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Quantum Computers in 2022

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After observing and contributing for nearly thirty years, I offer some perspective on the current state of play in quantum computers.

1 25 Years A-Building – as Assessment

This contribution is only very peripherally about numerically intensive computing. It is rather about the quest to, someday, have a fundamentally new tool to do numerically intensive computing, namely a quantum computer. That someday is today in the form of JUPSI, the D-wave quantum computer at Forschungszentrum Jülich[a]. But my work has as its objective a more ambitious use of qubits, in the form of gate-based quantum-coherent computation. This potentially more powerful alternative has been envisioned for many decades now, but may still be some decades away from its full realisation.

While there is perhaps never a good time to assess a work in progress, except when it is actually completed, I will nevertheless give a small try at it here. Getting to a quantum computer is no small thing, and one should not take such an ultra-marathon without a few breaks in between. I make no pretense to be systematic, comprehensive, or precisely predictive.

First, I will reflect on how some perspectives have remained immovable in 25 years. Immediately after the first big assessment of the possibilities of physical implementation of quantum computers in (I would say) 1994, it was observed that ion traps had many of the right ingredients for quantum computation. A first paper on quantum gates came out very quickly; its speed reflected the fact that hardly anything new had to be done, merely a relabelling of an experiment that was already being done. But pretty soon, a roadmap of sorts was drawn up, with, in my opinion (then as now) pretty much all the right ingredients[b].

This approach has seen steady progress. But why, with such a big head start, has it not been the clear front-runner? I think that here, as elsewhere, the physics-to-engineering transition of mindset has been very slow to be achieved. Maybe one problem can be traced to the varying interpretations of “scalable”. At one level, “scalable” just means being able to have more qubits to act on.

Perhaps unfortunately for subsequent developments, ion traps already had, in 1995, a modality to scalability: The linear ion trap could hold N atoms in a straight line. Even to this day, I see this discussed as an acceptable route to scalability, with even the advantage that any qubit can interact and be involved in gate operations with any other qubit. I think that this is a dangerous fantasy. First because all-to-all is not such a huge algorithmic

[a]See `https://juniq.fz-juelich.de/`
[b]See `https://qist.lanl.gov/`

advantage, but it is a huge pain when it comes to suppressing unwanted entanglements. Second, parallel operations throughout the qubit register are difficult or impossible in the linear arrangement. Third, the physics of taking N to infinity doesn't work anyway – the trap strength becomes weaker and weaker as N is increased.

I should of course make explicit my conflicts of interest, and also lack of foresight (but terrific hindsight) in making these critical comments. Right from the outset I was prejudiced towards realising quantum bits via the solid state, since that was my specialty circa 1994. This superficially matched with the orientation of my employer, IBM, which had been a leader in creating enormous infrastructures for constructing solid state (conventional) computers.

2 Quantum Computers and Solid State Physics

My first attempt at this, published in Ref. 1, is summarised in the table from that paper (Table 1). It contains some reasonable guesses, like electrons trapped on impurities or in quantum dots, and solid-state NMR. It was intentionally tongue-in-cheek in part (certainly in the case of the Mössbauer effect), and manifestly incomplete (electrons in gold).

Within a year, work on one of these possibilities really got going: NMR quantum computing, not in the solid state but in molecules in liquid solution. This initial splash pained me: I felt that there were some manifest no-gos in the way that this work got underway.

I perhaps had too snobbish an attitude then (as now) about what was the right way to go about this. Doing real experiments can always uncover unexpected opportunities and necessities for the future. The NMR effort to "factor 15" a couple of years later certainly showed the necessity of a systematic approach to efficient, high-precision pulse control, better than any abstract theoretical analysis of the problem ever could have done. But the violation of the prescription, elementary but perhaps not explicitly spoken yet in the later 1990s, that your computation should start with a cleared register (e.g., all 0s) was strongly violated by the nature of room-temperature NMR experiments. The clever expedient of the "effective pure state" somewhat obscured the unscalability of this approach. This, and the

TABLE I. Important times for various two-level systems in quantum mechanics, which might be used as quantum bits. t_{switch} is the minimum time required to execute one quantum gate; it is estimated as $\hbar/\Delta E$, where ΔE is the typical energy splitting in the two-level system. t_ϕ is the phase coherence time as seen experimentally. t_ϕ is the upper bound on the length of time over which a complete quantum computation can be executed accurately. The ratio of these two times gives the largest number of steps permitted in a quantum computation using these quantum bits.

Quantum system	t_{switch} (sec)	t_ϕ (sec)	Ratio
Mössbauer nucleus [35]	10^{-19}	10^{-10}	10^{9}
electrons-GaAs [36]	10^{-13}	10^{-10}	10^{3}
electrons-Au [37,7]	10^{-14}	10^{-8}	10^{6}
trapped ions–In [38]	10^{-14}	10^{-1}	10^{13}
electron-spin [9]	10^{-7}	10^{-3}	10^{4}
electron–quantum-dot [39]	10^{-6}	10^{-3}	10^{3}
nuclear spin [21]	10^{-3}	10^{4}	10^{7}

Figure 1. Table reprinted from Ref. 1.

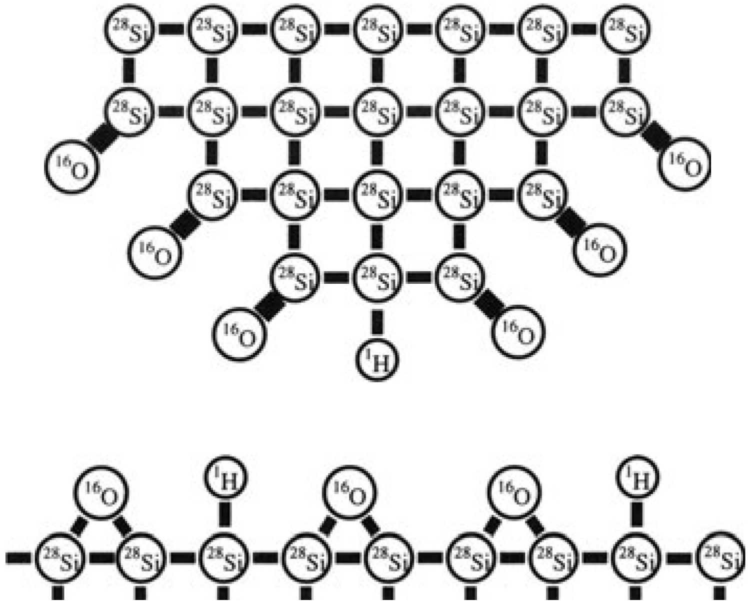

Figure 2. Concept atomic-force microscope quantum computer, reprinted from Ref. 2.

other unscalable aspect of living with an assembly of nuclear spins that could be present in a chemically stable molecule, meant that "factor 15" was not and could not be followed up by "factor 21", let alone "factor 637".

I sought to remedy this state of affairs in an initially scattershot way, by writing offhandedly in various papers some side-comments about what people should be focusing on. By the year 2000, I finally wrote out fully what I had been trying to say since 1994.

Not that I had the full story in 1994, or in 2000. To illustrate my lack of foresight, I would like to point out the best that I could offer in my 1994 paper. This is illustrated in Fig. 2, reproduced from Ref. 2. It posited some atom-scale engineering which made possible a scanning tip of an inert substance with a solitary hydrogen bonded at the end. "Inert" meant something with no unpaired spins, neither electronic nor nuclear, illustrated by a cartoon of silicon (with Si-29 intentionally left out). Thus, the spin of the single proton would be the clean qubit. The tip is to face a similarly constituted surface, with more proton qubits. Some rf pulsing for enabling operations is vaguely alluded to. Knowing that there had to be some idea for initialisation and measurement, I state some ideas for there being electronic paramagnetic centres on the surface, for the purpose both of swapping in a freshly polarised electronic state, and swapping out the nuclear state to make possible some kind of single-electron spin measurement. Scalability was fancifully dealt with by making the tips the ends of gear teeth, with a continually turning apparatus that bring the tips in contact with spin-containing grooves in the adjacent wheels.

3 Some Criteria

OK, enough of this Rube Goldberg (or perhaps Eric Drexler) nonsense. It is perhaps hard to say which aspect of this is the least realistic: structure fabrication is barely one iota closer to making this possible after the passage of 25 years, the mechanics of rapidly moving parts such as these will surely always remain unworkable. When continuing to defend myself five years later, I still felt it necessary to say that quantum computing is not science fiction. Any readers who had been paying attention to my writing would hopefully have come to this conclusion. I cannot blame the real nuclear-spin practitioners from cutting a lot of my fancy corners.

But I insisted that there had to be *some* answer to a minimal set of criteria. Since in the end I composed them as a set of carefully worded aphorisms, I will quote them now[4]:

1. A scalable physical system with well characterised qubits
2. The ability to initialise the state of the qubits to a simple fiducial state
3. Long relevant decoherence times
4. A "universal" set of quantum gates
5. A qubit-specific measurement capability

I am grateful to the anonymous Wikipedia author who quoted these exactly, with some abridgement (which I adopt here); I have seen them quoted far too freely, in ways that miss a lot of the meaning. Long exegeses could be written on the choice of the adjectives in these criteria, all of which were purposefully chosen.

In 1996 we thought (and published in 1998[3]) that the spins of individual electrons would be capable of addressing all these criteria. In our first paper we tried to say some thing on all five points. Our effort was quite influential, but it is clear that we were quite naive on several points. While we knew that nuclear spins would have to be discussed, we were not attuned to what a big role they would play in decoherence. We understood that holding electrons in quantum dots would be a good way to move the control problem to scales greater than the atomic, but we were not attuned to the issue that achieving entanglement over the "long" distance of, say, 200nm, would not leave enough room to fit in the control apparatus that is necessary for a multi-qubit scalable device. We now know that 20.000nm is a better target, and we have now spent many years of efforts to achieve this two order of magnitude increase of length scale in spin-qubit systems. This is, so far, without definitive success.

4 Onward with Superconductors

Superconducting devices did not look promising to me in the years 1994-96. I thought that such macroscopic structures would not have good enough quantum coherence. Events proved my understanding to be wrong, although I would say that it took until 2002 for that to be clearly the case. Many of us had not paid attention to the fact that, already in 1985, superconducting electric circuits had been shown to exhibit energy-level quantisation, confirming the concept of "macroscopic quantum coherence" which Leggett had advanced (and questioned) at that time[5]. But these "artificial atoms" of 1985, and 1995, and 2000, had very inferior properties to those of real atoms, with very limited manifestation of quantum coherence.

But among experts, Devoret expressed confidence[6] that the designability of electric circuits would provide ways of overcoming the low coherence of the early experiments. The 2002 paper of his group was a watershed moment for this specialty, in which he proved that such design efforts would pay off[8]. The "quantronium" type qubit introduced in this paper did not survive long in the annals of superconducting qubits, but this is because the design paradigm was indeed fruitful and led to rapid subsequent improvement, rendering quantronium obsolete.

Actually, the design flexibility of superconducting quantum circuits had already produced a fertile shoot from the tree in a rather different direction. Following these lines of thinking independently, Hans Mooij had proposed the "flux qubit"[9]. This qubit was designed largely for its ease of measurement, for it has two eigenstates that are magnetically quite distinct, so that the measurement could be done by standard magnetometry. But this distinguishability also made the coherence difficult to improve, although it ultimately was possible many years later. But before this was done, the inherent robustness of the flux qubit, both for measurement and qubit-qubit coupling, convinced the founders of the D-wave company, early in the 2000s, that they should fix on this design for their adiabatic optimiser. Since the concept of this optimiser did not require high quantum coherence, this choice was appropriate for them and remained unchanged through their many cycles of development. The coherence improvements of the mid 2000s onward were not needed in the D-wave strategy, and were not adopted.

For gate-based quantum computing neither the flux qubit, nor quantronium, were adequate, but Devoret's vision of improvement by electrical optimisation proved true. The Yale group made two related, and crucial, discoveries. First, they understood that resonant circuits could boost the sensitivity of measurement, and make the measurement of otherwise hard-to-measure qubits feasible. Then, they designed and made a much better (but hard to measure) qubit by a modification of a very basic qubit circuit, the Cooper-pair box. The resulting qubit, the "transmon", has been the workhorse of many successive generations of coherent processors, from the late 2000s onward.

In truth, the processors that have resulted, especially the universally accessible cloud offerings of IBM from 2016 onward, have sometimes barely deserved the name of "computer"[7]. It seems that, as currently designed, they are guaranteed to crash every 20 seconds or so due to a cosmic ray event[10]. I do not see this as a sign that this is on a track for failure. There are severe growing pains, but there is growth. One can foresee the overcoming of all obstacles. I take it as serious that IBM makes a mention of a million-qubit processor in its public roadmap[c]. The coming years will continue to be very interesting in the development of new quantum computer hardware.

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