We thank the Referees for their extensive comments on our manuscript, which have helped us reshaping it to make it clearer and more comprehensive.

Since we now resubmit the manuscript to PRB, we have utilized the availability of extra space to expand and clarify several aspects of our proposal, and what used to be the Supplementary Information is now integrated within the main manuscript.

The text has been altered and expanded almost everywhere throughout the manuscript, so we do not list here every tiny modification. Key changes are highlighted in blue in the responses below.

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Second Report of Referee A -- LF16680/Tosi

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*The authors state in their reply that they revised the Supplementary Information and added a figure in S4, which is important to my comment, but the current version is still the old one and the Sec. S4 is missing. I am confused.*

We have now integrated the main text and the supplementary files into a single manuscript, so the Referee will be able to see the entire contents. The figure mentioned here is now Figure 4.

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Second Report of Referee B -- LF16680/Tosi

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*Compared to the first version, the authors have improved the manuscript by eliminating most of the "minor issues". However, the main objection remains: The content of this paper is geared towards the 31P community, and it is not clear who else could learn something from this. While the authors claim that the paper is "sufficiently broad" for PRL, this is hard to see for the reviewer. This may also be related to their statements "We are actually unaware of published literature where nuclear spins are coherently controlled using only with microwave fields." They may, e.g. look at PRL 109, 137602 (2012).*

We thank the Referee for providing us a specific reference. We were aware of that work in the context of identifying individual nuclear spins via multi-pulse microwave control of their coupled electron, but we admit that we had not realized that the authors had also demonstrated coherent control of the nuclei with this technique. Very impressive.

*Some of their replies are also quite beside the point raised by the referees, e.g. to the remake that “experimental evidence for the feasibility" is absent, they reply that they provide "evidence for the experimental feasibility" - these are two different pairs of shoes.*

By providing “experimental evidence for the feasibility” of the proposal, the Referee obviously meant “conduct the experiment”. This is, indeed, a very different pair of shoes than providing theoretical evidence that a proposal is feasible (on the basis of a detailed Hamiltonian and realistic noise models), as we instead do here. We don’t think there can be much confusion as to what contribution is provided by our paper, given it’s a theoretical proposal (for now).

*Other points are contradictory: they claim that their calculations are reliable since they use "parameters values informed by the experimental state-of-the-art" but later they write that the most important parameter, the hyperfine coupling, is actually unknown :*

*"there is no prior experimental or theoretical estimate for AHF between the donor (31P) electron and nucleus, when the donors are subject to large electric fields". In summary, I still believe that this paper should be published in a more specialized journal.*

The sentence quoted above by the Referee was written in the context of the question of whether there will be an anisotropic component to the hyperfine coupling, when the donor electron wavefunction is strongly distorted in the presence of the electric field. To this question we provided an honest answer, namely that this detail has not yet been precisely worked out. It is rather exaggerated to go from here to stating that that “*the most important parameter, the hyperfine coupling, is actually unknown*”. The important component of this parameter, namely the (isotropic) contact hyperfine, is well understood. We should note that unlike the case of the NV-diamond system (which is probably what the Referee has in mind), where the hyperfine coupling is mostly dipolar, in the 31P system the Fermi contact hyperfine term dominates by many orders of magnitude, and it remains (by definition) always isotropic.

We have clarified this matter in the updated manuscript, with the sentence:

Here we have assumed that the hyperfine coupling between electron spin and $^{31}$P nucleus is purely isotropic \cite{Feher1959}, i.e. dominated by the Fermi contact hyperfine term. This assumption may no longer exactly hold when the donor electron wave function is distorted from its spherical symmetry in the presence of the strong vertical electric field, whereby a small dipolar component can be created (a related case, where the electron is shared between two proximal P donors, has been recently studied \cite{Hile2018}). However, it is known that the Fermi contact component of the hyperfine coupling for donor is silicon is always the dominant term, even for $^{29}$Si nuclei which are placed off-center with respect to the symmetry point of the wavefunction \cite{Ivey1975}. Therefore, we expect the isotropic approximation to capture the main physics of the problem.

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Report of Referee D -- LF16680/Tosi

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*It is very important can connect a nuclear spin, preserving its coherence time, to other quantum devices. In this paper by Tosi et al., the authors propose to connect a 31P nuclear spin to other systems, in particular to microwave resonators. To reach the goal, they couple the nuclear spin to an electron spin, this electron is shared with a quantum dot that represents the interface with the external world. The proposal is interesting, but I do not think that the paper is suitable for PRL. In this paper, the authors avoid to specify the coherence time of their system, they represent the results in terms of dephasing rates.*

The dephasing rate is simply the inverse of the coherence time. One of the reasons we preferred using dephasing rates instead of dephasing times is because they are the most natural quantity to describe the system in a circuit-QED context, where the strong-coupling regime is obtained for , all of which are typically expressed as rates, not times.

*Furthermore, they never make a comparison between the coherence time of their device and the coherence time of the 31P nuclear spin, or other qubits that can be easily connected to other devices, for example, artificial superconducting qubits. The coherence time of a 31P nuclear spin exceeds 1 second. In the case of Tosi, the dephasing rate is 1 kHz - 10 kHz, that corresponds to a dephasing time of 0.1 ms - 1 ms. Moreover the relaxation rate, when the nuclear electric dipole is relevant, is of the order of 5 kHz - 10 kHz [Fig. 2d], that corresponds to a relaxation time of 0.1 ms - 0.2 ms. This means that even if Tosi et al. minimize the charge noise, this is still relevant for a work that aim to be published in PRL. Indeed, the coherence time T2 of their system is of, at longest time, 0.4 ms, more than 3 order of magnitude less than 31P nuclear spin.*

*But, more relevant, artificial qubits have reached coherence time of milliseconds [Rigetti, C. et al. Phys. Rev. B 86, 100506 (2012), Reagor, M. et al. Phys. Rev. B 94, 014506 (2016)]. For these reasons, I do not think there is a real advantage to implement this proposal.*

The Referee argues that, since (some) superconducting qubits have similar coherence times as those predicted for the system described here, our paper should not be published in PRL. The issue of publication in PRL now irrelevant, since we are resubmitting the paper to PRB. Nonetheless, we respectfully disagree with the Referee’s negative point of view on our proposed qubit system, on the basis of how it compares with superconducting qubits. Let us recall that superconducting qubits are physically very large and there isn’t, to date, a plausible manufacturing strategy to scale them much beyond ~1000 qubits. Conversely, the system we propose here can be placed on regular arrays with nm pitch, and fabricated using conventional silicon CMOS technology. In the revised manuscript, we have added a few references [46 – 49] that discuss some of the prospects of scalability for dense arrays of spin qubits, of relevance to our present proposal. We (and the vast majority of researchers in our field) are of the opinion that judging the scalability of a qubit system solely on the basis of its coherence time is insufficient and often misleading. Issues such as addressability, tunability, connectivity within an array, are at least (if not more) important in the long term, and our proposal is a significant step forward in that direction.

*1) As the main application of the 31P nuclear spin is quantum memory, the authors should provide simulations that calculate the fidelity of an unknown quantum state that is stored in the device and after retrieved, specially because the eigenstates of the system are dressed*.

The application as quantum memory is what used to be considered the most plausible use for 31P nuclear spins, due to the difficulty of coupling them to anything other than their own donor-bound electron. The key message of the present proposal is that we now have a strategy for coupling nuclear spins to electrical signals at microwave frequencies, opening up the possibility of medium-distance electric dipole-dipole coupling, or long-distance coupling via superconducting resonators.

In the updated manuscript, we have further clarified the idea of moving away from using the 31P as a simple quantum memory.

*2) At the end of caption of Fig. 2 the dots are randomly inserted*.

The whole manuscript has been extensively rewritten and previous typos corrected.

FOR THE EDITOR ONLY

Referee D has made some pointlessly hostile remarks, such as insisting that we should indicate dephasing times instead of dephasing rates. The Referee seems to insinuate that we are trying to hide the coherence times, which is, frankly, ridiculous (times and rates are trivially the inverse of each other).

We have attempted to politely respond to the Referee’s comments, but we hope you are alert to the evident hostility of this Referee, when judging how to take into account her/his comments.