My collaboration with Luciano

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Luciano Girardello Memorial, Milano, January 16 2023

Luciano has been a central figure in my scientific life.

He is the coauthor with whom I have most joint papers **(26)** including some of my best ones

He was especially important in the early stages of my career. He was the expert guy which guided me in my first steps in physics.

I think of him not only as a wonderful person, a dear and close friend, and an excellent colleague, but also as a kind of "scientific father" of mine

Apart for some occasional paper on some specific aspects of supersymmetry/supergravity, my joint work with Luciano (and common collaborators such as Massimo Porrati, Sergio Ferrara, and others) can be classified in **five** topics/general directions:

1. the theory of *dynamical* spontaneous supersymmetry breaking (Luciano always insisted on the adjective "dynamical" to stress that we were concerned with spontaneous breaking at the full quantum level, not just semiclassically);
2. the theory of the superHiggs effect in (extended) supergravity;
3. early applications of techniques/results from supergravity/supersymmetry to superstring theory;
4. geometry of the conformal manifold of 2d $(2, 2)$ SCFT and relations with special and quaternionic Kähler geometries (c-map, etc.);
5. the non-perturbative theory of 2d $(2, 2)$ Landau-Ginzburg QFTs

1 *Dynamical* spontaneous SUSY breaking

In the Fall of 1981 I arrived at the Physical Department of Harvard University as a "honorary" Post-Doc (without having a PhD, as was the general case in Italy at the time).

There I found a small group of Italian physicists: Loriano Bonora, Paolo Cotta-Ramusino, and Luciano Girardello. All three have been in Harvard for some time, and they had to return to Italy in a few months.

I was young (24) and quite disoriented, and naturally joined the group to get their help and try to survive in the foreigner contest.

Soon we start discussing physics. Loriano and Paolo were studying anomalies from a higher geometric/topological perspective.
At the time I did not know enough mathematics to be involved in that project (or even understand its beauty).

Luciano was thinking about a fundamental question which at the time was in its very early stages (he was one of the initiators of that story)

Question

Supersymmetric QFTs have a lot of desirable properties and useful dynamical mechanisms. However the real world **does not look** supersymmetric. How can we keep the “good” properties of supersymmetry while avoiding its “unrealistic” predictions?

A few months before Luciano had given a first answer to the **Question** in a seminal masterpiece written with Marcus Grisaru:

L. Girardello and M. Grisaru, *Soft Breaking of Supersymmetry*, Nucl. Phys. **B194** (1982) 65 [1006 citations to date]

Luciano had found **one answer** to the **Question**: **soft SUSY breaking**. However he did **not** stop there, and went on, looking for other physical mechanisms to address the **Question**

When I first met him, Luciano was working around the following idea: SUSY is broken at non-zero temperature by the trivial fact that the statistical mechanics of bosons and fermions are different.

So we can use temperature to split bosons from fermions. However this does not lead *per se* to the kind of "good" breaking Luciano was after.

His strategy was to improve the situation by adding chemical potentials for the conserved bosonic charges

Question

Can we add chemical potentials μ_a for the conserved bosonic charges Q_a so that the partition function of the SUSY QFT

$$Z(\mu_a, \beta) = \mathrm{Tr}_{\mathbf{H}}\left[e^{-\mu_a Q_a} e^{-\beta H}\right]$$

has "good" SUSY breaking in Luciano's sense (whatever that meant)?

We started playing together with the idea. We realized almost immediately that if you take as conserved charge the Fermi number $\boldsymbol{F}$ and analytically continue to the **complex value** of the fugacity $\boldsymbol{\mu = -i\pi}$ you get a (unphysical) partition function

$$\mathrm{Tr}_{\mathbf{H}}\Big[(-1)^F e^{-\beta H}\Big] \equiv Z(-i\pi, \beta)$$

which has very good SUSY properties.

In particular $\boldsymbol{Z(-i\pi, \beta)}$ is given by a path integral in periodic Euclidean time with fermions periodic along the time circle, so that SUSY is not broken by the periodic boundary condition

Except for being "unphysical" (complex chemical potential) $\boldsymbol{Z(-i\pi, \beta)}$ was a "good" thermodynamical function according to Luciano's standards

We studied it, and (obviously) realized that only the vacuum states contribute to this "good" function which is then independent of β and invariant under continuous deformations of all parameters (as long as the deformation preserves SUSY)

Having arrived to this conclusion, Luciano said:

> *"This invariance under continuous deformations reminds me of something Witten was saying to me a few months ago"*

(Witten had been in Harvard the previous year, and Luciano had talked a lot with him expecially about SUSY)

Luciano looked in his (rather disordered) drawers and eventually found a two-pages informal note by Witten sketching the idea of an "index" which controls spontaneous breaking of supersymmetry

Witten's index was the same "good" quantity we were considering.

We gave a name to the "good" quantity: **Witten index**

The name **"Witten index"** that Luciano and I introduced for $\mathrm{Tr}[(-1)^F e^{-\beta H}]$ is now classical (it was introduced by us some two years *before* Witten's groundbreaking paper)
Luciano was proud of this

At this point Luciano and I had a deontological problem.
The idea that an index controls spontaneous supersymmetry breaking was totally due to Witten, and we acknowledged this fact by giving his name to the new gadget.

However we had many results and alternative interpretations of $\mathrm{Tr}[(-1)^F e^{-\beta H}]$ which were not hinted in Witten's two-page note, so we felt appropriate to write a short note of our own, giving appropriate credits to Witten for the index, and stating our results.

All these results look now **"pretty obvious"** to everybody, as it happens to all results after they become "classical" in a subject. But back then they looked **"surprising"** and **"unexpected"**, and many distinguished colleagues simply did not believe they were true. **For them they were "pretty obviously *false*".**

Besides other observations, we had an interpretation of the Witten index as a **topological degree for stochastic maps.**

At this point Luciano had yet another of his flash-backs:

> *"Now that you say that, I remember that a few months ago Herman Nicolai was saying to me something of this sort"*

We looked for the Nicolai paper, read it, and then introduced another name which is now common: **Nicolai map**

Luciano solved the deontological problem by calling Witten in Princeton and saying to him: "We have written a short note. After completing it, we realized that there is a significant overlap with some of your ideas. We are sending the note to you by over-night mail. Read it, and say to us if you think that it contains enough new results to deserve publication". The answer was positive, and we published the paper

S. Cecotti and L. Girardello, *Functional Measure, Topology and Dynamical Supersymmetry Breaking*, Phys. Lett. **B110** (1982) 39

This early paper spread the idea of the Witten index in the community. We wrote 4 or 5 following up papers, mostly on the Nicolai map.

This paper gave a major boost to my career: SISSA offered me a position (which I accepted 2 years later when was upgraded to associate professor)

2 SuperHiggs effect

In 1983 I was "ricercatore" at the Physics Department of Pisa University.

Meanwhile Luciano had become full professor in Milano-la Statale

The two of us were still collaborating on SUSY breaking in QFT

In Pisa, at the Scuola Normale Superiore, there was a brilliant undergradued student, Massimo Porrati, who wanted to do a thesis in Supergravity. He asked Luciano to be his adviser.

Luciano asked around about the guy, and all professors in Pisa said wonders of Massimo. Therefore Luciano was were glad to accept him as one of his students.

However he said to Massimo that, given the distance from Milano to Pisa, and the fact that I was in Pisa, it would be simpler and more effective if the three of us worked together. So Luciano, Massimo, and I started collaborating.

As our project Luciano proposed the theory of the super-Higgs effect. Luciano had 6 fundational papers on superHiggs in **$\mathcal{N} = 1$ supergravity**

E. Cremmer, S. Ferrara, L. Girardello and A. Van Proeyen, *Yang-Mills Theories with Local Supersymmetry: Lagrangian, Transformation Laws and SuperHiggs Effect*, Nucl. Phys. **B212** (1983), 413 [1261 citations to date]

but a systematic theory was still lacking and at the time there were many open questions. To name only a few:

Question

1. In extended supergravity is it possible to have *partial* super-Higgs i.e. break local $\mathcal{N}$ SUSY to local $\mathcal{N}'$ SUSY, $(0 < \mathcal{N}' < \mathcal{N})$ *while keeping the cosmological constant to zero?*
2. More generally, can we have a (large) hierarchy of SUSY breaking scales without generating a large Λ?
3. What about super-Higgs in AdS (both complete and partial)?

In rigid supersymmetry the answer to the first two questions would be **NO**

The three of us wrote 3 or 4 papers on the subject.

The most important result (in my opinion) was the **general Ward identity** relating the scalar potential to the "fermionic shifts" which holds in full generality (even in presence of higher derivative/curvature couplings) and relates in a simple manner the SUSY breaking parameters of the various SUSY sub-algebras (which set the scales of the different breakings), the masses of the several gravitini, and the cosmological constant.

We deduced it in a model independent way from the positive energy theorem in General Relativity proven by Witten a few years earlier.

The most important consequence of these works was that Massimo and I were now accepted as *properly initiated adepts* of the *secretive supergravity sect*, and could start working with Sergio Ferrara (a long-time collaborator of Luciano), and the group of people orbitating around him, on various aspects of Supergravity.

3 Early applications of SUGRA in string theory

We were studying various questions in SUGRA when in 1984 Theoretical Physics underwent a phase transition of 1^{st} order: Green and Schwarz wrote their paper on the anomaly cancellation in open superstring theory, and in a few weeks Witten and the group in Princeton started to flood the world with countless preprints full of exciting magical results in superstring theory

The string revolution had blown out!

People outside Princeton were taken a bit "in contropiede"

However **super**string theory was after all a supersymmetric theory, in facts contained supergravity as a sector.

So people, like us, with a strong SUGRA background, could apply their hardly-learned techniques to problems in superstring theory and to problems in supergravity *suggested* by string theory

A typical question suggested by the string was to construct higher derivative/higher curvature couplings in supergravity (e.g. the SUSY version of the Born-Infeld Lagrangian). This was the topic of a number of papers we wrote at the time

In that period we (Luciano, Massimo, Sergio F., I, and occasionally others) wrote quite a number of papers, most of them about *technical aspects* of SUSY/SUGRA which were purportedly relevant for this or that problem arising from (or motivated by) the string

Despite of being technical, they still are of interest nowadays, and a significant number of them are well cited (4? papers $>$ 100 citations)

According to the INSPIRE count the most important paper of that period, and the first one to be written (if I remember correctly) was

S. Cecotti, J.P. Derendinger, S. Ferrara, L. Girardello, M. Roncadelli, *Properties of E6 Breaking and Superstring Theory,* Phys. Lett. **B156** (1985) 318

written soon after Candelas *et al* published their paper on Calabi-Yau's as SUSY vacua for the $E_8 \times E_8$ heterotic string, leading to a 4d effective theory with gauge group E_6 in the visible sector

I must confess that my contribution to this particular paper was $O(\epsilon)$

4 Moduli spaces of SCFT, SUGRA & special geometry

At the end of that period we wrote the paper which I consider the **best one** Luciano and I ever wrote together (with Sergio Ferrara, in this case). The INSPIRE count confirms my impression:

S. Cecotti, S. Ferrara and L. Girardello, *Geometry of Type II Superstrings and the Moduli of Superconformal Field Theories,* Int. J. Mod. Phys. **A4** (1989), 2475 [452 citations to date]

(there is also a companion short note)

Seiberg (and also Gepner) had written papers on the Zamolodchikov geometry of the conformal moduli space of 2d (4,4) SCFT with $\hat{c} = 2$ by using the field geometry of 4d $\mathcal{N} = 4$ SUGRA and Differential Geometry

It was very beautiful work, but also a kind of baby case.
The real story was about the conformal geometry of 2d (2,2) SCFT with $\hat{c} = 3$, corresponding (in the vector multiplet sector) to the Weil-Petersson geometry of the moduli of Calabi-Yau 3-folds.

To address the question one had to use 4d $\mathcal{N} = 2$ SUGRA instead of the $\mathcal{N} = 4$ one, which is a much harder story because the $\mathcal{N} = 4$ Lagrangian is essentially unique, whereas the $\mathcal{N} = 2$ is not. Besides in the $\mathcal{N} = 2$ case we have two kinds of matter supermultiplets (vector multiplets and hypermultiplets) with two quite different target geometries (special geometry vs. quaternionic Kähler geometry)

The expertise in supergravity required to tackle the problem was high, but that **was** precisely our field of expertise, so we attached the problem with confidence

We had two viewpoints at our disposal: the world-sheet theory and the target space physics. We went back and forth between the two, comparing phenomena in the two set-ups, using symmetry, geometry, and consistency conditions.

In particular we realized that vector-multiplets and hypermultiplets, which look so different from the SUGRA perspective, should be related by a duality (later named **mirror symmetry**) which (roughly speaking) interchanges the two

This led us to the ***c*-map**, an explicit transformation which maps a special geometry (of $\mathcal{N} = 2$ SUGRA or $\mathcal{N} = 2$ SUSY) to a quaternionic Kähler geometry (hyperKähler in rigid SUSY)

The identification of the four geometries:

1. the Zamolodchikov geometry of the world-sheet (2,2) SCFT;
2. the Weil-Petersson geometry on the moduli space of 3-CY
3. the Griffiths-Bryant geometry of the underlying VHS;
4. the special Kähler geometry of 4d $\mathcal{N} = 2$ SUGRA

gave insights on all 4 subjects, and helped clarify the structure of $\mathcal{N} = 2$ SUGRA (correcting some previous misconceptions)

5 Non-perturbative 2d (2,2) Landau-Ginzburg QFTs

Meanwhile I became associate professor at SISSA (Trieste)

Luciano had an informal agreement with SISSA. He will send us his best graduates in Milan to get their PhD from SISSA, with the understanding that he would continue to be their PhD advisor.

Luciano had in mind to replicate the "Porrati mechanism" that had worked so well in Pisa. That is, Luciano, the student, and I working together on a project which would eventually produce more than enough material for the poor guy's thesis.

The weak point in the scheme is that you need a "Porrati" to make the "Porrati mechanism" work

The problem which kept busy Luciano, Andrea Pasquinucci (the student), and I was in line with the previous story about the identification/comparison of the **four** geometries arising from a Calabi-Yau 3-fold.

Our previous work inferred **indirectly** the Zamolodchikov geometry on the conformal manifold of the 2d SCFT using symmetries, consistency conditions, and deep theorems of Differential Geometry, but was **NOT** a **direct computation** of the Zamolodchikov metric from the first principles of 2d Quantum Field Theory

A direct computation (say, using the 2d path integral) was highly desirable from many points of view

Around that time Warner & Vafa, and independently Martinec, introduced a nice and simple description of (certain) 2d (2,2) SCFT in terms of Landau-Ginzburg models

Example/Claim

You want to compute the Zamolodchikov metric on the (complex) moduli space of (say) the quintic hypersurface in $\mathbb{P}^4$?
Promote the homogeneous coordinates $(X_0 : X_1 : X_2 : X_3 : X_4)$ of $\mathbb{P}^4$ to 2d (2,2) chiral superfields and consider the Landau-Ginzurg with superpotential

$$W(X_i) = \sum_{i=0}^{4} X_i^5 + \overbrace{\cdots\cdots\cdots\cdots}^{\text{101 monomials of degree 5}}$$

equal to the homogeneous equation of the hypersurface. The **claim** is that the Zamolodchikov metric in the 101-dimensional space of the complex parameters entering in W is identical to the Zamolodchikov metric of the σ-model with target the hypersurface, which in turn is the Weil-Petersson metric on the complex moduli space of the quintic 3-fold

The LG description gave a concrete (and easy enough!) QFT set-up to compute the metric on the conformal manifold, and check the **claim** that all **FIVE** metrics (the 4 mentioned before plus the LG one) are **equal**

We wrote 3 or 4 papers showing – by direct computation in the LG QFT – that the **claim** was indeed correct

In the last one of these papers we wrote a set of partial differential equations which characterize the Zamolodchikov metric in the LG SCFT ($\equiv$ LG with quasi-homogeneous superpotentials)

These equations are the ones we now call the **“tt^* equations”**

However while the equations are the same ones, the context in those early papers (too advanced for their time) was much more limited.

First: they were meant to describe the Zamolodchikov metric on the conformal manifold of a very special class of 2d (2,2) SCFTs.
The equations were “proven” using the foundations given by Zamolodchikov using conformal symmetry.
Second: there was no attempt to solve them. Our strategy was:

> *We show that the LG Zamolodchikov metric and the CY Weil-Petersson metric satisfy the same differential equations, with the same boundary conditions, then we have proven that they are equal by uniqueness of the solution*

A few weeks later Cumrun Vafa asked me how one would compute the geometry of the deformation space of a 2d $(2,2)$ QFT **in general**, that is, varying all sort of couplings, not just the marginal ones, in a theory which now is merely $(2,2)$ supersymmetric (non conformal) and without assuming a particular Lagrangian formulation (such as LG)

– *Elementary* – I said – *The deformation geometry is determined by the differential equations I wrote with Luciano and Andrea*

– *How can you say that? You used conformal symmetry in those papers.*

– *I say it because it is just true.*

– *Ok* – Cumrun replied – *Let us see if you are right. Apply these equations to the LG model with superpotential $W(X) = X^3 + tX$ and compute its geometry in the t-plane. Let us see what we get.*

When he saw the result, he immediately realized it was the exact non-perturbative answer. Now the problem was to construct a set-up, with the appropriate foundations, to make the obviously correct equations meaningful and "prove" them. We could not rely on Zamolodchikov work any longer, because no conformal symmetry was present

Cumrun and I used a new gadged that Witten had recently introduced: **topological field theory**. *TFT* (and its conjugate TFT^*) gave us the framework to give a precise meaning to the equations in the general context, without any *ad hoc* assumption besides $(2,2)$ SUSY, and we got a fully rigorous proof of them

Then we proceeded to "milk the tt^* equations" (Cumrun's language) for the physical lessons which we could learn from them

We got a τ function, which was instrumental for the first **BCOV** paper, which in its turn led to the second **BCOV** paper on Kodaira-Spencer gravity

The way I see it in retrospect, my personal contributions to those fundamental papers was largely a scientific legacy of my fruitful collaboration with Luciano: together we developed the required techniques and even wrote the correct equations (albeit we did not know **why** they were correct nor **when** they were correct)

Thank you for your attention

Special thanks to the organizers for giving me the opportunity to pay the due tribute to a dear friend which played so a prominent role in my life