Roman Jackiw and Chern-Simons Theories

by Robert D. Pisarski*

Abstract. I recount my personal experience interacting with Roman Jackiw in the 1980's, when we both worked on Chern-Simons theories in three dimensions.

Recently, the Center of Mathematical Sciences and Applications at Harvard initiated an excellent series of talks on Math-Science Literature. In the inaugural talk, S.-T. Yau spoke about S. S. Chern as a great geometer of the 20th century [1]. Particularly in the discussion section after the talk, Prof. Yau emphasized the essential role which Roman Jackiw played in bringing Chern-Simons theories into physics. In this note I wish to share some recollections from those times, and especially my interactions with Roman. I do this because I most strongly agree with Prof. Yau's assessment of Roman's contribution. In this as in other areas, Roman's work exhibits both the sheer joy of computation, while continually pushing to understand the greater significance of what is truly new and exciting.

As a graduate student, with David Gross and Larry Yaffe we computed the fluctuations to one loop order about a single instanton at nonzero temperature [2]. I then went off to Yale University as a postdoc [3], where I worked with Tom Appelquist on gauge theories in three dimensions. Our motivation was to understand the behavior of gauge theories at high temperature, which for the static mode reduces to a gauge theory in three dimensions. We computed the gluon self energy to one and (in part) to two loop order. The gluon self energy isn't gauge invariant, so the greater significance of our analysis wasn't clear. But we also computed in an Abelian theory for a large number of scalars, which is a nice soluble model, and from Tom I learned the most useful craft of power counting diagrams. We concentrated on the gluons, since quarks are fermions, and decouple in the static limit.

Sometime in the fall of 1980, I went to MIT to give a talk on this work. It was a joint seminar with Harvard, and I remember it well to this day. Talks were then on transparencies, and the evening before I thought I would be clever, and added a comment that while topological charge in four dimensions is quantized classically, perhaps it isn't quantum mechanically. Sidney Coleman was sitting near the front, in a purple crushed velvet suit which to me looked very much like that of Superfly. When he saw that slide, however, Coleman pounced, and did not let up until I surrendered abjectly, admitting to the idiocy of my suggestion.

During the talk and for the entire afternoon after, Roman grilled me about details of the calculation, how gauge theories in three dimensions work, what about gauge invariance, everything. It was very intense and quite exhilarating. Roman and S. Templeton then wrote a paper on theories in three dimensions [4], which because Roman is a superb calculator, appeared as a preprint a few weeks before ours.

When their paper came out, I remember looking at it, and thinking, ah, very good, they were spending their time on something irrelevant, two component fermions in three dimensions. Now if you forget about nonzero temperature and just do dimensional reduction, the natural thing is to go from four component fermions in four dimensions, to four component fermions in three.

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And thus I missed the really new physics, which Roman grasped. The most interesting part of the paper by Roman and Templeton appears a bit pedestrian, at the beginning of Sec. III, Eqs. (3.3) and (3.4). Under the Lorentz (or Euclidean) group in three dimensions, all one needs are two component fermions, since the Dirac matrices in three dimensions can just be taken as the Pauli matrices. What I did not work out is that under the discrete transformations of parity and charge conjugation, that a mass term for a *single* two component fermion is parity *odd*! Roman and Templeton discuss in their Ref. (11) [4] that the dimensional reduction of a four component fermion gives *two* two-component fermions. The masses for these are of equal in magnitude, but *opposite* in sign. This is how the mass for four dimensional fermions, which is certainly parity even, remains so after dimensional reduction to three dimensions.

This while it wasn't present in the original paper by Roman and Templeton, if one computes the gauge self energy for *two*-component fermions in three dimensions, then a parity mass term for the gauge field will appear *immediately*. Yes, it has nothing to do with nonzero temperature, but so what? It is an absolutely beautiful, novel, and gauge invariant mass term, special to three dimensions.

This was first proposed in two papers by S. Deser, Roman, and Templeton, in a Physical Review Letter [5], and a long paper in Annals of Physics [6]. The year before, Jonathan Schonfeld has proposed the same theory [7].

What Deser, Roman, and Templeton realized, however, and which Schonfeld did not, is that there is something extraordinary about a non-Abelian Chern-Simons term [8]: the ratio of the Chern-Simons mass term to the gauge coupling is an integer,

$$q = \frac{4\pi m}{g^2} = \text{integer}\,, \tag{1}$$

where m is the Chern-Simons mass, and g^2 is the coupling constant for the non-Abelian gauge theory. This quantization of the topological ratio q follows from invariance under large and topologically nontrivial gauge transformations, as the Chern-Simons term is related to the topological charge in four dimensions.

For years I didn't believe this result; it just seemed so *simple*, how could it possibly be right? For a while I went off in other directions. When I was at Fermilab a few years later, though, with Sumathi Rao we thought that if one computes perturbatively, as an expansion in g^2/m, then to one loop order the result will be a pure number, independent of g^2 or m. And *surely* perturbation theory can't know about large gauge transformations, right?

Sumathi and I did the computation to one loop order by brute force, which was not trivial. We had to sort out differences with the previous results of Deser, Roman, and Templeton [6].

In the end, by computing a ratio of (finite) renormalization constants, we discovered that in a SU(N) gauge theory the topological ratio shifts as

$$q \to q + N\,, \tag{2}$$

That is, even in perturbation theory q shifts precisely by the number of colors, and thus knows about large gauge transformations! This is an example of how in physics, one can do good things for all the wrong reasons. I set out to prove that Roman was wrong, and instead, completely confirmed his results in a beautiful and unexpected way.

I then continued working on Chern-Simons theories, considering the effects at nonzero temperature [9] and the effects of magnetic monopoles [10]. I showed that the topological ratio remains quantized at nonzero temperature [11], and that even in the Abelian theory, magnetic monopoles can produce the quantization of the topological ratio.

However, this was the time of the Second String Revolution, and my work had little impact upon the field. Except for Roman, who would include me in correspondence to others working in the field, such as Andrei Linde and Oleg Kalashnikov. I include copies of these letters in the figures below, from the time of our original work. The most important quote is from Roman, which unfortunately is partially obscured by a marker I used on the original. He wrote:

> *The gauge-invariant mass term that we have discovered may in fact provide the correct infra-red regulation at high temperatures. At present we do not see how it can be derived from the four-dimensional high-temperature theory: perhaps it has its origin in the $\theta F\widetilde{F}$ term. I would be very interested in any comments you may have about this.*

Even now I do not understand how a Chern-Simons mass term in three dimensions can arise from a θ term in four dimensions, but that is really besides the point. Roman's work illustrates the virtue of following things where they lead. Don't solve the problem you want to solve, solve the problem waiting to be solved.

In 1988, Edward Witten did his magesterial work on the relationship between Chern-Simons theories and the Jones polynomial [12]. Along the way, almost as an aside, he derived the shift in the topological ratio q. The work by Rao and I had so little impact upon the field that Alvarez-Baume, Labastida, and Ramallo [13], and also Chen, Semenoff, and Wu [14], rederived this result. In writing this article, I was rather amused to see that the shift in the topological ratio q was later derived by Axelrod and Singer using perturbative means [15].

I close with a personal comment. After Fermilab I went to the High Energy Theory Group at Brookhaven.

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Prof.R.Jackiw
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25 December 1980

Dear Professor Jackiw,

Thank you very much for your preprint with Templeton, I study it now with a great interest. I am happy that you investigate now the problems, which are important for the thermodynamics of the Yang-Mills gas. This problem is very interesting for me. Indeed, as it was shown in my papers Rep. Progr.Phys. 42 (1979) 389 and Phys.Lett. 96 (1980) 289, 293 (see also a large recent preprint by Gross, Pisarski and Yaffe), infrared problem in thermodynamics of the Yang-Mills gas at any temperature, at which gluons may be considered as free interacting particles (not only near the critical temperature), is connected with the 3-dimensional Yang-Mills theory, and that an infrared cutoff (or instability) may appear at momenta $|p^2| \sim g^2 T$ (at $|p^2| \sim g^2$ in the 3-dimensional theory). Understanding of the infrared behaviour of thermodynamics of the Yang-Mills gas is very important for the solution of the primordial monopole problem in grand unified theories, and I expect that there will be much work in this direction.

It might be of some interest for you that this problem has been studied also by O.K.Kalashnikov and V.V.Klimov from our institute. In particular in their preprint No 129 (1980) they have obtained the term $-g^2 c(-p^2)^{\frac{1}{2}}$ coinciding with the corresponding term in your eq. (4.14a) and in a recent preprint No 195 by O.K.Kalashnikov some part of the term $O(g^4)$ different from that given in (4.14a) was obtained. I will inform them about your work, and they will send you their preprints. Unfortunately all these results are not quite informative in the most interesting

Figure 1. Letter from A. Linde to Roman, page 1.

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region $|p^2| \sim g^2 T$ ($|p^2| \sim g^2$), since at small $|p^2|$ all higher-order corrections are large.

I would be greatly thankful to you for sending me your further papers, which always are very interesting and important for me. Renata Kallosh send you her best regards.

Yours sincerely

A. Linde A.Linde

Figure 2. Letter from A. Linde to Roman, page 2.

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O.K.Kalashnikov

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30.12. 1980

Dear Professor Yackiv,

Recently we have had an opportunity of acquainting with your work "How super-renormalizable interactions cure their infrared divergences", November, 1980 and have found it very interesting. In particular we are interested in your study of the 3-dimensional QED. As for the 3-dimensional Yang-Mills theory we would like to inform you that starting with 1979 we, too, are carring out the investigations of the infrared behaviour of the finite-temperature Yang-Mills theory which is determined by the 3-dimensional theory. Some of the results obtained in your work overlap with those found earlier by us and published as: O.K.Kalashnikov and V.V.Klimov, Lebedev Phys. Inst. preprint No 129, July 1980 (Jad. Fiz. 33 (1981) 595). More specifically, in these papers we obtained one-loop expressions (eqs. (2.8) and (3.5)) which are in agreement with your results (eqs. 4.9a) and (4.9b)). As for the higher approximations we think that your equation (4.14) is not valid at $p^2 < g^2$ and therefore it cannot be associated with the infrared behaviour of the Yang-Mills theory. We do not see, either, how your equation (4.10) can be put in accord with the Slavnov-Teylor identities.

To our opinion eq. (4.10) should not be solved by iterations but rather in a bootstrap way like it was done in paper: O.K.Kalashnikov, Lebedev Phys. Inst. preprint No 195, November 1980 (Zh. Eksp. Teor. Fiz; Pis'ma 20(181) No 2) what resulted in

We hope that you will make acquaintance with our works and will bear them in mind. We would appreciate very much in future exchanging information and preprints on the work in this field.

Very sincerely yours

O.K.Kalashnikov

Figure 3. Letter from O. Kalashnikov to Roman.

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Prof.T.Appelquist
Prof.R.D.Pisarski,
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23 January 1980

Dear Professors Appelquist and Pisarski,

Thank you very much for your paper "Hot Yang-Mills Theories..." This paper is very interesting for me and contains a lot of new results. However I disagree with your objections to my work on the monopole confinement and with your interpretation of the magnetic screening.

Indeed, topological arguments are absolutely unnecessary to prove magnetic confinement. The only thing which is necessary is to prove that the monopole creates some massive field H_i^a, which satisfies eq. $\partial_i H_i^a = 0$ and has nonvanishing energy (see my paper Phys. Lett. 96 (1980) 293). For quasiabelian monopoles (sources of the magnetic fields with only one isotopic component H_i^a for each unbroken subgroup in some gauge) this field is just the "short" magnetic field $H_i^a = \varepsilon_{ijk}(\partial_j A_k^a - \partial_k A_j^a)$ (without $\vec{A}_j \times A_k$). Indeed, if A_k^a is massive, then H_i^a is also massive, eq. $\partial_i H_i^a = 0$ is an identity, and it can be shown that the energy of the tube of this field is nonvanishing. This proves confinement of such monopoles.

Eq. $\partial_i H_i^a = 0$ for "short" magnetic field does not hold only if this field is singular. Therefore magnetic field H_i^a actually can be screened by singular sources of the magnetic field, e.g. by Hooft-Polyakov monopoles (in the unitary gauge they are singular) or by Wu-Yang monopoles. However singular Wu-Yang monopoles have infinite energy, they are not sourcelles, and their sources are not even distributions.

Figure 4. Letter from A. Linde to Tom Appelquist and myself, page 1.

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Therefore singular Wu-Yang monopoles are not contained in standard grand unified theories, and cannot screen magnetic fields in these theories.

Hope very much to receive your future works on gauge theories. Under a separate cover I send you my preprint "Grand Bang" on phase transitions in grand unified theories.

With best regards

Yours sincerely

A. Linde A.Linde

Figure 5. Letter from A. Linde to Tom Appelquist and myself, page 2.

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4 February, 1981

Dr. O. Kalashnikov, Dr. V. Klimov
and Dr. A. Linde
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Dear Kalashnikov, Klimov and Linde:

Thank you very much for your letters of 25 and 30 December, as well as for the preprints #129 and #195. I have read them all, and also passed them to my colleague, Templeton. Now I wish to make the following comments.

The main purpose of our paper has perhaps been misunderstood. We are not primarily studying finite-temperature field theories, rather we are considering super-renormalizable, for the most part three-dimensional, models. That is why we include Fermions in the QED and QCD examples (<u>c.f.</u> our comments on p. 2). Of course we are aware that such three-dimensional models are relevant to the high-temperature limit of four-dimensional theories. This I consider part of common physics knowledge, as also the fact that three-dimensional massless theories are infra-red divergent. I am happy to know that Linde noted this as well, and we shall be pleased to reference the Rep. Progr. Phys. review.

We agree that our results are informative only in the region $\sqrt{-p^2} \gg e^2$ (<u>c.f.</u> discussion on pp. 18, 25, 29 and 31). Indeed the detailed and lengthy analysis of pp. 17-21, is necessitated by our desire to use only reliable, large p results in the computation. Consequently we believe that the calculation of the coefficient of the logarithm is entirely reliable and our principal contribution, which is not found in your papers, is the establishment of this logarithmic effect.

It is good to know that we agree on the vacuum polarization magnitude, though I view it as a merely technical calculation which is preliminary to the principal purpose of finding the logarithm. [However, I have some difficulty with your numbers:

Figure 6. Letter from Roman to A. Linde and O. Kalashnikov, page 1.

Kalashnikov, Klimov and Linde 4 February, 1981

in #129 - (3.5) you have a handwritten (corrected?) value which agrees with ours, but in #195 - (3) you give a different expression which coincides with ours only in the Feynman gauge, $\alpha=1$. What have you actually calculated?]

I do not accept your criticism of our (4.10) and (4.14). For the reasons given above, the determination is reliable; also the Slavnov-Taylor identity for the vacuum polarization tensor is satisfied. Your expression #195 - (7) cannot be taken seriously, owing to its gauge-dependence (e.g. for $\alpha=1$, both "solutions" vanish.).

The gauge-invariant mass term that we have discovered may in fact provide the correct infra-red regulation at high-temperatures. At present we do not see how it can be derived from the four-dimensional high-temperature theory; perhaps it has its origin in the $\theta F\tilde{F}$ term. I would be very interested in any comments you may have about this.

Templeton has summed all the leading infra-red logarithms in QED, and shall send his results as soon as they are complete.

We look forward to hearing further from you and trust that we can come to an agreement on all points.

Please give my warmest greetings to Renata Kallosh and communicate to her my hope that I shall see her at the next Nuffield workshop in London.

Yours truly,

Roman Jackiw

P.S. Please note: I spell my name JACKIW, not Jackiv nor Yackiv.

RJ:gh

Copy to T. Appelquist
R. Pisarski
S. Templeton

Figure 7. Letter from Roman to A. Linde and O. Kalashnikov, page 2.

Years after I was hired, Bill Marciano mentioned to me once that Roman had most strongly supported my hire. Of course this wasn't the only support I had: surely kind words from my advisor David Gross, from Tom Appelquist, and from Larry McLerran [16], amongst others, also helped. Nevertheless, I was most struck by Bill's comment. In a very real sense, a good part of why I have been blessed to pursue theoretical physics, this bizarre and ahuman activity, is due to Roman. This note is my way of thanking him.

Acknowledgments

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References

[1] S.-T. Yau, "Shiing-Shen Chern as a Great Geometer of the 20th Century", https://cmsa.fas.harvard.edu/literature-lecture-series/.

[2] D. J. Gross, R. D. Pisarski, and L. G. Yaffe, *Rev. Mod. Phys.* **53** (1981) 43–80.

[3] I was a J. W. Gibbs Fellow, in fact the last. Since much of my career after working on Chern-Simons theories was devoted to the statistical mechanics of non-Abelian gauge theories, I am especially proud to have been associated, albeit tangentially, with the first great American theoretical physicist.

[4] R. Jackiw and S. Templeton, *Phys. Rev. D* **23** (1981) 2291–2304.

[5] S. Deser, R. Jackiw, and S. Templeton, *Phys. Rev. Lett.* **48** (1982) 975–978.

[6] S. Deser, R. Jackiw, and S. Templeton, *Annals Phys.* **140** (1982) 372–411; **185** (1988) 406 (erratum).

[7] J. F. Schonfeld, *Nucl. Phys. B* **185** (1981) 155–171.

[8] Back then we used the term topologically massive, but here I refer to the mass term as Chern-Simons, since they did invent it first.

[9] R. D. Pisarski, *Phys. Rev. D* **34** (1986) 3851–3857.

[10] R. D. Pisarski, *Phys. Rev. D* **35** (1987) 664–671.

[11] I was confused at to whether the quantization of q persists, and called Karen Uhlenbeck. Once I explained my problem, she immediately dispelled my doubts. As a physicist, I was thinking of large gauge transformations at spatial infinity: on R^3 at zero temperature, and on $R^2 \times S^1$ at nonzero temperature. Uhlenbeck pointed out that one should think of a compact manifold, so S^3 at zero temperature, and $S^2 \times S^1$ at nonzero temperature. Instead of spatial infinity, topological charge is then concentrated about a pinprick in the manifold. While reassured, I felt like the police officer in the movie *Ghost Dog: The Way of the Samurai*.

[12] E. Witten, *Comm. Math. Phys.* **121** (1989) 351–399.

[13] L. Alvarez-Gaume, J. M. F. Labastida, and A. V. Ramallo, *Nucl. Phys. B* **334** (1990) 103–124.

[14] W. Chen, G. W. Semenoff, and Y.-S. Wu, *Mod. Phys. Lett. A* **5** (1990) 1833–1840.

[15] S. Axelrod and I. M. Singer, "Invited talk at the XXth Conference on Differential Geometric Methods in Physics, New York, June 1991", http://arxiv.org/abs/hep-th/9110056.

[16] While at Fermilab, Larry and Peter Arnold developed their analysis of sphaleron transitions, and I spent many hours arguing with them, convinced that they were wrong. As with the quantization of the topological charge, again the error was mine.