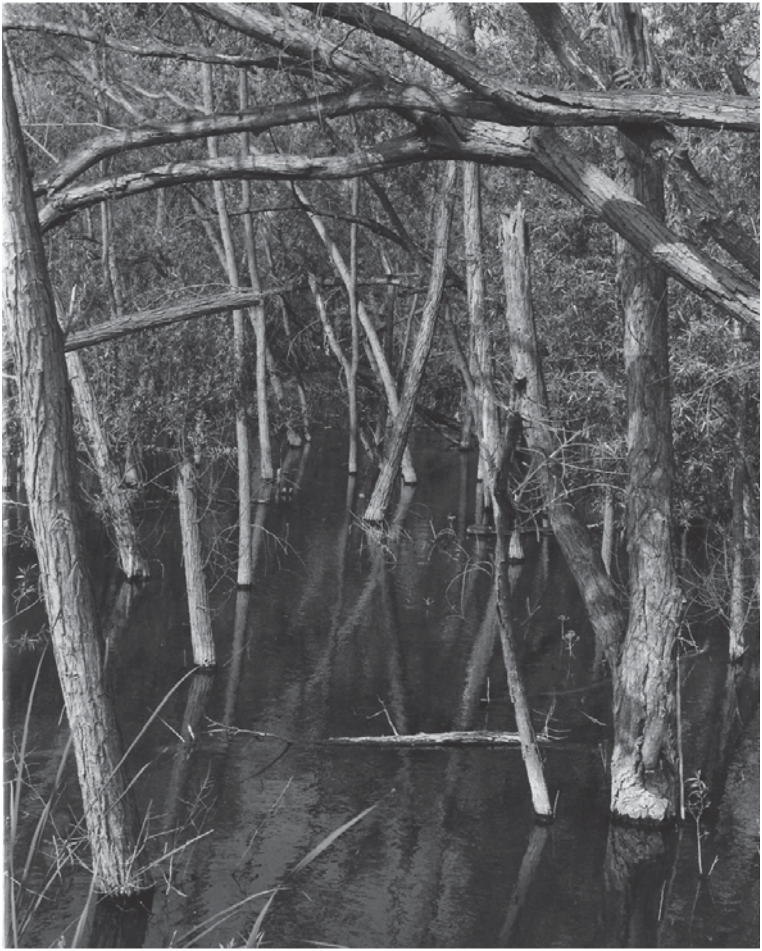


Five Talks with Myron

Thomas Curtright, University of Miami

Rather than five conversations with Myron Bander, as you might infer from my title, I will describe vignettes involving five seminars where he and I were both present, and I will try to put some aspects of these brief scenes into a broader historical context. The places and dates of the five seminars are

1) Caltech 1974 (Feynman & Gell-Mann)
2) Irvine 1976 (Pellam)
3) Irvine 1976 (Schonfeld)
4) Irvine 1977 (Freedman)
5) Fort Lauderdale 2012 (Bander)

I met Myron when I was a graduate student at Caltech, in the early 1970s.
Subsequently he hired me for my first post-doctoral position here at UC Irvine, 1976-78.
Those were important formative years for me, of course, and I greatly appreciated
the opportunities, guidance, and advice that Myron gave me during this period.

As most of you know, in addition to being a first-rate theoretical physicist with excellent intuition, Myron had a well-developed, keen sense of humor that ranged from the darkly sardonic to the gleefully juvenile, but which was always poignant. I liked this very much. I will always remember him for it.

1)

The first vivid memory of Myron I can recall was at a Caltech seminar. He and Paul Thomas had driven up from Irvine to attend a talk, a trip I would later repeat many times with Myron after Paul moved on and I became his replacement as an Irvine postdoc.

Myron and Paul were seated against the outside wall in the 4th floor Lauritsen conference room, opposite from me across the conference table in the center of the room. The talk (I cannot remember who gave it, or what it was about!) was well-attended. All the seats were taken except for one chair at the head of the table closest to the screen. This was invariably taken by Murray Gell-Mann.

Richard Feynman came in just before Gell-Mann, and seeing no available seats, except for the one reserved for Murray, he dramatically lay down on the floor between the table and the row of seats where Myron and Paul sat. Thus he was out of sight to anyone subsequently entering the room.

Murray came in last, and seated himself with a groan and a displeased look. This caught everyone's attention, perhaps intentionally, and in explanation Murray said, "I'm suffering from *rich man's disease*!" Immediately from the floor behind the table came a loud remark, "You mean *a fat butt*?" Completely surprised, Murray attempted to regain his composure and said something like, "Well ... no ... I think that would facilitate sitting." But the room had already erupted with hearty laughter, including Myron's.

However, as I looked across the room, I could see Paul was not so much amused as he was perplexed. It was immediately clear why. He turned to Myron, pointed his finger to the unknown (to him) guy on the floor and mouthed the words "Who's *that*?!" I suppose he was in disbelief that anyone would have the nerve to say something so coarse to Gell-Mann. I could see Myron respond, "*That* is Feynman."

2)

Later, I came to Irvine in the fall of 1976 as a postdoc in the particle theory group (with Myron, Gordon Shaw, and Dennis Silverman). In addition to carrying out research, the tasks given to me included organizing the preprint collection (articles arrived regularly in the US mail, printed on *real* paper!) and arranging some seminars. I can remember especially one talk attended by Professor John Pellam, in a wheel chair, during the later stages of his battle with ALS. I was seated behind him in the back of the room. John had been wheeled into the room by an attendant, and he wore weighted wrist bands to help hold his arms in their correct position on the armrests of the wheel chair. During the talk his right arm slipped off the armrest and fell down against the wheel of the chair. He couldn't raise it up.

I was distracted by this for the rest of the talk, concerned that Pellam's attendant would wheel him away at the end of the seminar without seeing that his arm was out of position, thereby injuring John as the chair's wheel turned against his skin. Even so, I waited until the end of the talk to offer any help. Then when the talk concluded, before I could do or say anything, Pellam quickly spoke to Myron who was seated next to him but who had not yet noticed John's predicament.

John said in a remarkably clear, firm voice, "Myron, would you please give me my arm?" Myron did so, gently but deliberately, with such great care and grace that the images of this simple kindness are something I have not forgotten. In a way that I cannot clearly express, this brief incident summarizes for me Myron's thoughtfulness and concern for others. From that moment onward I felt very comfortable working under his supervision.

(John Pellam died on July 23, 1977. There was a Pellam Symposium here at UC Irvine, 12 May 1978, in memoriam of the eminent low-temperature physicist. It was one of the last events I participated in at the Physics Department here, before I left to take up my second postdoctoral postion at the University of Chicago. As I recall, it is the only other such memorial symposium that I have attended. So now, some 35 years later, by participating in this symposium I feel like I have completed a long and circuitous journey back to its starting point.)

3)

When I came here in 1976 I knew many of the people at Caltech (having been a student there for so long!) and this furnished me with a ready pool of seminar speakers to invite to Irvine. At that time the theory group here provided a *whopping $15* to entertain each speaker. This was usually enough to cover the cost of an abalone dinner for both me and the visitor. Those were the days! I see that an abalone starter plate now goes for $22 at the Cannery (*if* they even have any).

Jonathan Schonfeld was then a new postdoc at Caltech, so I invited him to Irvine to talk about instantons in November 1976. He gave us an excellent seminar. Early in his talk, I asked an uninspired question about the axial anomaly. Myron joined in with Jonathan to provide a rapid response. I cannot remember the details, but their answers gave me the impression that they both thought I had asked a "dumb" question. Feeling somewhat uneducated as a consequence of this, I began to think about how to compute anomalies, more to make up for my own shortcomings than to do anything new.

Oddly enough, although I had worked on the Coleman-Weinberg mechanism of symmetry breaking for my thesis, I had never worked out from scratch the anomalous trace of the energy-momentum tensor. After dinner with Jonathan, I went back to the office before going home. I attempted a quick back-of-the-envelope calculation of the trace anomaly. Somewhat surprisingly, I got the known answer *spot-on* after a single line of algebra. That was fun.

Now, it was well-known even at that time that the anomalous trace of the energy-momentum tensor caused scale and conformal invariance to be broken. Having also worked on radiative corrections for supersymmetric theories for my thesis, another obvious exercise suggested itself to me: Compute the spinor trace of the supercurrent which I knew to be related to broken super conformal invariance. As before, a similar one line calculation gave an anomalous answer. But this time, it was not only fun, it turned out to be a *new* result.

The next day I discussed this with Myron. He was enthusiastic, and he encouraged me to work through the problem in complete detail to justify my quick calculation. So I plunged into that analysis. Looking back, I am surprised that he did not join in the calculation, but he did not. Perhaps it was because supersymmetry was such a logical stretch at that time. Unlike now.

In the meanwhile, I also had to finish up the last chapter of my thesis and return to Caltech to defend it all. (How I was a "postdoc" before defending my thesis is a long story for another venue. Suffice it to say Myron made it possible.) So there was a bit of delay before I completed all the details and checks for the supercurrent calculation. In fact it was about 6 months later that I recall asking Myron if I should write up the result and send it out as a preprint.

I don't remember his exact words, but the upshot was, why yes, of course ... why as in *why do you need to ask*?! After all, wasn't that what I was hired to do? Find and publish new results!

But Myron was more specific. He encouraged me to write a short version, and submit it quickly, and leave the bulk of the details until later. This was prescient.

I wrote it up and submitted it to the various preprint libraries, and to Physics Letters. Soon thereafter the members of the Irvine particle theory group dispersed for the summer. I went to SLAC. Myron went to Fermilab. A few days after going to Fermilab, he called me at SLAC to say that another preprint by Abbott, Grisaru, and Schnitzer on supercurrent anomalies had arrived at Fermilab. "Don't worry," Myron said, "your paper got here first." I think there was some institutional as well as individual competition underlying that statement.

4)

The superanomaly study led to some interesting developments. Regrettably, I never did write up all the details of my own calculations on the subject. I let it go at just that one Physics Letters article. Still, I did reap some benefits from the work. In particular, as a result I became friends with Dan Freedman and subsequently worked with him on another interesting problem.

Dan was at Caltech on sabbatical for the 1977-78 academic year. I met him there on one of my many trips back to Pasadena from Irvine. As I recall, I was introduced to Dan by John Schwarz, who described me as "yet another superman" or words to that effect. I replied that I was somewhat of an anomaly among supermen, a remark appreciated by Freedman. Later that fall I invited Dan to Irvine to give a seminar.

That seminar offered an opportunity for Myron to display his excellent intuition for theoretical physics. He did.

Freedman spoke about his research on antisymmetric tensor fields. The results had just appeared as a Caltech preprint and were very interesting.

Gauge Theories of Antisymmetric Tensor Fields

DANIEL Z. FREEDMAN*

California Institute of Technology, Pasadena, California 91125

...

October 1977

...

In the course of his talk, Dan exhibited a lowest order calculation of $2 \rightarrow 2$ scattering, as described in the preprint.

We now consider some aspects of the quantum field theory of our system. ...

To allay any suspicion that the theory is trivial ...

The scattering amplitude is $(S = 1 - i\delta(\ )\mathcal{M})$

$$\mathcal{M}(q_3 k,\ q_4 \ell;\ q_1 i,\ q_2 j) = 16\, g^2 f^{ijn} f^{k\ell n}(t-u) + \text{crossing terms.} \qquad (16)$$

Note that pole terms have cancelled for reasons to be discussed below. The amplitude has the same form as that of the non-linear σ-model, as it must from crossing symmetry and dimensional arguments, but it is of opposite sign.

=================== (*my emphasis*)

In his Irvine talk, Dan repeated his assertion that the 2 → 2 amplitude was of opposite sign to that in the nonlinear sigma model, and he therefore concluded that these two models were *not* equivalent.

It did not take long for Myron to object. He argued by an analogy with φ^4 theory, suggesting that only one sign of the amplitude would be consistent with a positive energy condition. And it was more or less clear from Dan's formalism that the energy density for his antisymmetric tensor model was positive. So, Myron suggested at the end of Dan's seminar here that *there must be a sign mistake* in the calculation of the 2 → 2 scattering amplitude.

Freedman said he would make some checks after he returned to Caltech. Eventually he did and, yes indeed, he found that his original sign was in error.

Kudos to Myron!

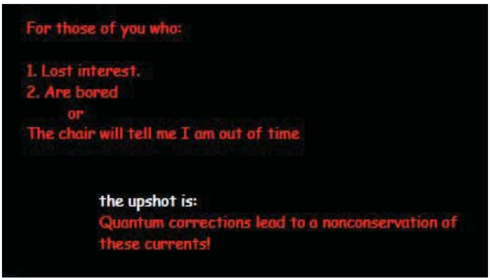
Let me re-emphasize that I describe this vignette as an example of Myron's excellent intuition, and not as criticism of Dan's original and very interesting preliminary paper. It is always possible to inadvertently flip a sign, especially when working alone without collaborative cross-checks. So far as I can tell, Dan's original 1977 paper on the subject only appeared as a Caltech preprint. However, in a later final published paper co-authored with Paul Townsend, Freedman established the equivalence of the antisymmetric tensor theory to the sigma model. As is evident in the following excerpts from that later paper, it always helps to have good students.

> These arguments demonstrate the classical equivalence of the antisymmetric tensor theory to the principal chiral non-linear σ-model. As a check on the argument a calculation of the tree approximation scattering amplitude was done using the second-order tensor lagrangian (2.3) and also using the conventional σ-model lagrangian (3.8). The results agree*.
>
> * We thank Mr. L. Alvarez-Gaumé and Mr. S. Mukhi for help with these calculations.

5)

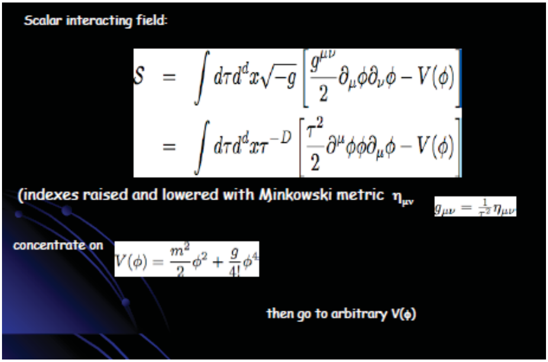
Now permit me to fast-forward 35 years to Myron's talk about de Sitter space at Miami 2012.

The slides from that talk are available online. Myron considered the supposedly conserved currents that generate the isometries of de Sitter space in light of quantum effects. The main point of his talk is summarized on this slide, with his usual humor.



In other words, he claims these currents are anomalous. So, 35 years after showing interest and offering critical advice to me on related matters, I learned that his enthusiasm for this subject remained unabated.

Myron backed up his claim about quantum corrections by considering the effects of a scalar field, a subject somewhat in vogue during the last year or so.



I will refer you to his talk and his published work cited therein for details.

Let me just use this to segue to some of my recent work, also on scalar fields, which perhaps may have some relevance to the other technical talks to follow in this session.

Allow me to offer some brief remarks about scale versus conformal invariance in the context of galileon models. This is something I have worked on during the last year or so with David Fairlie, one of the originators of galileon models.

I had hoped to interest Myron in this subject. But, sadly, circumstances did not permit that.

Galileon theories are a class of models for hypothetical scalar fields whose Lagrangians involve multilinears of first and second derivatives, but whose nonlinear field equations are nonetheless still only second order. These have been studied by numerous authors (please see references in the written version). The simplest example involves a single scalar field. This galileon field is usually coupled to all *other* matter through the trace of the energy-momentum tensor, $\Theta^{(\text{matter})}$, and is thus gravitation-like by virtue of the similarity between this universal coupling and that of the metric $g_{\mu\nu}$ to $\Theta_{\mu\nu}^{(\text{matter})}$ in general relativity.

But *surely*, in a self-consistent theory, for the coupling to be truly universal, the galileon should also be coupled to its own energy-momentum trace, even in the flat spacetime limit. Some consequences of this additional self-coupling are discussed in my papers with David Fairlie.

The action for the lowest non-trivial member of the galileon hierarchy can be written in various ways upon integrating by parts. Perhaps the most elegant of these is

$$A_2 = \tfrac{1}{2} \int \phi_\alpha \phi_\alpha \phi_{\beta\beta} \; d^n x \ . \tag{1}$$

where ϕ is the scalar galileon field, $\phi_\alpha = \partial\phi(x)/\partial x^\alpha$, etc., and where repeated indices are summed using the Lorentz metric $\delta_{\mu\nu} = \mathrm{diag}(1, -1, -1, \cdots)$. The field equation that follows from locally extremizing A_2 is

$$0 = \delta A_2/\delta\phi = -\mathcal{E}_2[\phi] \ , \tag{2}$$

$$\mathcal{E}_2[\phi] \equiv \phi_{\alpha\alpha}\phi_{\beta\beta} - \phi_{\alpha\beta}\phi_{\alpha\beta} \ . \tag{3}$$

As previously stated, the field equation is only second-order, albeit nonlinear.

It is straightforward to include in A_2 a covariant minimal coupling to a spacetime metric and hence to deduce a symmetric energy-momentum tensor. In the flat-space limit, the result is

$$\Theta_{\mu\nu}^{(2)} = \phi_\mu \phi_\nu \phi_{\alpha\alpha} - \phi_\alpha \phi_{\alpha\nu} \phi_\mu - \phi_\alpha \phi_{\alpha\mu} \phi_\nu + \delta_{\mu\nu} \phi_\alpha \phi_\beta \phi_{\alpha\beta} \ . \tag{4}$$

This is verified to be conserved upon using the field equation,

$$\partial_\mu \Theta_{\mu\nu}^{(2)} = \phi_\nu \ \mathcal{E}_2 \left[\phi\right] \ . \tag{5}$$

An interesting wrinkle now appears: $\Theta_{\mu\nu}^{(2)}$ is *not* traceless.

Consequently, the usual form of the scale current, $x_\alpha \Theta_{\alpha\mu}^{(2)}$, is not conserved. On the other hand, the action (1) is homogeneous in ϕ and its derivatives, and is clearly invariant in n dimensions under the scale transformation $\phi\left(x\right) \to s^{(n-4)/3} \phi\left(sx\right)$ for field configurations sufficiently localized so that boundary effects are insignificant. Hence the corresponding Noether current must be conserved. This current is readily found, especially for $n = 4$, so let us restrict our attention to four spacetime dimensions in the following.

In that case the trace is easily identified to be a total divergence:

$$\Theta^{(2)} \equiv \delta_{\mu\nu}\Theta^{(2)}_{\mu\nu} = \partial_\alpha \left(\phi_\alpha\phi_\beta\phi_\beta\right) \ . \tag{6}$$

That is to say, for $n = 4$ the *virial* is the trilinear $V_\alpha = \phi_\alpha\phi_\beta\phi_\beta$. So a conserved scale current is given by the combination,

$$S_\mu = x_\alpha\Theta^{(2)}_{\alpha\mu} - \phi_\alpha\phi_\alpha\phi_\mu \ . \tag{7}$$

Interestingly, this virial is *not* a divergence modulo a conserved current, so this model is *not* conformally invariant despite being scale invariant.

But our main interest was in the fact that the nonzero trace suggests an additional interaction where ϕ couples directly to its own $\Theta^{(2)}$. This is similar to coupling a conventional *massive* scalar to the trace of its own energy-momentum tensor. In that previously considered example, however, the consistent coupling of the field to its trace required an iteration to all orders in the coupling. Upon summing the iteration and making a field redefinition, the Nambu-Goldstone model emerged. But, for the simplest galileon model *in four spacetime dimensions* a consistent coupling of field and trace is much easier to implement. *No iteration is required.*

The first-order coupling alone is consistent, after integrating by parts and ignoring boundary contributions, so that

$$-\tfrac{1}{4}\int \phi\ \partial_\alpha\left(\phi_\alpha\phi_\beta\phi_\beta\right)\ d^4x = \tfrac{1}{4}\int \phi_\alpha\phi_\alpha\phi_\beta\phi_\beta\ d^4x\ . \tag{8}$$

(Similar quadrilinear terms have appeared previously, only multiplied by scalar curvature R so that they would drop out in the flat spacetime limit that we have emphasized here.) Consistency follows because (8) gives an additional contribution to the energy-momentum tensor which is *traceless*, in 4D spacetime:

$$\Theta^{(3)}_{\mu\nu} = \phi_\mu\phi_\nu\phi_\alpha\phi_\alpha - \tfrac{1}{4}\delta_{\mu\nu}\phi_\alpha\phi_\alpha\phi_\beta\phi_\beta\ , \quad \Theta^{(3)} = 0\ . \tag{9}$$

Of course, were quantum effects taken into consideration, say by dimensional regularization, it is clear that this trace may well be anomalous. We leave such quantum effects as (back-of-the-envelope?) exercises for the reader, and proceed with a *classical* field perspective.

Based on these elementary observations, we considered a model with action

$$A = \int \left(\tfrac{1}{2}\phi_\alpha\phi_\alpha - \tfrac{1}{2}\lambda\phi_\alpha\phi_\alpha\phi_{\beta\beta} - \tfrac{1}{4}\kappa\phi_\alpha\phi_\alpha\phi_\beta\phi_\beta\right)\, d^4x \ , \tag{10}$$

where for the Lagrangian L we take a mixture of three terms: the standard bilinear, the trilinear galileon, and its corresponding quadrilinear trace-coupling.

The field equation of motion for the model is now $0 = \delta A/\delta\phi = -\mathcal{E}\,[\phi]$, where

$$\mathcal{E}\,[\phi] \equiv \phi_{\alpha\alpha} - \lambda\left(\phi_{\alpha\alpha}\phi_{\beta\beta} - \phi_{\alpha\beta}\phi_{\alpha\beta}\right) - \kappa\left(\phi_\alpha\phi_\beta\phi_\beta\right)_\alpha \ . \tag{11}$$

As expected, this field equation is again second-order, albeit nonlinear.

Following the lead of many other studies, we discussed solutions of these classical equations in our papers, including some novel general relativistic effects that arise from coupling covariantly to gravity. We were surprised to discover the model readily exhibits *naked singularities*, unlike the usual situation where only a bilinear scalar field action is coupled covariantly to gravity. In terms of $r \to \infty$ data for static solutions in asymptotically flat spacetime, with constants M and C defined as

$$e^{L/2} \underset{r\to\infty}{\sim} 1 + \frac{M}{r} + O\left(\frac{1}{r^2}\right) , \tag{12}$$

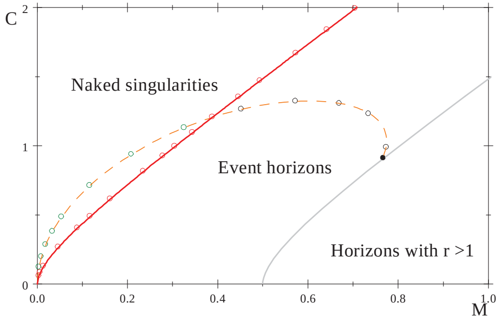
$$e^{N/2} \underset{r\to\infty}{\sim} 1 - \frac{M}{r} + O\left(\frac{1}{r^2}\right) , \tag{13}$$

$$\phi \underset{r\to\infty}{\sim} -\frac{C}{r} + O\left(\frac{1}{r^2}\right) , \tag{14}$$

and with the static metric expressed in Schwarzschild coordinates as

$$(ds)^2 = e^{N(r)}\,(dt)^2 - e^{L(r)}\,(dr)^2 - r^2\,(d\theta)^2 - r^2 \sin^2\theta\,(d\varphi)^2 \ . \tag{15}$$

We found the following (red) curve separating naked singularities from those with event horizons. This plot is based on a numerical study.



Computed points (red circles) and an interpolating curve (solid red) separating the $r \to \infty$ asymptotic data for solutions with naked singularities from that for solutions with event horizons.

Acknowledgments

I would like to thank the organizers of this symposium for the opportunity to speak here. It was an honor to be able to remember Myron in this forum. This work was supported in part by NSF Award PHY-1214521 and by a University of Miami Cooper Fellowship.