This paper investigates the effect of periodic pulse pumping on a spin system in which the energy-exchange processes are affected by the presence of an intermediate highly-excited decaying state. The latter is represented by a high-energy trion-state term which forms the model Hamiltonian together with the electron-spin, nuclear-spin, and spin-spin interaction terms. The presence of the trion state combined with the commensurability of periodic pulses (to the characteristic time of internal processes) allows one to show how long trains of pulses enact a distillation process leading to a "coherent" quantum state in which the disorder of the initial state is essentially suppressed. The evolution of the density matrix describing the system dynamics is governed by a Lindblad equation in which a standard damping term takes into account the dissipative effects relevant to the trion-state decay. Numerical simulations also allow to determine the entropy properties of the final "coherent" state and, particularly, to highlight the entropy reduction triggered by the combination of pulse trains and dissipative effects in the asymptotic quasi-stationary regime.

The paper is well organized. After briefly reviewing the basic information about the model Hamiltonian and the mathematical aspects of the evolution protocol (sections 2 and 3), focuses the reader attention on i) the entropy properties and ii) the dependence of the distillation process from the number of pulses. These aspects are extensively discussed in sections 4 and 5. Further details about important but technical aspects of sections 2 and 3 are given in three final appendices which concern 1) the linear mapping representing the pulsed dynamics in the Lindblad picture, 2) the mathematical derivation of the properties stated in section 3, and 3) the entropy for different distributions of the Overhauser-field hyperfine interactions.

The results presented in this paper are, in general, technically sound. The system dynamics is studied by using the well-established Lindblad formalism and is supported by a non trivial but detailed discussion on the density-matrix linear mapping, the core mechanism of the evolution.

The central result of this paper, the new (to my knowledge) mechanism inducing the entropy reduction and the transition to an ordered regime (due to the commensurability condition), is very nice and interesting. In general, I expect that the analysis developed in this paper should stimulate further experimental and theoretical work.

A revision, however, is necessary to improve the readability of this paper. To this end various unclear points or comments should be modified and several errors should be removed. These are mainly concentrated in Section 4, the most important part of this paper. This section also features a very technical style and many comments are made (or formulas are used) with the implicit assumption that any reader can understand them. The author does not provide the information necessary to understand (at least, in general) many aspects and intermediate steps of the discussion made in section 4. Useless to say that supporting comments and formulas by introducing many citations is not sufficient to compensate the absence of clarity.

Comments

1) Formula 5. The definition of operator U_{puls} is not clear as well as the terms "istantaneous" and "unitary". Since U_{puls} is a hermitian operator the use of term "unitary" should be justified and its role in describing a laser pulse should be explained.

2) At page 4, the origin of the distribution of couplings J_i (Overhauser-field distribution) should be briefly discussed and the (physical) reason why one can consider different distributions (within the current model) should be explained.

3) page 5, lines 5-6: if the time unit is $\hbar/J_q$, then the trion decay rate should be $2\gamma = 2.5\ J_Q/\hbar$ while the trion life time should be 0.4 ns instead of 0.4 ps.

4) The author considers distributions (8), (9) and (10) which, apparently, are constructed in an arbitrary way. As noted above, some information about this freedom could improve the clarity of this paper. Also, I cannot understand why these distributions depend on J_{max} but are independent from J_{min}.

5) Page 5. The comment below eq. (11) "The commensurability of these resonances is crucial for the advocated mechanism" must be improved. A similar comment is already present in the Introduction and the commensurability condition is again mentioned below eq. (8). The author does not state with the necessary clarity what are the internal processes (and the relevant characteristic times) which allow one to define the commensurability condition. Even if this is very obvious to the physicists of this specific research field, I am afraid that it is not so obvious to non expert readers.

6) A factor $\hbar$ is missing in $hT_{rep} = 2\pi n$ and $zhT_{rep} = 2\pi n'$.

7) The author should better explain how the two formulas defining the Larmor-precession resonances $hT_{rep} = 2\pi n$ and $zhT_{rep} = 2\pi n'$ (defined after formula (10)) are related to the periodic maxima (or minima) of S in fig. 1. Two nested resonances are apparently discernible in fig. 1 (see the relevant comment in the text). However, after stating the presence of such resonances, the author apparently contradicts this claim observing that these conditions are not applicable without pulsing. It is hard to understand this point.

8) The caption of Fig. 1 states that "Resonances of the electronic spin occur every $\Delta h = 0.5 J_{max}$". However, looking at the figure and observing that the h units are J_Q, the separation between subsequent electron-spin resonances seems to be $\Delta h = 0.5 J_Q$. Concerning the resonances of nuclear spins these should occur every $\Delta h = 500 J_{max} = 10 J_Q$: figure 1a (right panel) suggests that the shift is apparently 10 units, if this is referred to the value $h = 500 J_Q$. However, why this value should be the reference value

is not very clear.
The blue dashed line depicts the offset from the nuclear resonance (placed at $h = 509.5J_Q$) defined by $\Delta h = \pm J_{max}/(2z) = \pm 500 J_{max} = \pm 10 J_Q$, but figure 1b (left panel) apparently shows that the offset is $\Delta h = 0.5J_Q$. Concluding, it seems really hard to identify the shifts mentioned in the comment "The driven systems displays important shifts".

The caption of fig. 1 is very confused and the corresponding comment in the text does not clarify the rich information encoded in it. Due to its relevance, this part should be carefully checked and its clarity significantly improved.

9) The comment (see below eq. (11)) "where A_{max} is the maximum Overhauser field" should be "where A_{max} is the maximum value of the Overhauser field".

10) Page 9: To achieve a satisafactory understanding of the discussion concerning figure 4 (and of the benefits due to the spread reduction) is obiously related to the referee comment 4 about the alternative parametrization of J_i.

My evaluation of this paper is, in general, non negative. The authors analyze adiabaticity in nonlinear systems and study, in particular, the dynamical stability for the coherent population transfer based on the STIRAP process in a (nonlinear) 3-level quantum system with time-dependent parameters.

1) In the introductory part of this manuscript comment [8] rather obscure. However, the subsequent comments <<Mean field approach has shown...>> and <<We recast the mean-field model ... and use the classical adiabatic theory in [12] >> well clarify what is the theoretic framework to which this paper refers. This is the important information for the reader.

In my opinion, both comment [8] and the comment on BEC researchers (line 8-9, page 1) should be eliminated because are vague and thus useless. Certainly, physicists of the BEC area are not necessarily expert in nonlinear dynamical systems. It is true as well that experts in nonlinear systems some times propose models or study problems neglecting important aspects of real systems. What is sure is that this kind of dispute is essentially useless.

If the authors feel that some ideas, methods or applications in reference [9] are wrong they must state their criticisms in a clear way by writing a Comment rather than submitting a Letter. Elliptical comments do not improve the paper readability and can substituted with more useful observations.

2) Based on the recent paper [9] focused on the same topic, the authors point out counterintuitive difficulties inherent in performing an effective analysis of adiabaticity.

After showing that in case of zero detuning ($\Delta = 0$) the 3-level model becomes integrable, the authors study the regime where the constants of motion are zero ($I_1 = I_2 = I_3 = 0$) and thus only variables x_1, x_3, y_2 evolve in time while y_1, y_3, x_2 are zero. Within this regime (including the initial state considered in [9] where all population is in state $|a>$) and following reference [12], they derive the condition for adiabaticity embodied in formula 10. The comparison of the resulting adiabatic parameter with the adiabatic parameter of reference [9] is then effected. This aspect and the related discussion are very interesting.

Notice that A) in the caption of Fig. 1, interval $4.5 > t > 1.5$ rather than $3 > t > 1.5$ seems to describe correctly the adiabatic evolution in panel 1b, B) the A-dependent terms in the matrix of formula 4 contain an undue factor 1/2, C) the relation between variables ψ_q, $q = a$, e, g and x_k, y_k, $k = 1, 2, 3$ is not explained (see after equation 6).

3) In the second part of the manuscript, the authors consider the regime $H \neq 0$ where dynamics involves two degrees of freedom (four canonical variables). The definition of Q_i, P_i in terms of the old variables should be better explained. The authors could evidence how only x_2, x_3, y_2, and y_3 are contained in the quadratic part of H and thus are involved in the definitions of Q_i and P_i. Variables q_i and p_i are meaningless (see after formula 11).

This part, however, is rather confused and leaves the impression that it has been written hurriedly. Certainly it is not written in a way suitable for a broad audience. Formula 11 shows that $P_i = 0 = Q_i$ is a saddle in that $w_1 > 0$, $w_2 < 0$. What does it mean the comment << The frequencies are always real>>? Apparently, this fact is quite obvious. Are the presence of H_3 together with the condition $w_1 > 0$, $w_2 < 0$ the reasons why the equilibrium is not stable? This is not explained clearly.

4) The subsequent counterexamples [the authors should state that Formula 12 is derived by substituting in $H = H_2 + H_3$ the new action-angle variables (R_i, ϕ_i)] involving resonances 1:1 and 1:2 show that the time average of H_3 vanishes thus eliminating the only possible source of instability. This seems to be confirmed at page 8 line 1 (Even though the system is stable at fixed parameters...). At this point, however, one wonders whether the vanishing of the time average of H_3 is a condition sufficient to claim that dynamics is stable. This seems to be related to the phrase <<... stability is accidental>> in the abstract. This is another point that should be clarified.

The fact that the authors are writing the extended version of this letter (see comment [13]), in addition to the fact that parameter ϵ is undefined (page 8, line 4), does not help the reader in understanding the final part where the authors say that <<adiabaticity may be broken at resonances ...>>.

5) A stimulating result in this paper is that the criteria for adiabaticity used in nonlinear classical systems (ref. [12]) leads to a parameter A_{nl} giving predictions different from those evidenced by parameter r_{nl} of reference [9]. In my opinion, after reading paper [9], this is not sufficient to conclude that r_{nl} is not reliable. Comment <<predictions of r_{nl} are not very good>> in the caption of figure 3 is thus unjustified and should be substituted with a more appropriate comment.

Concluding, this paper contains interesting result and its publication might stimulate further work on the interesting problem of adiabaticity. However, the clarity of this paper is not satisfactory in several points and thus its present form is not suitable for publication in PRL.