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Fractional Angular Momentum, Fractional Charge, and Oscillators

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The monoaxial torsional pendulum's angle Θ relative to the lab is free real, neither mod 2π nor 4π. Hence the conjugate angular momentum is also *free real*, even quantum mechanically. If the $\frac{1}{2}k\Theta^2$ potential is relaxed at large Θ to $2\pi N$ periodicity, then the quantum of angular momentum is partially reestablished, to fractional $\hbar/N$. If the pendulum were built by tensor product from designated constituent atoms, addition of angular momentum would deny the foregoing; however, the tie to atoms is as a collective mode instead. Similarly, the $L-C$ oscillator has *free real electric charge* Q on *one plate* of the capacitor: The other plate, accommodating $-Q$, is here "the lab" or "ground"; observability of *current* breaks superselection of charge. These comments should alternatively develop from a Born–Oppenheimer approximation.

1. INTRODUCTION

The abstract essentially repeats my piece for the Washington APS meeting (Lubkin, 1982). This introduction is part update and part bridge.

I consider two separate issues of fractionalization: of charge, and of angular momentum; then a paragraph on $SU(3)$ and "quarks" touches on a third issue. By "fractionalization" I mean unusually small values for a physical quantum. Because I first encountered the phenomenon as infinitesimally small charge by considering the $L-C$ oscillator, I take that up first. Then a shift of scene to a different oscillator, the torsional pendulum, takes us to angular momentum associated with rotations about one axis, "monoaxial." There I seek to model the torsional spring or "tether" by which the bob is fastened to the wall, as a string of many other torsional elements of small inertia. Nearest neighbors in this string are to interact with a potential which it turns out must be anharmonic in order not to beg the question of

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building the collective fractional behavior out of ordinarily nonfractional ingredients. A sinusoidal potential leads to an impasse; a square well breaks through.

"Monoaxial" signifies that it is otherwise when the rotations are not confined to one axis. By attempting to nevertheless fractionalize three-dimensional angular momentum with the help of a spherical torsional pendulum, we will see on the one hand that the failure to fractionalize beyond spin-1/2 can be blamed on the instability of twists of the tether through angles greater than 2π; on the other hand that spin-1/2 (and $3/2, 5/2, \ldots$) may be considered to arise from integral spins by fractionalization.

2. CHARGE

The original reason I wished to look at the quantized $L-C$ oscillator was to show someone that the $(-1)^{1/2}$ in the phasor algebra of lowly electrical engineering was an anticipation of the $(-1)^{1/2}$ in Schrödinger's equation, and is thus fraught with significance, and is not just a cheap trick. Since the complex superposition of $\aleph_0$ state-space dimensions in solving Schrödinger's equation is literally infinitely more complicated than the one complex plane of a phasor diagram, I must however regard my original attempt as somewhat of a failure.

Consider the $L-C$ oscillator with Hamiltonian

$$H_{L-C} = \frac{Q^2}{2C} + \frac{1}{2}L\dot{Q}^2$$

where Q is the electric charge *of one plate* of a capacitor of capacitance C connected across a coil of inductance L. $\dot{Q}$ is of course the current in the coil. Quantize this classical Hamiltonian unimaginatively in parallel with the usual mechanical analog $\frac{1}{2}kx^2 + \frac{1}{2}m\dot{x}^2$, where x is a position, k is a spring constant, m is the particle's mass, and $m\dot{x}$ is the canonically conjugate momentum, with the usual correspondence $x \to Q$, $\dot{x} \to \dot{Q}$, $m \to L$, $k \to 1/C$. Then the canonical conjugate of the charge Q is $L\dot{Q}$ which, except for the c-number factor L, is the current.

The first amusing point is that the *current* constitutes an observable which does not commute with the charge Q; I take observability as evident because everyone knows that you can measure current "with an ammeter." This is a breaking of charge superselection briefer than other expositions (Lubkin, 1970; Aharonov and Susskind, 1967a,b; see also Lubkin, 1960, Rolnick, 1967, and Mirman, 1969, 1970, 1979). Because Q is the charge *of one plate* of the capacitor, with the other plate or the rest of the hardware

being a $(-Q)$-accepting "ground," the part played by the "other plate" is however very similar to that played by the "ground" in Lubkin (1970).

Not surprisingly, quantizing the $L-C$ oscillator is not new (Louisell, 1964). The problem of introducing resistances R, that is, of handling dissipation, avoided above, in particular was discussed in terms of modeling via transmission lines to infinity by B. Yurke and J. Denker (1982) at the Washington meeting.

The discussion of the quantized $L-C$ oscillator in Louisell (1964) has the slight blemish of discarding the zero-point oscillator energy, $\frac{1}{2}\hbar\omega = \hbar/2(LC)^{1/2}$; of course one can relate this to forces if one contemplates making L, C, or both variable; for example, if L corresponds to a fixed coil and C to parallel plates of fixed area A but variable separation ξ, and if k_c is the constant in Coulomb's law of force, then at low temperature T, $k_B T \ll \hbar\omega$, (k_B being Boltzmann's constant), the oscillator would be mainly in its ground state, and the plates are attracted to each other by the force $\frac{1}{2}\hbar(\pi k_c/L)^{1/2}\xi^{-1/2}$.

Now, in this simple harmonic oscillator discussion, the coordinate Q has free real spectrum (and also the momentum), it is the *energy* only which is more obviously "quantized" at discrete values $\hbar\omega(n+\frac{1}{2})$, $n = 0, 1, 2, \ldots$. That a continuous charge Q, not a multiple of the electron e, was a heretical originally implicit element of this discussion was pointed out to me by Nicholas Papastamatiou.

There are two obvious ways of dealing with this issue.

One is to weakly accept that "charge" *must* come in multiples of e, and to save the remarks on violation of superselection and on zero-point forces by limiting the examples discussed to such as have r.m.s. charge dispersion in the ground state $\gg e$, i.e.,

$$\frac{\langle Q^2\rangle_0}{2C} = \frac{1}{2}\left(\frac{1}{2}\hbar\omega\right) \gg \frac{e^2}{2C}, \qquad \text{with } \omega = (LC)^{-1/2}$$

this gives a limitation of the discussion to L, C such that

$$\left(\frac{L}{C}\right)^{1/2} \ll \frac{\hbar}{2e^2} \tag{1}$$

The second, bolder alternative is to accept the oscillator discussion literally, to regard the continuous charge Q as a phenomenon. This bolder point of view was suggested by Dale Snider.

There are two ways to seek to establish this bolder point of view. The first, a bit silly if perhaps publishable, is by simply asserting it as the faithful literal account of the harmonic oscillator Hamiltonian. It is in the style of building a discussion upon a Hamiltonian foundation.

A way to honestly establish the continuous-Q point of view is to derive the harmonic-oscillator Hamiltonian from accepted one-particle and two-particle terms, got by conceptually dissecting the lumped L and C elements, and the electromagnetic field, into ordinary charged particles with e-multiple charges only, and photons. Snider expects that such a discussion can be built on the model of the Born–Oppenheimer approximation: There, the nuclei of a molecule interact through a potential which takes care of the electrons, the lowest-lying states corresponding to an electronic ground state wrapped around a given arbitrary nuclear configuration. The idea is that, for understanding low-lying $L-C$ oscillator states, the excitations of higher frequencies are in evidence only in providing a potential for the behavior at low or lowest frequency. I have not developed this idea, but I believe it. If there exists such a way, then the "nuclear" phase of the Born–Oppenheimer story would exhibit a potential, well approximated in all likelihood near a minimum by a harmonic-oscillator potential. Even in an extreme case, where the r.m.s. charge fluctuation in the ground state is much *smaller* than e, so that *small* integer harmonic-oscillator n's all involved oscillation of charge in which only a small part of one electron's worth of charge shakes back and forth, one still should have a rounded, parabolic potential energy and a continuous coordinate, related to a continuous displacement of the center of charge of a sea of electrons distributed over a fixed, positive lattice, with no hindrance to the harmonic-oscillator character appearing even when the maximum charge displacement involves the net shift of only a small part of one electron.

The above $L-C$ oscillator story goes back several years, and seemed to me exciting because of its counter-superselection quality. I considered the continuous Q something of a liability, setting a background of implausibility for the main counter-superselection point. When Richard Sorbello showed me that the thing was already in an engineering text (Louisell, 1964), albeit without emphasis on the oscillator as a counterexample to superselection of charge, I held back my writeup, thinking that I should have to develop Dale's Born–Oppenheimer suggestion, at least, before bothering people. Reports on fractionalization by Jackiw, Schrieffer, and others (Su and Schrieffer, 1981; Jackiw and Schrieffer, 1981) however drew my interest back to my unelaborated $L-C$ oscillator story; continuous Q was no longer a liability.

3. THE TORSIONAL PENDULUM

Sometime last year it occurred to me that, since the driving force behind this is the simple harmonic oscillator, I could flee the complication

of thinking about coupling with the photon field by switching to another oscillator: the torsional pendulum. Fractionalization would be more dramatic, and clearer, because the usual quantization of angular momentum is the best understood quantization.

If we simply write

$$H = \frac{p^2}{2m} + \frac{1}{2}k\Theta^2$$

where m is a moment of inertia, k is a torsional spring constant, Θ is a free real angular coordinate for the pendulum bob, and p is the momentum canonically conjugate to Θ, hence an angular momentum, so that in a Dirac Θ representation $p = (\hbar/i)(\partial/\partial\Theta)$, then of course all questions have been begged. The spectrum of p is the whole real line, etc. What can one say about *justifying* this?

The obvious motivation is that, when a torsional pendulum is wound up a whole turn, the classical state is grossly distinguishable from the earlier less-wound-up state, because the elastic suspension is more twisted. Hence the incrementation of Θ by 2π, by 4π, 6π, etc., should not lead back to the same quantum state, but rather to new, orthogonal states. The discreteness of ordinary angular momentum imposed by the compactness of the 2π box for θ, is therefore lifted. (I use uncapitalized Greek letters to refer to unlifted angles.)

4. DISKS

Now, this is continuous rather than "fractional" angular momentum. If I wish to have it come out pedantically "fractional," then I can seek to enlarge my 2π box to $2N\pi$ and no more, in which case the quantum $\hbar$ of angular momentum will be split N-fold, to $\hbar/N$. Thus I wish to contemplate a torsional pendulum where twisting the elastic suspension N times around restores it to its original configuration. (Colleagues have called this the "quantized odometer.") For this purpose, I imagine the elastic mechanism to be for example a series of disks with N spaces between them, so that N turns of the whole series would distribute into a whole turn between each neighboring pair of disks. The entity between disks is to be a potential incapable of itself counting turns. This goal, of making "fractional" with a finite denominator, is a somewhat silly reason for introducing nonquadratic potentials, and so departing from the simplicity of a strict harmonic oscillator, but there is a better reason for looking at it: namely, the job of justifying the Hamiltonian as an approximate expression of something built

entirely out of ordinary, $\hbar$-sized, angular momenta. Because the ordinary ingredient links are not to display lifted angles, which are to appear only in a collective description, the ordinary interlink interactions must have a periodic rather than parabolic form.

5. DIRT

I think I should here interject a philosophical interlude on my overall motivation. Quantum mechanics is dirty![1] What I mean is, you speak of a state Hilbert space with precision to describe a system which is nevertheless *not* cleanly isolated from the rest of the world. If A, B are noncommuting observables, and if we first prepare an A-sharp state, then subsequently measure B in a Dirac–von-Neumann type of measurement, where after the measurement there is not only a B answer value but also the corresponding B eigenstate as a new system state, then the process of measurement has disturbed the original A value. Imagine also that A is an additively conserved quantity; then this disturbance entails Bohr's indeterminate transfer of the A quantity. Furthermore the B eigenstate is a *coherent* combination of different A values, so the tie back to the lab surround which has channeled A-stuff is a coherent aspect of the situation! You may *not* imagine the lab as first part of the quantum picture, look at a combined pure-state density matrix, and then "ignore" the lab by tracing out lab labels; that would ruin the A-value coherence! So the quantum mechanical game is to employ language which sounds referent to a completely self-contained microcosm, for a system which in fact is very much tied to an undescribed surrounding laboratory, by channels which transmit various additively conserved quantities. The objectionable quality of this dirtiness, this apparent inattention to transfers which one feels should be described, is mitigated by bringing the laboratory into the state space, even though now an outer undescribed laboratory still has an undescribed nexus with the enlarged system. Thus, there is a recurrent task of showing how a larger laboratory-and-system quantum mechanics reduces to a system-only quantum mechanics,[2] in which the laboratory and its channels to the reduced system evaporate from the formalism, but not by tracing-out (Aharonov and Susskind, 1967a, b; Lubkin, 1970, 1977). I say "recurrent" because whenever a new observable is somewhat questionably introduced, one can seek to clarify the story by

[1] This slogan (quantum mechanics is dirty) was a theme in a long article of mine (Lubkin, 1979). The idea is of course old, an aspect of Bohr's thinking.

[2] Of course there is von Neumann's discussion (1955) of independence of outcome on where you cut the system from the measuring device.

putting the key laboratory probing device also under the quantum mechanical microscope; I am *not* bewailing an infinite regress.

Another angle to this is that one should always be ready to pick out a portion of a system to be discussed as if it were a quantum mechanical microcosm, even if at first that seems too crude and dirty. My harmonic-oscillator discussion of the torsional pendulum, where the dynamical entity singled out for its own private Hilbert space has only one degree of freedom, ignores the independent dynamical degrees of freedom of its elastic tether. It is only because of the "dirtiness" theme underlying quantum mechanics that I like this: More dirt may make more quantum mechanics.

6. DISKS, CONTINUED

I now set forth a multidisk "spine" model of wall, tether, and bob (see Figure 1). N disks labeled $j = 0,\ldots,N-1$, an integer mod N, are linked, each to its nearest neighbors, by equal potentials $V_j = k[1-\cos(\theta_{j+1}-\theta_j)]$. The Hamiltonian is thus

$$H_{\text{spine}} = \sum_{j=0}^{N-1}\left(\frac{p_j^2}{2m_j}+V_j\right)$$

I had contemplated using $V_j = (k/r^2)[1-\cos r(\theta_{j+1}-\theta_j)]$ with rib count r a possibly larger integer than 1, in order to make the disks interact like multiribbed washers, but I find no advantage to this generalization. The p_j are *angular* momenta, the m_j moments of inertia.

Of course, the m_j will mostly all be equal to a common small moment of inertia "m," except for the j values corresponding to the two big disks labeled "wall" and "bob" in the illustration; the m_j will be M_{wall} and M_b with $M_{\text{wall}} \gg M_b \gg m$, possibly even $M_b \gg Nm$. I call the $m_j = m$ disks

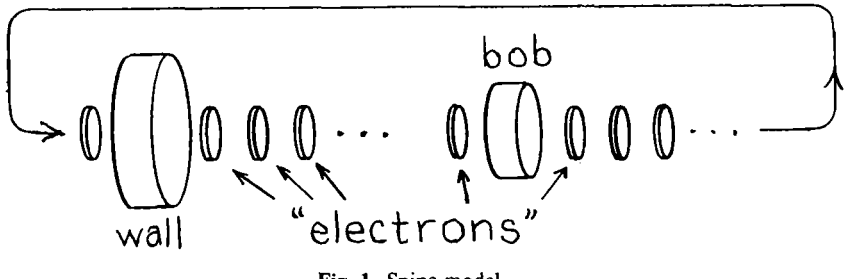

Fig. 1. Spine model.

"electrons" to signify their role vis-à-vis the Born–Oppenheimer approximation: there is to be nothing fermionic about them.

Each separate θ_j is to range over an ordinary 2π periodic compactum, so that the $p_j = (\hbar/i)(\partial/\partial\theta_j)$ eigenvalues are all whole multiples of $\hbar$, with even $\hbar/2$ forbidden. The fractional or free real angular momentum is to emerge from a treatment suppressing the electronic degrees of freedom except insofar as the spine of electrons can be twisted by turning the bob relative to the wall.

Of course variants with several bobs, no bob, and free ends are occasionally useful.

For small $\theta_{j+1} - \theta_j$, we have $V_j \approx \frac{1}{2}k(\theta_{j+1} - \theta_j)^2$; thus k is the interdisk "spring constant." V_j is not *exactly* $\frac{1}{2}k(\Theta_{j+1} - \Theta_j)^2$ so as not to beg beyond -2π lifting. Hence not only must each θ_j range over a 2π circle, but also the interdisk potential functions must be periodic in θ_{j+1} and in θ_j, of period 2π in each. Obviously $\frac{1}{2}k(\Delta\Theta)^2 \to k[1-\cos(\Delta\Theta)]$ seems simplest: in my recent "Breakthrough" (see below), I go however to the square well. The anharmonicity of these do somewhat compromise the simplicity of my program, of getting fractionalization directly from a *simple* harmonic oscillator.

My diagram tethers the bob to the wall with two strings of "electron" disks rather than one, so as to have an overall "circular topology" with each disk having two oppositely situated nearest neighbors, instead of having two "end" disks, on a *guess* that the analysis might thereby be slightly simpler. The special moments of inertia assigned to "wall" and "bob" may however so spoil the symmetry that having them be "end" disks too might do no further harm, and in the sequel I frequently pass to end-disk language.

7. CONSERVATION

I can easily inject here the obvious comment on conservation of angular momentum: One can string several bobs together in this way, and a joint rotation of everything by the same angle will commute with the Hamiltonian, and will be a sum of separate rotations, so that the unexceptional integral eigenvalue of the total infinitesimal rotation generator will be the sum of the separate infinitesimal rotational eigenvalues. Now, this is clear but misses the fractional point if my separate rotation operators are the $(\hbar/i)(\partial/\partial\theta_j)$. I wish to introduce a notion of a lifted angular coordinate Θ_b for each bob b, where Θ_b is free real (or at least ranging over some larger-than-2π circular domain). The point, then, refers to the $(\hbar/i)(\partial/\partial\Theta_b)$ operators. In the case of "wall and bob," the wall is here to be treated as a second bob.

8. LIFTING

The idea is to imitate the Born–Oppenheimer approximation. So we put the wall and bob, more generally the bob*s*, into a classical configuration, in imitation of what one does first for the nuclei in a molecule. "Classical configuration" means choosing the θ_j for the bobs = "nuclei" to have definite values (meaning mod 2π; no lifted Θ's yet), and neglecting the nuclear kinetic energy which would come from such literally severe nuclear localization. Then imagine solving for the energy eigenstates of the electrons. The eigenvalues would come out functions of the parametric nuclear θ_j's. Let us fix all bob angles but one, and focus on the dependence of this electronic energy on that one bob angle θ. The electronic ground state should describe the electron string in its least twisted configuration, consistent with the given pattern of bob angles, and in particular θ. Other, excited states should exist, corresponding to putting in (small) $\pm$whole numbers of extra whole-turn twists, between the bob belonging to θ and its (one or two) adjacent bobs (two if the N disks are linked around in a circle, one for an end bob). Thus I am expecting a twist number to be provided by an electronic-excitation label in a Born–Oppenheimer approximation. So far, the nuclear configuration label is an unlifted θ, but the electronic energy, which becomes an internuclear potential, has several possible branches corresponding to a twist number, and yet other branches, corresponding to other possibly higher electronic excitations, in which I am not interested.

The lifted angle Θ is to correspond to joining several twist-numbered branches together. As θ is increased to $\theta + 2\pi$ continuously, the least-twisted electronic ground potential will continuously vary to the once-twisted electronic excited potential; similarly the various twist-related potential branches over the 2π circle can be regarded as instead a single-valued function over a multisheeted domain, given by a free real Θ, or at least an enlarged, multi-2π circle, the picture of too highly twisted electronic states being false.

Now let me estimate the crudely expected behavior, for wall—string of $N-1$ electron disks—bob as a harmonic oscillator with spring constant K and inertia M, built out of N little springs k, the $N-1$ little inertias m, bob inertia M_b, and wall inertia ∞. The N springs k in series yield $K = k/N$. The $N-1$ electrons have net inertia $(N-1)m$, but in a mode where the bob end is moving the most, the very massive wall end negligibly, an electron a fraction x along from the wall will have its kinetic energy multiplied by x^2; since $\int_0^1 x^2\,dx = \frac{1}{3}$, the effective inertia of the string is about $\frac{1}{3}Nm$. But the bob inertia M_b is to dominate, $M_b \gg Nm$, say. Nevertheless, let me estimate the net inertia as

$$M = \mu^2 Nm$$

where μ^2 is some multiplier large compared to 1 if M_b does so dominate, but of order 1 if we try to do without a bob, and get our boblike behavior entirely from collective motion of the electron disks.

So the collective oscillation will have frequency $\Omega = (K/M)^{1/2}$, as compared to $\omega = (k/m)^{1/2}$ for one electron disk oscillating alone [of course $\omega = (2k/m)^{1/2}$ if we wish to count the *two* springs k by which it is attached to its neighbors, but never mind), where $K = k/N$ and $M = \mu^2 Nm$. Thus,

$$\Omega = N^{-1}\mu^{-1}\omega$$

So $\Omega \ll \omega$ both by virtue of having N large (many disks), and by using a massive bob (μ large). We expect a phenomenology where high frequencies are suppressed, say, at "temperatures" T with $\hbar\Omega \ll k_B T \ll \hbar\omega$, so that the electron disks will behave slaved to the nuclear bob(s), without independent motion, other than the hysteretic twisting phenomenon. So much for optimism; here comes trouble. But note the next section on "Breakthrough."

9. SUPERFRACTIONALITY AND TROUBLE

Let me seek a strong illustration of the importance of fractional angular momentum, "superfractionality," by looking at the ground state of the Ω oscillator, and ask about the r.m.s. dispersion of angular momentum p in that Gaussian state "0." Much as in the case of the $L-C$ oscillator, where particular L, C could be chosen (if they are simply assigned c-no. values) to control the zero-point charge dispersion (and in equation (1) I chose that to be large), we now seek a "superfractional" zero-point bob story where the r.m.s. angular momentum is *small* compared to $\hbar$, as a case where anomalously small angular momentum is truly dominant. The kinetic energy $\langle p^2/2M\rangle_0$ is half the total energy $\frac{1}{2}\hbar\Omega$; so

$$\langle p^2\rangle_0 = 2M\cdot\frac{1}{2}\cdot\frac{1}{2}\hbar\left(\frac{K}{M}\right)^{1/2} = \frac{1}{2}\hbar(KM)^{1/2}$$

We wish $\langle p^2\rangle_0 \ll \hbar^2$; hence superfractionality reads

$$\tfrac{1}{2}\hbar(KM)^{1/2} \ll \hbar^2, \qquad \text{or}$$

$$KM \ll 4\hbar^2 \tag{2}$$

Of course, less-than-$\hbar$ angular momentum uncertainty goes with more-than-2π lifted-angle Θ uncertainty; the zero-point spread-out of our pendulum covers more than one whole twist. Thus, $\Delta p\,\Delta\Theta \geqq \hbar/2$ is $\Delta\Theta = \hbar/2\,\Delta p$

for the Gaussian 0-point packet. $\Delta p \ll \hbar$ superfractionality reads $\Delta\Theta \gg \frac{1}{2}$; my words interpret this as $\Delta\Theta \gg 2\pi$.

Unfortunately, in terms of k and m, (2) reads $(K = k/N)\cdot(M = \mu^2 Nm) \ll 4\hbar^2$, and N cancels:

$$\mu^2 km \ll 4\hbar^2 \tag{2'}$$

But this may be read as saying that each electron disk considered as an isolated system of inertia m and tied to something else by spring k is *itself* in a state of superfractionality. Indeed, either $\mu^2 \sim 1$ because we are doing without a bob, or else $\mu^2 \gg 1$; in either case $km \ll 4\hbar^2$. Bob inertial dominance makes superfractionality *better* satisfied for the individual electron than for the torsional mode, but electron superfractionality cannot be avoided by trying to do without the bob.

Now, this is either terrible, because the spine of electrons will not then work as a reliable torsional element, or at least difficult, because it is hard to see that it works.

Let me explain further.

Each "electron" disk, in a classical Θ-net-twisted configuration, is to be only slightly twisted with respect to its nearest neighbors. If there are N-1 disks in a line, then each portion of the net twist will be only Θ/N. With N large, Θ can exceed 2π with Θ/N yet small. So each disk is classically near the minimum of its interdisk potential vis-à-vis (one or) both of its neighbors. *If* this is also true quantum mechanically, then the sinusoidal potential will be well simulated by an osculating parabola, and the $k(1-\cos\theta) \to \frac{1}{2}k\theta^2$ approximation will be good. But then the quantum mechanical spread in θ had better be small, and so the angular momentum spread conjugate to that had better be large. We do *not* want superfractionality for each *electron* disk. But we have it. Since that violates the $\frac{1}{2}k\theta^2$ approximation, the whole Hooke's law picture breaks down. The large $\hbar\omega$ electronic level spacing not only sends upper levels up to where they are happily negligible, but it also unhappily sends even the $\frac{1}{2}\hbar\omega$ ground state up to where one may no longer speak of a harmonic oscillator at all.

10. FREE WHEELING. WHAT CAN BE SALVAGED?

$\Omega \ll \omega$ when we do *not* seek any superfractionality, but just choose parameters to make the simple harmonic oscillators work. $\Omega \ll \omega$ for two reasons: because we make N large, and because we use a bob, "$\mu \gg 1$." The large N cancels out when we compare the Ω and ω oscillators' zero-point motions. Maybe we can vary parameters to gradually enlarge the zero-point

motions, and lose the one-electron-disk harmonic oscillators, while yet having $\Omega \ll \omega$ maintained by largeness of N. Then in spite of the whole-2π *free-wheeling* of each electron disk, an electronic ground state might yet respond torsionally rather than fluidly, to an imposed twist.

Indeed, it can be asked, what is the lowest energy of the chain of electron disks pinned between two classical nuclear disks as a function of the nuclear angles (it will be of their difference, of course), when the electrons strongly free-wheel (let this be my term for zero-point spread such that the wave function is near isotropic)? This will be the effective harmonic oscillator potential between the nuclei. Will this electron-bridge potential's minimum fade away too rapidly to be useful in designing a multielectronic (large-N) tether for a free-wheeling bob?

I have looked at perturbation theory, with p_j-eigen unperturbed electron states and the cosine terms of the V_j as perturbations, to see what an electron string provides as an effective potential between two classical ends, and the lowest-order nonvanishing term in the perturbed energy behaves as a cosine of the θ-end-difference. Were a tethering phenomenon persistent into the superfractional region, one would expect the effective potential to osculate to a parabola at $\theta = 0$ *better* than the simple cosine. Hence my look at perturbation theory makes me pessimistic about superfractionality here, and suggests that choosing superfractional parameters for the bob or collective motion does fluidize the tether, and so spoils the whole effect.

So tethering a bob to lift θ to Θ with sinusoidal washers obviously does work to give a language with fractional angular momentum, but the multisinusoidal tether is not good enough to support the hoped-for dramatic illustration of *dominance* of anomalously small angular momentum involved in having twisting important in the zero-point state: The attempt to make the tether that delicate leads to fluidity in its behavior. It will transmit a sinusoidal torsion, but of an ordinary angular argument, and so will not allow itself to be distinguishably 2π-twisted.

11. SUPERFRACTIONALIZATION BREAKTHROUGH

But if instead of a $k(1-\cos\Delta\theta)$ link potential I use an infinitely tall-walled *square well* link potential, the fluid failure of the tether in the superfractional limit must go away. Thus, $V = 0$ for $|\Delta\theta| < \varepsilon$, otherwise $V = \infty$. ($0 < \varepsilon \leqq \pi/2$ is less worrisome than $\pi/2 < \varepsilon \leqq \pi$; a detail.) Here the continuous springiness of a link is from quantum mechanical confinement, not from any classical parabolicity of V itself. The $V = \infty$ zones forbid treating V perturbationally, and I have not done the thing exactly. Of course one chooses $N\varepsilon \gg 2\pi$ to give the end bob the room it needs for a wide 0-point quantum state. Yet I regard superfractionalization as revived.

The Born–Oppenheimer electronic problem I have set up here but have not solved is best stated explicitly: The infinitely inert wall is set at θ_0, and the bob of large inertia M_b is also set, at $\theta_b = \theta_N$. The "electronic" Schrödinger equation is

$$-\frac{\hbar^2}{2m}\sum_{j=1}^{N-1}\left(\frac{\partial}{\partial\theta_j}\right)^2\psi = E\psi$$

with boundary conditions ("boxes")

$$\psi = 0 \text{ outside } "|\theta_{j+1} - \theta_j|" < \varepsilon, \qquad j = 0,\ldots,N-1 \tag{3}$$

with ε a fixed positive value best not larger than $\pi/2$. Each θ_j ranges over a 2π circle, say, $0 \leqq \theta_j < 2\pi$, with ψ 2π-periodic in each θ_j variable, and the allowed domain for $\psi \neq 0$, namely, $"|\theta_{j+1} - \theta_j|" < \varepsilon$, includes literal real values for $\theta_{j+1} - \theta_j$ within ε of 2π, also: the quotes are to remind one of this 2π modularity (see Figure 2). The ground state energy function and low-lying excited energy functions $E_0(\theta_0, \theta_N), E_1(\theta_0, \theta_N),\ldots$ are sought; determining these is the "electronic problem." These are expected to link into one multivalued function $E(\Theta)$, where $\Theta \equiv \theta_N - \theta_0 \bmod 2\pi$. This $E(\Theta)$, then, is the potential for the twisting of the bob through a lifted domain.

The observation that the electronic problem is equivalent to determining the low-lying energy levels of a point particle in a box within a Euclidean multi-2π toroid of $N-1$ dimensions, only makes the electronic problem *sound* easy.

Now, the classical configurations of the $N-1$ electron links are separated into disjoint connected sets by a winding integer, which I have been calling a twist count. Rafael Sorkin points out that therefore the "box" for

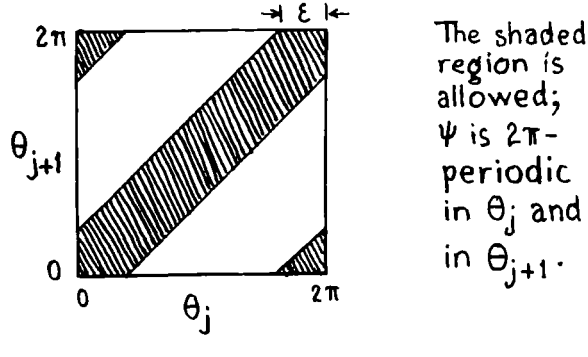

Fig. 2. Box inequalities.

the equivalent high-dimensional point particle problem must really be disconnected into separate boxes, which is why I have so called the bounding inequalities. Sorkin also points out that the E branches I want are each the energy of the ground state in *one* of the connected components. Thus the trick of using an infinitely tall potential defines which quantum states correspond to the twist phenomenon I wish to describe, and which correspond to the higher electronic excitations I wish to neglect: excited states *of the separate component boxes* are to be neglected. Of course, if we imagine the potential to be large but finite, there is no strict separation into disconnected boxes, and the "ground states" of the more twisted components become the *relevant* electronic excited states heretofore sought.

The angle between the θ_j, θ_{j+1} box-bounding hyperplanes and the ones for $\theta_{j+1}, \theta_{j+2}$ is 60°. If one deskews these to 90° by a linear transformation, then the heretofore nicely Laplacian kinetic energy gets cross terms.

12. THE THREE-DIMENSIONAL TETHERED TOP

Something must go wrong with indefinite fractionalization of full three-dimensional angular momentum, because of the exhaustiveness of the usual spin-0, -1/2, -1, -3/2,... representations of $O(3)$'s infinitesimal algebra. But let us anyhow try to proceed by using a spherical torsional pendulum. Roughly, let there be concentric wall and bob isotropically inertial rigid, say, spherical, shells, connected by elastic fill. In order to clearly keep any radial degrees of freedom of the elastic out of our story, I prefer to model the elastic by using a chain of $N-1$ further concentric isotropically inertial shells, of equal low inertia—these are now the "electrons"—with adjacent shells interacting by a potential of form $k(1-\cos\alpha)$, where α is the angle of twist between the pair of shells under consideration; of course a "breakthrough" square well form is advisable if something like superfractionalization is wanted, but that does not seem to be as interesting here. In more elaborate language, each shell has ψ, θ, ϕ Euler-angle coordinates (and no radial or other freedom), and a j to $j+1$ rotation matrix is defined as the difference

$$D_j(\text{six variables}) = R(\psi_{j+1}, \theta_{j+1}, \phi_{j+1})\left[R(\psi_j, \theta_j, \phi_j)\right]^{-1}$$

with the R's the usual 3×3 rotation matrices; that difference D_j is a rotation about some axis through angle α_j, and

$$V_j = k(1-\cos\alpha_j)$$

indeed Trace $D_j = 1+2\cos\alpha_j$.

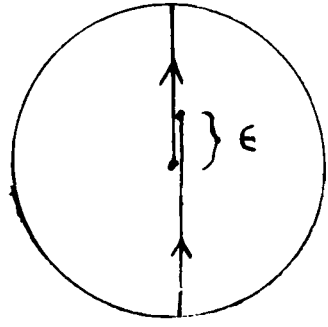

Fig. 3. A $2\pi + \varepsilon$ twist.

The primitive idea is that a twist of the bob relative to the wall through angle Θ about some axis will be distributed as N rotations of angles $\alpha_j = \Theta/N$, and hence if N is large, a twist Θ through 2π or even a small multiple of 2π, e.g., through 4π, would produce such a different situation in the elastic from the relaxed, untwisted condition, that the range of Θ will be lifted from its naive range of 2π, and not only to 4π but beyond.

But beyond 2π does not work, because the elastic is unstable when it is twisted more than 2π. Indeed, plot the $N+1$ rotations $R(\psi_j, \theta_j, \phi_j)$ for $j = 0,\ldots,N$, which define the $N+1$ classical orientations of the concentric spheres wall $= 0, 1, 2, \ldots, N-1$, bob $= N$. The electron spheres $1,\ldots,N-1$ are supposed to relax to a lowest-energy state consistent with the ad hoc winding applied to the wall and bob ends, so the $N+1$ rotations will march from 0th to Nth gradually, giving a pseudocontinuous path through the rotation group. Each D_j is trying to be small, so the path likely approximates some kind of geodesic, probably that belonging to the Cartan metric on the rotation group, and indeed one of length a local minimum, that is, minimum relative nearby varied paths connected to the same endpoints. Figure 3 illustrates an attempt to pass off a straight-looking path representing a twist through $2\pi + \varepsilon > 2\pi$ about one axis, as such a local minimum. I am using the points of a three-dimensional solid ball of radius π with diametrically opposite surface points identified, in the usual way, to represent rotations, the point α away from the center representing rotation through angle α about the axis from center to the point, in the sense given by the right-hand rule.

Figure 4 shows that the attempt fails. The path is continuously deformable to monotonically shorter paths, terminating with a rotation sequence about the opposite axis (or "going the other way about the same axis"), ranging over $2\pi - \varepsilon$ instead of $2\pi + \varepsilon$. I have set this up with, say,

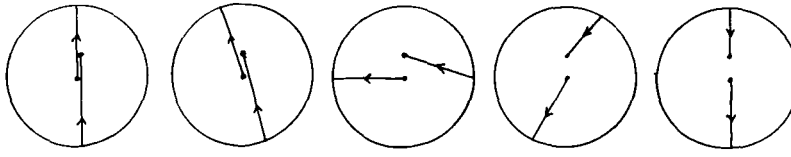

Fig. 4. Instability of the $2\pi + \epsilon$ twist.

"wall" being at the home (0,0,0)-Euler-angle configuration, represented by the ball's center, but of course the whole story can be shifted by an overall constant rotation, which would however complicate the diagrams. Also note that my intermediate paths have the awkwardness of being composed of two segments, along one of which the several rotations are coaxial, but along the other of which the several rotations are about varying axes, with an unsmooth bend at the point of diametric identification, but that this awkwardness in no way compromises the argument.

Now, the "Born–Oppenheimer" idea is to extend the classical configuration space of the bob object relative the wall, by using only metastably relaxed electronic interpolates of the tether. A 2π winding is, as is well known, nontrivial, but we must do without metastable $2\pi + \epsilon$ windings, in particular without any nontrivial 4π winding.

13. SPIN 1/2 (AND 3/2, 5/2, ETC.)

At least a 2π winding is *non*trivial, so we expect our $O(3)$ rotation-group-labeled configurations for the bob to double to its covering group. Wave functions describing the tethered top will then be complex functions on $O(3)$'s covering group, and will need all the $Y_{lmm'}$'s on the doubled Euler-angle domain to be fully based, including those indexed half-odd-integrally. The idea is, in building the model of $N+1$ potential-interlinked shells, to use not only the uncovered $O(3)$ notion of adjacent-shell twist angles α_j, but also rigid-body Hamiltonians for the separate shell kinetic energies which do *not* involve half-odd-integral spin. Thus, all the ingredients carefully avoid "spin-1/2," but the resulting collective behavior, described as a tethered top, exhibits "spin-1/2."

So either regard fractionalization through tethering as a failure here because we get no spin-1/3, -1/4, etc., spin-1/2 not being news, or else regard fractionalization as brilliantly successful in giving a construction of spin-1/2, -3/2, etc. out of purely integrally spinning ingredients.

In particular, a *small* zero-point motion of the bob should not reach out to 2π-sized twist, and the lowest wave function needs a sizably half-odd-integral component to interfere constructively near the identity with the

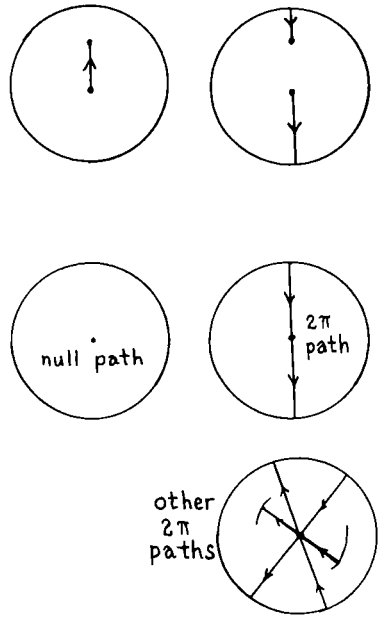

Fig. 5. Two locally shortest paths with given endpoints, except when the endpoints coincide.

integral component, but destructively near the 2π-twist point. So here small, classical-like zero-point spread may be more interesting than the large, near-isotropic coverage of the doubled Euler-angle domain of a "superfractional" situation.

14. COVERING GROUP?

My tethered-top configuration space was really a set of locally length-minimum geodesics, and not the covering group. Let us look to see that these are essentially the same, and worry some about the fact that they are not quite the same.

As before, without loss of generality, put the wall at the center or identity, and the bob in general position, so that the "bob" point may be taken as bob-relative-wall, or as $R(\psi_N, \theta_N, \phi_N)[R(\psi_0, \theta_0, \phi_0)]^{-1} = D$. Trace $D = 1 + e^{iA} + e^{-iA} = 1 + 2\cos A$ defines $\cos A$ unambiguously, hence A to $A \to 2\pi - A$ ambiguity. There are corresponding two locally shortest paths, illustrated in Figure 5. So the locally shortest paths seem to map two-to-one onto the $O(3)$ group, and seem so far a fine realization of the covering group. The trouble happens in the particular case when D is the identity; when the two points of the figure lie on top of each other. There

are two difficulties. The first is that no direction is distinguished, hence there is no unique longer path, but rather a whole projective 2-sphere's infinitude of them. The other difficulty is that any one of these is at only neutral equilibrium to being varied to any other; no one path is itself at a *strong* local minimum.

We can get to our obvious goal, the covering group, by replacing a classical path of locally minimum length with the *set* of classical paths at a common minimum length, connected to each other by neutral-equilibrium variations. Then instead of having our path-related construction cover most points of $O(3)$ doubly, but mess up by covering the single identity point by one null path plus a whole infinitude of 2π-long paths, that infinitude will be collapsed to one second object, hence the identity point will also then be doubly covered.

And "Born–Oppenheimer" thinking encourages us to accept this fusion of 2π-long paths. There is a unique long path corresponding to an almost but not quite 2π-twisted configuration. An actual bob "twisted 2π" relative the wall will be something like an angle and angular-momentum Gaussian-like wave packet spread out, in angle, somewhat about "2π," with 2π itself of measure zero. Each off-2π component of the state will at least naively go along with its near-2π-length uniquely twisted tether configuration; the whole lot of (near)-2π-long paths get used all at once.

This delocalization of the twist axis in the neighborhood of 2π is at first hard to believe. If you make a carefully isotropic model of concentric shells, and twist the inner one relative the outer, you will see the in-between shells corotate differentially about the same axis, and at 2π, you will think you still can pick an axis, but that there is a degeneracy associated with a gross instability if you fluctuate about 2π. This gross instability can be masked by friction in the bearings of an actual model. It is hard to imagine really building a model so delicate as to exhibit the gross delocalization of twist axis at "2π"! Of course, the tethered-top language is appropriate only when there is no independent dynamics of the tether—including no friction in the bearings.

It is of course easy to imagine the above delocalization as an onset of the worse gross delocalization of each electron shell in the three-dimensional analog of the superfractional disease already partly discussed for the one-dimensional rotation group with multisinusoidal tether. If my conclusion that the tether gets fluid is correct there and here too in that limit, then the tethered-top Euler angles will not even get lifted to the covering-group doubled domain in that limit, and no spin-$1/2$ there (except for the square well "breakthrough" alternative). A tethered top will require half-odd-integral spins only if its electron shells are fairly localized, and hence, *a fortiori*, the top itself well localized, except for a near-2π coherent delocali-

zation phenomenon among the electrons. Since the top is well localized on its doubled Euler-angle domain, its spherical-harmonic expansion will require a large spread of angular momenta. To discriminate a packet in one small part of the domain from a far-away packet over the other small part of the domain which co-covers the collapsed ordinary Euler-angle domain, half-odd-integrally-labeled harmonics, $Y_{lmm'}$'s with l, m, m' half-odd-integral, will be prominent, along with the integral ones. The least of these, with $l = 1/2$, will not particularly stand out.

15. TETHERING AND COVERING

One can seek to generalize to any non-simply-connected Lie group of "angles." First, there is a system whose configuration space is the given group. Next, one strings $N+1$ such systems together by means of potentials which tend to keep adjacent systems at the same angle. If N is reasonably large, a sequence of $N+1$ rotations gradually relaxed to a local minimum energy would approximate some sort of locally shortest continuous path from configuration 0 to configuration N, which two ends are to be held fixed.

Fixity of the $0, N$ ends while the $1,\ldots,N-1$ "electrons" adjust, is a Born–Oppenheimer picture, which may need support in having much more inertia vested in the ends than in the intervening electrons.

Obviously I have been for some time using a "segmental" picture with wall and bob at ends of a segment, instead of the "circular" one where each object has two neighbors. The segment looks more like a path with end points; the circle joins our objects by a pair of paths. But if we start trivially and wind up the bob relative to the wall, the two electron strings should wind up inversely, so that there is still a simple relation between configurations and paths with end points.

The thought occurs of connecting bob to wall with s electron strings, the segmental story being the case $s = 1$, the circular story $s = 2$. When $s = 3$, and one winds up along a certain path on one branch, how does the inverse winding arrange itself between the other two branches? The idea that one takes a product of the overall covering-group operations in all the s branches to equate to the identity involves an ordering not implicit in the unordered structure of the branches themselves. But if you think of creating the twist gradually, you see that the s branches will be gradually created as a time-ordered product...?

Building a chain of more-or-less copies of the original system is a strong move in the direction of a covering space of the original configuration space, because it replaces points in the original space by paths. The

universal covering space is obtained mathematically by joining paths together in equivalence classes I like to call "ways," where homotopic paths belong to the same way. The very large space of paths is reduced to the universal covering space of ways by means of homotopy.

Physically this would be too glib. Instead, paths are thinned out by a criterion of minimum energy relative neighboring paths, corresponding to a Born–Oppenheimer electronic ground state. Difficulty typified by the nonuniqueness of minimal 2π-twist paths discussed for $O(3)$ above, will appear, and may be resolved by emphasizing the importance of the quantum mechanical delocalization phenomenon always lurking behind the semiclassical thinking.

If N is small, the physical ways emergent will be only "sufficiently short ways"; long ways not being sufficiently discriminated from each other when only $N-1$ spots along the way, other than the ends, are marked. In the case of a noncompact group, this would prevent the covering from reaching the universal covering group, which would possess ways of arbitrarily great minimum length. But the easiest way to think about it is to think first of the universal covering group anyhow, then to note that overlong ways are unstable, and ways near a threshold of classical instability are only weakly metastable because of a considerable quantum tunneling phenomenon. Even if classical instability is made the formal cutoff, it is not clear that the overlong ways eliminated correspond neatly to a reduction to a particular nonuniversal covering, however.

The case of two electron disks for the original one-dimensional rotation group may be instructive. A total twist angle Θ is divided evenly into three $\Theta/3$ rotations, so $\Theta = 6\pi$ is indistinguishable from $\Theta = 0$. All these are

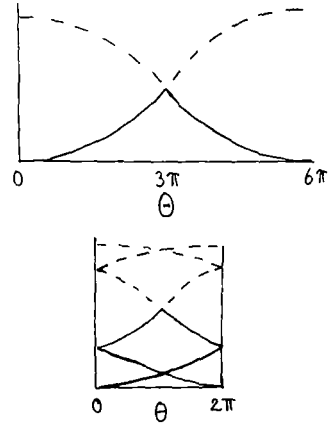

Fig. 6. Wall-bob effective potential, for two electron disks.

classically stable to small oscillations, except for the exact "midpoint" at $\Theta = 3\pi$, where each of the three links is at maximum potential energy. The wall-bob effective potential set by choosing the classical minimum energy for the two-electron-disk configuration, should look like Figure 6. If this is wrapped around to give a picture over a conventional 2π-sized θ domain for bob relative wall, one gets a three-branched minimum-effective-potential story, with instability at the cusp. Here, the threefold covering of the circle by a larger circle is clearly appropriate, except for the $\Theta = 3\pi$ instability.

16. TETHERING AND THE BREAKING OF SYMMETRY

A physically meaningful rotation or transformation of a system must not act on the laboratory's fiduciae; there must be some axes exempt from the action relative to which the system can be verified as having been rotated (transformed). Obvious as this may be, I am aware of David Finkelstein's emphasis of this point ("... the idea of a basic exact symmetry is counteroperational...," Finkelstein, 1980). The dirtiness of quantum mechanics guarantees that the system has roots in the laboratory. Hence a symmetry-breaking twist of the system relative *its own roots* is implicit in any *physical* symmetry. There is a philosophic tether here.

In order to do unusual things with angular momentum, I have tethered my bob system in such a way that its angular momentum is not a good quantum number; angular momentum oscillates instead of being constant for a torsional pendulum. My tethers are more robust than the ubiquitous philosophic tether, which is too nebulous to exert a restoring torque, yet I feel there is a kinship between the two sorts of tether.

17. QUARKS?

The particles of ordinary, "unconfined" matter, those that actually make it apart to asymptotic infinity, belong to representations of $SU(3)/Z_3$, and not to the other quarklike representations of its covering group $SU(3)$. (I ignore the indium spheres. I am not addressing the fractionalization of electric charge directly either.) Full $SU(3)$ behavior could be sought as the behavior of a chain of $SU(3)/Z_3$ ordinary objects, an example of covering by tethering. Quarks would appear as one of the ingredients of a representational analysis of a collective behavior of a string of ordinary hadronic matter. Confinement is automatic, but such quarks would seem to be large, not small objects. Perhaps the necessity for calling forth a whole string of ordinary hadrons, in involving large momenta, can have a Fourier-inverse aspect of positional concentration.

Such an attitude toward quarks would betray the hope that they are simpler than unconfined matter, and so might not be generally useful.

Yet it could be useful in bridging between 1960s Chew-style *S*-matric, non-field-theoretic physics, and quarks. If one is linguistically limited to what is available at asymptotic infinity and unitarity, then if one wishes yet to speak of quarks one *must* approach them synthetically. Covering by tethering at least suggests that this is not completely idiotic.

18. TETHERING, CONFINEMENT, AND SPIN 1/2

I have discussed entities with unusual quantum numbers, tethered together in composites. When a composite is itself untethered to anything, its overall quantum numbers are however usual. This is confinement.

I have also shown how spin 1/2 can arise dependent on tethering, within an integral spin context.

The two ideas together would have spin 1/2 confined. But spin 1/2 exists torsionally decoupled from the lab.

Hence a proposal that spin 1/2 always arises from the behavior of non-spin-1/2 constituted tethered tops, is unattractive.

19. VAGUE REFERENCES TO SIMILAR THINGS

I like to say that Dirac discovered spin-1/2 twice, once of course in his square-root relativistic wave equation, and again (1931) in his study of the motion of a spinless quantum electric charge in the field of a spinless magnetic monopole.[3] Relatively recent elaboration of pole-charge spin-1/2 (Jackiw and Rebbi, 1976a,b; Hasenfratz and 't Hooft, 1976; Goldhaber, 1976; Friedman and Sorkin, 1979) is considered by those authors as a building of spin-1/2 out of bosons. As far as I know now, the tethering idea seems to be a different construction, not involving gauge fields or dual charge.

Something like fractional monoaxial angular momentum has been independently introduced by Frank Wilczek (1982a, b), again in a context of gauge fields and dual charge.

ACKNOWLEDGMENTS

I have already mentioned useful formative conversations with Nicholas Papastamatiou and Dale Snider. I thank David Finkelstein for the thought on the implication of symmetry

[3]Though Dirac (1931) explicitly finds a doublet, hence spin-1/2, Dirac does not draw attention to that; Saha (1936) does.

breaking entailed in observation, and for his interest in this paper. Roman Jackiw's articles, talks, encouragement, and communication were the immediate stimulus for the present write-up. I thank Donald Witt for encouragement and suggestions. I have also bothered Donald Beck, Yutze Chow, Todd Leen, Leonard Parker, Luis Pimentel, Richard Sorbello, Rafael Sorkin, and Gilbert Walter.

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