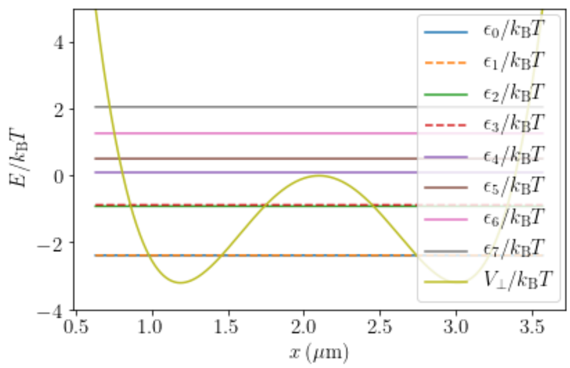
Reply to Report 1

We thank the referee for their positive assessment of our work and helpful comments. Our responses to the specific question the referee posed are as follows:

(1) The referee asks how strongly the third transverse level affects the low-energy physics. The answer is that this of course depends on the *precise* details of the splitting and phase imprinting protocols, and in particular how quickly the gas is split and the phase imprinted. In the parameter regime (for splitting and phase imprinting) we use in this work, which is motivated by the actual experiments, the effects of the third level turn out to be relatively small. Moreover, by changing the setup it is of course possible to make these effects even smaller. We believe that the referee has in mind retaining the lowest two levels (which is the minimum required to model the split condensate), and this indeed produces similar results to the model with three transverse modes. This point is discussed in section 6 of our manuscript.

(2) The two-fold degeneracy follows from the form of the potential vis-a-vis the energy of the lowest-lying states: after splitting the gases by raising the potential barrier, the lowest few states correspond to linear combinations of bound states in the two wells, with little mixing between the left and right wells. This causes the (anti-)symmetric combinations of left- and right-centered transverse wave functions to be nearly degenerate, as shown in the picture below (corresponding to the transverse potential at $t = t_{\rm r} = 5\,{\rm ms}$).



(3) We agree with the referee's statement that the experiments can indeed serve as a quantum simulator for the sine-Gordon model, provided that the state-preparation process is such that (i) the initial temperature is sufficiently low; (ii) splitting and phase imprinting are done sufficiently slowly in order to avoid occupying the higher transverse levels; (iii) the longitudinal potential is sufficiently shallow so that a description by a translationally invariant field theory can be justified. Our belief is that the main obstacle facing a description by the sine-Gordon model is in fact the presence of a longitudinal trap, and the finite size of the system. We have added to our discussion

of this point in section 8.

(4) The purpose of section 7 is in fact two-fold. First, it makes it explicit just how difficult it is to go beyond the time-dependent self-consistent HF approximation in the full model. We think this is a crucial point that cannot be overemphasised. Second, our view of the second Born approximation is different from the referee's: we feel that the issue is not its implementation – one of us has used this method in translationally invariant systems before – but rather how many of the modes (69) one can retain in the actual numerical computations. For shallow longitudinal traps this is indeed a very serious problem. However, the experiments in Ref. [10] were in fact carried out with a fairly tight longitudinal potential, and it is straightforward to make it even tighter. It is this (experimentally relevant) setting to which we have in mind applying the second Born approximation. We have added a comment along these lines in section 7.