time-ordering in the evolution operator. Then the result just follows from* *the definition of the Floquet Hamiltonian. We added the corresponding discussion.*

15) The Floquet Hamiltonian 61 is time-independent. In the introduction the authors says Using this criterion any Floquet system, which can be mapped to a static system via a local rotation (e.g. a static system in the rotating frame) is integrable because its folded spectrum contains infinitely many level crossings therefore I would now expect that this Hamiltonian 61 falls in this class of integrable Hamiltonians. Could the authors comment about that?

Reply: *Any Floquet Hamiltonian is time independent by definition. So the question is really whether it is a local operator or not. So the question is really related to convergence of Eq. (63) (in the updated version equation numbering is slightly different). We can only establish such convergence at short periods Eq. (61), which is known well from Stat. Mech. For longer periods convergence depends on analytical properties of* $\tau(\lambda)$ *matrix in the complex temperature plane and we do not want to make any exact statements here.*

16) Beginning of section V: I assume the authors meant discussed and identified.

Reply: *Yes, thank you!*

Finally, as a general remark, I believe this paper contains many innovative ideas and interesting arguments, but it seems hard to find some physical application. Indeed it is not clear at all how the Floquet protocols introduced here are physically realizable (how to realize a time evolution with the Boost operator?). This is a general point where I invite the authors to comment.

Reply: *The three classes introduced here can easily be realized in physical systems. The first class can be realized with finite-level systems, like artificial qubits coupled to bosonic few-mode cavity or transmission line. Interestingly, the implication of the second class of the models has been discussed already in Ref. [86] (new version) by Barmettler et al. and we comment on this in the bulk of the text, on page 10. The realization of the third protocol can be done with the tilted optical lattice by periodically switching on and off the tilt applied to the optical lattice. This is a standard procedure in the ultracold atomic systems.*

3 Second referee report

Report The subject of Floquet systems is very timely and interesting. Integrability in this context is an interesting issue, which is not well studied. The authors present classes of periodically driven systems that satisfy the property that the effective Floquet Hamiltonian is integrable. I consider this paper interesting, and potentially suitable for publication in Scipost Physics. However, I would like the authors to consider the following points before publication:

1. The title of the paper suggests a very general approach. However, the actual results are more restricted. Most of the paper (with the exception of the part on "Rotating frame") considers the two-step periodic process, for which the BCH expansion can be applied. I think the title should be chosen to more accurately reflect the content of the paper. The only element in the paper that conforms to the generality reflected in the title is the somewhat trivial definition of integrable Floquet system.

Reply: *In fact, first of all we do not claim that we found the most generic classes of integrable Floquet systems. We explicitly said this in the text. Second, we believe that the two-step protocol is already quite generic from the point of view of physical applications. Third, the first and the third classes are indeed more generic than the two-step protocol. When discussing the first class we explicitly mentioned that the time-dependent functions in the Hamiltonian could be arbitrary (not even periodic). The generalization of the third class to arbitrary periodic protocol is straightforward since it relies only on the BCH expansion in the rotating frame, and uses the same properties of the boost commutation relations. Let us also point that step like protocol is directly analogous to tight-binding (nearest neighbor) lattice models, where standard lattice integrability is normally discussed. Generic driving protocols are similar to generic periodic potential (or generic periodic spin-spin interaction). It makes the whole analysis much more complicated and is rarely done in the literature. Also we point that step like protocols are used in digital quantum simulations and have a full computational power, i.e. any continuous protocol (periodic or not) can be approximated to arbitrary precision by the step like protocol. So step protocols are very generic.*

2. The definition of integrable Floquet system used in the paper is very reasonable and intuitive. However, it has a strong limitation: the Floquet Hamiltonian is generally expected to be a very complicated nonlocal object even if the original Hamiltonian is local (e.g. a spin chain with short-range coupling). This seriously complicates identification of cases which are integrable since little is known about classification of nonlocal integrable Hamiltonians. The authors restrict themselves to the case of a two-step protocol and three very specific classes of algebras underlying the Hamiltonian, which do not have this problem.

Reply: *We are not aware of any examples of integrable Floquet systems with no local or quasi-local Floquet Hamiltonian. Usually non-locality of the Floquet Hamiltonian is equivalent to the infinite temperature heating. This is precisely why most Floquet systems are non-integrable. We do not know though whether there can be any exceptions and whether there are integrable non-local Floquet Hamltonians so we do not make a strong claim. Note that locality is usually the key to integrability as* *it allows one to define integrals of motion in the thermodynamic limit. In order to discuss non-local integrable Hamiltonians one has first to define what are they. There are of course known special cases with all to all interactions, but these are typically zero dimensional models, which we do not discuss here.*

3. The only explicit problem considered in the paper is the Mathieu harmonic oscillator, which is nothing new, as the eventual solution is from the 1969 paper by Perelomov and Popov (ref. [43]). The authors do not present any other nontrivial example that could convince the reader about the usefulness of the approach. It is also unclear if there are real systems that fit into the author's framework; I think that NMR experiments can be considered for inspiration, as the two-step protocol is well-known there, but it remains to be seen whether the authors' approach can say anything nontrivial there.

Reply: *We do not completely agree with the referee's statement. In the first class of the models we provide very general classification scheme (see Appendix A). The example of Perelomov and Popov is made for the illustration of the general procedure, the simplest one. Generalizations , for example for the case of two interacting bosonic modes is straightforward. However we decided not to overload the presentation with technical details. This can be done in separate papers. The second class of models in directly linked to very concrete protocols discussed in the text (Eq. (51)) and more specifically discussed in Barmettler et al (Ref. [86] of the new version). The third class can be realized with the periodically tilted optical lattices - a routine procedure in the field of ultracold atoms. An explicit example would be a Heisenberg chain with the Boost operator in the form of Eq. (37). Of course once the integrable Floquet Hamiltonian is identified one can study properties of specific systems, but then the problem becomes similar to the integrable quench problem and there is huge literature on this.*

4. A more technical point is that the text has some grammatical problems and a number of typos, like "to a different Cartan subalgebras" towards the end of Section III.A, CMT instead of CTM after eqn. (31), FLoquet before eqn. (36), "discussed identified" in the first sentence of Section V, "irrespective if" before eqn. (63). At some points the text is not really comprehensible, for example at the end of Section III. B where it refers to an Appendix. The present paper has no appendices, so it must refer to an Appendix in either reference [39] or [41], but which one?

Reply: *We would like to thank the referee for a very careful reading of the manuscript. We implemented corrections in the new version. Our paper has an Appendix A and B. The old version had a single Appendix which we referred to at the end of Section III. B.*

5. References: - when referencing GGE, [5] is an excellent work with respect to

the role played by quasilocal charges, but in my opinion the original paper M. Rigol, V. Dunjko, V. Yurovsky, and M. Olshanii, Phys. Rev. Lett. 98, 050405 (2007) also deserves to be cited as the originator of the concept. - fig. 2 is directly taken from the Perelomov-Popov paper, which must be acknowledged explicitly (just referencing [43] in the text when citing fig. 2 is not enough, at least in the usual practice of including artwork from other sources). - when citing the XXZ quench action work ref. [89] of the Amsterdam group, the authors omit the parallel contribution by the Budapest group. The original PRL papers

B. Wouters, J. De Nardis, M. Brockmann, D. Fioretto, M. Rigol, and J.-S. Caux, Quenching the anisotropic Heisenberg chain: Exact solution and generalized Gibbs ensemble predictions, Phys. Rev. Lett. 113, 117202 (2014). B. Pozsgay, M. Mestyan, M. A. Werner, M. Kormos, G. Zarand, and G. Takacs, Correlations after quantum quenches in the XXZ spin chain: Failure of the generalized Gibbs ensemble, Phys. Rev. Lett. 113, 117203 (2014).

indeed appeared back-to-back; both groups contributed unique and important pieces to the full picture. The authors cite the long version of the Amsterdam group's paper as [89]; the corresponding paper by the Budapest group is

M. Mestyan, B. Pozsgay, G. Takacs and M.A. Werner, Quenching the XXZ spin chain: quench action approach versus generalized Gibbs ensemble, J Stat. Mech. 1504 (2015) P04001.

Reply: *We have added the references mentioned by the referee. We produced our own picture instead of using the old one by Perelomov and Popov.*

Requested changes 1. The authors should give appropriate discussion of the issues raised in points 1-2 of the report, consider to change the title to reflect more the eventual scope of the results, and discuss the limitations of their approach more explicitly. In regard to point 3, the paper would be much improved by including at least one nontrivial and explicit novel example, or if the authors otherwise point out some more specific interesting systems where their approach can give new results.

2. The authors should eliminate grammatical mistakes and typos (as much as possible), and clarify the text where appropriate.

3. I suggest including the reference to the original GGE paper, and the authors must properly acknowledge the independent contributions of different groups in the context of the quench dynamics of the XXZ chain. It is also necessary to indicate explicitly the provenience of fig. 2 in its caption. The authors must also make sure they have the necessary permissions to reproduce it (this may be given under the terms the original journal is published, which may also specify the proper way of acknowledging the source of the figure).