

We appreciate the constructive observations of Referee A (apart from a minor issue of misunderstanding which has been clarified) and have modified the manuscript appropriately. We are, however, disappointed by the very biased and prejudiced criticism of Referee B. We have responded to all the specific points that Referee B raised. We hope that the version submitted now will be acceptable for publication in PRL.

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Response to Referee A -- LW14057/Roy

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**(A1) Referee A:** *The authors study a spin chain with static disorder under periodic driving. They find that the driving may significantly slow down the relaxation of an initial magnetization. They provide a numerical study of the time evolution and explain their findings by a mapping to a time-independent effective Hamiltonian.*

*The work is technically sound and presumably correct (some minor technical issues are listed below). Thus the question is whether it has sufficient impact to merit publication in PRL.*

**Authors:** We thank the referee for his/her positive evaluation.

The referee judges the novelty of the work based on two observations. The first is the suggestion that our results are somewhat expected and the other suggesting the results warrants a PRL publication. Weighing the one with respect to the other, the referee recommends publication in PRL.

Unfortunately, the referee misread the Hamiltonian, rendering the first observation (which the referee counted against acceptance) simply incorrect, as we clarify below. Hence, we expect that the referee would now have no hesitation in recommending the article strongly for publication in PRL.

**(A2) Referee A:** *On the one hand, the observed slowing down a tunnel dynamics by an ac driving is well known as dynamical localization or coherent destruction of tunneling. Thus, one may argue that the only novelty is the influence of disorder. Since the disorder Hamiltonian commutes with the driving, it is invariant under the mapping to the effective static picture. Therefore the results are to some extent expected.*

**Authors:** Contrary to the referee's observation, the disorder *does not commute* with the drive. Disorder is always present in the longitudinal interactions, while the drive is coupled to the transverse component of the spins (please see the Hamiltonians in Eqs. (2) and (4)). Hence, the simplicity envisaged by the referee is clearly absent in this case.

**(A3) Referee A:** *On the other hand, disorder affects the spectral properties and may lead to additional difficulties and surprises. Thus, the present numerical demonstration that dynamical localization works also for disordered systems is less trivial than it seems and has a certain impact, in particular since the time-evolution of a physical quantity is considered. Moreover, studies of driven disordered systems are scarce.*

**Authors:** We concur with the referee.

**(A4) Referee A:** *Balancing these two aspects, I judge that the second one slightly outweighs the first and, thus, tend towards publication in PRL.*

**Authors:** Since the first objection is void (as we pointed out above), we sincerely believe that our referee will strongly recommend the publication of this work in PRL based on the rest of his/her evaluation.

**(A5) Referee A:** *However, I recommend to consider the following issues:*

*1. The statement "Floquet showed that for a T-periodic Hamiltonian... [39]" implies that back in 1883, Floquet had studied time-dependent quantum systems, which is obviously wrong.*

**Authors:** We apologize for the misleading sentence. We have corrected it in the current manuscript.

**(A6) Referee A:** *Caption of Fig.2: the inequality  $J \gg 0$  is meaningless, because any (positive) quantity is "much larger than zero".*

**Authors:** We thank the referee for pointing out the typo - it should be  $\mathcal{J}_0(\eta) \gg \alpha/\omega^2$ . We have corrected it in the current manuscript.

**(A7) Referee A:** *Unlike stated by the authors, the limit  $\omega \rightarrow \infty$  is not "counter intuitive". In contrast, it is the limit in which the high frequency approximations considered in many of the cited papers works best. It also contradicts the authors' statement that the effective Hamiltonian is an expansion in  $1/\omega$ .*

**Authors:** We appreciate that the referee has brought up this point. We completely agree with the referee that the very fact that our approximation works better as  $\omega$  is increased is not counter-intuitive. This, in fact, lies in the very structure of the approximation scheme, as the referee has correctly pointed out.

However, we are pointing to a very different aspect which is counter-intuitive from a physical perspective. Suppose one applies a sinusoidal drive to a system with a frequency  $\omega$ . As one increases far beyond the characteristic response frequency of the system, the response to the drive becomes lesser and lesser. Thus, in the limit  $\omega \rightarrow \infty$ , no effect of the drive is expected to be felt at all. The rationale is based on comparison of two time-scales: the response time of the system (be it quantum or classical) and the drive period, and holds in several scenarios of driven systems - both classical and quantum.

Here, we see that this expected scenario applies for most values of drive parameters  $h_0, \omega$ , except for a few magic values for which  $\mathcal{J}_0(4h_0/\omega) = 0$ . At those values of the drive parameters, a strong effect of the drive is observed *even* when  $\omega \rightarrow \infty$ . This is what we considered to be counter-intuitive. We have made an attempt to make this more clear in the current manuscript.

**(A8) Referee A:** *The limit  $\omega=0$  is not well defined, because there the resulting time-independent Hamiltonian depends on a possible initial phase. I recommend to denote the undriven limit by  $\omega=0$ .*

**Authors:** We agree with the referee, and have changed the notation in the current manuscript.

**(A9) Referee A:** *The references account mainly for works on closed driven quantum systems, i.e., in the absence of any environmental degrees of freedom. However, the present disordered system has some relation to driven quantum systems that couple to a heat bath or an electron reservoir. I recommend a literature search and to add some references, as well as a reference to the following works on driven disordered quantum systems: Kaiser, New J. Phys. 10, 065013 (2008); Gopal, Phys. Rev. B 81, 195415 (2010).*

**Authors:** We thank the referee for his advice, and have implemented it in the current manuscript.

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Response to Referee B -- LW14057/Roy

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**(B1) Referee B:** *The manuscript reports on the effect of a strong periodic driving field on an Ising-type spin chain (with a transverse driving field). I did find some aspects of the manuscript quite interesting but am not convinced that it meets the criteria of broad interest and importance required for publication in Physical Review Letters. For one, it is the latest of a series of papers by the same authors examining suppression of the hopping terms (or here, equivalently the magnetic interactions between neighboring sites) of a tight-binding Hamiltonian (refs 29,30,31,26), which is itself a variant of the Coherent Destruction of Tunnelling (CDT) problem. The new ingredient in the present manuscript is the randomness in the intersite coupling terms and in Fig 3, the onsite energies.*

**Authors:**

The primary objection of the referee remains unclear to us. Does he object to the fact that the same set of authors have published works concerning the suppression of hopping in ordered systems, or is it his opinion that the fate of dynamical many-body localization in a strongly disordered system as reported in this manuscript for the first time (he implicitly acknowledges the originality in his 'new ingredient' remark) is not novel enough for PRL?

The first objection has no logical basis. Publishing works in a field earlier does not debar any researcher from publishing substantially new and long-awaited developments in the same field in PRL at a later time. We have evaluated the substantiveness of this development in our response to the next objection, detailed below.

The second objection concerning the lack of novelty of this development, has no scientific basis either. It is well-known that introduction of strong defects in a quantum system can change the physics to such an extent that almost no

conclusion can be reached based on the results known for defect-free systems. This holds even more strongly when the focus is on the coherent dynamics of an externally driven many-body systems (**Referee A** has acknowledges this). This is also reflected in the essential change in our formalism – in the previous (disorder-free) cases we could work with simple two-level systems, while here we need to consider L-level coupled systems (L, the system size, diverges in the thermodynamic limit), and a simple rotating wave approximation had to be replaced radically by the state-of-the-art machinery of Floquet - R.G. flow-equation formalism.

**(B2) Referee B:** *It is interesting that the elimination of the spatial periodicity does not impede suppression of the hopping terms.*

**Authors:** Contrary to our referee's observation, disorder **does indeed** impede the suppression of the decay of the transverse magnetization (the 'hopping term' alluded to by the referee). While in the absence of disorder the magnetization acquires a steady non-zero DC value which remains stable even as  $t \rightarrow \infty$  (see, e.g. Ref. 31 of the present MS), in the presence of disorder the average magnetization **decays to zero exponentially with time** with a finite characteristic decay scale  $\tau$  (our main result shown in Figs. 1, 3). This time-scale is finite for any finite drive frequency  $\omega$ . This remnant of dynamical localization manifests itself in steep jumps in the value of this decay time-scale  $\tau$  at certain special drive frequencies and amplitudes.

**(B3) Referee B:** *However, I am not sure how surprised one should be; CDT is often justified in terms of a simple two-site model so complete suppression of tunneling at high frequency does not necessarily require spatial periodicity.*

**Authors:** It seems, **referee B** him/herself is not quite sure about whether or not to be surprised with our results. We try to clarify this below.

As discussed in **(B2)**, we **do not** observe CDT happening here, so there is no question of justifying it.

So far as the spirit of the work is concerned, the entire point was to probe how robust the phenomenon of dynamical localization in many-body system is, when the scenario is not simple enough so that it can be mapped to two-level systems.

This, we believe, is a question of deep and broad significance, since the tantalizing phenomenon of dynamical localization has so far been observed only in *simple systems* e.g., single particle systems, the two-site systems (regardless of translational invariance) as the referee has pointed out or spin/bosonic systems mappable to simple two-level systems. For a long time, the folklore has been that dynamical localization can be expected only in especially simple settings, since it requires perfect cancellation of all transition amplitudes due to (apparently) fine-tuned destructive quantum interference. The strong disorder we introduced in this scenario is expected to test this long-standing picture, and it does so quite successfully with a dramatic outcome – dynamical localization does not persist, but it leaves behind a strong and spectacular remnant.

To summarize, the judgment of the referee is (at best) a result of his/her biased reading of the MS. He/she seems to have missed the entire point and reached incorrectly conclusions right from the outset.

**(B4) Referee B:** *Suggested applications to current experiments with cold atoms in disordered optical lattices are not very convincing: suppression of transport in that case is not an important consideration, quite the contrary given studies of Anderson localization.*

**Authors:** We of course agree with the referee! The experiment referred to did not explore the fate of dynamical localization in presence of disorder (otherwise we would not claim the novelty of our work). We referred to the experiment as a potential set-up where our ideas could possibly be realized experimentally.

**(B5) Referee B:** *The CDT renormalization has already been demonstrated with cold atoms (refs 22 and 23) (here I do not intend to suggest that this is not of interest, but am addressing PRL criteria of novelty).*

**Authors:** It is unfortunate that the referee could provide such a lame reason to justify his/her wish against publication of our work in PRL! That CDT has been demonstrated experimentally is obviously known, and one of the authors of the present article was also involved in the experimental demonstration of its many-body version for spin systems (Ref [26], present version).

But the present work has a completely different focus and content – it is **not about demonstration of CDT**, but about how **disorders** affect dynamical localization (or CDT) in a many-body system. **This is the first work to investigate the fate of dynamical localization in presence of disorder** (the issue of novelty has been discussed in detail in **B2** and **B3**).

**(B6) Referee B:** *Therefore I believe that this paper would be most appropriate for further consideration by Physical Review B.*

**Authors:** We honestly disagree with this biased judgment for the reasons discussed above.

**(B7) Referee B:** *It is not at all easy to read at present. Too many symbols are undefined and equations are carelessly written and defined. In fact almost every important equation has a problem or typo suggesting that the manuscript was produced in a rush.*

**Authors:** We sincerely apologize for the typos. We have corrected them all in the present version.

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