Tales and Tails and Stuff and Nonsense

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Abstract

In an informal way I review collapse models and my part in constructing them, and I recall some encounters with Abner Shimony. In particular, I address the question of the nature of spacetime reality in collapse models, stimulated by Abner’s criticism of the “tail” possessed by statevectors in such models.

0. Prehistory

Normally, when writing Physics Prose, one is expected to expunge all indications that physics is a human enterprise. But that expectation may cheerfully be waived in a volume honoring Abner Shimony. No one else embodies for me such a graceful blend of physics and kindly humanity. So I will take advantage of this opportunity to reminisce on the physics enterprise which has been the major work of my life, and weave in some tales of some of my interactions with Abner over the years which have been important and memorable to me. I also want to particularly focus on an issue called “tails.” Ten years ago, Abner took, and continues to take on this issue, a position with which I once agreed, but now I am in apostasy. In so doing, I will discuss the larger issue which is raised, namely the nature of the spacetime reality given by collapse models.

I first got to know Abner 29 years ago. We were introduced by Wendell Furry, in his office in the Jefferson lab at Harvard where I had my first job after getting my PhD degree. I was working on my first paper, which was critical of Standard Quantum Theory (SQT). From my undergraduate days I simply could not believe that SQT was a complete picture of nature. I couldn’t understand why my teachers and the rest of the physics community conveyed so forcibly the order “don’t question it, just use it.”

After a good deal of agony I decided not to obey that order. In that paper, among other things, I concluded that the Collapse Rule, the abrupt replacement of a statevector (hitherto smoothly evolving via Schrödinger’s equation) by a projection of that statevector was ill defined because no one could tell you precisely the circumstances in which it should be applied, nor precisely the time of application. Wendell was the only person I had found who was sympathetic to these concerns. In Abner I found another. I remember hearing for the first time about the Everett interpretation from him, which he pointed out had some features in common with some aspects of my discussion. But mostly I remember my delight at receiving his warm encouragement, and the realization that it was possible to earn a living working at Foundations of Quantum theory. After all, I had a data point—Abner.

Well, maybe it was possible to earn a living working at Foundations, but it wasn’t possible for me to do it at Harvard. A few years later, Abner came to visit Howard Stein at Case Institute of Technology, where I had my second job. I vividly recall a conversation we had in the open courtyard. Abner said he was interested in seeing if an experiment with photons could be devised which would test Bell’s inequality, and asked if I was interested in working upon it. I gave that proposal the deep consideration that it required—for one second—and answered no! My reason, I explained, was that I knew how the experiment would turn out, in favor of quantum theory and in opposition to local hidden variables. For some reason, in spite of my argument, Abner did not abandon his idea and the result was the celebrated paper by Clauser, Holt, Horne and Shimony. What did I conclude from this?

I was right! I was wrong!
In normal fields of human endeavor it would be impossible to reconcile such a conflict, but fortunately we are in Foundations and so we have recourse to

**Niels Bohr’s Unabridged Dictionary**

(first edition, Random House)

*truth* (trʌθ), n.; pl. *TRUTHS* (trʌθ̩z)

[Middle English trowthe, trowthe: from Anglo Saxon treowthu]

1. ordinary truth: opposite is false
2. deep truth: opposite is also deep truth

Clearly Abner had given me some personal insight into deep truth. I concluded

You should always listen to Abner!

Actually there is no need for deep truth (even in his humorous moments, Niels Bohr seems to have had a predilection to do public relations work for illogic, i.e., non-sense). I was right about one thing but wrong about another. I was right about the experimental result. But I was wrong not to get involved. I learned that one can think one knows the result of an experiment, but it is only after the experiment has been performed that one knows one knows the result. I also learned, by watching Abner at various conferences, how much fun it is, especially for a theorist, to go around talking about experiments.

Well, maybe it was possible to earn a living working at Foundations, but when Case turned into Case Western Reserve it wasn’t possible for me to do it there, and in 1969 I went to Hamilton College, where it is possible (it is worthy of remark that an unusually large number of people in this field have found a home at small liberal arts colleges).

At Hamilton I wrote a paper on a way of getting around Bell’s inequality if the particles in an EPR-Bohm experiment have three options: not only may they respond as spin-up or spin-down to the apparatus, but they can also elect not to be detected. If the experimenter throws away the data where the particles don’t appear, I showed by a local hidden variable model that one can then get from the remaining data at each angle the correct quantum correlations. This possibility has come to be called “the loophole.” I gave a talk about this at a conference in London, Ontario in 1971, and Abner gently informed me from the audience that an undesirable feature in my model would surely be spotted by an experimenter. It is that the number of detected particles is not constant, but depends upon the angle between the spin detectors. (This defect was later corrected in a model of Clauser and Horne.) I had also worked out a model-independent bound of 14% on the fraction of particles which would go undetected (the best bound I know of now, due to Mermin, is 17%), and concluded in my paper “...it is difficult to take this hypothesis seriously as a physical principle capable of extension to a large group of phenomena: had such large fractions of undetected events occurred in other already performed correlation experiments, it is hard to see how such behavior would have gone unnoticed.” I recall how flabbergasted I was to learn from Abner that photon counters are so inefficient that such large fractions of undetected events occur all the time. Only now, two decades later, is an experiment in the works that seems likely to close the loophole (see Ed Fry’s paper in this volume). I think I know what the result of this experiment will be, but ....

1. Early collapse models and Gambler’s Ruin

Just about this time I had an idea of how to go about correcting what I perceived to be the Achilles heel of SQT, the Collapse Rule. In 1966 Bohm and Bub published a paper in which they describe the collapse of the statevector as due to its interaction with Wiener-Siegel hidden variables. The hidden variables (which any system is supposed to possess with a given probability distribution) guide the superposition of states to evolve (collapse) to one of them. The probabilities and evolution are such that the ensemble of all collapsed states is identical to the ensemble obtained by applying the Collapse Rule. This showed that a modification of Schrödinger’s equation to describe collapse as a dynamical process, instead of an ill-defined postulate, was possible.
However, I felt that the Wiener-Siegel hidden variables had more mathematical than physical appeal, especially because you have to, in effect, collapse them to new values every time a statevector collapse is completed. Bohm and Bub made a suggestion about the characteristic time for this hidden variable evolution, but they never actually wrote down an equation that would do the job. It seemed an unnatural and unnecessary complication.

I thought that it is more natural for some randomly fluctuating quantity to be responsible for driving the statevector in its collapse. After all, many things in nature fluctuate—presumably one could eventually identify the appropriate candidate. But, best of all, I had a simple way for the fluctuating quantity to do the job. I will first describe the mechanism here by a precise analogy I stumbled upon only a decade later. The analog is the Gambler’s Ruin game.

Consider two gamblers, L who starts with, say, $30 and R who starts with $70. This corresponds to the statevector

$$|\psi, 0 > = \sqrt{\frac{3}{4}} |L > + \sqrt{\frac{1}{4}} |R >$$  \hspace{1cm} (1.1)

They toss a coin (= random fluctuation!). Heads, L wins $1 from R: tails, R wins $1 from L. The money each possesses fluctuates, although the sum is of course constant. This corresponds to the evolving statevector

$$|\psi, t > = c_L(t)|L > + c_R(t)|R >$$  \hspace{1cm} (1.2)

where the coefficients $c_L(t)$ and $c_R(t)$ likewise fluctuate, however keeping the sum of their squares constant. Eventually, one or the other gambler wins all the money, and the game stops. This corresponds to

$$|\psi, t > \xrightarrow{t \to \infty} |L > \hspace{1cm} \text{OR} \hspace{1cm} |\psi, t > \xrightarrow{t \to \infty} |R >$$  \hspace{1cm} (1.3L, R)

The punch line is that, if the game is repeated many times, L wins 30% of the games and R wins 70%. This corresponds to (1.3L) occurring .3 of the time, and (1.3R) occurring .7 of the time, just the probabilities assigned by SQT’s Collapse Rule to the $|L >$ and $|R >$ outcomes respectively!

The proof of this is really simple. Let $P(x)$ be the probability that, say, gambler L wins the game when he possesses the fraction $x$ of the total money. Then after the next toss there are two possibilities: either he loses the toss and then wins the game or he wins the toss and wins the game, so

$$P(x) = \frac{1}{2} P(x - .01) + \frac{1}{2} P(x + .01)$$  \hspace{1cm} (1.4)

The solution of this difference equation, satisfying the obvious boundary conditions $P(0) = 0$ and $P(1) = 1$, is $P(x) = x$.

A variant of this game is that when one of the gamblers is down to his last dollar they agree to play for 50 cents, and if he loses that they play for 25 cents, etc., so the game never ends. In the collapse dynamics, this means that a coefficient in Eq.(1.2), say $c_L(t)$, can get very small (and, it should be emphasized, with probability $1 - |c_L(t)|^2$ it will continue to get smaller yet), but it never vanishes. Such a small piece of the statevector is called a ‘tail’, and I will be saying a lot more about it later on.

The strategy I seized upon was to look for a modification of Schrödinger’s equation so that the ensemble of solutions obeys a diffusion equation which embodies (what I later learned is) Gambler’s Ruin behavior. For example, for the two-state system (1.2), set $|c_L(t)|^2 = x$ and suppose the ensemble of x-values obeys the diffusion equation

$$\frac{\partial \rho(x,t)}{\partial t} = \lambda \frac{\partial^2}{\partial x^2} x(1-x) \rho(x,t)$$  \hspace{1cm} (1.5)

($\lambda^{-1}$ characterizes the collapse time) with initial condition $\rho(x, 0) = \delta(x - x_0)$ .

It is easy to see from Eq.(1.5), without solving it, that it provides the essential aspects of the Gambler’s Ruin game. From integration of Eq.(1.5) over $x$ from 0 to 1+ (where $\rho$ vanishes) one learns that $\bar{\rho} \equiv \int \rho dx$ is constant (at value 1) in time. Likewise, multiplication of Eq. (1.5) by $x$ followed by integration over $x$ leads (after an integration by parts) to the result that $\bar{x} \equiv \int x \rho dx = x_0$. This constancy of $\bar{x}$ implies
that the “game” is fair, or what mathematicians call a Martingale. Similarly, multiplying by $x(1 - x)$, one finds that \( dx(1 - x)/dt = -2\lambda x(1 - x) \), so \( x(1 - x) = x_0(1 - x_0) \exp(-2\lambda t) \).

Since \( \rho \) and \( x(1 - x) \) are positive or zero, the only way that \( x(1 - x) \) can vanish as \( t \to \infty \) is for \( \rho \) to vanish where \( x(1 - x) \) doesn’t, and vice versa. This means that asymptotically there is collapse: \( \rho \to a\delta(1 - x) + (1 - a)\delta(x) \) (\( a \) is some constant: the sum of coefficients of the delta functions must add up to 1 since \( \mathcal{P} = 1 \)). But, because of the Martingale property, \( \bar{x}(\infty) = a \) must in fact be the initial value \( x_0 \). Therefore \( \rho \to x_0\delta(1 - x) + (1 - x_0)\delta(x) \) and we have (a continuous-in-time version of) the complete Gambler’s Ruin behavior. It can be shown\(^9\)-\(^11\) that the tails behavior, a game which never ends, is obtained if \( x(1 - x) \) in Eq. (1.5) is replaced by \( |x(1 - x)|^\gamma \), where \( \gamma \geq 2 \).

The only problem was to find a modified Schrödinger equation whose ensemble of solutions would obey the diffusion equation. I went on my first sabbatical in 1973 to the University of Geneva, and both Josef Jauch and John Bell (with whom I had corresponded but had not previously met) were encouraging. But it was only a year later that I found tools in a book of Prigogine\(^12\) and a paper of Chandrasekhar\(^13\) which enabled me to try out various modified Schrödinger equations and find the diffusion equations they imply.

Some weeks after submitting my first paper\(^14\) on this subject, I received a telephone call from Fred Belinfante. He told me that he had been sent the paper to referee, and that he liked it very much. But he said that there was a prejudice against such work in the offices of Physical Review D, and he didn’t want to criticize the paper in any way in his report, for fear it would be used as an excuse not to publish. He had some questions and some criticisms, and so we talked about them. Not long thereafter I was informed that the paper had been accepted, and I made a few revisions to respond to his concerns. For a while that prejudice abated, but in the last six years it has come back. A special set of rules has been framed, aimed at "speculative theoretical papers that lie outside the mainstream of current research," which emphasizes that such work, to be considered for publication, must have experimental consequences. The fact that this work does have such consequences, and a great deal of what they publish does not, doesn’t seem to affect their obduracy.

The equation I proposed was nonlinear in \( |\psi, t \rangle \):

\[
\frac{id}{dt} < a_n |\psi, t \rangle = < a_n |H|\psi, t \rangle + \lambda \sum_m < a_n |A|a_m \rangle \left[ < \psi, t |a_m > < a_n |\psi, t \rangle \right]^{r} < \psi, t |a_n >
\]

I give it here, although a far superior formulation will be presented later, because it illustrates all the issues which had and have to be struggled with.

2. Issues

In this first paper, I assumed \( H \) and the initial conditions are such that the phases of \( < a_m |\psi, t \rangle \) are essentially random. My argument was not so clean because I had not yet encountered the powerful methods of Ito for handling nonlinear differential equations which are linear in white noise. Then I came across a book by Wong\(^15\) which taught me just what was needed. It was possible to write a paper with a much neater argument,\(^16\) with the matrix elements \( < a_n |A|a_m \rangle \) in Eq. (1.6) set proportional to (complex) white noise (for those who care, Eq.(1.6) is then a Stratonovich stochastic d.e.).

White noise, given ab initio, has been used to drive collapse equations ever since. But I have always thought of this as a mathematical convenience: nothing in the physical world has the same behavior at all frequencies. Indeed, only recently I have been driven, by the need to get rid of infinities in relativistic collapse models, to consider non-white noise mechanisms\(^17\),\(^18\) and I regard being forced by the physics to adopt a possibly more realistic noise source as a hopeful sign. However, ultimately I think that the noise will turn out to be an approximation to some more dynamical and interactive quantity.

As to the issue of making a connection between the collapse mechanism and the rest of physics—what is the physical identity of the white noise?—gravity has presented itself as the leading candidate.\(^19\)-\(^23\) At the time I thought gravity was likely (in ref. 14 I gave an example with \( \lambda^{-1} = 10^{13}\) sec involving an apparatus whose collapse time was \( 10^{-15} \) sec, and remarked that \( \hbar \lambda \approx 10^{-8}\) erg, comparable to the
apparatus’s gravitational self-energy: I concluded ref. 16 with a homily on the involvement of gravity), on
the not very profound principle that what is understood least has the greatest potential for surprises and
might therefore be most responsible for collapse. I spent my second sabbatical in 1981 at Oxford with Roger
Penrose’s group, for very early Roger had argued for a connection between gravity and collapse and he has
been active in suggesting some tentative physical principles.20 But think it is fair to say that the formulation
of this connection is only in a rudimentary stage.

A word now about simplicity. Unfortunately, we have not yet even one experiment whose outcomes
disagree with the predictions of SQT to guide us in constructing collapse models—except that events occur
in every quantum experiment and SQT’s crude handling of them indicates the need for a revised quantum
theory. The constraints on a viable collapse model are enormous: to agree with every experiment while
describing collapse. It always has been my hope that these constraints and Dirac’s dictum to look for
simplicity in one’s equations would provide sufficient guidance to this program for at least a while, and that
attractive physical principles would eventually emerge along with constraining experiments, and I believe it
has worked that way so far. However, simplicity is in the eye of the beholder.

Which brings me to tails. I had favored Eq.(2.6) with r = 1 largely on the grounds of simplicity, but I
wanted a physical principle which would back up this choice. In 1984 Abner invited me to speak at Boston
University, and stay overnight at his house. We spoke in his study about choosing r = 1 with no tails or r = 2
with tails, and Abner convinced me (recall principle (0.1)!) that, as I put it in a paper (written because of
this interaction with Abner) on the collapse time, “... the reduction must take place in a finite time because
an experimental result reveals itself in a finite time.” I took this position in subsequent papers10,24 too, that
the presence of a piece of the statevector that corresponds to another outcome of an experiment, no matter
how small compared to the piece that describes the actual outcome, is ontologically undesirable. In this
paper I will eat my words.

There were two other problems which I could not figure out how to handle. From the first I mentioned
them, in every paper and every review11,24–26: I simply didn’t know what to do.

One I call the Preferred Basis problem. In Eq.(2.6) the basis states |a_n > to which collapse takes place
are put in by hand, and are not really described in any precise way. This is identical to, and therefore as bad
as, SQT’s inability to say which states end in collapse except in an ad hoc way. To be sure, I had some ideas,
suggesting13 that it “…depends only upon the the macroscopic distinguishability of appropriate position
variables characterizing the states.” But the preferred states couldn’t be position eigenstates because that
would require the collapse process to impart infinite energy to them.

The other I call the Trigger problem. The operator A in Eq.(1.6) should be small for states that differ
only in some microscopic way, since one doesn’t want e.g., two separated packets belonging to a single particle
to collapse to one packet in a short time, since that would mean the theory predicts no interference will take
place when Nature says it does. On the other hand, one wants rapid collapse when the two packets describe
a macroscopic apparatus variable in two different places. How do you make the additional term small for
microsystems, but large for macrosystems? I just did it by hand, but that was hardly a satisfying solution
to the problem.

3. GRW’s Spontaneous Localization (SL) Model

Then along came the work of Ghirardi, Rimini and Weber.27 They didn’t know about my work, they
didn’t have a modified Schrödinger equation, but they solved my two problems. Actually, I hadn’t appreciated
what they had done when I first read their paper. I was so misled by their emphasis on the density matrix’s
(neat!) behavior that I did not grasp their handling of the collapse mechanism for the individual statevector.
It wasn’t until I read a preprint by John Bell28 that I understood what they had accomplished. The image of
a jigsaw puzzle with only two missing pieces announcing their shape by their holes came to my mind, with
the pieces suddenly found and snapped into place, so clever and convincing were their ideas, and so tuned
were they to what I wanted but hadn’t been able to find.

To understand GRW’s model, consider first a particle described by two packets in one dimension. For
definiteness, let ψ(x) = φ(x) + φ(x − l), where φ(x) is centered on x = 0 with width much less than a = 10^{-5} cm
and l much greater than a (a is one of two parameters in the model). If we suppose the Hamiltonian is zero,
the wavefunction will just sit there until... WHAM, a HIT occurs. That is, the wavefunction is suddenly multiplied by a gaussian, say $\exp(-(x-a)^2/2a^2)$, and immediately renormalized to 1. The result of this is that the left packet is slightly altered in shape, but boosted in amplitude, and the right packet is almost wiped out. A collapse has occurred, with a small tail remaining.

I have deliberately chosen the center of the gaussian to be near the peak of one of the packets because that is a probable thing to happen. Along with the dynamical behavior, the model specifies a Probability Rule. It is that the probability that a hit occurs in the time interval $(t, t+dt)$ and that the center of the hit lies in $(x, x+dx)$ is $\lambda N^2 dxdt$, where $N$ is the norm of the wavefunction after the hit but before renormalization. If the center of the gaussian is far from both packets, $N$ is very small, so that is very improbable. The parameter $\lambda^{-1}$ was chosen by GRW to be around $10^{16}$ sec $\approx$ 300 million yrs, so a microscopic system is seldom affected. Then, why bother?

Here’s why. Let the wavefunction now describe an apparatus “pointer”, containing $n$ particles, in a superposition of two spatially separated states. If any particle is hit, the whole wavefunction is collapsed to one pointer (and a tail). Moreover, each particle in the apparatus has an independent chance to be hit, which makes the mean time for a hit to occur equal to $(\lambda n)^{-1} \approx 10^{-7}$ for a pointer containing $10^{23}$ particles.

So, the GRW solution to the Trigger Problem is that the mechanism is always on, but it is slow when the superposition differs in the positions of just a few particles, and fast when there are many particles with different positions. The Preferred Basis is position, but it is modulo a so as not to squeeze the wavefunction too much and impart too much energy.

The GRW model is very nice, but it isn’t a modified Schrödinger equation. Also, the hitting process destroys the symmetry of the wavefunction. Since they had solved my two problems, I wondered if I could combine what I had been doing with what they had done.

4. Continuous Spontaneous Localization (CSL) Model

I was on my third sabbatical in 1988, and through the intermediacy of John Bell it was arranged that I would first spend three months in Trieste with GianCarlo Ghirardi and, after that, a month and a half in Pavia with Alberto Rimini. GianCarlo and I became friends right away. No one else, I think, believed so much in the essential correctness of this approach as John Bell and we did. We spent the time trying to construct a relativistically invariant density matrix evolution describing collapse, with indifferent success. However, along the way, I tried to get the SL density matrix evolution equation from a modified Schrödinger equation.

One day, in spite of my experience with nonlinear collapse terms, I said to myself “if it’s fundamental, it ought to be simple, and linear is simple”, and I started looking for a way to get a linear term to describe collapse. To my amazement, once I had asked the question, a scheme appeared that seemed to have promise. However, GianCarlo and I were so focused on our relativistic problem that I put it away for some other time. Near the end of my stay we visited John Bell in Padua where he was giving a lecture. Afterwards we spoke with John about our difficulties with relativistic collapse, and also I mentioned this new idea. However, in trying to explain it, I realized that I didn’t understand it very well, and I determined to work on it first chance I got.

That chance came when I visited Alberto, who graciously didn’t object to my working on it, showing mild polite interest at first, which turned into strong interest as the work neared completion. As for myself, I never had an experience with a problem which was as wonderful as this. Everything I calculated turned to gold. Every equation simplified, every question I asked had stimulating answers. The whole thing took seventeen days, from start to completed paper. I would calculate during the day, write up the results at night, and my wife Betty would come to the University with me each day and type it up on the computer. Each late afternoon as we left the University and took the long walk through the streets of Pavia I kept repeating that I could not believe that this was happening to me. A few years ago I heard Roger Penrose give a talk at Syracuse University, arguing that mathematics is not invented, but it is out there, like Platonic Ideals, waiting to be discovered. I had felt that way, that I was uncovering something which had been waiting.

When I had finished the paper29 (which subsequently languished for 4 months at Physical Review D awaiting their development of the “outside the mainstream” criterion for rejection), GianCarlo came to
visit Alberto, as they both had a meeting in Milan. Apparently they spent the weekend working on my paper because on Monday we met, and they had some very nice things to say about it, and also some very interesting results of their own which followed from it. We decided to join some of my results with theirs and write a paper together. Alberto asked me what I wanted to call the new model. I thought deeply—for one second—and because I was combining my concern with continuous collapse evolution with their SL model I replied, “Continuous Spontaneous Localization.” Alberto said “But,” GianCarlo said “Shh,” and CSL it is.

Without any more ado, here is the structure of CSL. The evolution equation is

\[
\frac{d}{dt}|\psi, t >_w = -iH|\psi, t >_w - \frac{1}{4\lambda} \int d\mathbf{x} [w(\mathbf{x}, t) - 2\lambda A(\mathbf{x})]^2|\psi, t >_w \tag{4.1}
\]

I have to explain what is in (the Stratonovich) Eq.(4.1). \(\lambda^{-1}\) is GRW’s characteristic time. \(w(\mathbf{x}, t)\) is any classical field: you put it in, and solve for the statevector \(|\psi, t >_w\) which evolves under it. The operator \(A(\mathbf{x})\) is essentially the particle number operator for a sphere of radius \(a\) (= GRW’s length parameter) centered on the point \(\mathbf{x}\):

\[
A(\mathbf{x}) \equiv \int d\mathbf{z} \xi^+(\mathbf{z})\xi(\mathbf{z}) \frac{1}{(2\pi a^2)^{3/4}} e^{-\frac{1}{2a^2}(\mathbf{x} - \mathbf{z})^2} \tag{4.2}
\]

(\(\xi(\mathbf{z})\) is the annihilation operator for a particle at \((\mathbf{z})\). It is to \(A(\mathbf{x})\)’s joint eigenstates that collapse tends (any other set of commuting operators in place of the A’s would drive collapse to their joint eigenstates). Incidentally, it is this choice of \(A\) which ensures that the symmetry of the wavefunction is maintained during collapse. Note that the evolution is not unitary, so the norm of the statevector generally changes as it evolves.

You can put any field \(w(\mathbf{x}, t)\) you wish into the evolution equation (4.1), but what is the probability that Nature chooses that field? In addition to Eq.(4.1), we must specify a Probability Rule:

\[
\text{Prob}[w(\mathbf{x}, t)] = Dw \frac{<\psi, t|\psi, t >_w}{\psi, t >_w} \tag{4.3a}
\]

where \(Dw\) is the functional integration element

\[
Dw \equiv C \prod_{\mathbf{x}, t'} dw(\mathbf{x}, t') \tag{4.3b}
\]

\((C\) is chosen so that the total probability in (4.3a) is 1). Eq.(4.3) says that statevectors with the largest norms are most likely to occur.

That’s it! All that is left to do is draw the consequences. There are many ways to see how CSL works, but we will just give one illustration. One can think of CSL as providing “little hits.” Eq.(4.1), applied to a single particle in one dimension, becomes:

\[
\frac{\partial \psi(x, t)}{\partial t} = \int dz w(z, t) \frac{1}{(\pi a^2)^{1/2}} e^{-\frac{(x-z)^2}{2a^2}} \psi(x, t) - \lambda \psi(x, t) \tag{4.4}
\]

which may be written as

\[
\psi(x + dx, t) = \psi(x, t) + \int dz dB(z, t) G(x - z) \psi(x, t) - \lambda dt \psi(x, t) \tag{4.5}
\]

\((w \equiv \partial B/\partial t)\). Eq. (4.5) says that, in the time interval \(dt\), there are two terms which modify the shape of \(\psi(x, t)\). There is added to \(\psi(x, t)\) what amounts to a hit of infinitesimal amplitude: say that in a two packet situation, the left packet is thus augmented by an infinitesimal amount. There is subtracted an infinitesimal amount proportional to \(\psi(x, t)\): both packets decrease slightly. The net result in this example is an infinitesimal relative increase in the size of the left packet over the right packet. Instead of a sudden hit, the packets are playing the Gambler’s Ruin game.

In anticipation of the rest of this paper, it should be emphasized here that, just as GRW’s SL model leads to tails, so does CSL: this Gambler’s Ruin game is of the never-ending variety.

Often when a new development in physics takes place there is a new development in mathematics that goes along with it. In this case, abstracting to general operators \(A\), what was found was a linear
statevector evolution equation describing collapse which gives rise to the most general Lindblad (Bloch) evolution equation for the density matrix.\textsuperscript{30} Just before I left Pavia, Alberto received a paper\textsuperscript{31} from Nicolas Gisin who obtained, in the abstract case, a nonlinear evolution equation (essentially what we got when we combined Eqs.(4.1) and (4.3)) for the statevector which did the same thing. A year later, Nicolas brought to my attention a paper by Slava Belavkin\textsuperscript{32} who had arrived at the linear statevector evolution equation in the abstract case, in a context totally different from collapse models: he was modeling continuous incomplete measurements in SQT. I wonder why we (see also the work by Lajos Diosi\textsuperscript{33}) came across the same mathematical structure at almost identically the same time: it sometimes seems as if ideas have their own lives, independently of people.

5. Spacetime Reality

In the beginning, Schrödinger tried to interpret his wavefunction as giving somehow the density of the stuff of which the world is made.\hfill John Bell\textsuperscript{34}

At Erice in 1989, Alberto and GianCarlo reviewed SL and CSL\textsuperscript{35} and I spoke about attempts at a relativistic CSL.\textsuperscript{36} John Bell gave a marvelous criticism of the flaws in SQT,\textsuperscript{34} indicting it sarcastically with the useful acronym FAPP (For All Practical Purposes). John was in fine fettle (at the discussion following his talk, when David Mermin spoke up in defense of his friend and colleague Kurt Gottfried’s position on SQT which John had criticized, John snapped “FAPP-trap”). One evening at a festa, with Abner, Alberto, GianCarlo and others around, I raised the issue of tails to see what John would say, and I got roundly teased for my pains. We spoke about “stuff,” the word he had recently coined to characterize the squared wavefunction in configuration space $|\psi(x_1, \ldots)|^2$, which he saw as the appropriate quantity to sustain Schrödinger’s original vision of reality, at last feasible because of collapse models.

The last time Abner, David and I saw John, at a conference in Amherst (another small liberal arts college involved in Foundations!) in 1990, I characterized his teasing (I had spoken of tails as “what is contains a little bit of what might have been”) as a warning “not to express a new idea in an old language,” and John smiled and nodded broadly. His last gift to me was in his summation of the conference, when he said “The GRWP theory is the most important new idea in Foundations since I entered the field” (surely Bell’s Inequality deserves mention here?—but that was typical of John’s generosity). A few months later GianCarlo called me to tell me that John had died of a stroke. Like many others, we miss him greatly and we think and talk of him frequently.

At a memorial session in 1990 in Minneapolis to honor John, GianCarlo and I discussed nonrelativistic and relativistic CSL\textsuperscript{37} and Abner\textsuperscript{38} presented eight desiderata for such modifications of SQT. CSL fared fairly well under Abner’s sieve, except for desideratum d:

\textit{If a stochastic dynamical theory is used to account for the outcomes of a measurement, it should not permit excessive indefiniteness of the outcome, where “excessive” is defined by considerations of sensory discrimination.} This desideratum tolerates outcomes in which the apparatus variable does not have a sharp value, but it does not tolerate “tails” which are so broad that different parts of the range of the variable can be discriminated by the senses, even if a very low probability amplitude is assigned to the tail.

In what follows I would like to address Abner’s criticism. Essentially, I believe he has succumbed to my disease, of viewing a new theory in an old language. As a new language appropriate to the new theory I want to suggest a notion of stuff that resides in real space, not configuration space. I will distinguish three kinds of spacetime reality: objective, projective and subjective.

5.1. Objective Reality
In spite of the fact that my name is on the papers\textsuperscript{37,39} which introduce the definition of objective reality given here, it was really GianCarlo and Renata Grassi who saw the need for this in relativistic CSL. Indeed, one definition the dictionary gives of “objective” is “real,” so until we discuss the relativistic case it may seem that this subsection has a redundant title.

We begin by asking, when does a state $|\psi>$ possesses the property $A = a$? Here I use $A$ to represent a quantity like charge (sometimes $A$ will represent the associated operator), and $a$ stands for its value, e.g., $2e$. In SQT we have the criterion

$$<\psi|P_a|\psi> = \theta(a - a) = 1$$  \hspace{1cm} (5.1)

(here and hereafter we are taking the statevector to be normalized to 1). Then in fact $|\psi> = |a>$, and a measurement of $A$ is sure to give $a$. I think no one would object if we then say that $a$ is the objective value of $A$ for the state $|\psi>$. This is all very well, but this definition of objective reality is global, not local. While, above all, it is the correspondence

$$\text{reality} \leftrightarrow \text{statevector in Hilbert space}$$

which captures the fundamental ontology behind collapse models, we live in spacetime, not Hilbert space. What is the reflection of that reality in spacetime?

So let us specialize to quantities $A^V$ contained in a volume $V$, such as particle number (of any type), mass, charge, spin, energy, momentum, angular momentum, etc. For example, consider the particle-number-in-$V$ operator

$$A^V \equiv \int_V d\xi^\dagger(\xi)\xi(x) = \sum_{n=0}^\infty nP_n^V$$  \hspace{1cm} (5.2)

where $P_n^V$ is the projection operator on $n$ particles in $V$:

$$P_n^V \equiv \frac{1}{n!} \int_V \cdots \int_V \int_x \cdots \int_x \xi^\dagger(x_1) \cdots \xi^\dagger(x_n)|0>_V \cdots |0>_V \otimes 1_V$$  \hspace{1cm} (5.3)

($|0>_V$ is the vacuum state in $V$, and $1_V$ is the identity operator outside of $V$). We immediately encounter a difficulty with our definition of objective reality.

Let $V$ be a sphere of radius $10^{-6}$ cm, and suppose that a hydrogen atom lies with its nucleus at the center of the sphere. Let us ask the question: is the electron in $V$? We readily calculate that $<\psi|P^V_a|\psi> \approx 1 - 10^{-169}$. I would like to say that the electron is in $V$ (after all, even if one makes measurements every picosecond for the age of the universe, the probability is miniscule that the electron will be found outside of $V$) but the criterion (5.1) doesn’t allow it. It is not unreasonable to say that (5.1) is unreasonable, and so we adopt a new criterion for $a$ to be an objectively real value of $A^V$ for the state $|\psi>$.\n
$$<\psi|P_n^V|\psi \geq 1 - \epsilon$$  \hspace{1cm} (5.4)

This deserves some discussion. One may feel uneasy that a criterion for spacetime objective reality depends upon the (exceedingly small) parameter $\epsilon$ whose value however is to be determined by subjective agreement. But isn’t this true of everything we call real, indeed isn’t it the best that can be done? I say the dish is in the sink (or, more generally, assert the location of anything), even though quantum theory and/or statistical physics predicts a tiny chance that when one looks it will be found elsewhere. I say the egg is broken (or, more generally, assert that anything has occurred) even though there is a tiny chance that when one looks not only will the egg be whole, but all records of its breakage will be blank.

Indeed, as Abner suggests, the only reality we are able to account for, or should be required to account for, in a scientific theory is the reality apparent to the human senses and the human experience. Then it appears proper that the criterion for spacetime reality requires human agreement, to conform to that experience. I regard the tail as a tiny bit of noise in the presence of a large signal, and we have many examples of how the human system—indeed, any device—only sees the signal in such cases. Perhaps when...
we have a better understanding of the brain and its possible dependence upon quantum behavior we will understand more deeply how a brain in a superposed state does or does not perceive reality. But until then, I do not think it is reasonable, and worthy of a desideratum, that we should expect more of the human apparatus than of any other apparatus in terms of its ability to detect noise in the presence of a signal.

5.2. Stuff and Projective Reality

I believe that objective reality is not sufficient to capture the full reality content of CSL. Consider an electron in the state $|\psi\rangle = (1/\sqrt{2})|L\rangle + (1/\sqrt{2})|R\rangle$, where the wavefunction for $|L\rangle$ is nonzero only inside a volume $L$, and the wavefunction for $|R\rangle$ is nonzero only inside a different nonoverlapping volume $R$. Then

$$<\psi|P^L_0|\psi> = <\psi|P^R_0|\psi> = <\psi|P^L_1|\psi> = <\psi|P^R_1|\psi> = \frac{1}{2}$$

so neither particle number 0 or 1 inside $L$ or $R$ is an objective value for $|\psi\rangle$. Nonetheless, I want to be able to say that something is really inside $L$ and $R$. So I am going to define

$$<\psi|P^V_a|\psi>$$

and regard stuff as (projectively) real. This is another kind of reality than objective reality. The latter is what is produced in the usual von Neumann strong coupling impulsive measurements, designed so that, if collapse did not take place, the apparatus is forced into a superposition of macrostates, each corresponding to a different eigenstate of operator $A^V$, one of which collapse makes objectively real. While I don’t believe that a theoretical notion of reality has to include a method of experimental verification, it doesn’t hurt that Aharonov and Vaidman have shown that stuff can also be measured in certain circumstances by so-called protective weak coupling measurements, where the apparatus is never forced into a superposition of macrostates: it just indicates the projective value.

In what follows I will specialize to particle number $n$-stuff in $V$ which for short, and if there is no chance of confusion, I will sometimes call particle number stuff, or $n$-stuff in $V$, or $n$-stuff, or even just stuff:

$$<\psi|P^V_n|\psi> = \left(\frac{N}{n}\right)\int_V dx_1 \cdots \int_V dx_n \int_{\bar{V}} dx_{n+1} \cdots \int_{\bar{V}} dx_N |\psi(x_1, \ldots, x_N)|^2$$

(it is assumed that there are $N$ particles of this type in the world, and I have used the symmetry of the wavefunction to simplify). One could argue that this is the most important kind of stuff for human beings because our pictures of what the world looks like are literally shaped by the particle densities that surround us. But stuff for other variables can be discussed. Indeed, there is nothing to prevent one from discussing stuff associated with noncommuting variables (stuff is just a c-number), e.g., particle number stuff and momentum stuff are simultaneously in $V$. Indeed, it is a big relief to think of these quantities as actually being there together. This replaces the notion of “propensities” of Heisenberg which always caused me consternation: I find it hard to think of something that might show up in the future if the right experiment just happens to be performed, as having some real status.

When the $n$-stuff in $V$ achieves the value 1-$\epsilon$, then the projective reality becomes objective. Thus, instead of the statement “so much particle number $n$-stuff is in $V$” one may then say that “$n$ particles are in $V$.” It is important and interesting to consider the differences between stuff and particle number. For example, the sum of $0$-stuff+$1$-stuff+$\ldots$+$N$-stuff in any $V$ is conserved ($=1$), but the sum of particles in $V$ is not conserved. Conversely, if one divides all of space into cells, the sum of e.g., 1-stuff in all cells is not conserved. It can range from a maximum of $N$ (if, for example, the wavefunction is such that each particle is in a different cell) to zero (if, for example, each cell happens to hold two or more particles). However, the particle number in all cells is conserved (assuming fermions with no particle creation).

I think that the most interesting aspect of the difference lies in the flow in spacetime. For example, consider $N=2$. It is easily found from the evolution equation (4.1) that
reality would eventually become projective. Without collapse, I venture to say, objective reality would eventually disappear, and all collapse after a measurement takes place. This is the only mechanism which permits projective reality to replace objective reality.

Outside of V, no longer is \(|\psi(x_1, x_2, t)|^2\) vanishes unless one argument lies in V and the other lies in \(\bar{V}\), and the integrals cancel. (If \(\psi(x_1, x_2, t)|^2\) vanishes unless one argument lies in V and the other lies in \(\bar{V}\), the source term is still negligibly small). Moreover, the surface integral in (5.9) will vanish, at least for a while, if the stuff of either particle is far from the boundary. Thus when reality is objective, Eq.(5.9) describes the particle number’s conserved local flow in spacetime, at least for a while (which may be prolonged by moving or distorting V). On the other hand, when reality is projective, the 1-stuff flows nonlocally as well as locally.

The reason for this behavior is that collapse narrows wavefunctions in spacetime. For example, if the whole wavefunction of a single particle is contained in V, the collapse does not transfer any of it out of V. But if the wavefunction lies partially in V and partially out of \(\bar{V}\), there is a transfer regardless of the extension of the wavefunction. It is this nonlocal flow of stuff that makes a collapse theory violate Bell’s inequality and agree with experiment and SQT.

This allows some insight into the difference between classical behavior and quantum behavior.

The local flow of an objectively real particle number from one volume of space to an objectively real particle number in an adjacent volume is what classical physics is about. An objective number of particles (of every type) in a macroscopic object can be imagined wrapped in a volume V which moves like a rigid object in spacetime. Even a microscopic particle, as long as it moves freely, can be surrounded by a large enough or growing volume so that its particle number, charge, energy etc. are objective and flow locally in spacetime.

However, particle number can flow into stuff, and that is what quantum theory is about. For example, when a measurement with different possible outcomes takes place, many particles of the macroscopic apparatus move into different volumes of space. For another example, if a single particle scatters or if it spreads outside of V, no longer is \(\psi(x_1, x_2, t)|^2\) vanishes unless one argument lies in V and the other lies in \(\bar{V}\), the source term is still negligibly small). Moreover, the surface integral in (5.9) will vanish, at least for a while, if the stuff of either particle is far from the boundary. Thus when reality is objective, Eq.(5.9) describes the particle number’s conserved local flow in spacetime, at least for a while (which may be prolonged by moving or distorting V). On the other hand, when reality is projective, the 1-stuff flows nonlocally as well as locally.

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5.3. Relativity and Objective Reality

In this subsection we will see that the “peaceful coexistence” (one of many apt and useful terms coined by Abner) of relativity and a collapse model requires a new category of reality, and a redefinition of an old one. A relativistic version of CSL has been constructed, with some successes and some problems.\textsuperscript{36,37,39,41,42} The main point relevant here is that collapse in each Lorentz reference frame proceeds essentially just like nonrelativistic collapse.

The consequence of this for us can be made clear by the following example. Consider a particle in reference frame $o$ which, for $t < T$, is in a superposition of two equal amplitude spatially separated packets with worldlines at $L$ and $R$. Therefore, for $t < T$, the 1-stuff on each worldline is equal to $1/2$. Call $\alpha$ some such small spacetime neighborhood on $R$. Suppose that at time $t = T$ a particle-detecting apparatus situated near $R$ is turned on, and the result is that the particle is not detected. Therefore, in this frame, for $t > T$, the 1-stuff is $1 - \epsilon$ along $L$ and the 0-stuff is $1-\epsilon$ along $R$, so partielnumber has the objective values 1 along $L$, and 0 along $R$. Call $\beta$ some such small spacetime neighborhood on $L$.

Now consider a different frame $o'$, which, at time $t' = T'$, passes through $\beta$ and $\alpha$. In this frame, the measurement has not yet begun, so the 1-stuff at $\beta$ and $\alpha$ is equal to $1/2$. But this means that, the two frames differ concerning the amount of stuff at $\beta$: according to our definitions, frame $o$ says that that the reality is objective at $\beta$ while frame $o'$ says it is projective.

So the first lesson we learn is that the amount of stuff in a spacetime region can be frame-dependent. Of course, this is not a new thing in Relativity: the length of a stick, the time interval on a clock, the energy of a particle, etc., all are frame-dependent. Considering that, in collapse models, reality corresponds to a statevector in Hilbert space, and that the statevector is frame-dependent, it should not be surprising (although it may be disappointing to preconceived hopes) that reality can be frame-dependent. Yet, although reality \emph{can} be frame-dependent, it doesn't \emph{have} to be frame-dependent. I think that the circumstances in which it is not frame-dependent, first really appreciated by GianCarlo, is quite satisfying.

We are led to define three kinds of reality in a relativistically invariant way, contingent upon what is seen in all reference frames which pass through a spacetime region. If in all frames the stuff is projective, then we will say that reality is projective in that region. If in all frames the criterion (5.4) is satisfied, then we will say that reality is objective in that region. If in some frames the stuff does not satisfy (5.4) and in some other frames it does, we will say that reality is subjective (= frame-dependent) in that region.

Now we note that, in the example above, objective reality exists only on and within the forward light cone of the measurement region (on the $R$ worldline immediately after the measurement, but on the $L$ worldline only beyond where it cuts this light cone). That is, the message from a measuring instrument which calls objective reality into existence travels no faster than the speed of light. Isn't that nice?

A few more comments.

The collapse location (where a packet grows or diminishes) can be objective (agreed upon by all observers) or subjective (not agreed upon). In the above example, the location on $R$ is objective and the location on $L$ is subjective. The collapse location is not a thing that can be measured, so there is no need for this location to be objective.

In the above example, all frames agree that the cause of the collapse and the subsequent emergence of objective partielnumber at $L$ was due to the apparatus being turned on at $R$. But the cause of collapse is not always necessarily objective as it is in this case. Consider a situation in which there are two apparatuses, one at $L$ as well as one at $R$, and where both are turned on simultaneously in one frame. This leads one to conclude that the cause of collapse in some frames is the turning on of the apparatus at $L$, and in other frames it is the turning on of the apparatus at $R$. The cause of collapse is not a thing which can be measured, so there is no need for it to be objective.

Finally, I want to give one more reason for tails: I can’t see how to make a relativistic theory without them. If you have a tail, no matter how small, and you know the field $w(x, t)$ which the state vector evolved under, you can run the evolution equation (4.1) backwards and recover the statevector at any earlier time. If on the other hand, the tail was completely cut off, you get a nonsensical irrelevant earlier statevector, even in SQT. “So what?” you say. “There is no need to run a statevector backwards since time runs forward and one can’t stop that.” Well, but one \emph{can} go to another reference frame, and in so doing the frame sweeps backwards in time. I cannot see how you could get sensible results in another Lorentz frame without having the tail to tell you how to do it. And, this ends the tale I wish to tell.
Acknowledgments

Even though in this paper I have disagreed with Abner, I will always listen to him (see (0.1)) with great pleasure, as I have for three decades. I would like to thank Abner for being what he is.

I would also like to thank GianCarlo Ghirardi and Renata Grassi, who have recently completed their own insightful endorsement of tails, for animated and informative conversations earlier this year (my fourth Sabbatical). This, along with Abner’s comments on tails, stimulated this paper. In addition, I wish to thank the Hughes Foundation for a grant supporting this work.

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