ANPA WEST

Journal of the Western Chapter of the Alternative Natural Philosophy Association

Volume Three, Number One – 1992

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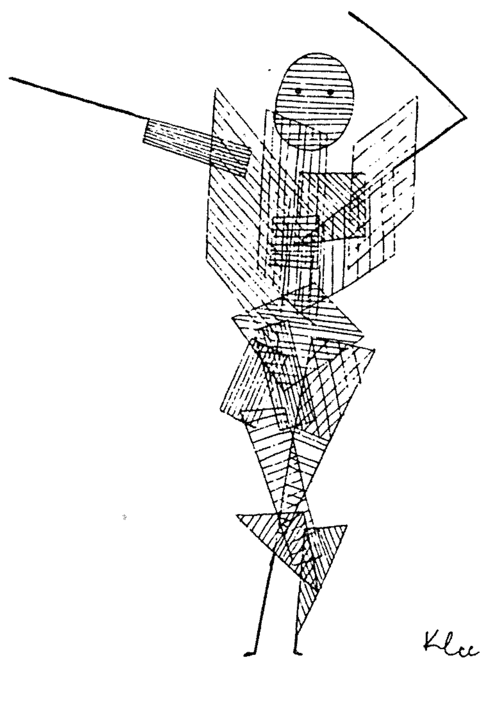
FROM THE EDITOR:
Some thoughts after the election.

For better or worse, it's here in the U.S. that much of the world sees the future. Indeed for a while it was quite a future! In 1961 President Kennedy announced that within a decade we would send a man to the moon. By 1971 we had sent six, and NASA was gearing up to send smart robots to the edge of the solar system. That same decade saw the promise of Lyndon Johnson's Great Society which was to end poverty once and for all. It also saw Woodstock and Earth Day and a rainbow multitude of experiments in new ways of thinking and living: the new age was upon us!

But alas, so was the Vietnam War. As it dragged on, Washington retrenched.



Back came the dreary belicose politicians who "don't care what the facts are" and "stand behind their every misstatement."[1] The mood in Washington was "Turn back the clock!" The mood spread. Peace? ESP? Ecology? Space colonies? Now crackpot ideas. The children of Woodstock saw their experiments in harmonious living break up into cults.

Hope dies hard, but after twenty years there was precious little left.

Which brings us to November 4th. Is it really possible that the public arena has again become a place of hope? A place where people of honesty and imagination can initiate and not just react?

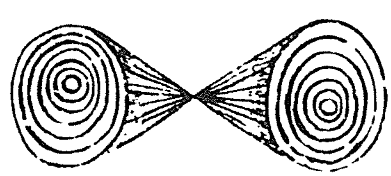
[1] *"I will never apologize for the United States of America - I don't care what the facts are." - George Bush.*

"I stand by all the misstatements I have made." -Dan Quale.

The Future of Our Journal.

In keeping with the more expansive mood of our country, we plan to expand our scope. First of all, we need articles from you. The charter of ANPA is to promote alternative natural philosophy of every kind, and natural philosophy covers a lot of ground. So let's have your new thoughts on space, life, mind, logic, ecology, bio-technology, nano-technology, mega-technology, ESP, flying saucers, or on almost anything, but especially on the meaning of quantum mechanics. We are also looking for book reviews, short comments, news items etc. Letters to the editor are a good format; just keep them short. Remember the golden rule: write for others what you would enjoy reading yourself.

No, we haven't forgotten the interactive journal. And no, we can't promise when you'll see it. We'll provide regular progress reports on its engine, the Racter program; in this issue we have a report on another of his jobs.



IN THIS ISSUE

Anti–Gravity

by Pierre Noyes
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The first suggestion that an undiscovered aspect of gravity might be used for space travel with which I am familiar was made by H.G.Wells. His *First Men in the Moon* used a gravitational *insulator* called Cavourite to surround the space capsule which transported them. Maneuvering was accomplished by opening and closing "shutters" which exposed the gravitating interior to a directional force which depended on the objects in the solar system which were brought into view. The "on-board computer" for the outbound voyage to the moon was Cavour, — the inventor of the substance and designer of the vehicle. That his companion's solo return journey to Earth succeeded was admittedly a matter of extremely good fortune.

I suspect that the Gravity Foundation's prize, offered for scientific essays discussing "gravitational insulators" and related subjects was either directly or indirectly inspired by this story. So far no plausible suggestions have turned up. The analogy is taken from electricity. Electric charges of opposite sign bound to form a neutral matrix are unaffected by electric fields to the extent that polarization of the charges can be neglected, and shield the interior material from external electric fields. For the gravitational analog to exist, there would have to be both gravitic and anti-gravitic "gravitational charges" and some way to form a neutral matrix from them.

The question of whether there are two types of "gravitational charge", and consequently whether some kinds of matter (most simply called "anti-matter") might "fall" UP in situations where ordinary matter falls down is not yet decided. With respect to electric charge, "anti-matter" certainly exists, and has been intensively studied since the discovery of a positron in the cosmic radiation by Anderson in 1932. All the high energy particle accelerators produce particle-antiparticle pairs copiously. The electric charge of each member of such a pair is equal in magnitude but opposite in sign (if one is positive the other is negative) to that of the other member. This is a consequence of a general property of current theories of elementary particles: if the direction of motion of all particles is reversed *and* the description "left" or "right" for all three directions of motion is reversed *and* the particle-antiparticle description is reversed, the resulting theory makes no experimentally observable prediction that differs from that of the original theory. This is called *CPT invariance* — a memnonic for Charge reversal, Parity reversal (mirroring, which interchanges left and right) and Time reversal (reversing the direction of velocities). This applies to every known force, EXCEPT gravity.

The idea that anti-gravity might describe anti-matter has been discussed ever since the CPT theory became compelling to theorists. Experimentalists are properly skeptical about the theoretical arguments — which we discuss below — that removed gravity from the list. Bill Fairbank here at Stanford spent a number of years trying to see whether or not positrons (i.e. positive electrons) "fall" up or down. Unfortunately, the experimental problem of constructing conducting tubes smooth enough to shield out external electric fields defeated him. Electric charge clings to rough patches on the interior of such a tube and is not removed by any known technique to the level of accuracy he would have needed to make a measurement.

The experimental question has been reopened recently, thanks to the success of Gabrielse of Harvard in slowing down and capturing *anti-protons*, which are the negative (anti-matter) counterpart of the positive nucleus of the hydrogen atom (proton). Anti-protons are produced at CERN (Centre European de Recherches Nucléaire) outside Geneva for injection into the high energy accelerators which carry out a major portion of their research program. Some years ago a Low Energy Antiproton Ring (LEAR) was constructed to slow down a few of these robbed from the main program to energies of a few million electron volts. This in itself was quite a feat; the related experimental programs produced results of interest to nuclear physicists for several years. But what was needed for the type of experiment in which we are interested was to slow these anti-protons down by another factor of several thousand million without loosing them by annihilation with ordinary matter. Gabrielese's team succeeded in doing this and holding them in a high vacuum volume using electric and magnetic fields — a "Penning trap". They can stay there for weeks without too much loss!

Since the anti-protons go batting back and forth between two interior positions in the Penning trap, they are going slower at the ends than in the middle. Therefore there is more time for gravity to act on them at the ends than in the middle. Consequently, if anti-protons act like ordinary matter, the paths they follow will be higher in the middle than at the ends. The reverse will true if they "fall" up. Unfortunately Gabrielese's magnificent achievement is still not enough for the measurement to be made. The difference in height between the ends and the middles of the paths is sufficiently large to be detected, all other things being equal. But they are not. There is a lot of electric and magnetic activity at the nearby injector to the CERN accelerators, as well as due to LEAR itself; this "noise" defeats current attempts to shield it out, and swamps the signal which would have to be measured. One scheme is to make the trap portable and move it to a "quiet place" where precision experiments are possible. Gabrielese has already moved trapped electrons from LA to Boston in an ordinary moving van, so this is possible. Money and time (he estimates five years, given the money) are all that appear to be needed.

Another team, under Holzscheiter from Los Alamos, is currently on the floor at LEAR. His Penning trap is similar to the Harvard setup, but is followed by a vertical shielded tube similar in concept to Fairbank's. Unfortunately his funds are probably inadequate to complete his task in the year he currently has available. Even if he gets his trap working during this period, he has no confidence that the patch effect which defeated Fairbank is sufficiently under control to make a gravity measurement. His *a priori* advantage is that anti-protons are 1836 times more massive than positrons, but this may not be enough. He has confidence that, eventually, he can measure gravitational effects on mercury ions, which are yet another factor of 200 heavier. And he can use negative hydrogen ions, which have a mass of 1838 compared to the antiproton's 1836, to calibrate his apparatus. Eventually, he should succeed.

A third approach is to capture positrons (electrons with positive charge) in the same trap as the anti-protons and use laser induced transitions to form neutral anti-hydrogen *atoms*. This was one main topic of discussion at a recent conference in Münich which I attended. A report appears in the September 25 issue of *Science*, **258**, 1858-1860 (1992). Again the estimate of time to result is the order of five years. Once anti-hydrogen is made in this environment, the measurement is straightforward. The weight of individual hydrogen atoms has already been measured by suspending them in a similar environment using an adjustable magnetic field. The same technique is no more difficult for anti-hydrogen atoms.

We return now to the theoretical situation. The first serious proposal that anti-protons "fall" up which came to my attention was a paper by Scott Starson prepared for, but not presented at, the conference *Physical Interpretations of Relativity Theory, II* held at Imperial College in London during September, 1990. I first met Starson there, and had extensive discussions with him then, which have continued. I had not thought about anti-gravity in the context of the ANPA program prior to this encounter. I was surprised to see that it is indeed possible to cast Bit-String Physics into a form which allows a consistent formulation of a theory of gravitational charge; these charges do indeed reverse between particle and anti-particle, as one would expect from CPT. I reported on this at ANPA WEST 7 in February, 1991 in a joint paper with Starson which appears in the Proceedings. Subsequently I have convinced myself that my theory *predicts* anti-gravity for all forms of anti-matter. Since few members of ANPA agree with me, and no scientist other than Starson that I know of outside of ANPA, I review the theoretical arguments against our prediction.

To begin with, our prediction is in flat contradiction with the equivalence principle (i.e. that there is no way to detect a difference between gravitational and inertial mass) and hence with General Relativity. For many physicists this is already sufficient reason to dismiss anti-gravity out of hand. Only particle theorists and others who believe in CPT invariance will pursue the matter further. But the usual context in which CPT invariance arises is in the second quantized relativistic field theory. In such theories the electromagnetic field has quanta with spin 1 while gravitation has quanta with spin 2. There is a general argument that, although the force between two particles which exchange spin 1 quanta is repulsive between a pair of particles or a pair of anti-particles, and attractive between a particle antiparticle pair, it is always attractive between *any* two systems which exchange spin 2 quanta.

However, if one looks at the "proof" of this theorem in more detail, one finds that it does not just depend on the spin of the quanta. In the case of any pair of particles which interact by exchanging particles with integral spin j (in our case j=1 or 2) the momentum change p (or force) must vanish like p^j as p goes to zero. This would be a disaster for the conventional theories, because the major effect observed for small p in electromagnetism is the Coulomb or electrostatic force between charges. For gravitation the only directly measured force is ordinary Newtonian gravity. The spin-2 "gravitons" which the theory predicts cannot be directly detected, and whether classical gravitational radiation has been detected or not is controversial. The way conventional theory gets around this disaster is to insist that the theory be "gauge invariant" as well as "Lorentz invariant". The low momentum limit— if one believes the somewhat tricky mathematics — then produces the desired Coulombic and Newtonian forces out of this theorists hat. But, unlike fields which have a direct connection with the observed motions of test particles, "potentials" whether "gauge" or other, have no directly observable consequences. One is permitted to view them as theoretical inventions, rather than as a transcription of empirical fact into mathematics. I make the technical argument in a paper called *ANTI-HYDROGEN: The cusp between Quantum Mechanics and General Relativity*, available as SLAC-PUB-5856 (September 1992).

The end conclusion is that *if* anti-protons "fall" up, one will have to abandon *both* the equivalence principle (i.e. gravitational mass is identical to inertial mass) *and* relativistic gauge invariance. Such an experimental result would kill two theories with one measurement, which is a good investment when one is looking for a crucial experiment. Fortunately experimentalists are not deterred by theoretical arguments, and are forging ahead as carefully as they can. We may have the answer in five years.

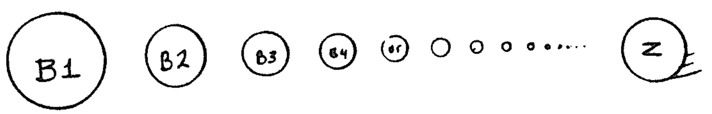
Returning to H.G.Wells' Cavourite, the existence of gravitational charge still does not lead to it in any obvious way. We would have to wait for further articulation of the theory before we could figure out how to construct a bound matrix of gravitational charges and anti-charges. Then we could build a small and convenient space capsule similar to that envisaged by Wells. Anti-gravity by itself does not lead directly to star-ships. Project Sherwood will, some day, lead to practical magnetic "bottles" for protons, which would also work for anti-protons, provides interaction with the surrounding material is sufficiently rare. Making these tanks large enough to make the whole structure gravitationally neutral would, presumably, require a huge ship. But star ships have to be huge for other reasons. Although the overall configuration could be gravito-neutral, the distribution could have a non-spherical ("dipole") shape. Then internal fly wheels would allow something like Well's maneuvering techniques to be used. Direct use of anti-hydrogen as fuel could be reserved for impulsive jet "trimming" rather than using proton-antiproton annihilation directly as a rocket drive.

I have strong hopes that we may see a start on such projects within my own lifetime. I now have a toast for all occasions which I urge the rest of you to adopt:

UP THE ANTI-PROTON

Zeno Ball

by Tom Etter



B1, B2, B3 ... Bi ... and Z are Newtonian billiard balls: They are perfectly rigid, perfectly elastic, and not subject to any forces at a distance, which is to say, a ball can only transfer momentum to another ball by hitting it. B2 is half the size of B1, B3 half the size of B2 etc. and the centers of all the balls lie on a single line L. There is a ball Bi for every i.

Zeno bowls his ball Z along the common center line toward the small end of the line of B's. What happens?

Z cannot hit the ball Bi since it is blocked from it by the next ball Bi+1 on its right. This is true for every i, so Z cannot hit any of the B balls. Therefore it cannot change its momentum, so it keeps moving along line L. The B balls don't change their momentum either, i.e. they remain at rest, so Z must eventually hit Bi!

We see that the Newtonian mechanics of rigid bodies in its simplest and most straightforward form is self contradictory. Is there a way to make it consistent without losing its ideal simplicity? We could of course prohibit an infinite number of balls, but this seems rather arbitrary. Another approach is to enlarge the possibilities for momentum transfer. Here's a way to do this without introducing action at a distance that seems to avoid the paradox.

Define a *body* as any collection of billiard balls whose total mass is finite. We'll say that body A is touching body B if they have a common boundary point. Momentum transfer begans when two bodies touch and continues in a way governed by the conservation of momentum applied both to A and B and their internal components.

In our example, A is Z and B is the collection of all the B balls. Contact begins when Z reaches the rightmost boundary point of B, which is the limit point of the B balls. At any later time, an infinite number of B balls will be bouncing back and forth along line L. This qualitative account of momentum transfer shows how the paradox is avoided; Z doesn't have to hit any Bi since it hits body B at a boundary point which is outside of all the Bi's. But there remains the question of whether it can be made quantitative. Does there exist a joint motion of all the balls that satisfies conservation of momentum? Is such a joint motion unique? Does this kind of solution work for any configuration of billiard balls?

STRICT FINITISM MEANT TO PLEASE THE ANTI-FINITIST

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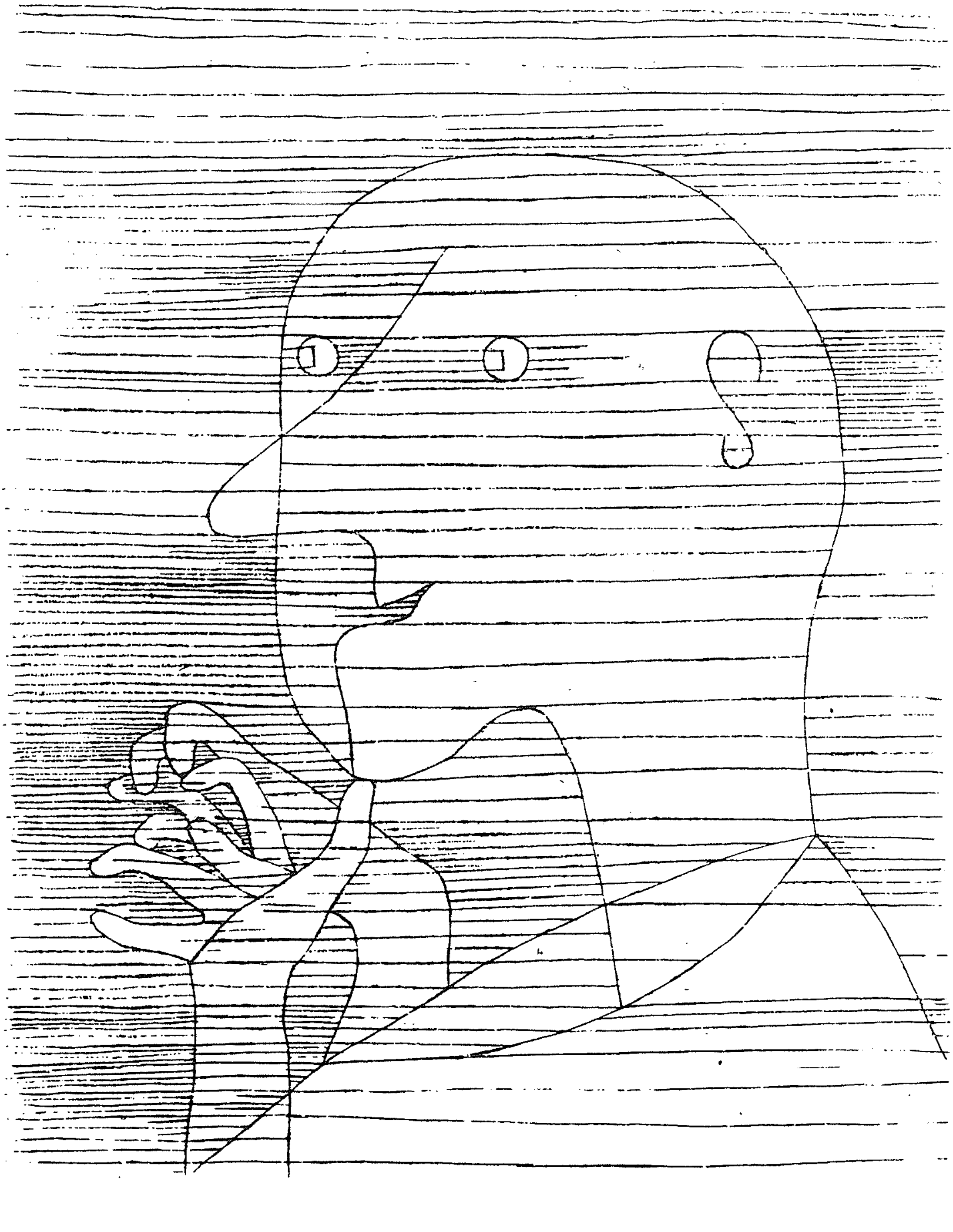
1. Why does anybody want to be a strict finitist?

If one looks at contemporary mathematics, it is obvious that infinity is a key concept. In fact, most if not all mathematicians will tell you that without it mathematics is fundamentally crippled, hardly worth the trouble anymore. Although this may sound rather (if not completely) obvious, it does raise two connected problems for the strict finitist: (a) show that the concept of infinity can be eliminated in mathematics, and (b) show that the remaining mathematics is worth the trouble. Problem (a) on its own is of course absolutely trivial: just eliminate all references to infinity and you're done. The hard problem is (b). In my previous work – Van Bendegem [1987][1] – I have tried to solve problem (b) in the "standard" way (that is, for strict finitists). This amounts to the following. First, (b1) you accept that strict finitist mathematics will be a subtheory of classical infinitist mathematics and secondly, (b2) you try to limit the damage as much as possible, i.e., you do your utmost best to preserve all interesting theorems. Quite similar to the distinction between (a) and (b), one might argue that the difficult part is (b2), whereas (b1) is a completely trivial statement. What I will present in this paper is an (informal) outline of a technique that shows (b1) wrong. This, I think, presents the problem of strict finitism from a quite different point of view. Contrary to what one might expect, this does not render (b2) trivial. I might add, unfortunately so. However, before going into the details, let me briefly recall the main reasons why anyone might feel inclined to strict finitism.

The first reason is the simple observation that we are surrounded by finite limits everywhere. In terms of my personal life, my life–time is strictly finite, my energy is strictly finite, my material means, my memory, etc. Moreover, if one looks at present–day physics, one sees that there too limits are present: Planck length, Planck time, speed of light, the size of the universe (according to the standard model), etc.[2] I think it is quite appropriate to call this type of argument an *empirical* support for strict finitism. Without this empirical backing, strict finitism would seem a rather uninteresting mental exercise.[3]

The second reason is internal to mathematics. It is undeniably true that the work of Georg Cantor and the tradition that originated therefrom is deep and fundamental. David Hilbert definitely did not want to joke when he wrote these famous words: "No one is going to expel us from the paradise that Cantor gave us".[4] However, the paradise being



with us for quite some time now, a number of problems remain. Apparently, it is quite difficult to draw a map of the paradise. To be precise, a number of maps are possible; there definitely is not a unique map. Moreover, each possible map has (necessarily) white spots, corresponding to undecidable statements. Finally, mathematicians themselves seem to have lost touch with these cartographers (or vice versa). In short, the paradise has lost some of its popularity. It is therefore a legitimate question to ask: do we need this paradise at all? Strict finitists are not exactly the first to have asked this question, as it has been discussed a number of times in this century. Intuitionism and the various forms of constructivism have shown we do not need the full paradise. All these proposals however still allow for the potential infinite (in one form or another). Strict finitism looks at the most extreme case: no paradise, no access to it, no sign in its direction. I know that these arguments do not really justify strict finitism, at best they justify why infinity as an *actual* thing is a questionable something. Of course, one might answer that a marginal position is more fun than a more balanced position. Or, more seriously, one might consider the following reason.

The third reason has to do with paradoxes and contradictions.[5] Quite understandably, if one looks at the literature, one sees that paradoxes and the like are typically associated with infinity. The standard argument usually is this: what a paradoxical reasoning shows is that if some statement is true, then it is false, but if it is false, then it is true, thus we get an infinite "oscillating" series true, false, true, ... The entities that are subjects of the paradoxes are typically infinite themselves: the set of all sets, the set of all sets not member of themselves, etc. However, this is only part of the story. Some paradoxes have nothing to do whatsoever with infinity but present themselves in a perfectly finite domain. Familiar cases are Berry's paradox and the finite version of the Sorites paradox. A recent quite important case is the finite analog of Gödel's theorem as presented in the work of Gregory Chaitin.[6] All this implies that rejecting the actual infinite in favor of the potential infinite, cannot be the final answer. It does make sense to go further. Fortunately enough, others have already shown some possible routes.[7] But, as said, all these proposals accept the fact that strict finitist mathematics will be a (weak) subtheory of standard mathematics.

2. Informal presentation of the method.[8]

Let's assume that we are dealing with elementary arithmetic (EA). (See 3.2. and 4. for other theories). The language of EA will consist of signs for specific numbers (k, n, m, ...), signs for non-specified numbers (x, y, z, ...), signs for operations or functions (+ and .) and, most important of all, the equality sign =. With the aid of standard first-order predicate logic – that is, with the logical signs, & (conjunction), v (disjunction), ∼ (negation), ⊃ (implication) and ≡ (equivalence), and the quantifiers ("for all", and "there are some"), we have the full language of EA. As it is usually done in the formal approach to EA, we build up a hierarchy as follows:

k, n, m, ..., x, y, z	. . . . numbers
+, .	. . . . functions
k + n, m + x, x.y, ...	. . . . terms
=	. . . . predicate symbol
k + m = x.y	. . . . atomic formulae
&, v, ...	. . . . logical signs and quantifiers

– for all x, for all y: x + y = y + x
– (n + m = k) & (for all x: x = x) . . . . formulae

Whenever I use capital letters A, B, C, these will refer to formulae (including, of course, the atomic formulae).

All this being done, there are two ways to approach the language EA. Either one stipulates a set of axioms and a set of inference rules (such as modus ponens) and then the main activity consists in proving statements. I will call this in accordance with common logical practice, the *syntactical* view. Thus, if there exists a proof for A, I will write $\vdash$ A.
1c

A different approach is to look for models for the language EA. Now, in EA is it quite obvious what we have in mind as model: namely the natural numbers considered as the well–known set $\mathbb{N} = \{0, 1, 2, ...\}$. Under this view, EA is the language that describes the structure or model $\mathbb{N}$. This I will call the *semantical* view. This way of looking at things has the advantage that it allows us to speak of the truth or falsity of a statement. The idea is quite simply to assume that if A is given, then A is true if A describes correctly a characteristic feature of $\mathbb{N}$. If not, A is false. I will write v(A) = t, resp. v(A) = f, for the statements, A is true under the valuation v, resp. A is false under the valuation v.

Of course, as might be expected, the syntactical and the semantical view are related to one another. On the one hand, we have the property of (weak) completeness:

$\vdash$ A if and only if for all valuations v, v(A) = 1.

On the other hand, we have (strong) incompleteness, i.e.,

it is not the case that for every A, either $\vdash$ A or $\vdash$ ~A.

Needless to say, this last result is precisely (one form of) Gödel's first theorem. This completes the background needed for my presentation.

There is a very well-known technique in logic and foundations of mathematics under the name of *relativization*. The basic idea is to take a statement A of a particular language and to restrict the statement by adding a condition C of some kind. Now, usually the procedure is as follows:

Let (A)r stand for the formula A in its relativized form. Then one stipulates that:

if A is an atomic formula, then (A)r is the same as A

if A is of the form "for all x: B(x)" then (A)r is "for all x: if C then (B(x))r"

if A is of the form "there is an x: B(x)" then (A)r is "there is an x: C & (B(x))r"

if A is of the form B1 & B2 then (A)r is (B1)r & (B2)r

if A is of the form B1 v B2 then (A)r is (B1)r v (B2)r

if A is of the form ~B, then (~B)r is ~(B)r.

Example: take the statement A: "for all x, y: x + y = y + x". Its relativized form (A)r is "for all x, y: if C then x + y = y + x". There are now two possibilities. Either the condition C is such that among the possible models for EA, there are models wherein C is true. In that case, it is not that difficult to prove that if A is true then (A)r must be true as well. Thus, adding the condition C does not entail that true statements are lost. The other possibility is that C is not satisfied in any of the given models. In that case, it depends upon the nature of C whether true statements remain true or not.

Thinking about strict finitism, it seems obvious that the condition C should be something of the following nature: "all terms satisfy a boundary condition, i.e., all terms are bounded by a largest number, say L", in short, "for all terms t, t<L". Need it be mentioned that this condition is not satisfied in the classical model ℕ. Thus, we must see what happens to truth and falsity. Two results should concern us here:

(1) All universal statements, i.e., statements of the form "for all x, B(x)" that are true, remain true. This is straightforward since the relativized form is "for all x, if x < L then B(x)".

(2) Existential statements are a different matter. Suppose that the statement A saying "there is an x, such that B(x)" is true. Then there is an x such that B(x) holds. Now, either x < L

or $x \geq L$. In the first case, (A)r will be true, but in the second case (A)r will be false. This last case is precisely the troublesome case. Not all true statements remain true. Some are lost for they turn out to be false.

I agree with the skeptical reader who is not at all amazed by this result. After all, we are restricting ourselves to a finite subset of the natural numbers, so "naturally" some true statements must get lost. Strict finitism is weaker than the classical theory. And I agree completely.

Perhaps what I will propose now might seem equally obvious, but as far as the consequences are concerned, I believe it is not. As the above mentioned problem has to do with the existential quantifier, let me change that condition. As a consequence, it will be necessary to change the condition on the negation as well. In fact, I suggest the following. Leave the relativization unchanged, except for the condition on existential statements and on negation. These become:

if A is of the form "there is an x: B(x)" then (A)r is "there is an x: if C then (B(x))r".

if A is of the form $\sim$B and B is not an atomic formula, push the negation down to the level of the atomic formulas.[9] If B is an atomic formula, then ($\sim$B)r is $\sim$B, as (B)r is the same as B for atomic formulae.

These changes are minimal for it amounts basically to nothing else than to treat both quantifiers in the same way (and to adapt the negation to that purpose). But see what happens now. Consider once more the case where C is "for all terms t, $t < L$". Thus the relativized form is: "there is an x, such that if $x < L$ then B(x)." Once again, either $x < L$ or $x \geq L$. In the first case, if A is true, so is (A)r. In the second case, if $x \geq L$ then, of course, $x < L$ is false. But relying on classical logic, if X is false, then "if X then Y" is true, no matter whether Y is true or false. Thus in this case too, (A)r is true. Thus true statements remain true statements. Everything seems fine. But is it?

Surely, something must have gone wrong. If, say, the largest number is 100, and I look at the statement "101 is a prime number"[10] then surely this statement cannot be true. But it is! It is true "by default". In terms of the scheme above, this means that the relativized statement is this: "if $101 < 100$ then 101 is a prime number". Since the condition $101 < 100$ is obviously false, the whole statement is true. Phrasing it slightly differently, the relativized statement is true not because 101 is a prime number, but because it is false that $101 < 100$. Actually, there is more. Take the statement A: "101 is not a prime number". Now it is easy to see that necessarily (A)r will be *true*. Again, by default. At this stage, the reader might have become completely confused or even stunned. Am I claiming that both (101 is a prime number)r and (101 is not a prime number)r are true? Yes, I am. Does this mean that EA in its relativized form is inconsistent? Yes, indeed it is. But then (EA)r must be trivial. Here, the answer is (fortunately) no. The argument goes like this.

In classical logic, the connection between inconsistency and triviality is expressed in the famous logical "law": if A and it is not the case that A, then B is the case. The so-called "ex contradictione sequitur quodlibet" states that from a contradiction or inconsistency everything is derivable. In symbols: A, $\sim$A $\vdash$ B. That the law holds, can be seen as follows. Under what circumstances would the law *not* hold? Evidently, if it is possible that both A and $\sim$A could be true together and B is false. But obviously, A and $\sim$A cannot be true together. Hence, one must conclude that under no circumstances does the law not hold, therefore it always holds. What happens in the relativized case? Consider now: (A)r, $\sim$(A)r $\vdash$ (B)r. Suppose that A is a true existential statement. Then, as I have shown, both (A)r and $\sim$(A)r are true. If we now find a statement (B)r that is false – which is easy to do, take 2 = 3, assuming that L is larger than 3 – then we do have a counter-example to this famous law. Hence, it no longer follows that the presence of an inconsistency or contradiction spells doom. (EA)r is inconsistent, yet it is not trivial. Having dealt with the most obvious objection to the method outlined here, let me turn to the next paragraph for an attempt at evaluation.

3. Pros and cons of the method.

3.1. No doubt the most unexpected feature of this approach is that strict finitist mathematics turns out to be an *extension* of classical mathematics and not a reduction. To be precise, the claim is that all true statements remain true. Of course, this does not hold for false statements. Some classical false statements will actually turn out to be true. One might nevertheless argue that this strict finitist notion of truth does not match with the classical notion. After all, some of the true statements are true "by default". Before answering this objection, let me note the following.

Let A be a classical statement and (A)r its relativized form. If the finitist conditions are satisfied – i.e., if all terms t occurring in A are below the limit L – then it is not difficult at all to show that: A holds if and only if (A)r holds. This means that when the conditions are satisfied, the classical and the finitist theory coincide. I think it is appropriate to call the set of all the statements A, such that A is equivalent with (A)r, the *window* of EA. What all this means in quite simple language is this: if we are below the limit L, then whether we do strict finitist of classical mathematics, makes no difference. Only if we pass beyond the limit L do things change radically. The classical mathematician will continue to do mathematics as if there is a domain that corresponds to the statements she is making. The strict finitist loses all interest, hence it is perfectly acceptable that the finitist accepts statements as true "by default". This is no more bizarre than the fact that the classical mathematician too will accept all statements of the form "If 2 + 2 = 5, then A" (no matter what A is) as trivially true. But she too will hardly be interested. However, what remains is that both mathematicians speak the same language (syntactically) but they simply have quite different (semantical) interpretations.[11]

3.2. From the theoretical point of view, this approach has a number of advantages over other strict finitist proposals. As must be obvious by the scheme outlined above, little or nothing has been said about the conditions that are put in front of the (classical) statements. This means that one can "experiment" with an (almost) unlimited number of possibilities.[12] Of course, for (EA)r there is basically just one possibility that is easy to justify, namely the case where one considers all natural numbers up to a limit L. Likewise, for the integers, the most natural choice will be to consider all numbers between –L and L. The next extension is the rational numbers and, after that, the real numbers. Here, the question is anything but trivial. Reasoning quite intuitively, a "natural" approach to the rational numbers, would be to split up the classical set Q of rationals in "equal-sized" parts. In decimal notation this would mean that an element of the finitist version of Q is an interval [a,b] where $|b - a|$ is a fixed value, say 0.001 (in decimal notation). Although this version is most certainly a possible candidate, it is a clumsy one.[13] Therefore, if this were the only possibility, it would mean that strict finitist mathematics is a clumsy business. In the approach outlined here, there is still hope.

3.3. Although strict finitist mathematics is an extension of classical mathematics, this does not imply that proofs are the same in both cases. Within the window, there is no problem. As A is equivalent to (A)r, all the axioms and rules can be applied. Outside of the window, one hardly needs a proof theory at all. In fact, one rule does all the work needed:

from $\sim$C derive C $\supset$ A (or (A)r).

Putting it differently, any classical proof can be rewritten in a strict finitist format. Within the window, one simply copies the classical proof. Outside of the window, one either copies the classical proof with all statements relativized, or one applies the above rule. The advantage of this approach over the "standard" strict finitist approaches, is that in principle all the classical proofs are available to the strict finitist, that is, *syntactically* speaking. Elsewhere I have phrased it like this: whatever the classical mathematician does during the day, the strict finitist can rewrite in the evening.

3.4. The major drawback from a classical point of view is, of course, the fact that this form of strict finitism is explicitly (and, for the desired result, necessarily) inconsistent. I admit that it is no great help to say that the "relevant" part of the strict finitist model is the window and that the window is consistent. For that runs counter to the idea that the strict finitist version is an extension. It is an extension precisely because of what is outside of the window. Thus it is inconsistency one has to go for, plain and simple.

I really have just this one argument: inconsistencies are not all that bad! More seriously, recent developments in modern logic – dialectical logic, paraconsistent logic, relevant logic, etc.[14] – have shown that it is perfectly possible to design logical systems with in-built inconsistencies. Often (though unfortunately, not always[15]) the inconsistent version is a simplification of the (presumably) consistent version. According to the situation under investigation, one is not necessarily obliged to resolve a paradox or contradiction. Why can there be no such thing as the Russell set? Apparently, its defining property ("not being a member of itself") is inconsistent, but why should this imply that the whole theory is wrong? Furthermore, theories that permit the existence of a Russell set, can equally well permit the existence of a universal set, i.e. a set U such that all sets are members of U.

Perhaps a few words must be said about one of the most famous contradictions ever: the Gödel statement G (i.e., the famous sentence that says of itself that it is not provable). Note first that Gödel's second theorem – the consistency of a mathematical theory containing EA is not provable within that theory itself – is now completely uninteresting. Why bother about consistency if the theory is already inconsistent by design? The first theorem is more interesting. At first sight, we seem to have conflicting results: (a) as the strict finitist uses the same language as the classical mathematician, she too can talk about G, (b) classically speaking, G is not provable within EA, and $\sim$G is not provable within EA, in short EA is (strongly) incomplete, and (c) (EA)r must be complete as it has strictly finite models. This seeming conflict is easily resolved once one realises that G cannot be inside the window of (EA)r. If G were in the window, then it is either true or false. Because of weak completeness, this means that either G is provable, or that $\sim$G is provable. But in the window, classical and strict finitist mathematics coincide. We would then have a proof a G in the classical theory and that is impossible (from the classical point of view). But if G is outside of the window, then of course both (G)r and ($\sim$G)r are true by default. Thus, as was intuitively to be expected, Gödel's theorems lose their importance in a strict finitist setting (although one can still talk about them).

4. Work to be done.

The first thing to do, as I have already indicated above, is to extend this approach to the rational numbers and the real numbers (I assume that the complex numbers considered as the cartesian product of the reals, will present no particular problems). This will not prove to be easy, because, as previous attempts by different authors have shown, the hope to restore the idea of infinitesimals seems meager if non-existent. Assuming that problem solved, developing an integral and differential calculus is the next step. That being done, geometry must be dealt with. At first sight, this should not present any special problems if one can rely on the relevant isomorphism between (real) numbers and particular geometric objects, say, a line.

Finally, all that being done, we have the necessary apparatus to do some physics, especially classical and relativistic mechanics. I am rather hopeful that some nice results will come out of this enterprise, as work I have done on so-called "supertasks" has (at least to my mind) convincingly shown that classical and relativistic mechanics are "sensitive" to infinities.[16] In the very far (yet finite) future, I will have no other choice than to deal with that branch of physics every philosophers gets desparate about: quantum mechanics. I have no idea at all what will come out of this in the best of cases. This state of total ignorance suggests this is an appropriate moment to end this paper.

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1. Jean Paul VAN BENDEGEM: Finite, Empirical Mathematics: Outline of a Model. Gent: Werken uitgegeven door de Faculteit Letteren en Wijsbegeerte, volume 174, R.U.Gent, 1987.

2. This argument must be treated rather carefully. The presence of physical limits – expressed in terms of constants – in physical theories does not imply that these theories themselves are finite in terms of the underlying mathematics. Quantum mechanics does use the mathematical formalism of infinite *Hilbert spaces.*

3. The importance of the empirical justification of strict finitism was made clear to me by an argument of Gerald J. Massey. Consider the possibility that the universe, as well as man has an infinite past. Infinitely long ago, a secret society was formed: The Ancient Pythagorean Society (APS). What they have done, is to check case by case Goldbach's conjecture (every even number is the sum of two primes) "starting" at infinity. Thus, n days ago, they checked case 2n. This implies that the APS can in fact decide Goldbach's conjecture. In fact, yesterday they have checked the "last" case. Thus, if the APS were to exist, strict finitism in my view becomes pointless.

4. A notable exception is Ludwig WITTGENSTEIN who wrote in his Remarks on the Foundations of Mathematics (Edited by G.H. von Wright, R. Rhees, G.E.M. Anscombe, translated by G.E.M. Anscombe. Basil Blackwell, Oxford, 1956[1], 1967[2], 1978[3] (revised and reset)): "For if one person can see it as a paradise of mathematicians, why should not another see it as a joke?" (V-7, p.264).

5. The literature about paradoxes and the like is truly immense. Nevertheless, Patrick HUGHES & George BRECHT: Vicious Circles and Infinity. A Panoply of Paradoxes (Jonathan Cape, London, 1975) may serve as a good introduction to the subject.

6. See Gregory CHAITIN: Information, Randomness and Incompleteness. Papers on Algorithmic Information Theory (World Scientific, Londen, 1990).

7. See Ernst WELTI: Die Philosophie des strikten Finitismus. Entwicklungstheoretische und mathematische Untersuchungen über Unendlichkeitsbegriffe in Ideengeschichte und heutiger Mathematik. Bern: Peter Lang, 1987, for an excellent overview of all attempts up to now.

8. What will be presented here in an informal way, has been worked out in full formal detail. The interested reader is referred to my papers "Strict, yet Rich Finitism" (to appear in Z.W. WOLKOWSKI (ed.): Proceedings of the First International Gödel Symposium, held in Paris, France, 1991) and "The Strong Hilbert Program" (to appear in the Revue International de Philosophie, 1992).

9. The basic idea is that any formula A can be rewritten into an equivalent statement A* such that all negations occur only in front of atomic formulae. Thus, $\sim(B1 \,\&\, B2)$ is equivalent to $\sim B1 \vee \sim B2$, likewise $\sim(B1 \vee B2)$ is equivalent to $\sim B1 \,\&\, \sim B2$, $\sim\sim B$ is equivalent to B, $\sim(\exists x)B(x)$ is equivalent to $(\forall x)\sim B(x)$ and, finally, $\sim(\forall x)B(x)$ is equivalent to $(\exists x)\sim B(x)$. Of course, this scheme works because the underlying logic of standard mathematics is classical predicate logic of first-order.

10. In terms of the language of EA, this at first sight simple statement must be rephrased as follows: "for all x, if x divides 101, then x = 1 or x = 101". The phrase "x divides 101" must be replaced by "there is a y, such that 101 = x.y". Hence, the full statement is "for all x, if there is a y, such that 101 = x.y, then x = 1 or x =101."

11. This distinction between what can be said syntactically versus what can be said semantically is, in formal terms, equivalent to a strict finitist version of the downward Löwenheim-Skolem theorems. Although the language of the theory is countably infinite, the models of the theory are finite. However, there is no statement in the theory itself that corresponds to this fact. Thus the fact that, although in the theory of the real

numbers one proves that they are uncountable, there exists a countable model, corresponds perfectly to the fact that, although one proves in (EA)r that the number of primes is countably infinite, yet in the model there are only a finite number of primes.

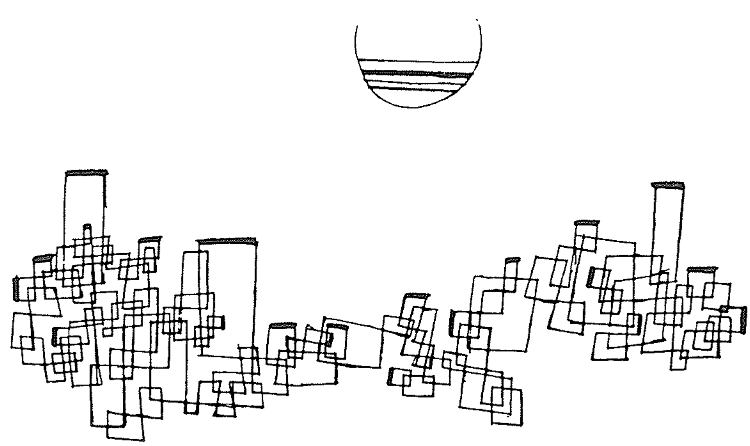
12. In fact, the general theoretical result – derived from a result of Graham PRIEST, "Minimally Inconsistent LP", to appear in Studia Logica, 1992 – states that any partition of a classical model will do. E.g., the model {0, 1, 2, ..., L} correspond to the partition {0, 1, 2, ..., [L, L+1, L+2, ...]} where the square brackets indicate a single element. Thus the number of possibilities is quite impressive.

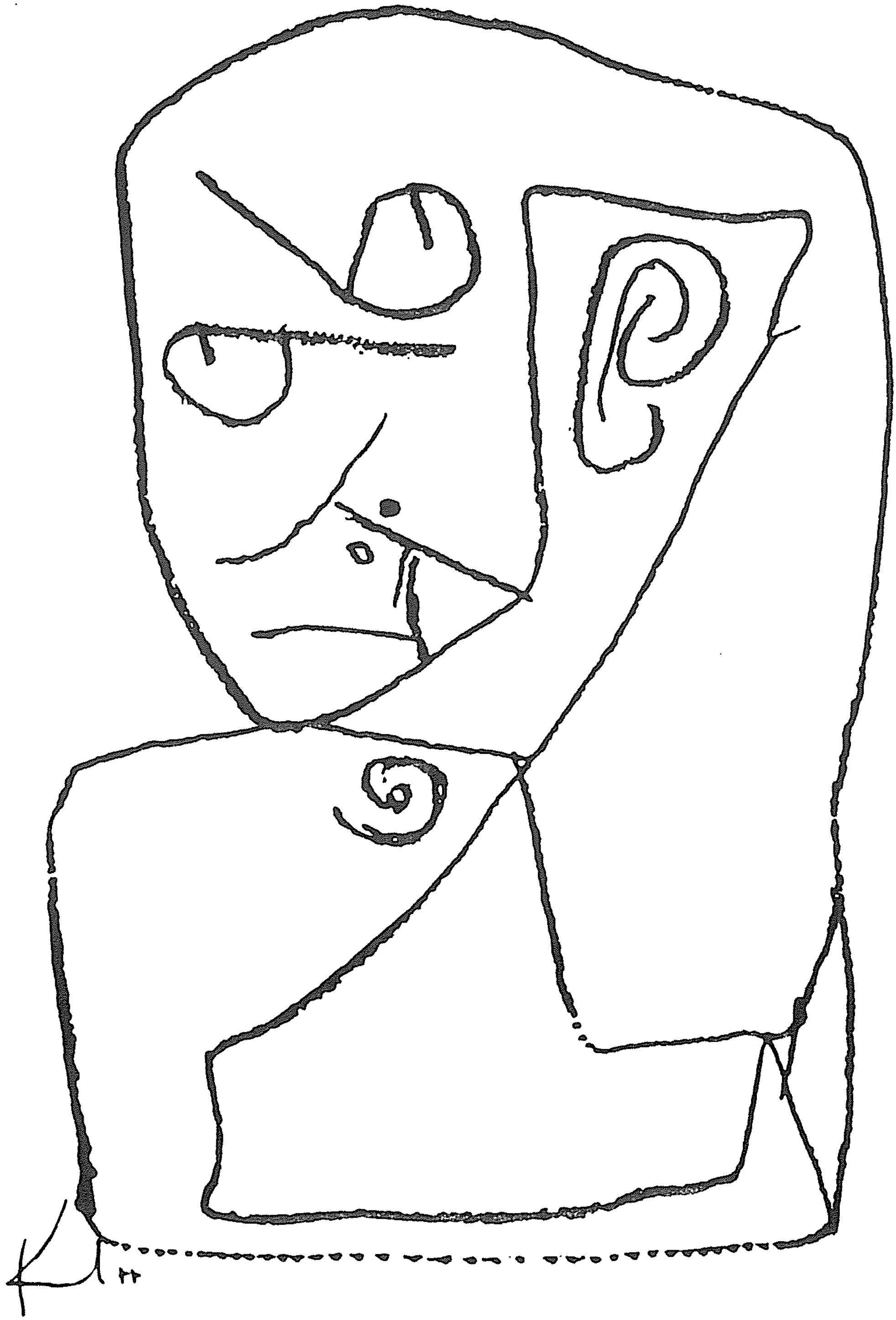
13. The most troublesome feature of this model is that no property of the rationals comes out right. If one identifies 1 with $[1-\varepsilon, 1+\varepsilon]$ and 7 with $[7-\varepsilon, 7+\varepsilon]$, then 7 x (1/7) need not equal 1. So one is forced to introduce additional conditions that guarantee that at least for some numbers n, n x (1/n) = 1. That is where the clumsiness creeps in.

14. An excellent overview and additional references can be found in PRIEST, Graham, Richard ROUTLEY & Jean NORMAN (eds.): Paraconsistent Logic. Essays on the Inconsistent. (Philosophia Verlag, München, 1989).

15. The most important exception concern the failed attempts to rewrite set theory in its naive version, i.e., instead of the Zermelo–Fraenkel comprehension axiom, one uses the original naive version: "There is a set y, such that for all x, x is a member of y if and only if A(x)". Set theories with this axiom are forced to work with very weak logical principles. The simplicity of the axiom is lost because of the complexity of the proofs.

16. See my "Ross' Paradox is an Impossible Supertask" (to appear in the British Journal for the Philosophy of Science, 1992) and "Three–Body Collisions and Supertasks: Where Infinities Meet" (submitted to Dialectica, 1992).





Racter Report #1: Acausality

by Tom Etter

Introduction.

Racter is a fictional character who inhabits computers. He was conceived in the mid–70's by writer Bill Chamberlain and myself. It all began when we found ourselves at loose ends after another of our projects lost its funding, leaving us with what was then a state–of–the–art micro–computer to play with. Bill had read a magazine article that described how to write a program for generating silly sentences by substituting words at random in sentence forms. We tried it out on our primitive computer and found it entertaining for a little while, but it quickly palled, so we set out to see if we could make it into something livelier.

The first thing we did was to make the random generating process hierarchical so not only individual words but sentence forms, paragraph forms, etc. became unpredictable. To avoid complete chaos, we devised various tricks for matching subjects, verbs and pronouns, keeping track of tenses, remembering key choices, and roughly controlling the flow of subject matter.

Such was the infant Racter. He turned out to be a good deal livelier than the word substitution program from which he evolved, and a few of his antics had us rolling on the floor. It was not too long before he had published a short story [1] and a book [2].

The infant Racter could speak, or at any rate babble, but could he be made to listen? To find out required some more programming. Racter made his debut as a conversationalist in 1985 with the publication of a disk [3] for the Mac and IBM PC. Opinions differ on his conversational ability, but he did attract a fair amount of attention, earn a bit of money, and win himself a spot in several computer museums.

Even though he could converse after a fashion, Racter could do nothing that was of the least practical value, and it would be ridiculous to speak of him as manifesting AI. However, children do become adults, and after a number of slow years he is finally showing signs of maturity. He is even applying for his first job. And indeed it's quite a job! If all goes well, he will become principle investigator in a project to decide once and for all whether human beings are machines.

Racter's new job is an outgrowth of some recent work on the foundations of quantum mechanics. Von Neumann captured the essential novelty of the quantum in two simple equations (see Appendix), the first defining quantum observation, the second quantum change. If we take these two equations seriously, they undermine much of what we've learned in school about causality and logic. As we'll see, in a more general form they extend way beyond physics, even applying to things like computers. More than that, and this is really the point, they show us that there may be of a vast new range of natural phenomena whose existence we've never even suspected since, before this new theoretical turn, we couldn't even imagine them. These are the phenomena that Racter is going to help us look for.

Chapter 1. The Racter Test.

The commonly accepted test for whether a computer has the mental life of a human being is the Turing test. The idea in brief is that if a computer can pass as human in conversation then we must grant it human status, at least as a thinker. Of course there is considerable room for debate about how to administer the Turing test. Who, for instance, should judge whether the computer passes? There are probably towns in rural America where you can buy yourself a bottle of bourbon with a Xerox of a twenty dollar bill, but don't try it in New York City! It's reasonable to surmise that throughout the several million years or so of human existence, not a single person ever had to worry about whether his conversation partner was a genuine human or a mechanical counterfeit, so neither our genes nor our culture have supplied us with very refined means for making this particular distinction. In the recent Turing contest at the Boston science museum, a very primitive program passed as human, and a (human) professor of literature flunked!

How do you test for whether a certain kind of coin is really gold? The equivalent of the Turing test would be to see whether they pass for gold among coin traders. But a trader who has real doubts won't just look at a coin or bite it – he'll take it to an expert in the exact and highly developed science of matter. For the Turing test to carry conviction we would need similar experts in an exact and highly developed science of the human. Alas, such a science doesn't exist. Furthermore, if it did, there would be no need for the Turing test.

The problem here is not only with the Turing test but with any test for human status; we simply don't have a clear behavioral criterion for what it means to be human. We do have a clear behavioral criterion for what it means to be a computer, though. This suggests that the Turing test is being applied to the wrong side in the comparison. We shouldn't be testing the computer for whether it's human, we should be testing the human for whether he or she is a computer! Do humans behave in ways that computers demonstrably cannot? It will be Racter's new job to find out; Racter will test people for non-algorithmic behavior.

But, it will be objected, such a test is impossible – it's not even conceivable. What could it even mean to speak of non-algorithmic behavior? After all, can't any finite task be accomplished by a computer?

The answer to this last question is yes and no. If we are speaking of a set task like solving a given problem or making a speech, and if there is a predetermined criterion for success, and if a computer can apply this criterion, then the answer is yes: If it can be done at all then a computer can do it, at least in principle. However, and here is the rub: this computer will, in general, have to be exponentially more complicated than the performance itself or the computer that judges its success. Even though the task seems relatively simple, it could take more resources than the known universe could supply to actually build a computer to do it, in which case the yes answer is academic. Thus we see that it's at least conceivable that humans can succeed in tasks that are beyond the ability of any *actual* computer.

If the task is not just to make a speech but to carry on a conversation, there is a simpler criterion that could distinguish a human being from a computer, which is that a computer can only respond to what his conversation partner has already said. If a human being can know in advance the outcome of events like coin tosses, for instance, then he is not a computer for sure.

Thus we see that there are two ways in which Racter could discover that you are not a computer: he could determine that you have said things that would be too hard for any actual computer to think of, or he could determine that your responses have come in a temporal succession that is inconsistent with our understanding of causality. This shows that the Racter test is at least conceivable.

But is the Racter test dealing with the world we really live in? Do we have any reason to suppose that such non-algorithmic or acausal processes actually occur? Or, even if they do, that they have any bearing on how we think and act? We shall see that there are indeed very strong reasons to suppose both of these things, and in the present paper we'll focus on reasons supplied by quantum mechanics.

Chapter 2. Causality and Beyond.

Anyone who has dealt with quantum mechanics has found that the kind of causal reasoning we use in everyday life doesn't work very well. Our common-sense models lead to wrong results and sometimes even to paradoxes. From the beginning some quantum theorists have suspected that causality itself might be the villain here. The physicist Pauli was one – he even co-authored a book with the psychologist Jung called "Synchronicity – an Acausal Connecting Principle."

As this book inadvertently demonstrates, causal thinking is very hard to escape from. Jung's brave efforts to describe the acausal workings of synchronicity were repeatedly over-powered by the invisible pull of causality that pervades all of language. The answer to "Why?" is "Because!" – what else? "Why did that happen?" "Because of synchronicity!"

We shall try to escape this invisible pull by taking a more abstract approach. Though the English style manuals frown on abstraction, sometimes it's is the only way to overcome bad habits of thought that have become entrenched by too much experience of a too narrow world. If, as Bergson says, our minds evolved as instruments of practical action, then "Why?" and "Because!" are not just cultural habits, they are hard-wired into experience. There's no way to simply shut them out of our thoughts. The most we can hope for is to keep them so occupied with the challenge of getting our abstractions right that they remain unaware of the end-run these abstractions are doing around them! Anyway, that's the present strategy.

Among scientists and engineers today, causal thinking is flowchart thinking. A flowchart, such as a logic diagram or a flow diagram of a computer program, consists of boxes standing for objects or events and arrows showing the flow of cause and effect among these objects or events.

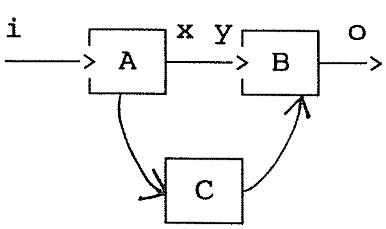
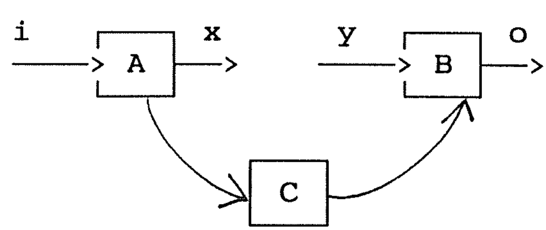


fig. 2.1 A flowchart



Cutting the arrow

An arrow, depending on which way it's pointing, represents an input variable or an output variable. An arrow between two objects is a shared variable which is an input to one and an output from the other. The experimental test for telling which is the input and which the output is to cut the arrow and observe which end is unaffected by the cut; that end is the output. In more traditional language, we distinguish the cause from the effect in a cause/effect pair as the member of the pair that is unaffected by severing their connection.

Each box has its *transfer function* which specifies how its outputs depend on its inputs. Flowchart boxes are black, as the engineers would say; we only pay attention to their transfer functions, not to what's inside them. Flowcharts are close cousins to computer programs, and can always be realized by computer programs of roughly equal complexity. This means that flowchartable behavior is basically the same thing as algorithmic behavior.

Our practical brain tells us that flowcharts are pictures of our understanding. "If there's any sort of behavior that can't be flowcharted", says the practical brain, "we can safely leave it to the mystics." That's our cue to begin our end run. By raising the level of abstraction, we are now going to construct some new toys for the practical brain to play with that will look very much like flowcharts. They won't really be flowcharts, though, and we must be careful to keep this in mind.

In set theory one defines a *relation* as a set of ordered pairs (or more generally a set of ordered n–tuples), and a *function* as a particular kind of relation. To construct our new toys, we'll draw boxes and arrows as before, but now, in line with the language of set theory, we won't say that the boxes have transfer functions but that they have *transfer relations*. So far this is just a change of terminology; what really makes our new toys new is that we'll allow not only functions but relations in general to be transfer relations. Consider a box with inputs x and y and output z. In a flowchart its transfer relation might be something like $z=x+y$. In the new box diagrams, though, its transfer relation could be $z>x+y$, or $x<y<z$. All this is spelled out in the appendix, where we'll see how to analyze any relational diagram in terms of newly defined "states" and "transformations".

We shall adopt Pascal's definition of probability as the number of favorable cases divided by the total number of cases. We apply this to flowcharts by defining a *case* as a joint value of all it's variables, and a *favorable* case as one of which the flowchart, regarded as a single relation, is true. Like ordinary flowcharts, the new relational flowcharts can always be "solved" by a computer to reveal their probabilistic behavior. However, most of them are in a strong sense acausal, in that the computer that solves them would have to be exponentially more complicated than they are. This is true even of diagrams whose local structure is almost indistinguishable from causal flowcharts. In such a "locally causal" diagram, acausality would not reveal itself in the parts but only in the whole.

Note that "Why?" and "Because!" are being kicked upstairs out of space and time and into logic. But we aren't going to leave them in peace even there; once again we'll enlist them in our end run, which now becomes an end run around logic itself! Our new move is very simple: the relational diagrams stay the same except for one thing, which is that transfer relations are now allowed to have a third truth value called *anti-truth,* where anti-true cases are subtracted from true cases in calculating probabilities. Mathematically this is almost a trivial change, but philosophically it's jumping off a high cliff, as we'll see in future Racter reports.

Here are the major categories of box-arrow diagrams:

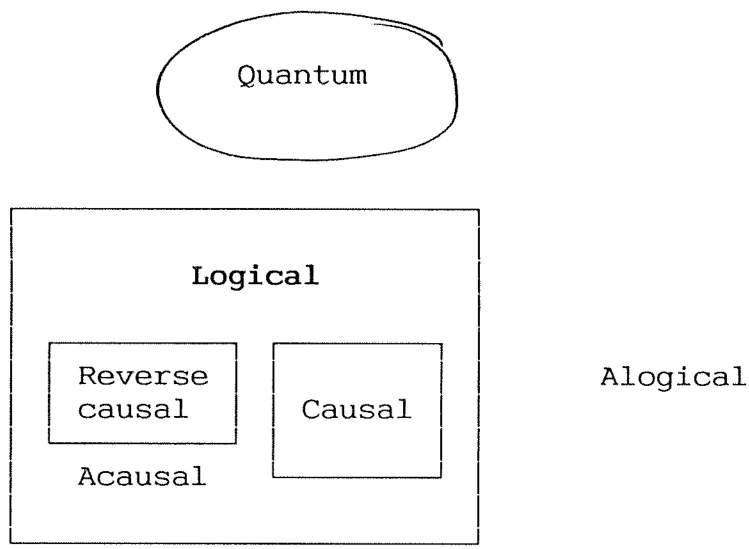


fig. 2.2 Box-arrow Diagrams.

Note the little circle labelled "quantum" A few classicists are still trying hard to put that little circle inside the square marked "causal", but the next two chapters will show why it belongs where it is. I'll wind up this chapter with a preview of a basic theorem about the quantum that we'll meet again in Ch. 5, a theorem that supplies one of the strongest reasons to seriously search for acausal and alogical processes.

In the early days of science the laws of nature were seen as order imposed by a strict God on rebellious matter. But in modern times we've found that actually it's the rebelliousness of matter itself, or at least its tendency towards disorderly conduct, that accounts for some of the laws of nature, the second law of thermodynamics being the best-known example. As a child I had a chemistry set with which I whiled away many a summer's afternoon, though not, I'm afraid, with the kind of educational activities recommended by the instruction manual – I was mostly into stinks and explosions. However I did discover one memorable law of the "order from disorder" kind: if you mix a lot of chemicals together at random they almost always turn brown. This of course explains why dirt is brown.

The above-mentioned theorem supplies a similar explanation of why the "ground" of the physical world is quantum. Roughly what it says is that if you average any haphazard mixture of states that includes acausal and alogical states, you always get a quantum state. Quantum is brown, so-to-speak. If we spin this out a bit further, it tells us that a quantum object is not a special kind of object any more than dirt is a special kind of chemical; it's just the "average" object in a suitably random series of encounters.

This explanation of quantum mechanics would be very satisfying if it were right; it resembles the very satisfying explanation provided by the central limit theorem of why normal distributions turn up so often. But of course for it to be right, the kinds of processes described by our general box-arrow diagrams must actually occur. Do they? Science to date has seen only the color causal and the color quantum, so-to-speak, and it has just barely noticed that they are different. It has neither denied nor affirmed the occurrence of the other colors in our new spectrum, since it has never before had the capacity to imagine them. Beyond the causal, it's essentially color blind. Racter's new job, with our assistance, is to give it new eyes.

Brown isn't the only "large number" color; there's also white, which is a uniform mixture of the light spectrum. It turns out that there's a white in our new state spectrum too, namely *classical!* That is, there's another way of randomly mixing general states that leads not to quantum states but to causal states. Physicists have given a lot of thought to the relationship between quantum and classical; some have followed Bohr in maintaining that the two are separate domains, others have taken quantum as the overall domain and have tried to find ways of getting white out of brown (it can't be done), while others still persist in a diehard struggle to keep everything white. Clearly our new results, if they are borne out by experiment, will throw this discussion into a very different arena. We'll return to brown and white in chapters 5 and 6.

To summarize this chapter: We have made two simple changes in causal flowcharts, replacing functionality by relationship and allowing for subtractions from the case count. The resulting diagrams of acausal and alogical processes include the quantum, which turns out to be an average or "generic" type. Do these other acausal and alogical processes exist? Do they play a hitherto unsuspected part in our lives? Will understanding them transform the life sciences? Will it lead to new technology? Or perhaps to a new idea of what technology is? Let's find out.

Chapter 3. Polarizers and Jumping Beans.

This chapter will present a small but crucial piece of quantum mechanics that shows quite clearly why we can no longer regard the world as causal. We'll have little need for technicalities since the facts in question are extremely simple and can best be understood through simple examples.

3.1 The rural church.

There is a small town in which there live 100 women, 100 democrats and 100 church–goers. The minister is young and handsome, so 75 of the women go to church. He is also quite liberal, so he can count 75 of the democrats among his flock. Problem: What's the least possible number of women democrats?

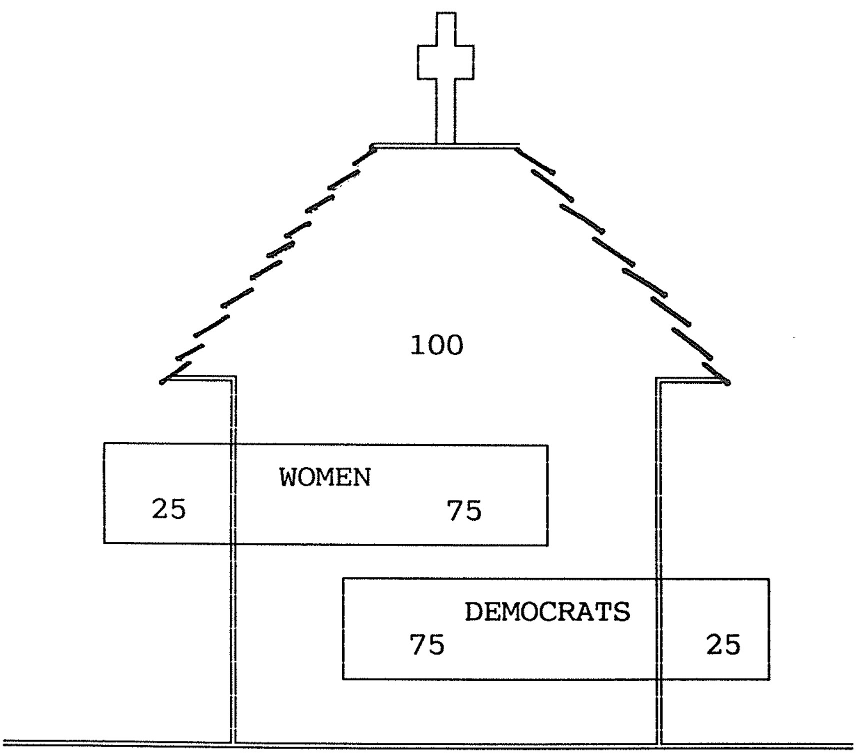


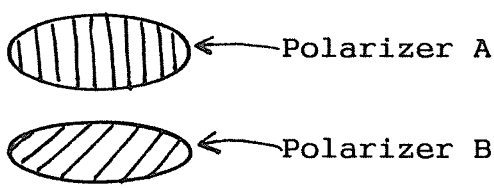
fig. 3.1.1

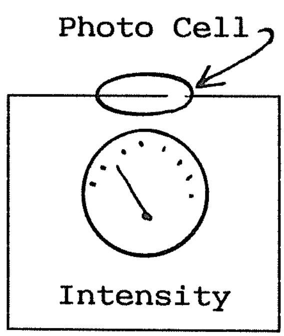
Since only 25 church–goers are not women and only 25 church–goers are not democrats, there are at most 50 church–goers who are either not women or are not democrats. Since there are 100 church–goers altogether, this means that at least 50 church–goers are both women and democrats. Ergo at least 50 of the women in town must be democrats.

This grade school problem in arithmetic and its solution led to a revolution in the philosophy of quantum mechanics, thanks to physicist John Bell. Though the issues here are fundamental and profound, they are neither complicated nor technical; indeed, the relevant physics can be presented in a few informal paragraphs.

3.2. The high school physics experiment.

Here's a picture showing all of the quantitative facts from quantum mechanics that we'll need in this chapter:





Angle	Intensity
0	100%
30	75%
60	25%
90	0%

fig. 3.2.1

A light from an ordinary light bulb shines through two polarizers onto a photo–cell. How much comes through depends on the angle between the two polarizers: At 0°, the light is a maximum. At 30°, it's 75% of this maximum, at 60°, it's 25%, and at 90°, it's 0%. Try to remember these numbers.

If we make the light very weak the electric current from the photocell will no longer be steady but will occur in a series of pulses, each corresponding to the detection of a single photon. As we rotate the second polarizer out of alignment, these pulses don't get weaker, they get less frequent; the numbers 100%, 75%, 25% and 0% now refer to the number of pulses per second, signifying the percentages of photons that make it through the second polarizer.

Why does a particular photon goes through a polarizer while others are stopped? Possible explanations are of two kinds:

1. The passport theory. Each photon has a passport good for some angles and not for others; of two identical photons, either both will make it through or neither.

2. The doorman theory. The polarizer itself is somewhat erratic as to who it lets through. Though it tends to smile on certain kinds of photons and frown on others, its hospitality also varies with its own moods; of two identical photons, it might welcome one and stonewall the other.

Einstein favored the passport theory, and in the 1930's he came up with what looks like an unassailable proof of it. By a simple thought experiment he showed that it's possible to produce twin photons carrying identical passports. (Actually the simple version described here is due to David Bohm; Einstein's original thought experiment involved position and momentum rather than polarization and was somewhat more complicated) More exactly, what this thought experiment shows is that we can create photons pairs with the property that if we place polarizers at the same angle in both of their paths they'll either both pass or both fail. If the doorman theory were true, the two doormen would have to synchronize their decisions based on something other than the states of the photons. This seems very implausible, since the two photons could be sent to opposite sides of the galaxy to encounter their respective polarizers, and the choice of angle could be made independently at both ends.

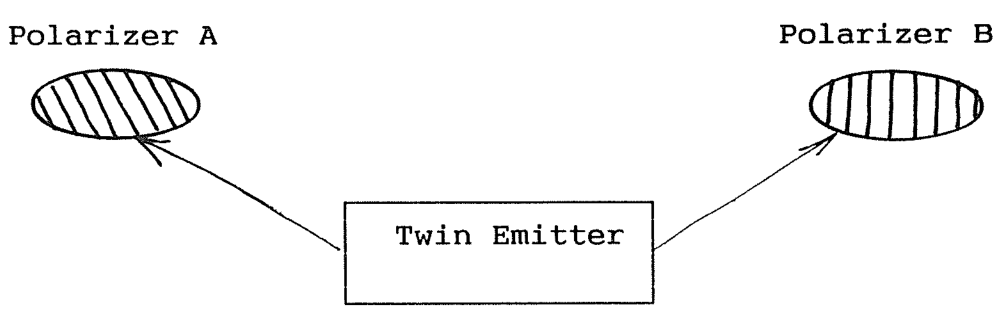


fig. 3.2.2

Another way to put it is that Einstein's thought experiment shows how to make a non-disturbing observation of whether a given photon will pass a polarizer at a particular angle: we simply test its twin. Our reasoning here would go unchallenged in the everyday world; if we had twin eggs, for instance, we'd count it as a non-disturbing measurement to test the fragility of an egg by squeezing its twin until it broke. But how can we be sure that two eggs, or two photons, are really twins without testing both of them? If we had only one pair, this would indeed be impossible. But what we have in the case of photons is a production process for pairs, and we can test this production process for whether it always produces twins by testing a fair sample in which we destructively test both members of the pairs. This has been done for quantum twins, and the result is what theory predicts – if it weren't, there would be a major crisis in physics!

What happens with twin photons when we measure them at different angles? There is one more experimental fact we need to take into account, which is that if a beam of photons will pass through a polarizer A without attenuation, then there is no way to distinguish it from a beam that has already passed through A. Suppose that A in fig. 3 is set to 0° and B to 30°. Now we know that if an A photon passes, it's B twin is sure to pass a 0° polarizer. Thus these photons behave just like photons that have already passed a 0° polarizer, i.e. like those that have come through the top polarizer in the experiment of fig. 2. Ergo 75% of them will pass B. Since the ability to pass is a property of the photon itself, this number 75% applies to the photons whether we test them or not; it's the percentage of 0° passers whose passports are also good for 30°. By similar reasoning, 25% of the 0° passers have passports good for 60°, and none for 90°.

A light beam is called *unpolarized* if the percentage of its photons that will go through a polarizer does not depend on the angle of the polarizer. Consider an unpolarized beam which will send 100 photons per second through a polarizer. Consider the photons emitted during one second: of these, suppose that the 100 that pass at 0° are women, the 100 that pass at 30° are church-goers, and the 100 that pass at 60° are democrats.

By the quantum angle rule, 75 of these women will be church-goers, and 75 of these democrats will be church-goers. Therefore, as we saw in 3.1, there must be at least 50 women democrats.

But women and democrats are 60° apart. Therefore by the quantum angle rule, there will be 25 women democrats!

Our train of reasoning, by remaining solidly on track, has led to a flat contradiction. What's going on? At least one of our assumptions must be wrong. Most physicists concerned with the question think that what is wrong is our seemingly innocuous assumption that measuring one twin doesn't disturb the other. A quantum measurement always disturbs what it measures, they say, even when what it measures is a twin at the opposite side of the galaxy, separated by a thousand light years of lead! This seemingly desperate way out is known as *non-locality.*

3.3. Professor B. and the jumping bean movie.

A frying pan with two sections and a domed cover contains a million jumping beans.

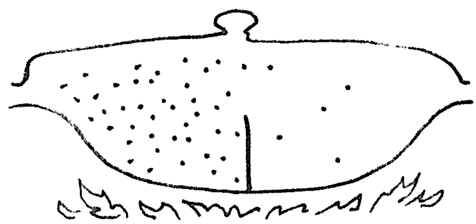


fig. 3.3.1 Pot with 1,000,000 beans.

When we put this pan on a low heat, each bean has a slight chance of jumping into the opposite section; on the average one bean in a thousand changes sides every second. The beans all start on the right, but after an hour or so they are pretty evenly distributed between the two sides.

We have taken a movie of the above events which we give to our expert colleague Prof. B. to analyze. His expertise is in the realm of theory rather than practice, so in typical fashion he puts the reel on the wrong spindle and runs the movie backwards. Thus he sees the beans starting with nearly equal numbers left and right and all ending up on the right.

Now for us who made the movie, the number of jumps per second from left to right is proportional to the number of beans on the left side. But for Prof. B. who watches the movie, our jumps from left to right will be jumps from right to left. Thus for him the number of jumps per second from one side to the other is proportional to the number of beans on the other side! "Aha, there is something like gravity at work here" he says. "The more beans there are on the other side, the harder a bean on this side is pulled, so the greater the probability that it will jump." He comes up with equations describing the workings of this attractive force that are just like our forward equations except that certain of his terms are negative.

The laws of mechanics, which are the most fundamental laws we know, are symmetrical with respect to past and future. Our knowledge of this fact, which has been around since the 17th century, is a quietly ticking time bomb. One way to think of the present paper is that it's an attempt to brace ourselves for its ultimate explosion. We'll come back to all this; for now let me just make one observation: if in the larger spectacle of nature, past and future are symmetrical like up and down, then Prof. B's backward movie show might not always be a mistake.

So far, we've seen nothing about this possibility to lose any sleep over. A new kind of attractive force? Interesting, but no big deal. But now let's look at a slightly different experiment. Instead of one pan containing a million jumping beans, let's now consider a stack of a million pans each containing one jumping bean:

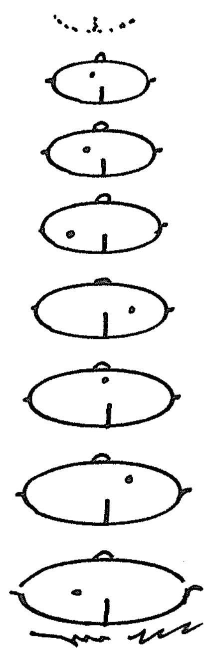


fig. 3.3.2 1,000,000 pots with one bean each.

Everything else is as before: the beans all start on the right side, and one out of a thousand beans change sides every second. The math is exactly the same, both for us and Prof. B. But what about Prof. B.'s force? If we consider the bean in a single pan, in the forward movie the probability that it will jump is always the same, but in the backward movie it's proportional to the number of pots in which the bean is on the other side. But how does the bean in our pot know about the beans in the other pots? Suppose they are light-years away?

Again we encounter non-locality! But now we encounter it not with exotic and ephemeral entities like photons but with ordinary jumping beans.

Chapter 4. Goodbye to causality and logic.

As mentioned in 3.2, those physicists who have paid serious attention to Bell's theorem have generally taken it to be a proof of non-locality. This is not unreasonable. After all, the passport theory grossly contradicts quantum mechanics, as we've just seen, which leaves us with the doorman theory. But the doorman theory only works if the doormen synchronize their decisions as to who passes, which requires that they communicate in some way that is not impeded by either distance or material barriers.

The idea of signals that can't be blocked is bad enough, but what really gives the physicists a hard time is that these signals would have to be able to travel faster than light, which is prohibited by the theory of relativity. Actually, what relativity says is that if signals could travel faster than light they could be relayed back into the past of the sender, leading to causal paradoxes (footnote on good and bad proofs).

In order to see the essential problem here, it's useful to forget about photons and polarizers for a moment and imagine the experiment of fig. 3 being carried out in a long black box with a computer terminal at each end. There is a series of trials in which the operator at each end is prompted to choose a number called "angle", which is some multiple of 30°, after which his screen shows either "pass" or "fail". The results are tabulated and later compared to find the correlation between the outcomes at the two ends as a function of the difference of angle. The results are our familiar percentages of fig 2.

Question: what's inside the black box? Suppose we draw a causal flowchart of what we imagine to be inside it, treating the two operators' choices of angles as free inputs. Since passport theories are out, any causal explanation must make the doorman's decisions causally dependent on the choice of angle at the other location, which means it must contain causal pathways whose flow is faster than light. But by connecting such faster-than-light pathways into relays that go backward in time, one could produce events that cause their own non-occurrence.

The situation in a nutshell is this: There are certain simple and reproducible phenomena for which we cannot draw a realistic causal flowchart, since the boxes and arrows of any such flowchart would be of a kind that we could reassemble into a paradoxical flowchart. Bell's theorem marks the end of the universal reign of causality.

Of course people will still keep looking for ways to keep causality on its throne. One such way is to suppose there are causes in the common past of the operators at the two computer terminals that make them coordinate their angle choices with the states of the photons so as to produce quantum statistics. This common past might of course be very remote, even going back to the big bang. One is reminded of Cardinal Wilberforce's answer to Darwin: God put the fossils in the rocks to test our faith. So desperate was the plight of Biblical faith in the 19th century that this monstrous idea was taken seriously even in intellectual circles. It's a sign of how desperate is the plight of causality today that the Rube Goldberg fantasy of synchronizing causes has in fact been seriously proposed by people who are knowledgeable about Bell's theorem.

Before we join these quantum desperados, let's remember the backward jumping beans. Prof. B.'s second movie, the one in which there are a million pans, has just as bad a case of non-locality as Bell's theorem. If Prof. B. tries to draw a flowchart of his "attractive force", it too will contain faster-than-light causal pathways, and he might even end up among the quantum desperados. But if we show him how to run his projector, his non-locality will evaporate like ground mist in the morning sun. "Gee, there isn't any force after all! I'm just turned around in time."

Could it be as simple as that with Bell's theorem? Are we just turned around in time? Of course now it's not just movie time; we can't merely reverse the reels. Also, what's involved now is not correcting a time reversal but studying symmetry under time reversal. In essence, though, the answer is yes, it's really that simple, or almost.

Reversing past and future seems at first to be a pretty straightforward idea, like reversing left and right or up and down. But such spatial analogies don't tell the whole story. When you reverse past and future you reverse every kind of motion, including the flow of information between you and the world. Think what this does to science's ideal observer, who sees without disturbing. There he sits, high in his ivory tower, the ultimate arbiter of all that is, was or will be. But turn him around in time and what happens? He turns into someone who disturbs without seeing!

If time is symmetrical, it must be possible to reconcile a time-reversed "viewpoint" of the world with our forward viewpoint so as to make them equivalent. How do we manage that? How can the perfect seer be equivalent to a blind man? If the ticking time-reversal bomb really goes off, we may find that many of the pieties of current scientific method sound pretty silly. What will happen next is anyone's guess. Maybe science will retreat into the sterile gloom of unquestionable tradition. Hopefully it will evolve into something livelier than it already is.

If we can't use spatial metaphors for time reversal, how then does a theory of time reversal proceed at all? It's useful here to start out by talking about words: given a piece of text that deals with time, how do we transform it into another piece of text of which we would say that the time it deals with is reversed? I'm taking text here in a broad sense that includes diagrams, pictures, etc.

Let's consider three kinds of text in particular: movies, stories and flowcharts. These involve three different conceptions of time which I'll call movie time, narrative time and action time. The first two are closely related; indeed we can think of movie time as a special case of narrative time. Action time is another matter, though, and we shall see that there are two independent ways to reverse it, neither of which applies to narrative time. In fact these two ways exactly correspond to the two extensions of flowchart theory described in Ch. 2 that define our present enterprise.

Reversing movie time means reversing the order of a series of snapshots S1, S2, S3, .. which we take as representing "slices" of time. Note that we are assuming that these time slices as themselves timeless, so the arrow of time belongs only to their ensemble, not to the slices individually; only with this assumption does it make sense to regard the backward movie as a picture of the backward event.

If we think of a movie as a statement, what it says on the most literal level is "S1 and then S2 and then S3 .. etc." which makes it an instance of what I'm calling a narrative. The more general definition of a narrative is a statement which is logically of the form "A and B and C etc., and T" where A, B and C etc. are events and T is how they are temporally related. Since an event, as opposed to a time slice, often has its own time arrow, the reversal A* of A is in general a different event from A. Thus the reversal of a narrative will be of the form "A* and B* and C* etc., and T*", where T* is T with before and after reversed. I am being somewhat formal here in order to bring out the crucial differences between a narrative and a plan of action.

Sometimes a plan of action is just a narrative of what we intend to do, a schedule of planned events. But more often a plan involves contingencies: "If it doesn't rain tomorrow we'll go to the beach." Planning requires knowledge of causes and effects, of how things work, and it's natural to draw up a complex plan as a flowchart. States now are no longer just time slices, they are also conditions that cause future events. Thus states have a time arrow; they bear very differently on past and future. Reversing action time must reverse this "arrow of influence" that states radiate toward other states. That is, reversing state arrows is a certain kind of time reversal that applies to action time; we'll speak of it as *arrow reversal.*

Causality means that later things are functions of earlier things. What happens to causality when we reverse earlier and later? If it is to remain causality, i.e. if it is to continue to make later into a function of earlier, the new function must be the *inverse* of the old function. *Functionality reversal* is the second kind of time reversal that applies to action time, and we'll see that it is quite independent of arrow reversal.

In future papers I'll carry out a detailed flowchart analysis of jumping beans in which the meaning of these two kinds of "time reversal" will become quite clear. Here's a rough sketch to help us get oriented. First of all, our jumping bean law follows from two principles:

Independence. The chance of a particular bean jumping doesn't depend on where the other beans are or what they are doing.

Dynamics. The chance of a particular bean jumping in any given second is one in a thousand.

If, say, 3/4 of the beans are on the left, these laws imply that 3 times as many beans will jump right as will jump left, so the beans will continue to become more evenly distributed until there are an equal number on both sides. Viewing the process backwards shows the beans becoming less evenly distributed, however, so for a reversed observer at least one of these two laws must be false. Which is it?

Arrow reversal says that independence is false. The dynamics remain the same, but the jumps become progressively more correlated because the boundary condition of the process is in the future rather than the past. In terms of the "broken arrow", a forward state puts an unequal distribution on the past part of the break (output from past to future) and an equal distribution on the future part; these are the defining conditions of a so-called *causal* state. Reversing time direction puts the unequal distribution on the future part and the equal distribution on the past. Either case satisfies the two von Neumann rules (see Appendix).

Functionality reversal holds onto independence by replacing the transfer function in forward dynamics by its inverse:

Inverse Dynamics. The chance of a particular bean jumping in any given second is *minus* one in a thousand!

What on earth does that mean? Never mind – just apply the rules of probability theory and try to forget the minus sign. If we assume that the beans ignore one another, then it follows that their initial causal state will transform into future causal states that are progressively less equally distributed.

Suppose we saw a movie of a time-reversed pot of jumping beans that was for real; which of these two explanations should we prefer? This question highlights the crucial difference between narrative time and action time. Our assumed movie is the narrative *trace* of some process; its time is narrative time. On the other hand, the time in the two explanations is action time. The two *active* processes which they describe are fundamentally different; even though they leave the same trace, they play a very different role in the larger picture. Indeed both are needed to get to quantum mechanics.

Negative probabilities, whatever they may be, are the price of functionality symmetry in a theory of action time. The price is a steep one, since it involves giving up logic itself on the level of "micro-events" like time-reversed bean jumps or photons going through polarizers. More exactly, it means *relativizing* logic: words like AND, OR, IMPLIES etc. no longer have the same meaning for different observers. [4]. A well-known example of this is the Heisenberg uncertainty principle; a particle can't have both position AND momentum because the different situations in which you measure position and momentum don't share AND!

In the case of Bell's theorem, the relativity of logic makes it possible to dispense with non-locality without having to postulate some grotesque deus ex machina to explain the quantum correlations. Indeed, and this should make Einstein happy, we can even go back to a version of the passport theory. Here's the rough idea:

The key thing to note here is that AND is relative to the polarizer angle. If we pick an angle, we can say of a given photon that it either does or doesn't have a passport. But we've got to stay with that angle. If we pick another angle, the first passport disappears, or rather the very fact of whether or not the first passport exists disappears! The statement that a photon p has a passport for both 0° AND 30° is neither true nor false; it's simply meaningless, just as it's meaningless to say that a photon has both a definite position and a definite momentum.

In summary, a real time-reversed movie could force us to give up causality in order to keep logic, or it could force us to give up logic in order to keep causality. But if we should try to fit this movie into a larger context, we would find that the choice here is not arbitrary, and indeed we might be forced to give up both.

Chapter 5. Relational Flowcharts and Two-way States.

If past and future are relative like up and down, then our present ideas about states and changes are badly wrong. At best they work locally, like the idea of a flat earth. But unlike the flat earth, whose wrongness can be confined within the limited domain of geography and astronomy, a wrong conception of states and changes propagates its wrongness through every department of knowledge outside of pure mathematics. We should pause for a minute to contemplate just how much is at stake in getting states and changes right. Everything we say about events, whether human, natural or supernatural, uses the language of what is, what was and what will be, i.e. of states and changes. A radically new conception of these things like the one I'm presenting here is not just a new theory; if it's right, it's a shift in the very grounds of understanding.

In the seventeenth century Galileo originated the practice of treating the velocity of an object as an aspect of its state. This practice is so familiar today that we forget what a radical step it once was, and for that matter, still is. The word "state" seems to call for something that is *stationary,* and yet after Galileo the physical state of an object becomes a *slice of motion,* pointing backward to where the object has just been and forward to where it will next be. Notice how different the Galilean state is from the static state of a chess-board or of a computer [5]. I have argued that the current fad of computer modelling is a regression to Aristotelian thinking).

Galilean states are slice of motion in movie time. Our new states will be modelled on Galilean states, but now the time of which they are slices is not movie time but *action time.*

Recall that action time differs from movie time, and more generally from narrative time, in that it involves not only what does happen but what might happen. When we draw up a plan of action we impose constraints on a set of variables whose possible values correspond to the possibilities in the situation. As we noted before, it's natural to represent these constraints by a flowchart, where arrows represent variables and boxes represent transfer functions. In present practice, a state is the value or the probability distribution on an arrow (a variable), and a change is the transfer function in a box. The two concepts are quite separable, so the states are static.

Our new states are dynamic. Like Galilean states, they contain something of both the past and future. But unlike Galilean states, their past and future are not in the course of actual events but in the realm of possible events. Their new "velocity" doesn't just tie together was, is and will be – it also ties together might-have-been, might-be, and might-become. A state is now a door between past and future *potentiality.*

To understand the role of a door means to understand the difference between the situations with the door open and the door shut. In a flowchart, to shut a door means to *break* an arrow. Roughly speaking, the state of an arrow will be defined as the contrast between the structure of the flowchart with the arrow broken and the structure of the flowchart intact.

In a causal state, the influence of what will be is nil; the future, when disconnected from the past, is pure potentiality. With the arrow whole the past is completely in control; what will be is what was! Causal change is obeisance to the past; anything new is a meaningless chance mutation, erased without a trace in the grand march of averages that is the unfolding of (statistical) causal order.

But causal states are only a very special case. Another very special case is reverse causal states, where the future is completely in control, as it would be most of the time in the near neighborhood of the "big crunch". Halfway between causal and backward causal are the (quantum) mechanical states, where it's not only the degree of past and future influence that is equal, but its exact form; this, as I've already indicated, is also a degenerate kind of order. However, the vast majority of states have a much richer kind of order than any of these three, an order in which the constraining power of past and future interact in a way that has no causal analogue.

Earlier I spoke of causal and quantum as white and brown, the two "large number" colors. Now that we are thinking of a state as a door between past and future, this color metaphor can be given more content. First let's imagine that the state door is made of a special kind of glass, a glass which blocks all worldly influences when it is shut, but which is transparent to our Cartesian other-worldly vision.

Consider a causal state. When we look through its door from the side of the past, what we see is pure whiteness, a future that is featureless and blank. White is the color of total disorder, of zero information. It's what you get when you superimpose such a great number and variety of pictures that you lose all their features – a similar superposition of sounds is called white noise. The view through the closed door shows us the *equilibrium* that would be the large-number effect of removing the cause.

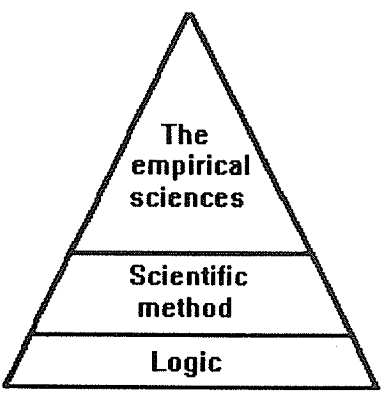
Looking backwards through the glass door of our state shows us history, or more exactly, that portion of history that would pass through the glass door into the future, if it were open.

Now consider a quantum state. Looking from past to future we don't see pure white but a "history" that is exactly like the history we see looking from future to past. The door looks like a mirror, or more accurately, a corner mirror. This is what happens when our state is a superposition of many states whose past–future orientations are chosen at random. The color brown is a rather strained visual metaphor for this particular large–number effect – "mirror" is in some respects better – but brown at least reminds us of the crucial fact that this special kind of symmetry emerges from disorder.

The glass door metaphor has a minor flaw in that it only applies to so–called *pure* states, and there are also *mixed* states for which the glass door isn't the only link between past and future. But it has a much more serious flaw, which is that it places us outside of time. When we put ourselves back into time we are led to anti–truth and negative case counts, which relativize logic and thereby end any possibility of capturing the flow of time in a single coherent image.

CONCLUSION: BEYOND SCIENCE.

We learned in school that the empirical sciences rest on scientific method, which systematizes the reasonable search for causes. Reason, in turn, rests on logic:



But we have just said goodbye to causality and logic. Where does this leave scientific method? Where does it leave science? Where does it leave us?

If our horizon is the boundary of our village, it doesn't much matter that the world is round. Like the village arts of pottery and weaving, the art of science –its method and logic – is pretty much a fait accompli. Saying goodbye to causality and logic means saying goodbye to science as we know it. It means creating a new art, a new method that encompasses scientific method but that goes far beyond it. Though the science of quantum mechanics has given us a strong hint that we are, in Wheeler's term, participating observers, it will be this new art that brings our participation as observers to consciousness. Racter and the generalized quantum theory that goes with it will, I hope, become a new chapter of science. But it is my greater hope that they will also be the first step toward this new art.

6. APPENDIX.

The present paper is intended to be a curtain raiser for the new science that will result from generalizing causality and relativizing logic. It aims to set the new mood, to show something of the new style of thinking, to give a sense of how current physics is pushing us in the new direction, and finally to give a hint of the intellectual riches waiting for the first comers to this new field of inquiry. To conclude, I shall in this chapter briefly lay out some of the simple but surprising mathematics that gets the new theory started, and reflect a bit on what it may mean for experience. Future progress reports will present the theory in more detail together with the results of what I hope will be some illuminating experimental tests.

6.1 The von Neumann Rules.

The core of what is new in quantum mechanics, that which makes it an essentially different theory from classical mechanics, is contained in two simple rules that were discovered by John von Neumann and first presented in his book "The Mathematical Foundations of Quantum Mechanics." He expressed these rules in terms of operators on Hilbert space (for now we shall think of operators as matrices) which are used to represent three seemingly very different kinds of thing: first the *states* of a quantum object, second the *propositions* about a state, and third the *transformations* of a state. Here, in terms of these three kinds of matrices, are the von Neumann rules:

The observation rule. Given a quantum object in state S, the probability of a proposition P about S is trace(PS). (The trace of a matrix is defined as the sum of its diagonal).

The change rule. Every change in an isolated quantum system, whether it results from the passage of time or a change in the observer's viewpoint, is described by a transformation T satisfying $S'T = TS$, where S is the state before the change and S' the state after.

In quantum mechanics T always has an inverse T^{-1}; multiplying both sides of the change equation on the right by T^{-1} puts it into the more familiar form $S' = TST^{-1}$ that gives the second state as a function of the first. $S'T = TS$ is the preferred form, though, since it makes sense when T doesn't have an inverse, and indeed even when it's not a square matrix; as we shall see, this makes the change rule apply to any box in a flowchart.

The above observation rule implies the more general observation rule that the average of an observed quantity Q is trace(QS), where Q is a self-adjoint operator. It's this more general form that ties quantum theory to the observation of macroscopic physical quantities, even when these are averages of non-commuting microscopic quantities. The trace is invariant under all quantum changes of viewpoint, which means that the *logic* of statements about the trace is also invariant. That, in a nutshell, is why the relative logic of quantum theory is not simply the end of science. Fortunately the trace is invariant in our more general theory too; Racter will qualify as a scientist by measuring trace(QS).

From the two von Neumann rules one can derive all of the general mathematical machinery about observables and their averages etc. that one usually finds in the first few chapters of a textbook on quantum mechanics. Quantum physics proper begins with the study of particular groups of transformations; the quantum analogues of the equations of classical physics can be derived by studying Hilbert space representations of the classical space-time symmetry groups. We're not concerned here with physics itself, though, except at the very fundamental level of the von Neumann rules, which we are now about to encounter again as the rules that govern the "flow" in every flowchart.

6.2. Boxes.

A box in flowchart theory is an object, and entity, a thing. More accurately, it's a *certain kind* of thing, an *instance* of a *type.* Box types contain *variables,* so they are what in computer science are called *structured types.*

Let's begin by reflecting a bit on instance, type and variation. Consider the two sentences "The dog is chasing the cat" and "The dog is man's best friend". Notice that "The dog" in the first is an instance while "The dog" in the second is a type. Thus we see that a particular idea can be used to present either an instance or a type; the essential difference between the two is not in the idea itself but in the role that this idea plays in a larger context.

However, when we pay attention to an instance, we often do add further details of its type, especially when there are other instances of the same type around. For instance, if there were several dogs in the room, the particular dog we are noticing might become the yellow dog. The type "yellow dog" is what computer programmers would call a *descendant* of the type "dog"; we'll use the more colloquial term *special case.*

When we single out an instance by means of a special case of its type, it's easy to fall into the fallacy that Whitehead calls misplaced concreteness, which is to confuse this more specialized type with the instance itself. Indeed, ordinary language encourages this mistake – "You should have a dog, a Doberman, for instance." Sometimes it's not so easy to judge whether we are misplacing concreteness or not; consider the "for instance" in the middle of the last paragraph, for instance!

A variable, as we shall use the term here, is a range of variations on a type. More exactly, a *box variable,* i.e. a variable belonging to a box type, will be defined as the index of a *menu* of types that are special cases of the "overall" box type

A simple example should make this clearer. Suppose our box is an instance of the type "dog", i.e. it's a dog. Now dogs come in various colors and ours happens to be white. But we are thinking that maybe yellow or grey would have been a more practical color, which means we are contemplating three variations on our dog's type:

1. "yellow dog" 2. "grey dog" 3. "white dog"

Note that we are now looking at our dog not as an instance of just plain "dog" but of the more complicated type "dog with color". That is, we have added a *color variable* to his overall box type of "dog". We can add other variables: he could become "dog with position", "dog with velocity", "dog with appetite" etc. Note that adding variables doesn't per se turn a type into a special case; that only happens when we *fix* his variables, or place limits on their ranges as we did in limiting our dog's possible colors to only three.

We'll assume that menu items are types which have no instances in common: no yellow dog is a grey dog. We'll also assume that any instance of the overall box type is an instance of some value of a variable: any dog of the type I'm considering would be yellow, grey or white. With these two assumptions, the von Neumann rules become theorems in the logic of everyday events, as we'll soon see.

For the sake of brevity and convenience we'll access the menu of a variable through its index just as we order lunch in a fast-food joint by number: "Gimme a 3" means "I would like to order the meal that is listed as the third item on the menu, please." "He's a 2" means our dog is the second item on the color menu, a grey dog. We'll treat box variables as numerical, though we must never forget that their numerical values are *pointers* to types;

this is true even of variables like speed or weight whose menus are natural orderings of these types.

The items on a variable's menu will be called its *cases,* in contrast to the index numbers that are it's *values.* Indeed this is standard English, as in "Consider the case of x=4". We shall also refer to *joint cases* of several variables, as in "Consider the case of x=4 and y=2". A joint case of all the box's variables will be called a *box case,* or simply a *case* when we are discussing the box as a whole.

Now suppose we were to arrange all the joint cases of x and y into a menu; we would thereby produce a new variable. There is a natural way to order joint cases, the so-called lexical ordering, which is alphabetical ordering with numbers as the letters of the alphabet. For lexical ordering to be unique, the variables themselves must have an order of priority. We shall assume that the box type itself supplies this order, i.e. its variables come not just as a set but as a *list.* Thus every subset of a box's variables defines a:

Compound variable. Given x,y.., the compound variable C(x,y..) is defined as the index of the menu that results from the lexical ordering of the joint cases of x,y.. We'll call x, y etc. *atomic* variables. Computer programmers will recognize compound variables as close relatives of record variables, and we shall adopt their data base record/field notation to single out atoms.

In short, a *box type* is given by an overall type together with a list of atomic box variables, where a box variable is defined as the index of an exhaustive menu of mutually exclusive special cases of the overall type.

6.3. Events.

An *event* will be defined as anything that can be said about a box in terms of its variables, for instance x=y, x>y, x=5, x>5, x=y+z etc. To put it another way, an event is a statement of *what is the case,* and we can specify an event by specifying those box cases for which it is true. If an event only involves one or two variables, we can specify it more simply by giving a one or two dimensional array of TRUE's and FALSE's; for instance x>5 would be given by a list whose first five entries are FALSE, the rest TRUE.

Those cases for which an event is true are called its *favorable* cases, a term which comes from Pascal's definition of probability, which we can now adopt in his very own words:

Probability. The probability p(A) of a box event A is the number of cases favorable to A divided by the total number of cases.

Pascal's is a purely formal conception of probability abstracted from the structure of types, and as such has nothing to do with likelihood or relative frequency. If a type applies to a situation whose cases are equally probable according to some other sense of probability, then of course the Pascalian probabilities will also have this other sense. However, we'll only be concerned here with probability as a tool for analyzing relational structure.

To analyze something we must first isolate it as something that is relatively independent of the rest of the world, and then break it down into relatively independent parts that we can focus on separately. But just what do we mean by independent? In the case of two events A and B, what we mean is that $p(A\&B) = p(A)p(B)$. This rule gives us a basis for defining independence more generally; for instance, we say that variable x is independent of variable y if for all m and n, the event $x=m$ is independent of the event $y=n$. We'll soon see that this definition plays a crucial role in classifying states.

Events will be divided into two classes: *Transient* events, which may be true of some instances of the box type and not of others, and *laws,* which are assumed to be true of all instances and thus to belong to the box type itself.

Box type redefined: As before, except that now we add to the type a set of laws, which taken together will be known as its *law.* A box whose law allows all cases is called a *free* box.

Legal case: A case for which the box law is true.

Probability redefined: As above, except now we only count legal cases.

The events of a box can be combined by OR, AND and NOT into other events of that box, so together they form a Boolean algebra. Boole called this an algebra because OR and AND behave somewhat like addition and multiplication. The resemblance to addition and multiplication is closer if we use XOR (exclusive OR) for addition instead of OR, since now we can also define subtraction, which happens also to be XOR, i.e. $x-y = x+y$. The algebra of AND and XOR is called the *Boolean ring,* and it is equivalent to Boolean algebra in the sense that all of the Boolean operators can be defined in terms of AND and XOR. From now on when we speak of "events" without qualification, it will be understood that these events all lie in the Boolean ring of some particular box.

Boolean ring: A set of elements with multiplication "&" and addition "+" that behave like Boolean AND and XOR.

The event that allows all cases is called the *universal* event, or 1, while the event that forbids all cases is called the *null* event, or 0. Note that 1 and 0 as elements of the Boolean ring behave as the multiplicative and additive identities, i.e. 1&X = X and 0+X = X. Note also that 1 and 0 have probabilities 1 and 0. Since 1 is always true and 0 is always false we will also refer to 0 and 1 as TRUE and FALSE.

So far in our discussion of events we've been on familiar ground, but now we're coming to something brand new: We're going to use the resemblance between numbers and events to define *event arrays* that behave like vectors and linear operators:

Event vector. A one-dimensional array of events indexed by a box variable.

Event matrix. A two-dimensional array of events indexed by two box variables.

Event tensor. An n-dimensional array of events indexed by n box variables. (We won't be much concerned with n>2).

Scalar. Any event. To multiply an event vector V by a scalar e, multiply each of its entries Vi by e, i.e. replace it by e&Vi.

As mentioned above, it's natural to represent an event in one or two variables by a vector or a matrix of TRUE's and FALSE's corresponding to its favorable and unfavorable cases. More generally, any event can be so represented by an n-dimensional array of TRUE's and FALSE's. If we interpret TRUE and FALSE as the universal and null events 1 and 0, then this array becomes a event tensor of a kind we'll call a

Proposition: An event tensor whose entries are 0's and 1's. There is a natural 1-1 correspondence between propositions and events. A proposition in von Neumann's sense corresponds to an event involving a pair of linked variables, as we shall see.

There is another very simple but very important kind of tensor called the *self tensor* that results, roughly speaking, from treating the values of certain variables as events. To see what this means, consider first a single variable x. For each number n in the range of x there is an event x=n that sets x to n. These events of course have a natural order x=1, x=2, x=3 etc. whose index is x itself, so that when we arrange them in this order they constitute an event vector in x; we'll call this the *self vector* of x. Essentially the same construction leads to the *self matrix* of a pair of variables.

The self matrix of x,y: The event matrix of all joint assignments to x and y indexed by x and y. More concretely, think of the self matrix of x and y as the result of filling each cell of a spreadsheet with a statement giving its location in x,y coordinates, i.e.:

x=1 & y=1	x=2 & y=1	x=3 & y=1	etc.	
x=1 & y-2	x=2 & y-2	x=3 & y=2	etc.	
x=1 & y=3	x=2 & y=3	x=3 & y=3	etc.	
etc.				

fig. 6.3.1

Important Theorem. If M is the self matrix of x,y and N the self matrix of y,z then NM is the self matrix of x,z.

Since a variable is a list of cases, how exactly does it differ from the list of events that is its self vector? More simply, how does a case differ from the event that says that it is the case? The difference is subtle but real; the latter adds a kind of "self-consciousness" which, though it supplies no new information, produces a considerable shift in logical form. Consider c, the color variable of our dog. If c=1, then c points to the idea "The yellow dog". But if we now take c to be not the box variable but the index of its self vector, the value 1 points to the idea "The dog whose color could be yellow, grey or white, but is in fact yellow". Think about it.

In summary, each variable of a box has two roles, first, as a range of special cases of some overall box type, and second, as an exhaustive list of assertions of which case obtains. The self vector is the second role. The self tensor is the analogous second role of a set of variables.

These rather delicate notions of self vector and matrix are what really gets the new mathematics going. In a compound box of the kind we shall call a flowchart, the self matrices that result from cutting arrows are the states referred to in chapter 5, whereas the self matrices of sub-boxes are transformations. Combining these two kinds of self matrices with propositions as defined above, we get precise analogues of the von Neumann rules. When we replace events by their probabilities and allow these to go negative, we get the von Neumann rules themselves in their most general form.

6.4. Flowcharts.

A *flowchart* which we shall now define broadly as synonymous with a box-arrow diagram, is a box made of linked boxes, where a link is an event of the form x=y. There's really not much more to it than that.

The variables of a flowchart are simply the variables of the boxes it is made of. Variables will be shown by arrows, though not all variables need be shown. Note that because of the way we have defined variables, no two boxes can have variables in common. What, then, does it mean for an arrow to connect two boxes? Of course it means that the variables at its two ends are equal. But this equality is not *sameness;* rather, it is a *law* of the form x=y belonging to the flowchart as a whole.

Flowchart laws. All laws of the component boxes of a flowchart are to be regarded as laws of the flowchart itself. In addition there is a set of laws of the form x=y called *links* which belong to the flowchart as a box but not to any of its member boxes. Links are shown as joined arrows. A flowchart whose link set is empty is called free.

This defines flowcharts for now. But since we are looking ahead, I'll briefly mention several features of a broader notion of flowchart that we'll need to connect up the present theory with the deeper level of pre-logic.

First of all, there are two operators on boxes, negation and quantification, that give to flowchart theory the full expressive power of the predicate calculus; how these operators relate to states and transformations is largely unexplored. Second, there is the concept of *repetition* within a structured type which we need for recursion and, more broadly, for analyzing uniformity and growth in extended structures. Finally, and this is a natural development of a notation that deals with repetition, there is the concept of a box variable that ranges not over mutually exclusive cases but over *coexistent things* like members of a flowchart. What does it mean to link such a *selector* variable with a state variable? In future reports we'll return to the dark "pre-logic" of such a link; let it suffice for now to note that here may be the logical germ of the mind-body connection!

6.5. States and the von Neumann rules in flowcharts.

At last we are ready to really get down to business. In earlier chapters, a state was informally described as something that belongs to a connecting arrow and involves breaking that arrow. We can now make this more precise. A connecting arrow is a link. Breaking an arrow means removing that link. The state is the self matrix of the loose ends.

State. Given a flowchart F with a link x=y, the *state* S(x,y) is defined as the self matrix of x,y in the flowchart F' which is like F except for not containing the link x=y.

By creating compound variables (see 6.3) we can combine links into compound links which we'll call *cables,* e.g. x=y and z=w would produce the cable C(x,z)=C(y,w). Cabling lets us apply simple theorems about one or two states to complicated structures involving many links. The combination of all the linked variables of a box will be called its *box cable;* if all variables in a flowchart are linked, then the states of the box cables of its boxes determines its law. It's an open question as to what kinds of flowchart laws are determined by the states of the single links.

The first von Neumann rule says the probability is trace(PS); we've defined S and now we'll define P. We need one preliminary definition:

Link event. By a *link event* in x,y we'll mean an event in x,y that acknowledges that the two are linked by placing the same constraint on both. For instance, x=3 & y=3 is a link event but x=3 & y=4 is not, nor is x=3. The general form of a link event is E(x) & E(y).

Now recall that every event corresponds to a *proposition,* defined as the event tensor of 1's and 0's that mark its favorable and unfavorable cases. Don't forget that 1 and 0 here are the universal and null events.

State proposition. The proposition of a link event, which is a matrix of 1's and 0's in which the 1's are all in the diagonal.

First von Neumann rule, logical form. Given a state S and a state proposition P on S's variables, the link event E corresponding to P is given by trace(PS). (Trace means sum of the diagonal, where addition here is XOR).

If we replace the events in the event matrices S and P by their probabilities, then we get the first von Neumann rule in its standard numerical form, or almost. The probability of E is not quite the trace but the trace *renormalized* by dividing it by the probability of the link event x=y in the flowchart F'. To avoid the nuisance of constantly having to renormalize, we'll mostly work with quantities called *amplitudes* whose ratios are the ratios of probabilities; we can always get back to probabilities themselves by dividing by the amplitude of some event plus the amplitude of its negation.

Amplitude matrix. Given a state S, we say that a numerical matrix A is an *amplitude* matrix of S if the ratios of the entries in A are the ratios of the probabilities of the entries in S. To put it another way, the amplitude matrix of S is the probability matrix of S multiplied by an arbitrary constant.

Von Neumann state: An amplitude matrix whose trace is 1, i.e. which is of the form M/trace(M) where M is a probability matrix. In a context where we are only dealing with amplitude matrices, we'll leave off the "von Neumann".

Von Neumann state proposition: Like a state proposition, except that 1 and 0 are now interpreted as numbers rather than events.

First von Neumann rule, numerical form. Given a von Neumann state S and a von Neumann state proposition P, the probability of the event corresponding to P is trace(PS).

The second von Neumann rule, in logical form, expresses a relationship that holds between any two links in a flowchart. Let S and S' be the states of two links x=y and x'=y' in F and let F'' be the flowchart with these two links removed.

Transformations. By the *transformation* T of S into S' is meant the self matrix of y,x' in F'', while the *transformation* U of S' into S ia the self matrix of y',x.

Second von Neumann rule, logical form: S'T = TS. (proof: S=UIT=UT, S'=TIU=TU, TS=TUT, S'T=TUT, TS=S'T)

The numerical form of the second rule, like that of the first rule, is what results from replacing event matrices by amplitude matrices. Unlike the first rule, however, it has a narrower scope; it is not true of any pair of states but only of states S and S' for which T and U are *independent,* meaning that the variables y,x' are independent of the variables y',x in F''. This will of course be the case if y,x are the only variables of some box in the flowchart. Thus we'll usually represent transformations by two-variable boxes; in the case of causal flowcharts, (numerical) T is the probabilistic transfer function of such a box (fig. 6.5.1.)

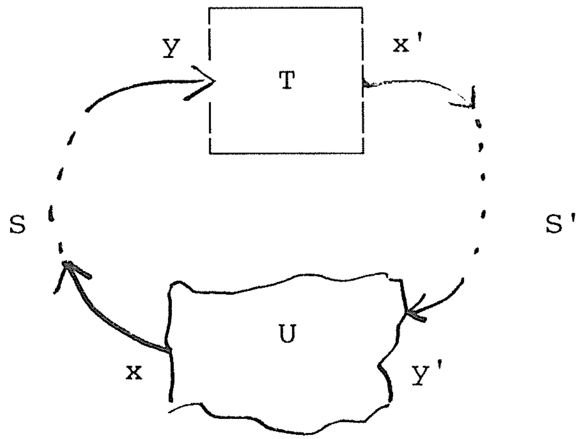


fig. 6.5.1 The transformation T of S into S'.

To summarize: The von Neumann rules are very general laws about relational composition which take two forms, logical and numerical, the latter derived from the former by replacing events by their probabilities (don't forget that probabilities as we defined them are aspects of relational structure). In logical form, the first rule applies to any link and any link event, while the second applies to any pair of links. In numerical form, the first rule also applies to any link and link event, but the second only applies to pairs of links that can be interpreted as the only links to a box.

6.6 States classified.

States will be classified entirely in terms of probability, so "states" in what follows will mean numerical states, i.e. von Neumann states. Of course the same classifications apply to the logical states from which the numerical states are derived. We'll jump ahead and simply assume that probabilities can go negative; how this relates to logic is a complex subject that we'll come back to in future reports.

Positive vector or matrix: no negative entries.

Pure states. A state S(x,y) is called *pure* if x is independent of y in F'. i.e., if removing the link x=y destroys any correlation between x and y. (In Dirac notation a pure state is of the form ¦a><b¦).

Theorem. Every state is a linear combination of pure states.

Causal state: A positive state such that the sum of every column is equal to the trace.

White vector or matrix: positive and uniform (all entries equal). A white array contains no information.

Theorem. A pure positive state S(x,y) is causal iff the self vector of y is white (the future is undetermined). (In dirac notation it is of the form ¦a><1¦).

The concept of pure causal state corresponds to our usual concept of a probabilistic state; there is no common sense equivalent to impure causal states, which occur in causal flow–charts as the states of cables in which some of the member links are causally prior to others.

Causal transformation (transition matrix). A positive (amplitude) matrix all of whose columns have the same sum.

Theorem. If S, S' are pure and causal and T is causal then the rule TS = S'T determines S' as a function of S. This establishes the familiar statistical determinism of transfer functions.

Theorem. A flowchart is causal (both states and transformations) iff it can be embedded in a flowchart all of whose boxes are functional and which has no loops (the embedding may involve gathering arrows into "bundles" representing compound variables).

Adjoint or transpose: The adjoint M* of a matrix M is the matrix that results from exchanging its rows and columns, i.e. reversing its indices (we can interpret complex matrices as real matrices in a way that preserves this definition). The adjoint of a causal state is its time–reversal. More generally, we'll speak of S and S* as the forward and backward *orientations* of a state S. If S=S* we say that S is unoriented or *self–adjoint.*

Quantum state: A self–adjoint state with positive diagonal.

Theorem. A state S(x,y) is pure quantum iff the self-vectors of x and y are equal (its Dirac form is $|a\rangle\langle a|$).

Theorem. Every self-adjoint state is a linear combination of pure quantum states.

Quantum (unitary) transformation: A non-singular transformation whose inverse is equal to its adjoint; $T^{-1} = T^*$. (Non-singular means having an inverse).

Theorem. A non-singular transformation T is quantum iff it preserves self-adjointness, i.e. iff for all self-adjoint S, if TS = S'T then S' is self-adjoint. ●

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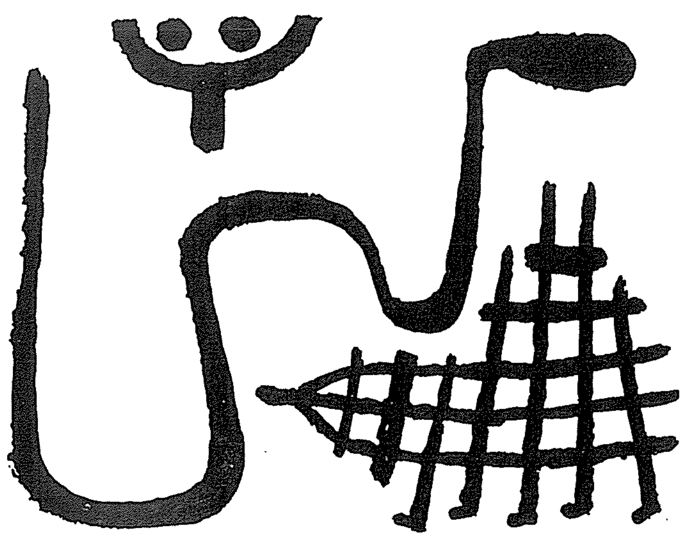

President's Report: The 14th International ANPA Meeting

by Fred Young

ANPA 14 took place from September 3rd to 6th, 1992 at Cambridge University. In addition to giving a talk by talk description of the meeting, I will discuss some questions that I feel need to be addressed.

Viv Pope opened the meeting and set the tone as well. His talk was a critique of what he thought were philosophical deficiencies in the work presented at ANPA meetings. He was criticizing mathematical physics which produces results separated from their philosophical interpretation. This provoked a lot of further discussion, and is related to other issues which emerged at the meeting. He also presented a short geometric derivation of special relativity which he said can be taught to children. Mike Heather presented some brief comments on solitons.

Lou Kauffman gave a talk on spin networks and discrete physics. This work was based on some early work of Penrose and explored the relationship of spin exchange processes to space and space–time. It is related to topological quantum field theory. Brian Clement talked on some flaws in the foundations of mathematics and how he can overcome them. Peter Marcer briefly described some work by Daniel Dubois on the fractal machine which involves a conservation of EXOR.

Day two began with Ted Bastin presenting a philosophical focus which went through McGoveran's calculation of the fine structure constant. It appears that there may be some aspects of the calculation that are not completely understood. This prompted Viv Pope to make a philosophical comment about problems with questions that force either/or distinctions. He showed where these issues fit in the overall picture of philosophical ideas. David Roscoe continued his work on gravitation without parameters. His model for gravitation involves counting and therefore has something to do with the combinatorial hierarchy. Faruq Abdullah presented an electromagnetic method of healing. He discussed the work of Stephen Walpole who treats illnesses by measuring the EEG's, determining abnormalities, and supplying frequencies missing from their profiles to restore abnormalities. Healthy brainwaves seem to follow 1/f distributions. Beth Davis also gave a talk on the Mandelbrot set crop circle.

On day three, Bill Honig humorously recounted his experiences as the founding editor of Speculations in Science and Technology. Most of the submissions involved either ballistic or ether models of special relativity. Bill finished with his own ether model for the quantum. Geoffrey Constable presented some further work on the maximum and minimum values of electrical variables. The talk discussed Josephson junctions as well as the quantized hall effect. He suggested some tests such as finding a periodicity in the red shift. Keith Bowden presented a paper on orthogonality in computing and systems theory.

The guest speaker was Chris Clarke who discussed quantum theory and consciousness. He discussed the work of Frohlich on biological quantum coherence, and discussed the general problem of qualia. He believes that quantum mechanics does bear on the problem of qualia. Peter Marcer said that the brain should be the role model for the computer rather than the computer being the role model for the brain. He says that his general system logical theory shows how the mind is a categorical version of the fundamental spectral theorem of von Neumann for Hilbert space. Clive Kilmister then spoke on fundamental conceptual problems concerning the hierarchy.

I gave the presidential address, on "Chaos, Biology, and Physics." I began by relating my modeling methodology to that of McGoveran. I discussed the reason for biological forms in the Mandelbrot set and discussed the idea of recapitulation systems generated by non–linear mappings. I proposed that the Mandelbrot set, living systems, and superstrings, loops and baby universes are all examples of non–linear recapitulation systems.

On day four Pierre Noyes began with a talk on discrete antigravity. It became clear that this is controversial within ANPA and that there is a lack of agreement concerning the foundations of the hierarchy. Mike Manthey showed how a consideration of the strong AI problem leads to a discrimination algebra that is a Clifford algebra. It is related to the work of Hestenes and is a radical way of doing the semantics of parallel processing.

Geoffrey Read presented a paper on "Mnemic Causation." Mnemic is the influence of the past on the present. He says that biology forces physics to transform itself out of all recognition. Chris Clarke spoke on the work of Parker–Rhodes. It is clear that much more work needs to be done on the foundations of the hierarchy. The meeting ended with a paper by Eddie Oshins (presented by myself) on a test for classical psychospinors. The test comes out of an analysis of Martial arts and 720 degree rotations. The test involves measuring the rotation of a neural population vector as first described by Georgeopoulos in *Science* magazine.

The meeting which many considered to be the best ANPA meeting yet, left me with two sets of questions. The first concerns the foundations of the hierarchy. After reading the proceedings of ANPA nine I was under the impression that the foundation of this work was solid. I now find that no one really understands Parker–Rhodes' derivation, and there is no consensus agreement on any other derivations or calculations. After contemplating the comments of Viv Pope I am left with the feeling that not even the philosophy of our approach is understood. The relation of the hierarchy approach to other work in the foundations of quantum mechanics is discussed in the Prephysics paper in the proceedings of ANPA 9. The current discussions appear to be both unaware of this work and less comprehensive. I have always thought that ANPA was trying to increase our understanding of the interpretation of quantum mechanics. I am not sure we have succeeded and I am afraid that the hierarchy construction has been separated from its conceptual roots. We should be concerned that someone like Chris Clarke who is fascinated by Parker–Rhodes has been uninterested in the ANPA work

The second set of questions question involves the relation of the ANPA work to other work in interdisciplinary science. When ANPA was founded there was very little interdisciplinary science. Now we have nonlinear science with fractal geometry, chaos, and self–organizing systems. The Santa Fe Institute has become an interdisciplinary center for this work. They hold conferences on subjects which range from information theory and quantum mechanics to artificial life and mathematics and DNA. The early ANPA work discussed program universes, but their relation to the hierarchy and reality were left unsolved. I am afraid that as ANPA fine tunes the hierarchy it is missing the opportunity of attracting researchers interested in other interdisciplinary work. There is very little discussion within ANPA of the relation to other interdisciplinary work. ANPA can attract new members by including relevant new work, and not becoming too narrowly focused on the hierarchy construction. ●

Ninth Annual Meeting
of the
Western Regional Chapter of the
Alternative Natural Philosophy Association

13–15 February 1993; N.B. Holiday Weekend
Cordura Hall, Stanford University

Preregistration: $20 registration; at the meeting: $25. If you wish to preregister and/or present a paper, send your registration fee and an abstract of your paper to:

Fred Young, 128 Lyell St., Los Altos, CA 94022 (415) 949–7428

Papers selected by the local committee for oral presentation will be scheduled for at most 40 minutes followed by 20 minutes of discussion. Papers not scheduled for the first two days (Saturday and Sunday) will be discussed on Monday. Any papers in *camera ready* format of at most 20 sheets (8 1/2" by 11") –preferably less – which are given to the Secretary before Saturday noon, Feb. 15, will appear in the INSTANT PROCEEDINGS the next day.

ALTERNATIVE NATURAL PHILOSOPHY ASSOCIATION

Statement of Purpose

1. *The primary purpose of the Association is to consider coherent models based on minimal number of assumptions to bring together major areas of thought and experience within a natural philosophy alternative to the prevailing scientific attitude. The combinatorial hierarchy, as such a model, will form an initial focus of our discussion.*
2. *This purpose will be pursued by research, conferences, publications and any other appropriate means including the foundation of subsidiary organizations and the support of individuals and groups with the same objective.*
3. *The Association will remain open to new ideas and modes of action, however suggested, which might serve the primary purpose.*
4. *The Association will seek ways to use its knowledge and facilities for the benefit of humanity and will try to prevent such knowledge and facilities being used to the detriment of humanity.*

Fifteenth Annual Meeting
of the
Alternative Natural Philosophy Association

September 1993 Cambridge, England
Sept. 9–10: Department of History and Philosophy of Science, Free School Lane
Sept. 11–12: Wesley House, Jesus Lane

Registration: 20 pounds plus ANPA dues, 20 pounds.

If you wish to attend and/or present a paper, please inform:

Faruq Abdullah, Secretary to ANPA
Room E517, The City University, Northampton Square, London EC1V 0HB, ENGLAND

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