TOPOLOGY IN PHYSICS - A PERSPECTIVE

A.P. Balachandran

Department of Physics, Syracuse University,
Syracuse, NY 13244-1130

ABSTRACT

This article, written in honour of Fritz Rohrlieh, briefly surveys the role of topology in physics.
When I joined Syracuse University as a junior faculty member in September, 1964, Fritz Rohrlich was already there as a senior theoretician in the quantum field theory group. He was a very well-known physicist by that time, having made fundamental contributions to quantum field theory, and written his splendid book with Jauch. My generation of physicists grew up with Jauch and Rohrlich, and I was also familiar with Fritz’s research. It was therefore with a certain awe and a great deal of respect that I first made his acquaintance.

Many years have passed since this first encounter. Our interests too have gradually evolved and changed in this intervening time. Starting from the late seventies or thereabouts, particle physicists have witnessed an increasing intrusion of topological ideas into their discipline. Our group at Syracuse has responded to this development by getting involved in soliton and monopole physics and in investigations on the role of topology in quantum physics. Meanwhile, especially during the last decade, there has been a perceptible shift in the direction of Fritz’s research to foundations and history of quantum physics. Later I will argue that topology affects the nature of wave functions (or more accurately of wave functions in the domains of observables) and has a profound meaning for the fundamentals of quantum theory. It may not therefore be out of place to attempt a partly historical and occasionally technical essay on topology in physics for the purpose of dedication to Fritz.

This article has no pretension to historical accuracy or scholarship. As alluded to previously, particle theorists have come to appreciate the importance of topology in the classical and especially in the quantum domain over the years, and that has evoked a certain curiosity about its role in the past, and about the circumstances leading to its prominence since the late seventies. The present article is an outgrowth of this idle curiosity. I have been greatly helped in its preparation by the book on soliton physics written by Russian colleagues, the essay on Skyrme by Dalitz and a speech by Skyrme recon-
structed by Aitchison\textsuperscript{a}, and have relied on these sources for information when necessary.

Our recent changed perceptions about topology is well brought out by the following incident. Some time in the early part of this year, I received a book from Physics Today entitled \textit{Knots and Physics}. It is written by the mathematician Louis Kauffman and Physics Today wanted me to review it.

Now, twenty five years ago, it would have been remarkable to send a book on knots to a physicist for review. Indeed, a book with a title \textit{Knots and Physics} would have been considered bizarre by physicists and mathematicians alike. Twenty five years takes us back to 1968. It was a time when the phenomenon of spontaneous symmetry breakdown was only beginning to be widely appreciated, and electroweak theory had not yet been fully articulated. Particle physicists were immersed in studies of symmetry principles, and Ken Wilson was yet to launch lattice QCD. Physicists and mathematicians had cordial, but generally distant relations. Ideas on topology were far from our minds. A knot to me meant no more at that time than what is tied at Hindu weddings. True, there were a handful of physicists, like David Finkelstein and Tony Skyrme, who talked of solitons and fundamental groups. But they were the oddities. We were content with Feynman diagrams and current commutators.

But this was not always so. The first idea on solitons had already occurred to Scott Russell in 1842. He was observing the motion of a boat in a narrow channel and discovered that the water formed by its wake formed a remarkably stable structure. He coined the phrase “solitary elevation” while discussing the phenomenon he witnessed. I reproduce his report on what he saw below.
Report on Waves. By J. Scott Russell, Esq., M.A., F.R.S. Edin.,
made to the Meetings in 1842 and 1843.

Members of the Committee

- Sir John Robison*, Sec. R.S. Edin.
- J. Scott Russell, F.R.S. Edin.

I believe I shall best introduce this phenomenon by describing the circumstances of my own first acquaintance with it. I was observing the motion of a boat which was rapidly drawn along a narrow channel by a pair of horses, when the boat suddenly stopped—not so the mass of water in the channel which it had put in motion; it accumulated round the prow of the vessel in a state of violent agitation, then suddenly leaving it behind, rolled forward with great velocity, assuming the form of a large solitary elevation, a rounded, smooth and well-defined heap of water which continued its course along the channel apparently without change of form or diminution of speed. I followed it on horseback, and overtook it still rolling on at a rate of some eight or nine miles an hour, preserving its original figure some thirty feet long and a foot to a foot and a half in height. Its height gradually diminished, and after a chase of one or two miles I lost it in the windings of the channel. Such, in the month of August 1834, was my first chance interview with that singular and beautiful phenomenon which I have called the Wave of Translation, a name which it now very generally bears; which I have since found to be an important element in almost every case of fluid resistance, and ascertained to be the type of that great moving elevation of the sea, which, with the regularity of a planet, ascends our rivers and rolls along our shores.

It was not Scott Russell alone who came across topological notions in the last century. Sir William Thomson had published his ideas about atoms being vortices in a fluid in 1867. Thomson later became Lord Kelvin. Kelvin did not like the rigid point-like atoms
of chemists. He very much wanted to describe them visually as extended structures. He was much impressed by Helmholtz’s discovery of “vorticity” in fluids. On the basis of experiments with Tait on smoke rings and analytical results on vortices, he developed his vortex atom which maintained that atoms are vortices in a perfect fluid. Some of the vortex atoms of Kelvin are shown in Fig. 1. He already had a good intuition about certain knot invariants and seemed upset that he knew Riemann’s “Lehrsätze aus der Analysis Situs” only through Helmholtz.

![Fig. 1. Kelvin proposed that atoms are vortices in a perfect fluid. The figure shows some of his vortex atoms.](image)

But Kelvin’s ideas did not catch on. Fantasies of his sort soon came to be regarded as reactionary desires to preserve a dissolving mechanistic world. The ancien régime was
losing its ability to rule in the face of the revolutionary onslaughts of the emerging relativists and quantum theorists. Physicists generally soon ceased to be seriously bothered by knots and topology for many years.

But not all physicists. The young Dirac had published his remarkable text book on quantum mechanics in 1930. Soon thereafter, he came to recognize certain basic features of wave functions with implications which sharply differentiate classical from quantum physics. I can outline his insights from a modern perspective as follows.

The dynamics of a system in classical mechanics can be described by equations of motion on a configuration space $Q$. These equations are generally of second order in time. Thus if the position $q(t_0)$ of the system in $Q$ and its velocity $\dot{q}(t_0)$ are known at some time $t_0$, then the equations of motion uniquely determine the trajectory $q(t)$ for all time $t$.

When the classical system is quantized, the state of a system at time $t_0$ is not specified by a position in $Q$ and a velocity. Rather, it is described by a wave function $\psi$ which in elementary quantum mechanics is a (normalized) function on $Q$. The correspondence between quantum states and wave functions however is not one to one since two wave functions which differ by a phase describe the same state. The quantum state of a system is thus an equivalence class $\{e^{i\alpha}\psi|\alpha \text{ real}\}$ of normalized wave functions. The physical reason for this circumstance is that experimental observables correspond to functions like $\psi^*\psi$ which are insensitive to this phase.

In discussing the transformation properties of wave functions, it is often convenient to enlarge the domain of definition of wave functions in elementary quantum mechanics in such a way as to naturally describe all the wave functions of an equivalence class. Thus instead of considering wave functions as functions on $Q$, we can regard them as functions on a larger space $\hat{Q} = Q \times S^1 \equiv \{(q, e^{i\alpha})\}$. The space $\hat{Q}$ is obtained by associating circles $S^1$ to each point of $Q$ and is said to be a $U(1)$ bundle over $Q$. Wave functions on $\hat{Q}$
are not completely general functions on \( \hat{Q} \), rather they are functions with the property 
\[ \psi(q, e^{i(\alpha + \theta)}) = \psi(q, e^{i\alpha})e^{i\theta}. \]
[Here we can also replace \( e^{i\theta} \) by \( e^{in\theta} \) where \( n \) is a fixed integer.]

The behaviour of wave functions under the action \((q, e^{i\alpha}) \rightarrow (q, e^{i\alpha}e^{i\theta})\) of \( U(1) \) is thus fixed. Because of this property, experimental observables like \( \psi^*\psi \) are independent of the extra phase and are functions on \( Q \) as they should be. The standard elementary treatment which deals with functions on \( Q \) is recovered by restricting the wave functions to a surface \( \{q, e^{i\alpha_0} | q \in \hat{Q} \} \) in \( \hat{Q} \) where \( \alpha_0 \) has a fixed value. Such a choice \( \alpha_0 \) of \( \alpha \) corresponds to a phase convention in the elementary approach.

When the topology of \( Q \) is nontrivial, it is often possible to associate circles \( S^1 \) to each point of \( Q \) so that the resultant space \( \hat{Q} = \{\hat{q}\} \) is not \( Q \times S^1 \) although there is still an action of \( U(1) \) on \( \hat{Q} \). We shall indicate this action by \( \hat{q} \rightarrow \hat{q}e^{i\theta} \). It is the analogue of the transformation \((q, e^{i\alpha}) \rightarrow (q, e^{i\alpha}e^{i\theta})\) we considered earlier. We shall require this action to be free, which means that \( \hat{q}e^{i\theta} = \hat{q} \) if and only if \( e^{i\theta} \) is the identity of \( U(1) \). When \( \hat{Q} \neq Q \times S^1 \), the \( U(1) \) bundle \( \hat{Q} \) over \( Q \) is said to be twisted. It is possible to contemplate wave functions which are functions on \( \hat{Q} \) even when this bundle is twisted provided they satisfy the constraint \( \psi(\hat{q}e^{i\theta}) = \psi(\hat{q})e^{in\theta} \) for some fixed integer \( n \). If this constraint is satisfied, experimental observables, being invariant under the \( U(1) \) action, are functions on \( Q \) as we require. However, when the bundle is twisted, it does not admit globally valid coordinates of the form \((q, e^{i\alpha})\) so that it is not possible (modulo certain technical qualifications) to make a global phase choice, as we did earlier. In other words, it is not possible to regard wave functions as functions on \( Q \) when \( \hat{Q} \) is twisted. [We are assuming for ease of presentation here that wave functions are always smooth functions. That is not of course always the case. The significance of \( \hat{Q} \) is that smooth functions on \( \hat{Q} \) can provide us with physically acceptable domains for observables. Discussions involving domains of operators tend to be technical. We will not therefore pursue this remark further here.]

It was a great merit of Dirac that already in his seminal paper of 1931 on the role of
phases in quantum theory, he isolated a physical system where $\hat{Q}$ was twisted and these phases had an important role. This was the system of a particle with electric charge $e$ and a particle with magnetic charge $g$. Now we all know that if there is a magnetic monopole (or monopole for short) at the origin, the Maxwell equation $\nabla \cdot \vec{B} = 4\pi g \delta^3(x)$ excludes the existence of a smooth vector potential for the magnetic induction $\vec{B}$ and causes endless trouble in formulating quantum theory. The idea of Dirac was to replace the monopole by a semi-infinite, infinitely thin current loop [see Fig. 2]. This semi-infinite loop is often called the Dirac string. The effect of the string away from itself is that of a monopole,

![Fig. 2](image)

With a magnetic monopole at the origin, $\nabla \cdot \vec{B} = 4\pi g \delta^3(x)$, $\vec{B} = \text{magnetic induction}$. Dirac represented magnetic monopoles by semi-infinite, infinitely thin current loops (Dirac strings). He showed that the loop can not be observed if electric charge $e = \frac{\hbar}{2g} n$, ($n = 0, \pm 1, \ldots$). Thus if a magnetic monopole exists, electric charge is quantized.

But that is not so exactly on the string. Dirac argued that the effect of this string is undetectable if $e = \frac{\hbar}{2g} n$ (in units where the speed of light $c$ is 1), $n$ being an integer. When that is the case, shifting the string amounts only to changing the phase of the wave function in a way that cannot be observed. Thus, the position of the string, and hence the
string itself, ceases to be an observable when $n$ is an integer. In that case, then, Dirac’s approach yields a quantum theory for a charge and a monopole [whereas it gives only a theory of a charge and a semi-infinite current loop when $n$ is not an integer]. In this way, Dirac derived the quantization of $eg$ in a charge-monopole theory.

This paper of Dirac is fundamental to quantum theory, and much of modern mathematics as well. On the physical side, it predicts charge quantization in units of $\frac{\hbar}{2g}$ and charge quantization is a basic experimental fact. Further, the charge-monopole system has the remarkable feature that it can have half-odd integral angular momenta even if the charge and monopole have integral spin. We are thus led to understand from Dirac’s work and later developments that composites can have half-odd integral spin even if its constituents have integral spin. Dirac’s paper also suggests the ideas which later were discovered, developed and forcefully articulated by Aharonov and Bohm in the context of their celebrated effect. As for mathematics, it must be among the first publications on fibre bundles, the integer $n$ defining what the mathematicians call the Chern class. In Molière’s book, *Le Bourgeois Gentilhomme*, M. Jourdain somewhere exclaims to his philosophy teacher that he was making prose for more than forty years without knowing it. The following is a reproduction of the relevant passage.

M. Jourdain: Quoi? quand je dis: ‘Nicole, apportez-moi mes pantoufles, et me donnez mon bonnet de nuit’, c’est de la prose ?

Maître de Philosophie: Oui, Monsieur.

M. Jourdain: Par ma foi! il y a plus de quarante ans que je dis de la prose sans que j’en susse rien.

M. Jourdain: What? when I say: ‘Nicole, bring me my slippers, and give me my night-cap’, is that prose?

Philosophy Teacher: Yes, Sir.
M. Jourdain: Good heavens! For more than forty years I have been speaking prose without knowing it.

*Le Bourgeois Gentilhomme* (1670), II. iv.

It would seem that Dirac was unknowingly making mathematics in the same way that M. Jourdain was unconsciously making prose.

All this happened in 1931. But we knew nothing about Dirac’s work when we learned quantum theory in Madras. Indeed, quantum theory is still taught in universities missing out on all the beautiful and fundamental discoveries coming from the youthful Dirac of 1931.

Dirac’s paper, I suppose, must have been too difficult for physicists for several decades. What is surely true is that it was largely ignored till the ’70’s. Then events began to unfold in particle and condensed matter theory bringing topological issues to centerstage, and reviving forgotten memories of Dirac. But before describing these events, let us first go back to the late ’50’s and the ’60’s. At that time, Tony Skyrme and David Finkelstein had also discovered novel topological ideas which they were developing independently. They were working alone, or with a colleague or two. Their ideas too remained uncomprehended by the community for many years.

Tony Hilton Royle Skyrme was born in England in 1922 in the house of his maternal grandparents. His maternal great-grandfather Edward Robert knew and admired Kelvin, and was associated with the construction of the Tidal Predictor under the direction of Kelvin and Tait. This machine was for predicting tides worldwide. The admiration went to the extent of naming his son Herbert William Thomson Roberts. This machine was in the house where Tony was born. Tony has said in a speech that he was greatly impressed by the ingenuity of its mechanism.

Tony grew up in a world beset with increasing turbulence. In 1943, after Cambridge, he joined the British war effort in making the atomic bomb. It was only in 1946 that he began
fundamental research. During 1946-61, he was associated with Cambridge, Birmingham
and Harwell and was engaged in wide ranging investigations in nuclear physics. It was this
work, especially the work on nuclear matter and the fluid drop model, which eventually
culminated in his beautiful proposition that nucleons are solitons made of pions.

In the speech of Tony I mentioned above, he has described the reasons behind his
extraordinary suggestions. He knew of Kelvin from the Tidal Predictor, and was vaguely
aware of Kelvin’s vortex atoms. Like Kelvin, he too desired a model of the nucleon which
was visualizable and extended. He felt that fermions can emerge from self-interacting
Bose fields just as bosons arise as bound states of fermions. Moved by these imprecise and
intuitive desires, Tony began his work on nonlinear field theories, the sine-Gordon equation
and the chiral model and was led to the proposition that nucleons are twisted topological
lumps of pion fields6. In his papers, Tony also had initiated ideas on bosonization, vertex
operators, and quantum theories on multiply connected spaces, all years and years ahead
of his time, and all topics of central interest today.

David Finkelstein had a grasp of topology and differential geometry which was excep-
tional for physicists in the ’60’s. Like Skyrme, he had understood that solitons can acquire
spin 1/2 from the topology of the configuration space. He must have realized what little
role relativistic quantum field theory played in the theory of solitons, and struck by the
fact that all existing proofs of the spin-statistics theorem relied on relativistic quantum
field theory. But solitons can acquire spin half in nonrelativistic models and can not
always be described by relativistic quantum fields. He and Rubinstein, I suppose, were
led by such thoughts to seek and find an alternative proof of the spin-statistics theorem.
Their proof was published in 19687. It is this proof which is important for chiral soli-
tons. There are grounds to expect that it is the Finkelstein-Rubinstein approach which
will be found significant in quantum gravity as well. An absolutely fundamental result,
namely the spin-statistics theorem, is getting topologized. Even more striking, it is still
The closing ’60’s and the ’70’s herald the dawn of modern times for theorists. In condensed matter physics, attention began to focus on vortices in superconductors and superfluids, and defects in liquid crystals. Soon a classification of defects based on its topological properties emerged. In particle physics, the bootstrap theory of Chew was leading to unexpected developments. According to Chew, there is nuclear democracy, all particles are bound states of each other and none is more elementary than another. These ideas evolved into string theory with its explosive implications for mathematics and mathematical physics. Physicists in search of the ultimate solution claim that “strings are “TOE”, “TOE” meaning “Theory of Everything.” If that is so, physicists have found a scientific substitute for God. QCD and electroweak theory also began to take shape in the late ’60’s and the early ’70’s. It soon became plausible that they had the ability to account for physics at energies less than about 100 GeV, and perhaps up to much higher energies as well. Attention turned to unification of strong and electroweak theories by the construction of “grand unified” models.

It was in this ambience that topology was discovered in particle theory. There was first the striking discovery that grand unified theories predicted monopoles and vortices. Realization soon dawned that the correct approach to their study involved the ideas put forth by Dirac in 1931 and the defect theories of condensed matter. There was then the discovery of instantons in QCD and its implications for vacuum structure, time reversal violation and neutron electric dipole moment. The remarkable observation was also made by ’t Hooft that electroweak theory predicted baryon and lepton number violation, although at that time, it was felt that the effect was too small to observe.

There was another dimension to this story. These developments involved intense cross fertilization between fields. There were basic contributions to physics by leading mathematicians. It involved as well greater interaction between condensed matter and
particle theorists at least because of their shared interest in defect theory.

The result of all this activity was that we were bombarded with papers on topology, and by sheer exposure, if not by effort, began to be aware of topological issues. For some of us, this was a period of excitement at learning beautiful new ideas. It was also a period of hope that nonperturbative effects with topological roots can now be investigated with greater ease. Instantons, vortices and monopoles began to play a role in papers on phenomenology and have continued to do so to this day. The startling suggestion was made that monopoles can catalize proton decay at strong interaction rates with life times of the order of perhaps $10^{-19} - 10^{-20}$ seconds. The discovery of ’t Hooft about baryon and lepton number violation was also given teeth by the realization that their rates were greatly enhanced at high temperatures. It is nowadays widely speculated that this effect has a significant bearing on the observed baryon number asymmetry in the universe.

It was during this time that we discovered Skyrme’s work at Syracuse\textsuperscript{9}. There was, before us, the paper of Pak and Tze\textsuperscript{10} reviewing Skyrme’s research. That too suggested to us that it was worth our while to work on Skyrmions. It was in this way that we came to write our papers on Skyrmions and tell Witten about these ideas. Soon there followed Witten’s remarkable papers\textsuperscript{11}. It did not take much time thereafter for the general acceptance of Skyrme’s ideas.

Topological notions have flourished with extraordinary vigour in particle physics, and what is at times called physical mathematics, for ten or more years. Much of the recent impetus comes from string and conformal field theories and their relation to complex manifolds.

Topology is not likely to go away from classical and quantum physics in the foreseeable future. It is now appreciated that it has at least two important roles in physics. Firstly, it can suggest the existence of stable structures like defects, vortices, monopoles and Skyrmions. Secondly, it has a profound influence on the nature of quantum states. This
influence comes about because, as mentioned previously, the phases of wave functions can have serious consequences in quantum theory. This second feature is still poorly understood. Even its existence is not widely known even though sixty two years have passed after Dirac’s paper. There is also a nonabelian generalisation of these phases which we have not discussed in this article.

There is also an entirely new role topological physics has recently begun to assume, brought about because fundamental mathematical developments are nowadays significantly influenced by quantum field theory. The contributions to knot theory by Witten, the discovery of certain mirror manifolds in string theory and the recent developments in Riemann surface theory are all dramatic examples of this role of physics in mathematics. This then is one more reason for us to anticipate the vitality and longevity of topological trends in physics.

It is time to come to an end. Although there are certain prominent names associated with topological physics and its developments, it is good to be conscious that physics is a social activity in which many humans, not all well-remembered, participate, and that whatever we do necessarily partakes of the collective knowledge and creativity of the physics community. From this perspective, all the advances and insights which have emerged from studying topological aspects of physics are also ultimately the fruits of labor of generations of physicists. I conclude this essay by quoting a poem by Brecht which aptly describes these thoughts.

A WORKER READS HISTORY

Who built the seven gates of Thebes?
The books are filled with names of kings.
Was it kings who hauled the craggy blocks of stone?
And Babylon, so many times destroyed,
Who built the city up each time? In which of Lima’s houses,
That city glittering with gold, lived those who built it?
In the evening when the Chinese wall was finished
Where did the masons go? Imperial Rome
Is full of arcs of triumph. Who reared them up? Over whom
Did the Caesars triumph? Byzantium lives in song,
Were all her dwellings palaces? And even in Atlantis of the legend
The night the sea rushed in,
The drowning men still bellowed for their slaves.

Young Alexander conquered India.
He alone?
Caesar beat the Gauls.
Was there not even a cook in his army?
Philip of Spain wept as his fleet
Was sunk and destroyed. Were there no other tears?
Frederick the Great triumphed in the Seven Years War. Who
Triumphed with him?

Each page a victory, At whose expense the victory ball?
Every ten years a great man,
Who paid the piper?

So many particulars.
So many questions.

I am most grateful to Paulo Teotonio for drawing the figures and typing their legends, and to Carl Rosenzweig and Paulo Teotonio for help with the text. This work was supported by the Department of Energy under contract number DE-FG02-85ER40231.
References

A long list of references is not appropriate in an informal essay of this sort. Only a limited number of publications are therefore cited.

1. V.G. Makhankov, Yu. P. Rybakov and V.I. Sanyuk, “The Skyrme Model, Fundamentals, Methods, Applications” [Springer-Verlag (in press)].

This book has been especially useful for information on Kelvin and on his vortex model.

2. R.H. Dalitz, *Int. J. Mod. Phys. A* 3 (1988) 2719.

3. T.H.R. Skyrme, *Int. J. Mod. Phys. A* 3 (1988) 2745. This speech was reconstructed by I.J.R. Aitchison.

References 2 and 3 have been freely used while discussing Skyrme.

4. P.A.M. Dirac, *Proc. Roy. Soc.* (London) A133 (1931) 60.

5. A.P. Balachandran, G. Marmo, B.S. Skagerstam and A. Stern, “Classical Topology and Quantum States” [World Scientific, 1991].

6. T.H.R. Skyrme, *Proc. Roy. Soc.* (London) A247 (1958) 260; A260 (1961) 121; A262 (1961) 237; *Nucl. Phys.* 31 (1962) 556; *J. Math. Phys.* 12 (1971) 1735.

7. D. Finkelstein and J. Rubinstein, *J. Math. Phys.* 9 (1968) 1762. For recent developments involving the Finkelstein-Rubinstein ideas, see R.D. Tscheuschner, *Int. J. Theor. Phys.* 28 (1989) 1269 [Erratum: *Int. J. Theor. Phys.* 29 (1990) 1437]; A.P. Balachandran, A. Daughton, Z-C. Gu, G. Marmo, A.M. Srivastava and R.D. Sorkin, *Mod. Phys. Letters* A5 (1990) 1575 and *Int. J. Mod. Phys.* A (in press); A.P. Balachandran, T. Einarsson, T.R. Govindarajan and R. Ramachandran, *Mod. Phys. Letters* A6 (1991) 2801; A.P. Balachandran, W.D. McGlinn, L.
O’Raifeartaigh, S. Sen and R.D. Sorkin, *Int. J. Mod. Phys.* A7 (1992) 6887; A.P. Balachandran, W.D. McGlinn, L. O’Raifeartaigh, S. Sen, R.D. Sorkin and A.M. Srivastava, *Mod. Phys. Letters* A7 (1992) 1427. These papers develop proofs of spin-statistics theorems which do not use relativity or quantum field theory.

8. G. ’t Hooft, *Phys. Rev. Letters* 37 (1976) 9; *Phys. Rev.* D14 (1976) 3432 [Erratum: D18 (1978) 2199].

9. A.P. Balachandran, V.P. Nair, S.G. Rajeev and A. Stern, *Phys. Rev. Letters* 49 (1982) 1124; *Phys. Rev.* D27 (1983) 1153.

10. N.K. Pak and H. Ch. Tze, Ann. Phys. (N.Y.) 117 (1979) 164.

11. E. Witten, *Nucl. Phys.* B223 (1983) 422, 433.