Reply to Andrew Hodges

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We separate the criticisms of Hodges [1] and others into those against the algorithm itself and those against its physical implementation. We then point out that *all* those against the algorithm are either misleading or misunderstanding, and that the algorithm is self consistent. The only central argument against physical implementations of the algorithm, on the other hand, is based on an assumption that its Hamiltonians cannot be effectively constructed due to a lack of infinite precision. However, so far there is no known physical principle dictating why that cannot be done. To show that the criticism may not be a forgone conclusion, we point out the virtually unknown fact that, on the contrary, simple instances of Diophantine equations with apparently *infinitely precisely* integer coefficients have *already* been realised in experiments for certain quantum phase transitions. We also speculate on how central limit theorem of statistics might be of some help in the effective implementation of the required Hamiltonians.

Introduction

Since our first proposal in 2001 of an algorithm employing quantum adiabatic processes to render the classically noncomputable Hilbert's tenth problem into that of a quantum mechanically computable [2, 3, 4, 5], there have been a lot of interests as well as criticisms. That situation is inevitable for such seemingly radical a claim against the accepted and accustomed wisdom. On the other hand, it is also a natural and healthy state of scientific research wherein all new proposals must be subjected to rigorous examination against what is currently known. In our case, however, most of the criticisms are not in the formal literature but only in informal discussions in private or on internet forum and at the many seminars and conference presentations that we have given. We have tried to collect all the criticisms known to us and evaluate them one by one, for instance, in one of our recent postings [6].

This paper is to analyze all the points raised by Andrew Hodges [1]. We first note that most of those points have already been raised before by others to which we have already discussed and addressed accordingly, for example in [6]. But as Hodges has also included few new objections among them, we will address all of them here again in some details.

We will make the distinction between the algorithm itself and its physical implementation in the below.

Against the algorithm, all the points, except one, raised by Hodges are just misunderstandings, which we will try to clarify in the next Section. The only exception is Hodges's reference to a paper by Smith, who has provided a counterexample [7] to our previous claim about a sufficient condition for a criterion that employs measurement probability to identify the final true ground state among the infinitely many available states in the quantum adiabatic algorithm. We have investigated the counterexample and identified the cause for the insufficiency of our previously stated condition [8]. Subsequently, we have given a mathematical proof [9] for the same criterion, validating the mathematical and logical consistency and correctness of our proposed algorithm. We will not repeat those arguments here.

With respect to the question of physical implementation of the algorithm, Hodges has reiterated once again the issue of infinite precision which has been raised elsewhere by others before. We will discuss our responses [6] to that issue further in more details in the second last Section.

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Fundamental concepts for a basis for further discussions

We should note firstly that there is a whole hierarchy of the noncomputables [10]; that is, some are more 'noncomputable' than others. Computability of certain noncomputable, if could be ascertained, does not mean that *all* the noncomputables are then computable. Our claim of (quantum) computability is restricted in that sense, it is only applied to Hilbert's tenth problem, or equivalently the Turing halting problem, or any equivalent problem. (Note also that such equivalence is not trivial at all; it took more than 70 years and generations of mathematicians to establish the equivalence of Hilbert's tenth problem and the Turing halting problem.)

What constitutes, in particular, the noncomputability of Hilbert's tenth problem? For each Diophantine equation without any parameter, there is nothing noncomputable about whether that particular equation has any integer solution or not. There always exists a finite mathematical procedure to find the answer for such a question for any single Diophantine equation (but not always with a *family* of parametrised Diophantine equations). If we have not yet had a (recursive) procedure to obtain that answer then it does not mean that such a procedure is ever out of reach. In that regards, noncomputability is similar to the concept of randomness; there is nothing random about a *single* bit or number by itself. (The notion of randomness only applies to a series of such bits or numbers which does not have any statistical pattern or, if we appeal to algorithmic information theory, does not have a pattern which can be encoded more effectively and economically than the length of the series.)

Nor there is anything noncomputable about a collection of *finitely* many Diophantine equations, because we can always concatenate all the procedures for all the equations (as there is always a procedure in principle to determine the existence of solution for each equation) into a *finitely* collective procedure that can be applied to the whole collection.

What constitutes the noncomputability of Hilbert's tenth problem is the fact that we ask for a *single* finite procedure which can be applied to all the elements of sets of *countably* many Diophantine equations. The application of Cantor's diagonal arguments to the Turing hating problem, see for example [11], has established that were there a finite and recursive Turing machine that can be applied to determine the halting of, or lack of, any Turing machine then contradiction and inconsistency would have to follow. What that means for Hilbert's tenth problem is that there is *no* single finite recursive algorithm for *all* Diophantine equations, but for each given equation we have to find a recursive algorithm *anew* each time.

We could confirm the fact, which Hodges has also mentioned, that if we could resolve Hilbert's tenth problem in the positive then we would be able to resolve not only the Riemann hypothesis but also the Goldbach conjecture, Fermat's last theorem, and the four-colour problem, to name a few. And we would be able to do that with a *singly unified* approach too. This is because these problems belong to a class of problems each of which has a Diophantine representation – namely, the resolution of each problem can be rephrased in terms of the existence or lack of solution of some Diophantine equation corresponding particularly to that specific problem. (On the other hand, as there is nothing sacred or noncomputable about a single parameter-free Diophantine equation, the resolution of Hilbert's tenth problem has no other consequence otherwise on the individual equation.)

To illustrate that Cantor's arguments cannot rule out hypercomputation (for a general discussion on this issue, see [11]), we have pointed out that the probabilistic arguments of our algorithm can avoid the usual Cantor's arguments and thus leads to no contradiction. Nor there would be any contradiction if our quantum algorithm cannot be encoded with a Gödel integer. And this is how we could reconcile the quantum adiabatic algorithm with Cantor's diagonal arguments in a consistent manner. Certainly the probabilistic nature of the algorithm is not sufficient to imply that it is hypercomputational. For that, we have had to construct and show explicitly that it could, as a single algorithm, resolve any instance of Hilbert's tenth problem. A criticism, as one of Hodges', on the probabilistic nature of our algorithm not only misses the point but also is not supportable.

We always have explicitly stated and repeatedly emphasized the probabilistic nature of our algorithm in that it can determined up to any predetermined probability whether or not a particular Diophantine equation has any solution. That probability can be chosen arbitrarily close to one, but can never be one (except in the event the equation has a solution, in which case we could verify by substitution). If Hodges and other mathematicians are not willing to accept that as a way to nonrecursively resolve Hilbert's tenth problem then we would not be able to share any common ground at all, and perhaps no further exchange is possible. If that is the case, we still must point out, as a parting remark, that there is as yet *no* classical recursive algorithm which can resolve the problem in the same probabilistic manner as the quantum algorithm.

The questions of various infinities in the algorithm

Hodges and others often are puzzled by the apparent ability of the quantum algorithm to *explore an infinite space in a finite time*. That puzzlement comes from a simplistic expectation that in order to determine whether a Diophantine equation has any solution or not one would have to explore the *whole* integer space. Such an exploring task apparently could not be accomplished in a finite time in the case the equation has no solution at all. As a matter of fact, the expectation is too simplistic and not quite correct. Were it correct then we would easily be able to, and certainly not need the sophisticated Davis-Putnam-Robinson-Matiyasevich theorem [12] and then Cantor's diagonal arguments, to establish the noncomputability of Hilbert's tenth problem. The fact of the matter is for each Diophantine equation we only need to explore a *finite* domain in the space of appropriate tuples of integers; the equation has a solution *if and only if* the solution resides in that finite domain (bounded by the so-called test function, see Davis [13]). This property is quite remarkable and is applicable to a wider class of so-called *finitely refutable* problems, see a theorem in [14]. Once such a finite domain is known for a Diophantine equation, it is just a matter of substitution of a finite number of integers to determine if the equation has any solution at all. The noncomputability of Hilbert's tenth problem comes from the fact that there can be no *single* finite recursive procedure to determine such a finite domain for each and every Diophantine equation. There must be, in other words, at least one Diophantine equation that is not susceptible to the treatment of any given recursive procedure.

On the other hand, we claim that a single quantum adiabatic procedure, as described by the quantum algorithm, can be applied to each and every equation. Surely, in a finite time the quantum mechanical (normalisable) wave function can only spread out from its initial wave form to explore an effectively finite domain of the underlying Hilbert space. But the domain so explored is the relevant domain sufficient for the purpose of finitely refuting or confirming the existence of solution for the equation. The finitely refutable character of Hilbert's tenth problem manifests itself in the quantum algorithm as the finiteness in the energy of the final ground state and in the time that this ground state can be obtained and identified (by our identification criterion via a probability measure [9]).

To drive home the point that only a finite portion of a space can be explored in a finite time, a fact with which we do agree, Hodges has also mentioned Grover's search algorithm [15] in an unstructured database to erroneously imply that quantum adiabatic computation cannot search more efficiently than the time complexity offered by Grover's algorithm (which is of the order of the square root of the database size) and thus cannot compute Hilbert's tenth problem – a statement with which we disagree. Such a statement is misleading and incorrect in at least three aspects. Firstly, quantum adiabatic computation could accomplish the search in a time *independent* of the size of the database, provided sufficient energy must be supplied, see the references [16, 17, 18, 19]. The energy, secondly, need not be proportional to the square root of the database size, quantum entanglement can reduce substantially the required energy, as demonstrated in a quantum adiabatic algorithm for the NP-complete travelling salesman problem [19]. And thirdly, for any instance of Hilbert's tenth problem, as we only need to search in a finite domain appropriate for the specific parameter-free equation under consideration thanks to the finitely refutable property, both the energy required and the time taken are *finite* for a successful execution of the quantum algorithm in the infinite underlying Hilbert space. Some finite information about the final ground state is that all we could have, there is no infinite amount of information here as misunderstood by Hodges.

Metaphorically speaking, the initial ground state of the quantum algorithm provides one end of a *finite* string along which we can trace to the final ground state at the other end. This can be done quantum mechanically because of the quantum adiabatic theorem, which asserts that a particular eigenstate of a final-time Hamiltonian, even in dimensionally *infinite* spaces, could be found mathematically and/or physically *in a finite time*. This is a remarkable property, which is enabled by quantum interference and quantum tunneling with complex-valued probability amplitudes, and in principle allows us to find a needle in a particular infinite haystack! Such a property is clearly *not* available for classical recursive search in an unstructured infinite space. Being built on these principles, our algorithm is far from being a simple brute-force search as claimed by Hodges.

The applicability of quantum adiabatic theorem here is warranted as we have shown that [5], for the quantum adiabatic processes of the algorithm, there is no level crossing in the spectral flow not only between the instantaneous ground state and the instantaneous first excited state but also between *any* pair of instantaneous eigenstates. Interestingly, there are other versions of quantum adiabatic theorem [20] which do not require non-zero gaps between the energy levels. We will not need it here but may need it later to resolve a technical degeneracy problem for our algorithm, see below.

Incidentally, we have also been able to derive a lower bound on the computation time T for a general quantum adiabatic computation,

$$4 < g(\theta)T\Delta_I E.$$

This lower bound on the computation time incorporates the initial ground state and the spectrum of the final Hamiltonian together in $\Delta_I E$, which is defined as the energy spread of the initial state in terms of the final energy. The manner of the time extrapolation is further reflected in $g(\theta)$. This condition is applicable to finite and infinite spaces, and states that the more the spread of the initial state in energy with respect to the final Hamiltonian, the less the lower bound on the running time. It thus also emphasizes the fact that energy must be considered in the running of quantum adiabatic computation, and perhaps in all computation if they all are indeed physical in the end. Energy, thus, should be another dimension of algorithmic complexity in addition to those of time and space.

Because of the finitely refutable character of Diophantine equations, we do not, contrary to naive expectation, really need an infinite space to carry out the algorithm for any given parameter-free Diophantine equation. A finite but sufficiently large space will do. Once the final ground state has already been included in this finite space the addition of further Fock states which are highly excited states of the final Hamiltonian will only change the dynamics negligibly, as can be inferred from a set of infinitely coupled differential equations that we have derived in [4].

We might or might not be able to devise a procedure according to which we could tell whether, for a given Diophantine equation, the underlying Hilbert space even though finite is sufficiently large. But in order to keep the algorithm simple at this level, we prefer to employ infinite spaces in the algorithm, which are quite appropriate for the set of all Diophantine equations (but not quite necessary because an unbounded underlying space might do). We have to point out here again that dimensionally infinite Hilbert spaces are not exceptions in quantum mechanics, they are, rather, the norm. Without infinite Hilbert spaces, for instance, we would never have had any realisation of the fundamental commutator

$$[\,x, p\,] = i\hbar.$$

(To wit, were x and p finite matrices, the mathematical trace of the lhs would vanish, contradicting the non-vanishing trace of the rhs!)

The last point of this Section is about possible degeneracy of the final ground state which originates from possible multiplicity of the globally minimum value of the square of the Diophantine equation. We have proposed the use of additional symmetry breaking terms in the final Hamiltonian to avoid the degeneracy, before asymptotically removing these terms in some perturbation-theory treatment in order to recover the original Hamiltonian. We do not quite understand Hodges' objection to the application of perturbation theory and therefore cannot comment on his objection, except only wish to mention here that we can show that any degeneracy (even with infinite multiplicity) can indeed be lifted with the introduction of the symmetry breaking terms. A mathematical proof for this is exactly the same as that for the no crossing of any pair of instantaneous eigenstates in the spectral flow we have already mentioned above.

Apart from the proposed use of symmetry breaking terms, the problem of ground-state degeneracy can certainly be removed if there is indeed a *single-fold* Diophantine representation for every listable set [13]. The existence of such a representation is, however, still an open problem at present time. The same degeneracy problem for our algorithm could perhaps be removed with a suitable application of the versions of quantum adiabatic theorem which require no gap between the levels [20]. But this is only a speculation at present and would require further investigation elsewhere.

Is the algorithm physically implementable?

As distinct from the criticisms above on the quantum algorithm, we now move to the question of its physical implementability.

The issue of infinite precision that Hodges mentioned has in fact been raised and discussed before [6]. Martin Davis was the first person who brought to our attention around 2002 that a lack of precision in an implementation of the integer coefficients of Diophantine equations, for example, those in the equation

$$x^2 - 2y^2 = 0,$$

would lead to a wrong representation of, and thus to a wrong answer to, the original equation. And it is thought that this precision problem cannot be overcome in any possible implementation of the quantum algorithm.

Before analyzing the issue, we have to point out here that Hodges' use of the example of quantum simple harmonic oscillator to dismiss the physical feasibility of our algorithm is inappropriate. The lack of infinite precision in the characteristic length, or equivalently in the frequency, of the oscillator is irrelevant here. We only need to distinguish one quantum from two quanta and in order to do so we do not need infinite precision in the oscillator frequency, $\Delta\omega = 0$, because it suffices to have $(\Delta\omega/\omega) \ll 1$. That is how we are able to count photons, and confirm the fundamental quantisation nature of light, even though we do not have absolute precision in their frequencies. Likewise for our algorithm, sufficiently precise finite precision also suffices for the purpose of separating the integer-valued energy eigenvalues of the final Hamiltonian.

Coming back to the issue of precision of the coefficients in a Diophantine equation, we need to distinguish the infinite precision from the unbounded precision. Infinite precision, the kind of perfect precision for any digit and all digits of an infinite number of digits of a measured value, is what Hodges and some other people claim to be necessary for an implementation of the algorithm. With an assumption that such an infinite precision *per se* is not available to our physical instruments, even in principle, they have come to a negative conclusion accordingly. What irrefutably available to our instruments, instead and at least in principles, is the second kind of precision wherein we can obtain an accuracy for an unbounded but finite number of digits, as many digits as we want provided we have the time and the resources to do so. There exists no physical principle against this kind of precision – even though we may have to pay the price in, apart from the time and resources required, some correspondingly worse accuracy for a conjugate quantity as demanded by some quantum mechanical uncertainty principle, but that is alright for our purpose. Our opinion is that it may *not* be a forgone conclusion, as some may have thought, that the physically available precision is not adequate for an implementation of the proposed algorithm.

Firstly, our ultimate objective is to obtain the ground state of a desired Hamiltonian, and the algorithm is 'just' a means to that end, but a universal means nevertheless. In saying that the negative resolution of Hilbert's tenth problem of the (platonic) mathematical world also carries over to the physical world in such a way that we could never achieve the above objective is equivalently to saying that we would never be able either to identify some ground states, or to construct some suitable Hamiltonians, or both. That would have been a very stringent constraint on the physical world and would have resulted in an entirely new physical principle – but we do not have any reason or anything to support this negative conclusion.

We, as yet, have absolutely no 'no-go' physical principles which dictate that there must exist a physical system which we cannot cool down to the ground state, or in which there must exist a physical limit of a distinctively non-zero temperature beyond which we cannot proceed any further. The third law of thermodynamics, and its generalisation to the quantum domain, only states that we should not be able to obtain exact absolute zero temperature, or equivalently, that we should not be able to obtain a ground state with total certainty, but that is fine as we do not need to obtain a ground state with unity probability in order to identify it – see our probability criterion for ground state identification [9]. But the third law does not demand that there must be a non-zero lower bound on achievable temperature, or equally, that there must be an upper bound of obtainable probability for a ground state. (If anything, the postulate of projective measurement in standard quantum mechanics may contradict some classical statement of the third law; we may discuss this interesting issue elsewhere.)

The experimentally confirmed phenomenon of the *thermal* phase transition of Bose-Einstein Condensation in some kind of traps, for example, otherwise demonstrates that collective quantum effects do enable us to obtain and identify some particular ground state with arbitrarily high probability and achieve temperature arbitrarily close to absolute zero.

Likewise, there exists no known physical principle against the possibility of effective construction of any desirable Hamiltonians. In fact, an effective realisation of the quantum adiabatic algorithm for the special case of linear Diophantine equations has already been achieved recently [21], even though it is only known by others as the *quantum* phase transition of superfluid to Mott-insulator. We refer the readers to the Appendix for some detailed discussion of our claim that this interesting and seemingly unrelated quantum phenomenon is indeed connected to the quantum algorithm. In a few words, quantum phase transition is different from thermal phase transition in that, while the latter originates from the *thermal* fluctuations due to the competition between the opposing requirements of minimising the energy on the one hand and of maximising the entropy on the other, the former phase transition originates from the *quantum* fluctuations due to another kind of competition between opposing quantum mechanically conjugate terms in the same Hamiltonian. The particular superfluid - Mott insulator transitions are captured by the Bose-Hubbard model which contains as apart of the Hamiltonian the term

$$(a^\dagger a)^2 - 2\tilde{m}(a^\dagger a) + \tilde{m}^2,$$

and thus can be viewed as some physical realisations of instances of Hilbert's tenth problem, namely the Diophantine equations,

$$x - \tilde{m} = 0.$$

This fact is surprising, extraordinary and of great consequence; and will be explored further elsewhere.

Granted that this is a very simple type of Diophantine equations, but our point here is to emphasize that we should not rule out the possibility of effective implementation of the quantum algorithm, because we may be able to count on the help of collective quantum mechanical behaviours as in the quantum phase transitions above or of something else.

Is there any other way that unbounded precision may suffice in general? We speculate that the Central Limit Theorem of statistics might be of some use here. Let us illustrate the idea with the first Diophantine equation above, and let us denote its lhs by $P(u, v; x, y)$,

$$P(u, v; x, y) = ux^2 - vy^2.$$

In realistic implementation, we can treat the parameters u and v as random variables with some *finite* variances and aim to have the averages $\bar{u} = 1$ and $\bar{v} = 2$. In an instance of implementation, the parameters take some random values (u_i, v_i), which may or more likely may not be $(1, 2)$, resulting in $P(u_i, v_i; x, y)$ as the input for the quantum algorithm, and consequently in a subsequent randomly distributed output (x_i, y_i), which is the location of the corresponding ground state. Central limit theorem, if applicable, would then ensure that the average, for a large number of repetitions N, of such outputs (x_i, y_i) from our algorithm should tend to a central value corresponding to the output from the input $(u, v) = (\bar{u}, \bar{v}) = (1, 2)$. As the average of these outputs should have a statistical spread that is inversely proportional to $\sqrt{N}$, we may be able to reduce this variance, by having sufficiently large N, until we could confidently confirm the (integer-valued) central value, which should be effectively the output for $P(1, 2; x, y)$.

While it is certainly less restrictive to assume infinite precision for the average values of the coefficients rather than the coefficients themselves, it is still unclear whether this assumption on the average values could be satisfied. (The in-principle unsatisfiability of this, on the other hand, would imply that we can never, as a matter of fundamentals, get rid of systematic, as opposed to statistical, errors.) Note also that in order to apply the central limit theorem we need to have a *finite* variance for the algorithm outputs. But, as the outputs (x_i, y_i) for randomly distributed inputs (u_i, v_i) may not be bounded from above, it is yet to be shown that the former could have a finite variance even if the latter does.

Obviously, this speculation of ours requires further investigations.

Concluding remarks

We separate the criticisms of Hodges and others into those against the algorithm itself and those against its physical implementation. We then point out that *all* those against the algorithm are either misleading or misunderstanding, and that the algorithm is self consistent – which is the best one could do for such a mathematical entity as the algorithm.

We suspect that all these confusions might perhaps be originated from a mix-up in thinking that Hilbert's tenth problem is the same as a search problem in an unstructured and infinite database. While the latter unstructured search problem is quite general and difficult, each parameter-free Diophantine instance of Hilbert's tenth problem *does* provide some structure, which is the information mathematically encoded in the equation itself, for a search for the global minimum of its square. Furthermore, the search space for such a minimum is always finite. To further highlight possible pitfall for such a simplistic comparison with unstructured search, let us consider the counterpart of Hilbert's tenth problem over the real numbers, that is, the question of a single universal procedure to determine the existence or lack of *real* solution for any given multivariate polynomial with *real* coefficients. Were the brute-force approach of unstructured search in the real numbers the only one available, one would have concluded that this would be another noncomputable problem because such a search in an infinite space of the reals, which is even 'larger' in cardinality than that of the integers, could have never been completed in a finite time. On the contrary, and perhaps fortunately, such a conclusion is wrong: Tarski showed that [22], unlike Hilbert's tenth problem, this counterpart problem is classical *computable* – thanks to the structures mathematically encoded in the polynomials themselves.

Thus, general considerations, as those of Hodges, borrowed from a general unstructured search not only are inapplicable to our algorithm but could also be quite misleading.

On the other hand, the only central argument against physical implementations of the algorithm is based on an assumption that its Hamiltonians cannot be effectively constructed due to a lack of infinite precision. To show that this may not be a forgone conclusion, we draw attention to the virtually unknown fact that, on the contrary, simple instances of Diophantine equations with apparently *infinitely precisely* integer coefficients have *already* been realised in certain experiments known as quantum phase transitions! We also speculate on how central limit theorem might be of some help in the effective implementation of the required Hamiltonians.

Computability should constitute of both consistency and also implementability. However, the issue of implementability can only be settled either in the negative by citing prohibiting physical principles, of which there is none known at present, or in the positive by actual and general demonstrations, of which there are so far only special cases connected to certain quantum phase transitions. The awaited final outcome must and can only be bounded by physical laws. Until it is settled one way or another, it should be remembered that premature and prejudiced judgement has never served us well.

APPENDIX A: LINEAR DIOPHANTINE EQUATIONS AND THE SUPERFLUID TO MOTT-INSULATOR QUANTUM PHASE TRANSITION

The recent experimental demonstration [21] of the superfluid - Mott insulator phase transition can be captured mathematically by the Bose-Hubbard Model [23]:

$$H_B \;=\; -J\sum_{\langle i,j\rangle}\left(a_i^\dagger a_j + a_j^\dagger a_i\right) + U\sum_i n_i(n_i - m), \tag{A1}$$

where J is the tunneling rate between neighbouring lattice sites i and j, and U is the strength of the on-site interactions. Here, $n_i = a_i^\dagger a_i$ is the number operator at site i, and m is some positive integer number. Initially, the system is in the superfluid phase which is the ground state of the first J-term of the above Hamiltonian, $|g_I\rangle = \left(\sum_{i=1}^M a_i^\dagger\right)^K |0\rangle$, where K is the total number of 'atoms' in the superfluid and $|0\rangle$ is the zero-occupation Fock state. This is approximately the coherent state at each site for large K and large number of sites M. A lattice is then raised adiabatically throughout the superfluid, leading to an exponential suppression of the tunneling rate J relative to U. At a certain critical value of the ratio U/J, the system undergoes a phase transition to a new state $|g\rangle = \prod_{i=1}^{M}\left(a_i^\dagger\right)^m|0\rangle$, which is the ground state of the second U-term in (A1). In considering a single site i and in the mean field approximation, we replace a_j $(j\neq i)$ in the Hamiltonian by its mean field $\langle a_j\rangle \sim \alpha$, which is also a measure of the coherence in the system. That leads to, up to c-numbers and with some α,

$$\begin{aligned}\text{the } i\text{-th term in (A1)} &\to -J\left(a_i^\dagger\langle a_j\rangle + a_i\langle a_j^\dagger\rangle\right) + U\,n_i(n_i-m)\,, \\ &\to \tilde{J}(t)\,(a_i^\dagger-\alpha^*)(a_i-\alpha) + \tilde{U}(t)\,(a_i^\dagger a_i-\tilde{m})^2 \equiv \tilde{J}(t)\,H_I + \tilde{U}(t)\,H_P\,.\end{aligned} \tag{A2}$$

In other words, this is just an implementation of our quantum adiabatic algorithm for the simple equation $x-\tilde{m}=0$. Physically, at each site we have initially the coherent state which is the ground state of some initial Hamiltonian H_I. At and beyond the phase transition, the coherence vanishes, $\langle a_j\rangle = 0$. We are then effectively left with H_P, completing an extrapolation to a final Hamiltonian whose ground state has $\tilde{m}$ as the occupation number, which is also the solution for the simple equation.

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