Peer Review File

Manuscript Title: High-fidelity laser-free universal control of two trapped ion qubits

Redactions – Mention of other journals

This document only contains reviewer comments, rebuttal and decision letters for versions considered at *Nature*. Mentions of the other journal have been redacted.

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1:
Remarks to the Author:
The authors present microwave-based universal control of trapped-ion qubits — that is, the entangling and single-qubit operations that form the basis for quantum computing — that achieves the record fidelities previously only obtained with laser-based control. Moreover, those records hold not only for trapped ions but also for any quantum computing platform to date. The control scheme, which combines microwave magnetic fields with radiofrequency magnetic field gradients, was recently introduced by the authors in Ref. 42. Here, what is notable is that they apply the scheme to achieve high-fidelity universal control, which they demonstrate through the preparation of two maximally entangled states, a symmetric and an antisymmetric state. The preparation of the latter requires not only entangling operations but also individual control, and the fidelity of the antisymmetric state is higher than any previously reported.

I believe that the manuscript is well suited for publication in Nature because we do not yet have a clear route to scale up any quantum computing platform to the point where it can solve relevant problems better than existing computers. The trapped-ion platform is among the top candidates, but scaling up laser-based gates to achieve universal control over hundreds or thousands of ions poses a serious challenge. The microwave approach pursued here, implemented using currents in the trap device, has the potential to scale much more readily. Here the authors answer the crucial question of whether such approaches can match or even exceed the performance of laser-based approaches, and thus in my opinion the results represent an important milestone. Let me note that the gate times demonstrated here are still significantly slower than those achieved with lasers, but the speedup of a factor of four demonstrated here with respect to the state of the art is encouraging.

The manuscript is written in an exceptionally clear style and lays out the results methodically and precisely. Previous work is referenced thoroughly and is used to place the current results in context. The supplementary information provides extensive details on the setup and its characterization that will be useful for experts, but this material is a good fit as a supplement and not necessary to understand the main results. My only question is about Fig. 2b, which is not discussed until after Fig. 3. Also, it seems to me that Fig. 2b can only be understood in this order. Would it not make sense to separate Fig. 2b as a fourth figure? Also, the authors don't mention in the main text what the error bars in Fig. 2 correspond to. (Generally, the statistical treatment is laid out very clearly, and I appreciate the discussion of the "trigger" technique used to choose the reported dataset.)

Referee #2:
Remarks to the Author:
In this manuscript, a novel trapped-ion quantum gate is experimentally demonstrated that uses only magnetic couplings driven at rf- and microwave frequencies instead of laser pulses (coupling to electric dipole or quadrupole transitions). The proposal to realize trapped-ion quantum gates by using magnetic field gradients for creating state-dependent forces goes back twenty years and was followed by experimental demonstrations using either static field gradients or gradients oscillating at microwave frequencies. However, until very recently, these experimental realizations had substantially lower gate fidelities than laser-based gate operations.

Srinivas and colleagues have achieved a breakthrough in terms of gate fidelity by implementing a novel type of laser-free gate that makes use of magnetic field gradients oscillating at rf-frequencies. This approach, which was proposed by the NIST group and experimentally tested with a single ion in refs. 39, 42, 44 overcomes two important drawbacks of the previous implementations: (1) Contrary to gates using a stationary gradient, the gate speed does not drop drastically with increasing trap frequency. (2) It features a strongly reduced heat dissipation as compared to gates with field gradients at microwave frequencies, which is important in view of the limited heat transport in microfabricated ion traps having high-current carrying wires. Moreover, as outlined in the mentioned references and the current manuscript, the gate is intrinsically robust to a lot of sources of gate errors.

The data presented in the manuscript convincingly demonstrates that the potential of this approach for high-fidelity gate operations can indeed be realized experimentally and achieve Bell state fidelities that are on par with the highest ones achieved in any physical system. Here, I’m inclined to trust the authors’ analysis that the relatively large state preparation and measurement errors have been characterized accurately enough to support the claimed high gate fidelities, even though it would have been nicer to provide additional evidence by concatenating several gate operations.

I believe that this gate has an excellent potential as a building block for experiments in quantum information processing and in precision spectroscopy. For this reason, I support publication of this manuscript in Nature. The presentation of the manuscript is very clear and its length appropriate (the only information I was missing in the main manuscript was a statement about the orientation of the vector describing the field gradient with respects to the direction of the motional modes used for mediating the gate operation).

Referee #3:
Remarks to the Author:
The manuscript entitled "High-fiedlity laser-free universal control of trapped-ion qubits" by Srinivas et al. demonstrates two-qubit entangling gates as well as a single-qubit gate based on a novel microwave control of trapped Mg+ ions. Notably they achieved very high fidelities for the generated symmetric and anti-summetric Bell states, arguably the best so far in any quantum systems. The experiment was executed with an incredible precision, almost to perfection. The results are well presented and supported by excellent agreement with a theoretical model through careful data analyses. Therefore I do not question the quality and validity of their work (except for some technical details I am interested in, which I listed at the end.) My question is whether this work constitutes a major breakthrough in the field of quantum optics, quantum computing and potentially beyond, to be worth a publication in Nature. Then my answer is negative. I assessed the impact of this work from both quantitative and qualitative perspectives with the following questions.
1. Quantitatively does this work provide a major leap in important figures of merit such as the gate fidelities and gate speed?

2. Qualitatively does this work provide a new insight or a novel concept that would appeal to a large readership including those outside their own expertise?

Even though the fidelities achieved in this work are one of the highest, the difference from the previous records (ref.15 and 16) is minimal (actually statistically indistinguishable). The authors claim that the fidelity for the anti-symmetric Bell state is the highest among all the reported values. However I speculate that the settings in ref. 15 and 16 could have also achieved a similar or even higher fidelity if the corresponding authors were bothered to do so. This is because the additional cost of generating the anti-symmetric Bell state from the symmetric one is just a single qubit rotation which is presently not a major technological hurdle. The gate speed was improved from the previous microwave gates by about a factor of 4. This is a nice improvement but is not sufficient to be called a major leap. Besides it is still one order of magnitude slower than the laser-based gates. It would have made a resounding and game-changing impact to many if their gate had matched the speed of the laser-based schemes. But it is not there yet as of now.
On the other hand if we look at the qualitative side, this is the first realization of a novel two-qubit gate scheme proposed in ref.33 which uses an oscillating magnetic field instead of a static one as originally proposed in ref.19. This simple modification brings about notable and interesting advantages over the existing microwave techniques: smaller power consumption, higher Rabi frequencies (and hence shorter gate times) and intrinsic dynamical decoupling. It is certain that these features will be highly appreciated in the ion trap community. However they are still very technical and their importance would be difficult to comprehend for those who are outside the community.
In conclusion both the questions listed above have to be answered negatively. In my opinion the work is a progress being made and therefore one step short of being a Nature article. Having said that, this takes nothing away from the work. The quality of the experiment is superb. It deserves a publication in a more specialized journal such as [redacted].

In the following I list relatively minor and more technical questions/comments that I came across:
○ It is difficult to understand the relationship between the IDD, spin echo and Walsh modulation. They all seem to address the dephasing due to a qubit frequency offset. But do they address different parameter spaces such as different frequency ranges? Or do they just provide additional layers of protection?
○ The authors should provide more information about the pulse sequences presented in Figure S2. In the pulse sequence for the entangling interaction (Figure S2(A)), does each pulse in the sequence complete a circle in the phase space? Which Walsh function was used ?(W(7, t)? But no mention). Very little information is provided for the pulse sequence of single ion addressing (Figure S2(B)). An explanation using Bloch spheres will be appreciated.
○ What is the gate fidelity if only one pulse is used in the pulse sequence in Figure S2(A)? In other words how much do the Walsh modulation and spin echo contribute to the fidelity improvement?
○ In the single-qubit addressing, the ions are displaced from the trapping axis. Are there any adverse effects from the excess micromotion?
○ In ref.30, it was shown that when there is a residual magnetic field it causes a spin flip transition (S15 in supplementary material of ref.30). In the current work how do the authors take this effect into account during the single-qubit addressing?
○ The current scheme inherently requires the use of magnetic-sensitive sates. When comparing the current scheme to others on a fair ground, one needs to take the additional costs of transferring the population back and forth between the field-insensitive and -sensitive states whenever performing gates. The authors should comment on this.
○ What is the reason behind choosing the Hamiltonian (1) in the main text? It should have been possible to use a different detuning and choose a different Hamiltonian. Especially the one with the first order Bessel function and YY coupling. What is the benefit of (1) over this one?

Author Rebuttals to Initial Comments:

We thank the reviewers for their careful reading of the manuscript and their feedback. We respond to their comments below with the reviewer comments written in red.

Reviewer 1

> My only question is about Fig. 2b, which is not discussed until after Fig. 3. Also, it seems to me that Fig. 2b can only be understood in this order. Would it not make sense to separate Fig. 2b as a fourth figure?

We had also considered making this change previously, but in the end decided against it. Our rationale was that the qubit-frequency-shift insensitivity shown in 2b enables the single-ion addressing, which is described in Figure 3. We felt that the current order was more consistent thematically, even though we recognize that this means that Figure 2b is not discussed in the text until after Figure 3. Figures 2a and b highlight two important new aspects of our entangling operation, namely intrinsic dynamical decoupling and insensitivity to qubit frequency shifts or offsets enabled by the sigma_z*sigma_z nature of the entangling operation, when combined with a spin echo. Figure 3 summarises the main results of our paper: the creation of both the symmetric and antisymmetric entangled states.

> Also, the authors don't mention in the main text what the error bars in Fig. 2 correspond to.

We have added a description of the error bars for Figure 2 in the caption.

Reviewer 2

> (the only information I was missing in the main manuscript was a statement about the orientation of the vector describing the field gradient with respects to the direction of the motional modes used for mediating the gate operation)

We added a statement in the caption of Figure 1b) saying that we adjust the orientation of the modes so the gradient is maximally aligned along the mode at omega_r. We added a description of mode orientation with respect to the gradient in the supplement, along with the relevant citations.

Reviewer 3

> It is difficult to understand the relationship between the IDD, spin echo and Walsh modulation. They all seem to address the dephasing due to a qubit frequency offset. But do they address different parameter spaces such as different frequency ranges? Or do they just provide additional layers of protection?

The IDD provides robustness to sigma_z (spin-dephasing) errors at frequencies below delta/(2*pi). The Walsh modulation removes static qubit shifts similarly to a spin echo in this case. A simple Hahn spin echo is provided by a two-loop gate with Walsh modulation, and would provide robustness to static sigma_z offsets during the gate. With our Walsh-modulated 8-loop gate, the qubit pi pulses which perform the Walsh modulation also suppress the effects of linear-in-time and quadratic-in-time (over the duration of the gate) terms proportional to sigma_z, similar to higher-order dynamical decoupling sequences. Fluctuations which vary more rapidly on the timescale of the entangling interaction (in other words, fluctuations with frequency content above ~Delta/(2*pi), roughly ~1 kHz), are not suppressed by Walsh modulation.

At the same time, the Walsh modulation provides insensitivity to mode frequency offsets or drifts (for drifts which are static, linear, and/or quadratic in time). We have changed the description on page 2, right column to:

"We can also interleave the application of the interaction in Eq.~\ref{eq_hamiltonian} with a sequence of global qubit π pulses. These pulses suppress errors due to static or slowly-varying (relative to $1/\Delta$) qubit frequency offsets, which are proportional to $\hat{\sigma}_{zi}$ and thus commute with the entangling interaction~\cite{supplementary}. These same π pulses simultaneously implement Walsh modulation, which provides robustness to static offsets and slowly-varying (relative to $1/\Delta$) drifts in the motional frequency or in the control field amplitudes~\cite{Hayes2012, supplementary}. This combination of techniques yields an entangling interaction with substantial protection against decoherence of both the qubit and the ion motion, as well as experimental miscalibrations. "

> The authors should provide more information about the pulse sequences presented in Figure S2. In the pulse sequence for the entangling interaction (Figure S2(A)), does each pulse in the sequence complete a circle in the phase space? Which Walsh function was used ?(W(7, t)? But no mention).

We now specify that we use Walsh 7 modulation in the caption of Fig. S2B.

> Very little information is provided for the pulse sequence of single ion addressing (Figure S2(B)). An explanation using Bloch spheres will be appreciated.

We added more detail in the main text about the single-ion addressing sequence.

'The differential ac Zeeman shift generates differential phase evolution of the two qubits, enabling universal control when combined with global control pulses. For example, we can flip the spin of one of the two qubits using a spin-echo sequence of approximately 70\,μs duration~\cite{supplementary}. With this individual control of our qubits, we transform the symmetric entangled state $\ket{\Phi}$ into an antisymmetric entangled state $\ket{\Psi_-}$:' We also include more detail in the caption of Fig. S2B.

> What is the gate fidelity if only one pulse is used in the pulse sequence in Figure S2(A)? In other words how much do the Walsh modulation and spin echo contribute to the fidelity improvement?

Our leading error was due to motional frequency fluctuations. Using higher-order Walsh modulations suppresses such errors at the cost of an increased entangling operation duration. Practically, we increased the Walsh modulation until we stopped seeing fidelity improvements, which in this case was the Walsh 7 modulation. We have some data for Walsh 1 and Walsh 3 sequence as specified in Ref. 58, but they were not as carefully characterised as the Walsh 7 sequence presented here. We add a note referring the reader to this reference for that comparison.

> In the single-qubit addressing, the ions are displaced from the trapping axis. Are there any adverse effects from the excess micromotion?

We see no measurable effects of increased micromotion, or on our single-qubit rotations.

> In ref.30, it was shown that when there is a residual magnetic field it causes a spin flip transition (S15 in supplementary material of ref.30). In the current work how do the authors take this effect into account during the single-qubit addressing?

Ref. 30 in our original submission (Leibfried et al. Nature 422, 412 (2003) does not discuss this effect, nor is there an equation S15 in the supplement. However, we believe that Ref. 42 (Srinivas et al., PRL 2019) may be the reference in question. Eq. S15 of that work is not applicable here, since it considers a situation where both a radio-frequency field at omega_g and a microwave field at omega_0+omega_g are being applied. During our individual addressing, only the field at omega_g is being applied. The field at omega_g, when applied in isolation as during the single-qubit addressing sequence, can only provide a qubit frequency shift (ac Zeeman shift) but cannot by itself drive spin flip transitions because it is so far detuned.

> The current scheme inherently requires the use of magnetic-sensitive sates. When comparing the current scheme to others on a fair ground, one needs to take the additional costs of transferring the population back and forth between the field-insensitive and -sensitive states whenever performing gates. The authors should comment on this.

These population transfers would be performed using microwave pulses. For our system these errors would be less than 1e-4, much smaller than errors in the entangling operation. We note that Refs. 5 and 7 (in the revised manuscript numbering) both use field-sensitive states, as do the implementations using the static gradient scheme.

> What is the reason behind choosing the Hamiltonian (1) in the main text? It should have been possible to use a different detuning and choose a different Hamiltonian. Especially the one with the first order Bessel function and YY coupling. What is the benefit of (1) over this one?

Any qubit frequency offsets which are proportional to sigma_z commute with the interaction in 1, and thus can be removed using a multi-loop sequence with a spin echo pulse in the middle. These errors would not commute with an effective YY interaction, and would thus reduce the entangling gate fidelity. We add a sentence to the text explaining this: *'Although our method can also generate an effective $\hat{\sigma}_{y1}\hat{\sigma}_{y2}$ interaction~\cite{Sutherland2019} with a different choice of δ, such an interaction would not commute with $\hat{\sigma}_{zi}$ errors and is therefore less desirable.'*

We also choose this interaction as it does not require calibration of the pi pulses to implement Walsh modulation.

List of changes

1. Page 1, added reference to Ref. 7 and updated the abstract and introductory paragraph accordingly.
2. Page 1, added reference to Ref. 24.
3. Page 2, modified Fig. 1b caption to include description of mode orientation with respect to gradient.
4. Page 2, right column, modified the paragraph to include more detail about the Walsh modulation and why we chose the sigma_z*sigma_z coupling instead of the sigma_y*sigma_y coupling. *"We can also interleave the application of the interaction in Eq.~\ref{eq_hamiltonian} with a sequence of global qubit π pulses. These pulses suppress errors due to static or slowly-varying (on the timescale $1/\Delta$) qubit frequency offsets, which are proportional to $\hat{\sigma}_{zi}$ and thus commute with the entangling interaction~\cite{supplementary}. These same π pulses simultaneously implement Walsh modulation, which provides robustness to static offsets and slowly-varying (relative to $1/\Delta$) drifts in the motional frequency or in the control field amplitudes~\cite{Hayes2012, supplementary}. This combination of techniques yields an entangling interaction with substantial protection against decoherence of both the qubit and the ion motion, as well as experimental miscalibrations. Although our method can also generate an effective $\hat{\sigma}_{y1}\hat{\sigma}_{y2}$ interaction~\cite{Sutherland2019} with a different choice of δ, such an interaction would not commute with $\hat{\sigma}_{zi}$ errors and is therefore less desirable."*
5. Page 3, added description of error bars for Fig. 2. Modified Fig. 2B to specify that it was the symmetric state infidelity. Changed caption accordingly.
6. Page 4, left column added citation to Ref. 7 for laser-based methods.
7. Page 4, left column, fixed citation to individual addressing to use Ref. 39 instead of Ref. 59. They were both by U. Warring in 2013 and were mixed up.
8. Page 4, added more detail about the single-ion addressing sequence. *'The differential ac Zeeman shift generates differential phase evolution of the two qubits, enabling universal control when combined with global control pulses. For example, we can flip the spin of one of the two qubits using a spin-echo sequence of approximately 70\,μs duration~\cite{supplementary}. With this individual control of our qubits, we transform the symmetric entangled state $\ket{\Phi}$ into an antisymmetric entangled state $\ket{\Psi_-}$:'*
9. Page 4, added Ref.51 when discussing parallel operations and Ref. 24 for integrated detectors.
10. Page 5, added description of error bars in Fig. 3a.
11. Page 5, added Ref. 7 to Fig. 3b, updated caption to indicate that Ref. 7 did not correct for their measured fidelity.
12. References, updated citation to Ref. 67 which has since been published.
13. Page 8, added that we tune the motional mode to be along the gradient. *'For these experiments, we tune the mode alignment to be along the gradient by maximizing Ω_g experimentally~\cite{Srinivas2020}; the mode angles are similar to those specified in Ref.~\onlinecite{Warring2013b}.'*

14. Page 10, specified Walsh 7 modulation in Fig. S2a caption. Described how we perform a phase space loop in each of the 8 pulses, *‘Each of these eight sets of pulses nominally corresponds to a single closed loop in motional phase space; deviations from perfect loop closure are mitigated by Walsh modulation~\cite{Hayes2012}.’*
15. Page 10, added more detail on single-ion addressing in Fig. S2b caption. *‘ This sequence is appended to the sequence in $\textbf{(a)}$ to create the antisymmetric Bell state. We perform a spin-echo sequence, using the gradient oscillating at ω_g in only the first arm. The ac Zeeman shift from the magnetic field at ω_g imparts a phase rotation of π on the second qubit relative to the first. The phase of the global qubit rotation pulses in this sequence, here denoted by ϕ, must be calibrated relative to the entangling interaction in $\textbf{(a)}$ to create the desired state $\ket{\Psi_-}$. The π pulse and second $\pi/2$ pulse have no effect on the state $\ket{\Psi_-}$; however, for a general initial state, the second $\pi/2$ pulse is necessary to perform the individually addressed spin flip, and the π pulse implements a spin echo to undo the effects of common-mode qubit frequency offsets.’*
16. Page 10 left column, added reference to comparison of different Walsh sequences in Ref. 56. *‘We tried both higher and lower orders of Walsh modulation, with the highest fidelity entangling operations obtained using the Walsh 7 sequence~\cite{Srinivas2020}.’*