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Keeping the Entropy of Measurement: Szilard Revisited

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What happens to von Neumann's entropy of measurement after we get the outcome? It becomes entropy of erasure. This is cribbed from Szilard (1929). Also, two errors in that celebrated paper are corrected.

1. INTRODUCTION

The Second Law of Thermodynamics forbids a net gain of information. Yet a measurement "provides information." Measurement itself thus becomes paradoxical, until one reflects that the gain in information about the system of interest might be offset by a gain in entropy of some "garbage can" gc. Indeed, it *must* be so offset to save the bookkeeping of the Second Law. This apparent and paradoxical gain in information attendant upon observation, presumably due to neglect of some dissipant gc, has long prompted aberrant speculation that intelligent beings and even life in general somehow indeed violate the Second Law, an erroneous view I cite only for perspective. For some time I have fallen prey to a version of this paradox, developed in the context of standard quantum theory of measurement as delineated by von Neumann (1955), a trap I have recently been able to escape with the help of Szilard (1929), the celebrated paper in which the related paradox of Maxwell's demon is broken. Here I describe a precise formulation, then resolution of my paradox of information through measurement.

2. REVIEW OF VON NEUMANN

Von Neumann finds *no* paradoxical loss of entropy through measurement, but rather a *gain* of entropy quite in conformity with the Second

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Law. I go over this well-known ground to explore the paradox of the *missing* paradox!

Indeed, if we start with a pure state vector $|x\rangle = \sum a_i|e_i\rangle$ resolved on the orthonormal basis of states $|e_i\rangle$ separated by distinct outcomes of the measurement, then the $p_i = |a_i|^2$ are the probabilities of the various outcomes, or, in an early way of saying it, the probabilities of the different possible "quantum jumps." Interaction with the measuring device makes the system "jump" with probabilities p_i, which introduce nonnegative entropy

$$\sum p_i \ln \frac{1}{p_i} \equiv \left\langle \ln \frac{1}{p} \right\rangle \equiv vN \tag{1}$$

(in Boltzmen or e-folds). Perhaps I should attribute this notion of production of entropy to Dirac (1938–39), who noted that the quantum jumps not only introduce entropy, but might account for *all* production of entropy, a thesis with ancient roots (Lucretius ~55 B.C.; see Latham, 1951, p. 66) that I also long ago found support for in a calculation (Lubkin, 1978).

Von Neumann makes this already familiar production of entropy vN unambiguous by dealing with very concrete ensembles. Thus, the pure state $|x\rangle$ before the measurement is presented as some large number N of copies of an $|x\rangle$-prepared system. After measurement, p_iN estimates the number of copies cast into state $|e_i\rangle$, hence the originally pure ensemble $|x\rangle\langle x| = P$ gets replaced by the mixture $\sum |e_i\rangle p_i \langle e_i| = \Upsilon$. The entropy per copy of P is 0, the entropy per copy of Υ is quite unambiguously the vN attributable to measurement, or to "quantum jumps."

The clarity of this mixing of the multicopy ensemble unfortunately reaches its resulting increase vN of entropy by so well bypassing the phenomenon of *gain* of information through measurement that we simply do not learn enough about our paradox from it. Of course, more generally, the original ensemble P need not be pure, and the mutually orthogonal outcome spaces E_i [of a sharp test (Lubkin, 1979b), definition on p. 550; also Lubkin (1974)] need not be one-dimensional, yet the final ensemble $\Upsilon = \sum_i E_i P E_i$ if distinct from P is of greater entropy than P, which happens if any E_i fails to commute with P, this greater generality being, however, equally useless to us in regard to our paradox.

3. THE PARADOX

So, to formulate the paradox, as it were to look for trouble, I focus attention as before upon the *individual* trial, rather than on the statistical behavior of the very many trials dealt with by von Neumann. We still have the probabilities $p_i = |a_i|^2$ (in the case of initial pure state x) as predictions of the likelihoods of our single outcome for our individual trial. Indeed, after the trial is over, *but before we look* at the outcome, the original state matrix P is replaced by the new state matrix Υ. Yet when we do learn the outcome, Υ is in turn replaced by $E_i \Upsilon E_i$ (unnormalized), with i the index of the single observed outcome, or more simply (and normalized) by E_i itself if E_i is one-dimensional (dimension of its image space). The threefold sequence

$$P \to \Upsilon \to E_i \tag{2}$$

(to keep to the simple one-dimensional case, which illustrates the point nicely), with recognition of a state Υ after the essential interaction of measurement but before transition of the answer "i" to the observer, emphasizes the increase in entropy in the first step $P \to \Upsilon$, so that we may be properly shocked at the loss of this entropy in the second step, $\Upsilon \to E_i$. Also, the $P \to \Upsilon$ increase in entropy is of course precisely the $\langle \ln (1/p) \rangle$ entropy of measurement vN of von Neumann, which we bet is the "right answer." We are now in the uncomfortable position of betting on a "right answer" for an entropy of measurement provided that we do not look at the outcome, yet getting zero if we do look, as if a proper measurement does not involve looking at the result, which is surely an unhappy state of affairs!

4. DEBATE ON THE SINGLE TRIAL

Some may fault the notion of probability without an ensemble as the root of my trouble; to relate the probabilities p_i to experience, we must run many trials, not one, and so the probabilities, to be physically meaningful, *must* refer only to the very many trials. Yet let us run our N trials one by one. If *each* time we generate no entropy, then we will not have generated any entropy in the *string* of N trials, either. And the per-trial "entropy" vN of the "N-ensemble" of N trials in my above version of von Neumann appears only in describing the *nonuniformity* of the ensemble. Our knowledge of each outcome after it happens is replaced by the randomness of entropy in the amount vN only if the state we produce for a next experiment is *randomly* chosen from that ensemble. To repeat, the generation of entropy by measurement, if nil in the particular trial, is nil in the N trials together, and entropy of the ensemble for a subsequent test is produced only by the possible policy of disregarding the separation into cases provided by the particular outcomes, and using the whole ensemble in a future experiment *with no regard for the known sorting into cases of the earlier individual outcomes.*

Is there, then, no true entropy of measurement, but just entropy from a refusal to accept the sorting provided? No, there must be more to it than that, because of the $P \to \Upsilon \to E_i$ paradox; Υ, the situation before we look, but after the measurement has taken place, already possesses entropy of measurement vN, which cannot be lost in the $\Upsilon \to E_i$ phase; *therefore, there must be a garbage can gc.*

5. BOHREAN DOUBTS

Or is the situation too vague? Increase of entropy is, after all, not universal. Define your system of interest to shrink to nothing, then its entropy of course goes to zero. If, as another but less trivial case, the system is so well isolated as to obey a law of unitary motion, its entropy remains constant, and frustratingly for Boltzmann, will not increase. The Second Law is for situations in which the system is imperfectly isolated, so that it tends to develop new correlations with the outside, yet where somehow the gross content of the system remains fixed, on the average, so that shrinking to nothing, for example, is forbidden. Then the density matrix of the system of interest, in having the increasing external correlations lopped off through Landau tracing over externalities (Lubkin 1978), becomes increasingly mixed. Another reason that having "the system" perfectly isolated is suspicious is that specification of a precise value of some observable forces a detailed correlation of the system with the outside in regard to complementary observables (e.g., Lubkin, 1979b, p. 537). Perhaps these various philosophical demands attending definition of the type of "system" that should indeed obey the Second Law engender an incompatibility with the "systems" in a paradigm of measurement; perhaps "systems" for clear Second-Law bookkeeping and "systems" in measurement are complementary. But I feel that these Bohrean misgivings anent the clarity of my paradox are mooted by finding gc with the help of Szilard's example. More strongly, it *must* be that the Second Law and measurement *go together*, because they both refer to the empiricism of everyday life, the particularization of definite outcomes in everyday measurement serving as the injector of randomness that drives the Second Law, as noted already by Lucretius and Dirac.

6. AMPLITUDES IN SUPPORT OF THE SINGLE TRIAL

Having drawn attention to the philosophical objection to probabilities for a single trial, I should make it clear why I nevertheless regard the $P \to \Upsilon$ phase to be meaningful for a single trial, again for the clear pure case $P = |x\rangle\langle x|$. We may regard state x times the equipment's original state y_0 to evolve, $\sum a_i|e_i\rangle \otimes |y_0\rangle \to \sum a_i|e_i\rangle \otimes |y_i\rangle$, to a new *pure* state, with density matrix

$$\bar{\Upsilon} = \sum_{ij} a_i a_j^* |e_i\rangle \otimes |y_i\rangle\langle e_j| \otimes \langle y_j|$$

with $\Upsilon = \sum_i |a_i|^2 |e_i\rangle\langle e_i|$ obtained from $\bar{\Upsilon}$ by the Landau tracing-out of the equipment. That is, the various *amplitudes* for different "outcomes" are in principle capable of subsequent interference, and our option to disregard this by confining attention to the x-system subsequently is what reduces the amplitudes to probabilities and engenders entropy. So the branching into distinct channels is there, physically, in the Schrödinger equation, which gives us $\bar{\Upsilon}$, even before the neglect or approximation that limits our attention to Υ. And this Υ already formally has entropy $vN = \mathrm{Tr}\,\Upsilon \ln(1/\Upsilon)$, even though operator Υ acts on the Hilbert space appropriate to *one* trial, *not* on a tensor product of N copies of that Hilbert space. The fact that entropy vN times N is most easily *displayed* by the scatter in an N-copy ensemble *associated* to Υ should not be used to obscure the fact that the entropy vN times 1 is already there without any such display. Of course, to emphasize the already physical qualities of this amplitudinous branching has become known as the "many-worlds" point of view; the reader will follow better if I say that for me, this is the *right* view.

7. PLURAL REALITIES

Indeed for clarity I restate the paradox in grossly multiworldly language.

First Multiworldly Statement: $vN = \sum p_i \ln(1/p_i)$ explicitly contemplates the multiplicity of branches issuing from a node of measurement, hence is obviously appropriate for a contemplator of that multiplicity, say, for an early observer who has not yet read the outcome, or for an outer observer or friend outside the laboratory. But in the reality relative to an inner or late observer who *has* read the outcome, the other branches are excluded, and the entropy expression vN in seeming to yet take those other branches seriously seems no longer appropriate.

Preliminary Resolution. Now, the amount of entropy vN or more must nevertheless yet be there, even in the bookkeeping of an inner observer, because the reality of experienece is the steady progress "inward" of an observer, through a series of particularized outcomes, and it is to this common experience or *stream* of consciousness that the Second Law and indeed the notion of time applies, else the Second Law could not have been found out in the 19th century. So while vN for the *outer* or early observer is simply a feature of multitudinous branching, there must also be a vN *inner* to each branch harbored in a gc in each branch.

Second Multiworldly Statement. Branching realities might be feared inconsistent with laws framed in a philosophy of one unique reality, hence inconsistent with all the traditional "*First*" laws of conservation, and inconsistent, too, with the Second Law. Since "each branch has all the baryons" (Lubkin, 1979a, p. 174), laws of conservation are fine—of course such laws are also tied to symmetry, and enough symmetries will remain. The present investigation may be taken to deal with the scare that the *Second* Law might fail.

Resolution Again. But it must not fail. When the probabilities are swept away by a specific outcome, their entropy must yet be swept into a gc, not actually annulled. This is the usual caution, in an entropic embarrassment, that one may not have been sufficiently careful in defining the problem's thermodynamically relevant "universe."

8. THE ANSWER, gc

Where, then, is the garbage can? Szilard (1929) finds it for me: gc is the damper on the register that receives the outcome, the damper that allows that register to get rid of its former configuration. I call this dissipation *entropy of erasure* (er), and argue that $\mathrm{er} \geq vN$.

Lemma. Let the k-labeled orthogonal states of the register reg occur with Boltzmann's probabilities

$$q_k \propto \exp(-\varepsilon_k / T_0) \tag{3}$$

in the mixed state of reg at temperature T_0 that is to correspond to "erasure"; note that by choosing the energies ε_k of the levels, one can adjust the q_k at will. Outcome k of a measurement is assumed to set reg instead to pure energy level k. Then, if the probability of this outcome is p_k, the entropy of erasure is given by the "cross entropy" expression

$$\mathrm{er} = -\sum p_k \ln q_k \tag{4}$$

Proof. Suppose that, due to the outcome having been k, the reg starts at pure state E_k before erasure to T_0; to calculate the entropy of that erasure "er_k" from such a start: er_k is a sum of two terms.

$$\mathrm{er}_k = \Delta S_{\mathrm{reg}} + \Delta S_{\mathrm{res}} \tag{5}$$

The part ΔS_{reg} is S_{reg} at T_0 minus S_{reg} at E_k. As the latter is pure, its entropy S_{reg} is 0, whereas S_{reg} at $T_0 = -\sum q_j \ln q_j$. So also $\Delta S_{\mathrm{reg}} = -\sum q_j \ln q_j$. The other term ΔS_{res} is the heat gained by the T_0 reservoir used for the quenching, divided by T_0. If the thermodynamic internal energy of reg after quenching is U_0, then the heat gained by reg is $U_0 - \varepsilon_k$, whence the heat gained by res is $-(U_0 - \varepsilon_k)$; so

$$\mathrm{er}_k = -\sum q_j \ln q_j - (U_0 - \varepsilon_k)/T_0 \tag{6}$$

is the result for the entropy er_k of erasure from the definite outcome k. Of course,

$$U_0 = \sum q_j \varepsilon_j \tag{7}$$

Finally, the expected value er for the entropy of erasure if reg starts from an unknown setting k but with probability p_k is

$$\text{er} \equiv \sum p_k \cdot \text{er}_k \tag{8}$$

This convex combination alters (6) only in replacing ε_k by $\sum p_k \varepsilon_k$, giving

$$\text{er} = -\sum q_j \ln q_j + \sum (p_j - q_j)\varepsilon_j / T_0 \tag{9}$$

Let

$$z \equiv \sum \exp(-\varepsilon_k / T_0) \tag{10}$$

Then $\varepsilon_k / T_0 = -\ln q_k - \ln z$ eliminates the ε's, to give

$$\text{er} = -\sum q_j \ln q_j + \sum (p_j - q_j)(-\ln q_j - \ln z) \tag{11}$$

which indeed simplifies to $-\sum p_j \ln q_j$, hence to (4). ■

Theorem:

$$\text{er} \geq vN \tag{12}$$

Proof. It is to be shown that $-\sum p_k \ln q_k \geq -\sum p_k \ln p_k$, that is, that $-\sum p_k \ln q_k$ considered as a function of the arbitrarily assignable quenching probabilities q_k for fixed values of the experiment's own probabilities p_k is minimum at $q_k = p_k$. This is immediately verified, using a Lagrangian multiplier λ for the constraint $\sum q_k = 1$. Thus, $d(-\sum p_k \ln q_k) = \lambda d \sum q_k$, $-\sum p_k \, dq_k / q_k = \lambda \sum dq_k$, $-p_k / q_k = \lambda$, hence $q_k \propto p_k$, hence $q_k = p_k$ since both p's and q's must sum to 1. It is also easy to check that this stationary point is indeed a minimum, e.g., $-\sum p_k \ln q_k \to +\infty$ at the $q_k = 0$ boundaries. ■

Varying instead on the p's for fixed q's produces no interesting result.

Significance of the Theorem. The reservoir that quenches reg, thereby erasing its old contents, indeed is an adquate gc for upholding the Second Law, in that quenching the outcome of a former trial of the same experiment in that gc produces enough entropy to compensate for the loss of entropy vN upon learning the outcome of a new trial. Quenching indeed produces *more* than enough entropy, unless the quenched or erased mixed state of reg is selected to have the same probabilities as the experiment's own. In this way each trial of a long sequence needed to establish empirical probabilities will indeed contribute in the mean at least its proper share vN to increase the entropy of the universe, and will do that by dissipation in the gc cribbed from Szilard.

9. CONTEMPLATION OF SZILARD (1929), WITH MINOR CORRECTIONS

Erasure in Szilard. I first explain wherein lies my debt to Szilard (1929). Szilard is exorcising the Maxwell's demon. This demon is an entity "who" by observing molecules can operate an engine in a way that reduces the entropy of the universe, or equivalently, extracts work from a single heat reservoir, in violation of Kelvin's principle. Szilard argues convincingly that the essence of the demon is the storage of information about one dynamical variable x in another one y, and concentrates on cases where the set of possible values for y has two elements: in modern jargon, y is embodied in a one-bit register. Szilard gives examples to show that if such writing of x on y could be done without producing entropy, then the demon would work, and the Second Law would fail, but that if each such writing somehow entails the production of one bit of entropy, the demon fails. (In 1929, "amount $k_B \ln 2$ of entropy, where k_B is Boltzmann's constant.") He concludes abstractly from the Second Law that such writing must produce the required bit of entropy to compensate the deficit, but he is not satisfied with that, and he accordingly builds a model of a one-bit register, to see precisely where entropy gets produced. He is so careful not to produce entropy unnecessarily that he frighteningly manages to write demonically on his register *without* producing entropy, if reg is in a known state before writing. This disaster is avoided when he does find the requisite production of entropy upon *erasing* back to a known state, for the next cycle. His known state is equilibrium at some temperature T_0; the erasure is effected by plunging reg into a reservoir at T_0. (Szilard does not use the word "erase," but that is the idea.) Since I also find my gc through entropy of erasure, I have now explained my debt to Szilard; I note that for a two-state register er $= -p_1 \ln q_1 - p_2 \ln q_2 \leq \ln 2$ and is ln 2, one bit, only when $p_1 = p_2 = q_1 = q_2$, and that the more general expression appears also in Szilard; I have simplified for readability.

I will now attempt to discharge my debts to Szilard and to the patient reader by correcting some mistakes; the excitement of discovery carried the brilliant author past some fine points.

Degeneracy g. Szilard uses a two-level quantum system or atom for his register. To record one answer "0," the atom is to be cooled to its ground state, which is unobjectionable; cool (near to) absolute zero, $1/T \to +\infty$. To record the other answer "1," the atom should be heated to its *nondegenerate* excited state. The easy way to do that is to heat to $1/T \to -\infty$, or $T \to 0^-$, the "other absolute zero" of negative temperature, but Szilard refuses to anticipate the discovery (Purcell and Pound, 1951) of negative temperature. He instead makes his excited state *highly degenerate*, giving it a multiplicity

$g \gg 1$. Then a large, positive temperature, $1/T \approx 0$, which is really only halfway to $-\infty$, seems to work, as the odds of occupation of the upper level are g times the odds for the lower. Unfortunately, the hot mixed state has large entropy $\ln(g+1)$, and will produce entropy more than the one bit ln 2 on being plunged into the resetting reservoir at T_0. (Or if T_0 is near ∞, resetting the *cold* state will produce excessive entropy.) As Szilard wishes to show that it is possible to *just* compensate the deficit of entropy, such excessive production of entropy would contradict his point about that. To see how the g-foldness of the upper level actually leads to the trouble of excessive heat/temperature upon cooling, note that although the energy ("heat") transferred is independent of g, the T_0 denominator does involve g: In the easy case, $q_k = p_k = 1/2$, and in particular is 1/2 for the ground state, we have $e^0 = 1 = ge^{-\varepsilon/T_0}$, where ε is the step between levels, hence $T_0 = \varepsilon/\ln g$ is indeed depressed by the largeness of g, and entropy $\sim\varepsilon/T_0$ is enhanced and excessive.

If one does not like my glib repair with negative temperature, one may instead write upon a nondegenerate upper level as follows: First cool to the lower level. Then apply a causal Hamiltonian motion to "rotate" that to the upper level.

I of course wish to extend optimal management of a two-level reg to an n-level reg. For writing, I must be able to set reg to one of these n levels, not only to the ground state (by cooling to 0^+) or to the top state (by heating to 0^-). There are enough *other* absolute zeros available (Lubkin, 1984) through chemical potentials to indeed select any level, approximately, by direct Gibbsian equilibrium. Here the alternative method of causal Hamiltonian motions subsequent to cooling to the ground state is much plainer.

Bits and Pieces. Szilard tries so hard to avoid unnecessary production of entropy that he unwittingly lets his crucial bit slip by even in erasure, and so seemingly *creates* a demon: I erase by plunging reg directly into a T_0-bath, which may seem a gratuitous crudeness, to be replaced by a more quasistatic scheme . . . but which would, if possible, reinstate the demon! Szilard at first tries to avoid this seeming crudeness. His equipment is a cold T_A-bath, a hot T_B-bath, the erasing T_0-bath, body K that is the register, but also extra pieces A and B, and a large number of other reservoirs for implementing quasistatic non-entropy-producing processes. After having been written on, K is either at T_A, signifying one of the two y values, or at T_B, signifiying the other. If it were generally known which of these, T_A or T_B, was K's temperature, then K could indeed be reset to T_0 quasistatically, hence without producing entropy. Szilard wishes to convince us that when it is, however, *not* known which of T_A, T_B is K's temperature, *then* erasure does entail the production of entropy demanded by the Second

Law: *indeed this is his essential and correct contribution.* Unfortunately, he has used his pieces A and B "too well." One of A, B is in contact with K. Piece A is at T_A, B is at T_B, and K is at the temperature of the piece touching it, but we do not know which that is. Yet in order to bring K to T_0 *without* producing entropy, we need only to move quasistatically *both* A *and* B to T_0 separately! Then K will go to T_0 automatically and gradually, by conduction of heat through whichever piece it touches.

Having thus seemingly exploded Szilard's central point, I must somehow patch it up: It seems to me that the shifting of contact of K, sometimes with piece A but sometimes B, *itself* requires a lever with two settings, and it is exclusion of this lever's budget of entropy from the discussion that allows a bit to escape scrutiny. Indeed, Szilard's *first* contraption instructs me about levers. It is a cylinder of volume $V_1 + V_2$ containing one ideal-gas molecule, the volume being then split into V_1, V_2 by slipping in a piston sideways. Then if the molecule is in V_1, its pressure will force the piston in one direction; if the molecule is in V_2, however, the force will be oppositely directed. *A lever* is provided, to in either case cause the force to *raise* a weight, thus seemingly achieving a demonic engine—if we forget to bookkeep entropy for the lever, which Szilard does not let us do in this case.

But adding *detailed* consideration of a lever, except to fend off Szilard's unwitting A–B–K demon, is *not* instructive. The purpose of Szilard's body K is to see the dissipation happen: If that dissipation instead happens elsewhere, in some extra lever, then that will involve another body K', and we will have made no progress at all! So I got rid of pieces A and B, and let K (or reg) itself touch the dissipant entity. Indeed Szilard's *mathematics* pays no attention to his pieces A and B. The strategy is to refuse to complicate with extra registers, and so to show the fallacy of simple demons. Then the Second Law itself, having survived Maxwell's assault, gains our confidence, and so causes us to lose interest in building other demons.

10. CLASSICAL DEMONS AND ORTHOGONALITY IN HILBERT SPACE

Is it possible to revive the paradoxical disappearance of entropy by changing the construction of reg to make er smaller? No; to have unambiguous separation of cases, the different outcomes must write reg into mutually *orthogonal* states, which already fixes the model. It would be silly to go on for my original problem, the statement of which stems from a quantum mechanical context. But the problem of Maxwell's demon antedates quantum mechanics, which may make us wonder whether Szilard's solution is as essentially quantal as it seems to be, from his use of a register with two energy levels. Indeed, if classical logic is allowed (Birkhoff and von Neumann, 1936; Jauch, 1968; Finkelstein *et al.*, 1962), er can be made arbitrarily small, thus breaking Szilard's solution: Just let the several pure recording states of reg all make arbitrarily *small* angles in Hilbert space with one common pure state vector y_0, and erase to y_0. E.g., have Szilard's two settings of y be two linear polarizations of a photon, but separated by only a small angle. If we think about this classically, the electric vector will have slightly distinct directions, and that is classically enough to cause unambiguously distinct consequences. The demon does work classically. I leave conversion of my blend of Hilbert space with "classical logic" into a thoroughly engineered classically mechanical demon as an "exercise"!

From the Second Law to Wigner's Principle. Contrapositively, we may choose to assume the Second Law, which demands that $\text{er} \geq vN$, and so reach a denial of the usefulness of a set of nonorthogonal states as a register. This, then, is a *thermodynamic* foundation for Wigner's familiar principle (Wigner, 1952) that if a measurement unambiguously separates states in always leading to distinct settings of some dial, those states must not only be distinct, they must be orthogonal. Of course, Wigner's argument from unitarity of the overall process in time is undoubtedly clearer . . . unless you set out to *build* time from observation.

11. NOT LANDAU TRACING?

The entropy of any single mixed state Y may be imagined found from Landau tracing of an encompassing pure state: Diagonalize $\Upsilon = \sum p_i |x_i\rangle\langle x_i|$, and use $\psi = \sum p_i^{1/2} x_i \otimes y_i$ for an encompassing pure state's vector, where the y_i are orthonormal and orthogonal to all the x's. Is the entropy of erasure er also of this character?

It has *not* been so computed: The computations of separate er_k were done first, *then* convexly combined to $\text{er} = \sum p_k \cdot \text{er}_k$. The nonlinearity of $x \to -x \ln x$ in Boltzmann's definition of entropy guarantees that if the convex combination were done first, the result would be wrong. In particular, for the optimum case $q_k = p_k$, one would "get" *no* production of entropy upon quenching to T_0 were the wrong order used. The computation of er did not investigate one single density matrix Y; indeed, naively replacing the separate E_k by the single density matrix $\Upsilon = \sum p_k E_k$ just gave a wrong answer.

Yet there *is* in a sense an "encompassing" pure state ψ: the wave function of the system and reg, in interaction together. Nevertheless, the analysis of details within ψ was *not* done by selecting some factor Hilbert space to be Landau-traced out. The physical analogue of such Landau-ignoring of a factor space is subdividing a system into a system of interest and a complementary part to be ignored. This Landau philosophy may, however, not be general, in that *reality* is not subdivided. In the calculation of er, we instead used a *different* simple reality for each outcome k, namely reg set at E_k, we T_0-quenched *that,* and then convexly combined the produced entropies er_k on the excuse of calculating a mean entropy over a long run: a time average rather than an ensemble average. This is also what Szilard does. Hence, since 1929 we have had a calculation of entropy production outside the scope of Landau tracing, and based upon relative reality, albeit disguised as an old-fashioned averaging over time.

It should be noted that Landau tracing does implicitly play its part here: If, in contemplating any single $E_k \to T_0$ quench, we imagine following the detailed unitary motion of reg in interaction with a T_0-reservoir, then no entropy will be produced until we Landau-neglect that reservoir; and that will get you er_k. What I suspect may *not* be attainable by Landau tracing is a unified derivation of er, as distinct from er_k.

A related trouble—for reviving my paradox, not Maxwell's—is the thought that you need never erase if you have enough "clean paper" to write on. My answer to this is that the entropic debt is then paid in advance, when you manufacture all that clean paper. It is roughly if not precisely analogous to "getting work from heat without a cold reservoir" by letting cylinders of ideal gas expand without restoring their original condition.

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REFERENCES

Birkhoff, G., and von Neumann, J. (1936). *Annals of Mathematics*, **37**, 823.

Dirac, P. A. M. (1938-39). *Proceedings of the Royal Society of Edinburgh*, **59**, 2, 122.

Finkelstein, D., Jauch, J. M., Schiminovich, S., and Speiser, D. (1962). *Journal of Mathematical Physics*, **3**, 207.

Jauch, J. M. (1968). *Foundations of Quantum Mechanics.* Addison-Wesley, Reading, Massachusetts.

Latham, R. E. (1951). *On the Nature of the Universe, A translation of Lucretius* (~*55* B.C.), Penguin Books, Baltimore, Maryland.

Lubkin, E. (1974). *Journal of Mathematical Physics*, **15**, 663.

Lubkin, E. (1978). *Journal of Mathematical Physics*, **19**, 1028.

Lubkin, E. (1979a). *International Journal of Theoretical Physics*, **18**, 165-177.

Lubkin, E. (1979b). *International Journal of Theoretical Physics*, **18**, 519-600.

Lubkin, E. (1984). Lie algebras of first laws of thermodynamics for physics without time, *XIIIth International Colloquium on Group Theoretical Methods in Physics*, W. W. Zachary, ed., pp. 275–278, World Scientific, Singapore.

Purcell, E. M., and Pound, R. V. (1951). *Physical Review*, **81**, 279–280.

Szilard, L. (1929). *Zeitschrift für Physik*, **53**, 840–56 [Translation in Wheeler and Zurek (1983).]

Von Neumann, J. (1955). *Mathematical Foundations of Quantum Mechanics*, Princeton University Press, Princeton, New Jersey [Reprinted in Wheeler and Zurek (1983).]

Wheeler, J. A., and Zurek, W. H. eds. (1983). *Quantum Theory and Measurement*, Princeton University Press, Princeton, New Jersey.

Wigner, E. P. (1952). *Zeitschrift für Physik*, **133**, 101.