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COMMENT

Comment on 'Bennett clocking of quantum-dot cellular automata and the limits to binary logic scaling'

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Abstract
It is shown that a number of claims in the subject paper are incorrect and/or misleading.

In the paper by Lent *et al* [1], the authors make certain claims about our work [2, 3] to which we feel compelled to respond.

Unfortunately, we believe that Lent *et al*, in section 6 (pages 4248–50) have misinterpreted and misunderstood our analyses of physical limits for binary switching. We would like to clarify several points made by Lent *et al* that we believe are incorrect.

(1) Lent *et al* state that the energy–momentum relation $p^2 = 2mE$ is true only if the potential $V(x) = 0$. This position is perplexing to us. We would like to note that E in the above relation is the kinetic energy, which we would mark as E_k for better clarity. The above energy–momentum relation relates momentum to the kinetic energy, and it is universally valid (the relativistic case is described by a more general equation, but the conclusion will not change), and it is not related to the potential energy.

The binary switch requires an energy barrier to prevent uncontrollable (and undesired) over-barrier transitions between binary states. If the barrier height is E_b, the kinetic energy of the particle must be less than E_b and the result from (16) immediately follows. The $k_BT \ln 2$ term in (16) follows from Boltzmann's probability for an over-barrier transition, i.e. $\exp(-\frac{E_b}{k_BT})$ due to thermal activity, and the requirement that the over-barrier error probability is less than 0.5. The result $E_b > k_BT \ln 2$ is immediate.

One final note on the relation (16) is that it is a simple estimate of the conditions of significant tunnelling using the Heisenberg uncertainty principle, as is often done in the texts on the theory of tunnelling [5]. Different modifications of this approach are known, for example Haensch *et al* [6] discussed possible refinements to our model by using non-rectangular barrier shapes, using effective electron mass rather than free-electron mass, etc.

(2) Lent *et al* dismiss our claim that the barrier height, E_b, is related to the energy dissipated in a controlled binary transition. The chain of reasoning for the affirmation of our position is (a detailed analysis can be found in [3] and [4]):

- According to Poisson's equation, changes in barrier height require changes in charge density that involve charging or discharging of a capacitor.
- The energy to 'deform' the barrier is equivalent to the energy of charging the capacitance.
- Charging and discharging of a capacitor is always accompanied by energy dissipation.
- The reason for the energy dissipation is the presence of finite resistance in the circuit (regardless of how small the resistance is).
- Adiabatic schemes to conserve energy are also subject to these charging or discharging losses as well as other overhead losses (for detailed discussion see [3] and [4]).

Lent *et al* believe that the resistance is not a fundamental issue: 'as [76] points out, there is some heat dissipation . . . due to the small resistance of the wires themselves, but (contra [76]) it can be minimized and is not a fundamental limitation.'

We take issue with Lent *et al*'s dismissal of our position that one cannot manipulate the energy barrier without dissipation proportional to E_b, in effect, arguing that the resistance does not enter into energy dissipation. This surprising position, that the resistance 'is not a fundamental limitation', flies in the face of physics and our experience!

(3) Lent *et al* say on page 4249 that 'Zhirnov *et al* argue that it is important to distinguish between 'charge-based' switching devices and other more exotic devices based on representing information as spin, for example .... The conclusion of [75, 76], that only those possible CMOS successor technologies which are based on something other than charge warrant pursuing, is therefore unwarranted'

In fact no such claims are made in [75, 76] ([2] and [3] in this response). On the contrary we have written several papers showing that different physical entities used for representing information, such as charge, spin, photons, etc., are governed by the same physics and therefore share similar limitations [7, 8].

References

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REPLY

Reply to Comment on 'Bennett clocking of quantum-dot cellular automata and the limits to binary logic scaling'

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In the comment on our paper [1], Zhirnov and Cavin defend several of their claims in [2] and [3]. Their specific comments are addressed in turn below.

(1) At issue here is Zhirnov and Cavin's claim that the fundamental minimum size of a 'computational element' is 1.5 nm (equation (2a) in [2]), based on their use of an uncertainty principle argument. It is clear from the scaling properties of the Schrödinger equation that one could always scale lengths down and potentials up to achieve a bistable computational element as small as desired. Moreover their argument would suggest that molecules which are smaller than 1.5 nm would be thermally unstable. We point out in [1] that the error in their simple argument is identifying the uncertainty in momentum with an energy term, $\Delta p = \sqrt{2mE}$, which can only be true when there is no structure to the confining potential, i.e. no double well. In that case, and only in that case, the total energy is equal to the kinetic energy and the identification they make is correct. The fact that room temperature bistable molecular switches have actually been fabricated which are significantly smaller than their 'fundamental minimum size' [4] should also be noted.

They point out that the energy barrier height which separates two binary states must be larger than $k_B T \ln 2$. On this distinguishability criterion we have no disagreement. Where we differ is on whether or not it follows that each time the bit is switched, it is necessary to dissipate that amount of energy as heat. The idea (of Landauer's [5]) is simply to lower the barrier before switching the state.

(2) There is not a fundamental minimum energy which must be dissipated to compute a bit. What does the term 'fundamental' mean in this regard?

No heat engine works at 100% efficiency. All have losses due to friction between moving parts. Designers can lower those losses by clever design and better lubrication. When we say there is a fundamental (upper) limit on the efficiency of the heat engine given by the Carnot efficiency η_C, we mean that even if friction vanished, 100% efficiency ($\eta = 1$) cannot be achieved. In real engines there will always be friction losses to deal with, but one knows that minimizing them will never result in an efficiency $\eta > \eta_C$. The Carnot limitation is fundamental; friction losses are inevitable but not fundamental.

The Landauer–Bennett (LB) result, that there is not a fundamental lower limit to the amount of heat that must be dissipated to compute a bit [5], does not mean that one can ever do computation without dissipation. It simply means that the heat dissipated can always be made lower, perhaps at the cost of speed. Resistive losses are inevitable but not fundamental. It therefore becomes a practical and quantitative question as to how much heat will be dissipated at reasonable speeds. For adiabatically switched CMOS [6] it may be that the speeds required are unacceptably slow for most applications. For molecular QCA the outlook is much better [7] because intrinsic molecular switching times can be as fast as 10^{-13} s [8], so adiabatic switching can still be very fast. It is a quantitative question which must be addressed by analysing particular computational systems.

Zhirnov and Cavin correctly perceive that if they are right, the LB result must be wrong [3]. In our paper we provide a concrete example by direct calculation which supports the LB conclusion. A single example cannot prove LB correct, but it is sufficient to show that the general Zhirnov–Cavin conclusion that there is such a lower limit is incorrect.

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(3) The fundamental energy considerations for bistable switches (distinguishability, adiabaticity, dissipation, etc) are entirely independent of whether the two states are defined by spin, charge or other degrees of freedom. In fact, the arguments of LB were formulated for magnetic systems initially [5]. Zhirnov and Cavin contrast their analysis of the limits of 'charge-based' systems with the possible exploration of other systems: 'the predicted end of scaling of charge-based binary switches in the far nanometre size regime has stimulated the search for alternative physical state variables...' [9]. Exploration of digital devices beyond CMOS is an excellent idea, and QCA is one such alternative. But charge is not the issue. If they have now come to that conclusion, it is a welcome point of agreement.

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