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Theory and Applications in Mathematical Physics

Conference in Honor of B. Tirozzi's 70th Birthday

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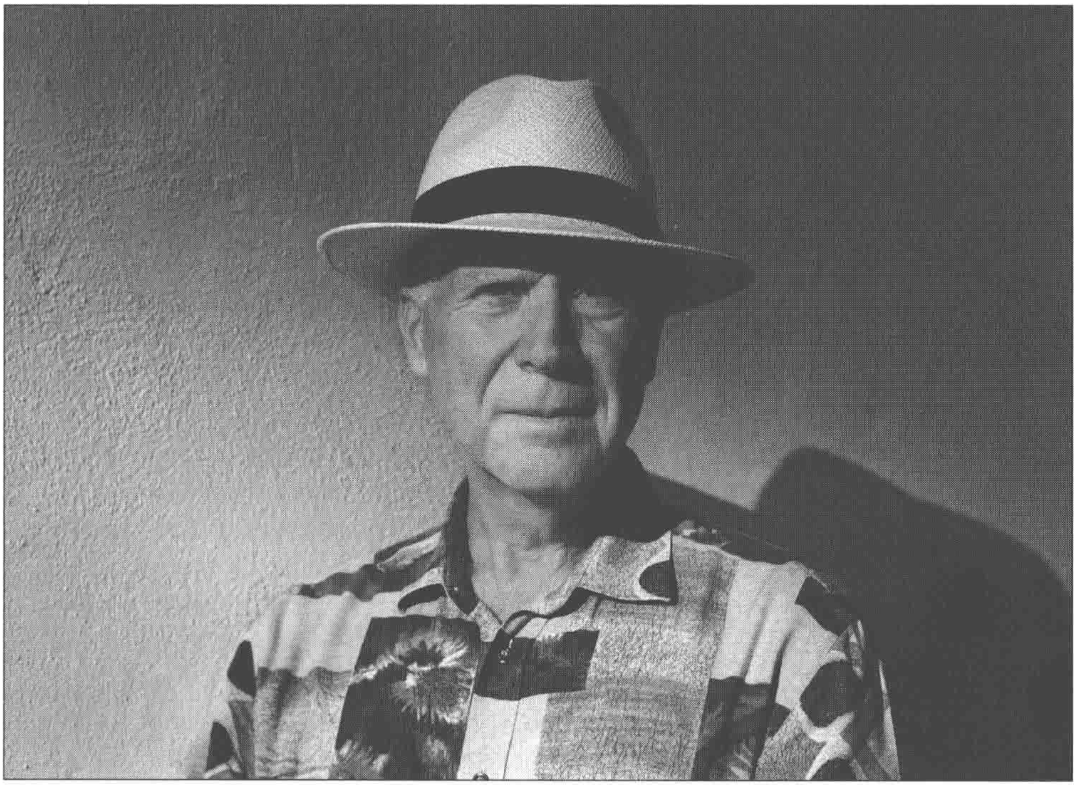

B. Tirozzi

(*Photo by Dino Ignani*)

Preface

The conference for my 70^{th} birthday has been very amusing for me. I had the possibility to meet very nice colleagues with whom I shared a nice time of my life, both for the research we have done and for the company. Also for them it was an occasion to meet each other. I have to say that the enjoyment had always been an important part in my life. When I speak about enjoyment I mean to have fun making research about some interesting topics in physics or mathematics or both. I have always found that physics is full of very attracting topics and that treating them with some good mathematical formalism was even more exciting. Thus my scientific career has always been based on the search of the arguments that at that moment looked to me interesting and attracting. So I did not work all the time on the same topic, many young researchers remain stuck on the same argument all the time for being sure that they will produce a lot of papers. I think that this is not a good choice.

I started my activity at the Department of Physics at the University "La Sapienza" in 1968. It was a very hot time. There was an intense activity of research of the theoretical group in many directions and there was an intense political activity at the same time. I was among the young people who participated to all the rallies, meetings, occupations but I took part also in the research activity which was also very extraordinary. I took the degree on physics with Marcello Cini as supervisor and we worked on the electromagnetic mass of the elementary particles, application of group theory to particle physics. The coworkers where Franco Bucella, who continued in this direction and Mimmo De Maria who decided later to dedicate himself to the history of physics. But at the same time there were lot of seminars on strong interaction among elementary particles, Regge poles, analytic structure of scattering amplitudes, perturbation theory and so on. Just to mention the main persons I cite Nicola Cabibbo, Luciano Majani, Francesco Calogero, Tony de Gasperis, Gianni Jona Lasinio, Giuliano Preparata, Carlo Di Castro. But the methods applied by these brilliant theoretical physicists were not so satisfactory for me because theoretical physics often hides the hypothesis and the assumptions at the base of their theories, or make some approximations that are not clearly written. So I got interested in the rigorous derivations made by Gianni Jona and Francesco Calogero. But my first approach to rigorous mathematical physics started with the group founded at that time by Giovanni Gallavotti.

A lot of young people participated to his seminars and started to develop interesting applications of probability theory to statistical mechanics, quantum systems, dynamic of infinite systems of particles. At that time I produced very good papers in collaboration with Errico Presutti, Mario Pulvirenti and others. So we discovered that the right formulation of statistical physics can be done using probability theory. On those years, around the '70, very important and nice publications appeared on Russian journals, and being translated in English. I am speaking about the papers by Yasha Sinai, Roland Dobrushin, Bob Minlos. But at that time there was also the cold war, the iron curtain, no internet and so we received these important papers one or two years later. These scientists were masters of applications of probability theory to phase transitions since they were all coming from the school of Kolmogorov. I wanted also to see the life in Soviet Union and to understand how they constructed the communist society. With these two strong curiosities, I make the big move to go to Soviet Union for one year using the exchange program among CNR and the Academy of Science. I got a very warm welcome by the group of Sinai, Dobrushin and Minlos. Of course I had to learn Russian since their talks were mainly in Russian. The discussion at their seminars were even more intense than in our seminars. I remember that it was not possible to speak more than 3 minutes without being interrupted by a lot of questions. Since all the big scientists were not allowed to move due to the iron curtain, Moscow was full of very good seminars held by other well known people. I quote the Gelfand's seminar, Katok's seminar and many others. So I learned Russian and probability theory at the same time. The life in Soviet Union was much less exciting than the scientific activity I must say. When I came back I brought some techniques that were useful for all the group. After that year I often went to work with this exciting group in Moscow. In one of these contacts I got acquainted with some rigorous result obtained by Sinai and Khanin about the spin-glass. This result was used in a very good paper I have written with Enzo Olivieri and Marzio Cassandro. I have also generalized a nice result about periodic orbits by Sinai and Vul and this was my first paper where I used the computer. In the '82 I also explored other ways of doing research, I always remained in contact with the Russian group but I was curious to see how mathematical physics was done in other part of the world. So I decided to ask the hospitality of Joel Lebowitz at Rutgers University who had a big group of statistical physics. I had the experience to teach a course on differential equations there.

In the second part of that year I went to visit IHES (the Institute of Hautes Etudes Scientifiques) in Paris. Of course the atmosphere in Paris was very exciting and the scientific activity was at a very high level. The director was in fact David Ruelle. At this institute I started a very interesting collaboration with Kristoff Gawedzky and Antti Kupiainen with whom we published very good papers on renormalization group. On that time I was commuting between IHES and the Mathematical Department in Rome. In that time string theory came up very strongly and in fact there were seminars everywhere and also at IHES and in Rome. I took part in an interesting seminar about string theory held by Claudio Procesi, De Concini, Enrico Arbarello and the physicists Massimo Testa and Giancarlo Rossi. But somehow, even if I have published a paper with De Concini and Fucito I was not satisfied with this theory because there was no experimental counterpart. That is why I again changed topic and came back to statistical physics but at this time I was fascinated by the Hopfield model which incorporated the property of memory, associative memory, in the statistical physics landscape. The model also describes in a simple way the properties of the neurons. I created a new course at the Math. Department named Reti Neurali (Neural Networks) and many students attended my lectures. So I got a big number of enthusiastic young disciples. Some of these remained at the University or in the research. Our relationships were always very constructive and we helped each other very much. I would like to mention the following: Enrico Ferraro, Enrico Rossoni, Silvia Puca, Sara Morucci, Daniela Bianchi, Giulia Rotundo, Gabriele Stabile, Marco Piersanti. With them we had experience of different applications of neural networks. With Enrico Ferraro and Enrico Rossoni we modeled the behavior of the oxytocin neurons, with Giulia Rotundo and Gabriele Stabile we made different models of markets and economy, with Silvia Puca, Sara Morucci and Stefano Pittalis we applied neural networks to the wave motions and tide motions of the sea. The oxytocin problem was coming from a European collaboration organized by Jianfeng Feng about neurobiology. Daniela Bianchi was a very precious coworker for studying biology and for the study of the network of the neurons of the hyppocampus made with the NEURON program, very difficult to manage, especially if trying to use it on parallel computers. Marco Piersanti was also another very good student of mine who entered in this research.

The problem of the motion of the Italian seas was coming from a contract of a government agency. I met Janfeng Feng in Beijing University where he invited me for one month. I was also very interested to visit Beijing. We started a very nice collaboration about neural networks which went on for many years with a rich production of papers. In those years I went also many times to Bochum, Bielefeld, Bonn where I worked with Sergio Albeverio and his group getting very interesting results. But I had also the fortune to be contacted on those times by Masha Shcherbina and Leonid Pastur of Kharkov Ukraine. They are leaders in the application of the probability theory to the Hopfield model. We arrived to solve rigorously the Hopfield model without using the non-rigorous replica trick used by Amit, Gutfreund and Sompolinsky for solving the model. We also continued with many other interesting rigorous results. I found a very good fellow in the Math. Department and after in the Physics Department who shared with me the desire of solving the disordered models with rigorous probability theory. I am speaking about Francesco Guerra who has shown very important results with rigorous methods about the Parisi's solution of the replica symmetry breaking of spin glass theory. Our researches were going on in parallel and it was encouraging for me to talk with some person about this approach because nobody had the courage, at that time, to go through such problems. For two reasons, the first was that it was very difficult to find rigorous proofs, the second was that the physicists solving their models with the replica theory were doing opposition against our approaches. Francesco has helped me a lot in this struggle and also helped me on the practical level. At the end both of us won this battle and got the prizes we were looking for.

In Italy I became a member of CINFAI, an Interuniversity Consortium for studying the Physics of Atmospheres and Hydrospheres. I was interested to study this topic. Sergio Albeverio organized a meeting for me with Sergey Dobrokhotov of Moscow. He is applying the asymptotic methods developed by P. Maslov to the problem of propagation of typhoons and tsunami. We started a very fruitful collaboration on these arguments with very original results. This collaboration still goes on, fortunately and we also succeed to use asymptotic methods for the problem of Plasma in the Tokamak. By the law of the Italian government I am obliged to retire in 2014. But again my collaboration with Russian groups was lucky for me because the Plasma Laboratory of Frascati showed interest in these new methods and I got the hospitality at their center. So at the end of all my going around in the world and in the science I am still in the active research. I think that this is a consequence of my tendency to follow only the things that looked exciting to me.

I retire from the University to start another life in the research. I am glad to see here all my best companions of my long life in the science. I am also very happy to have here many of my best disciples and students. Further, above all, I have to thank one million times Adriano Barra and Elena Agliari who organized the heavy work of this lucky conference. There is also Biancamaria with whom we shared a lucky life and other relatives. So I have just to say goodbye, the story is not finished but continues with some changes.

Thanks to all.

B. Tirozzi

Contents

Chapter 1

Motifs stability in hierarchical modular networks

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Recent advances in our understanding of information processing in biological systems have highlighted the importance of modularity in the underlying networks (ranging from metabolic to neural networks), as well as the crucial existence of *motifs*, namely small circuits (not necessary loopy) whose empirical presence in these networks is statistically high. In these notes, mixing statistical mechanical with graph theoretical perspectives and restricting on hierarchical modular networks, we analyze the stability of key motifs that naturally emerge and we prove that all the loopy structures have systematically a broader steadiness with respect to loop-free motifs.

Introduction

Network theory, coupled with Statistical Mechanics, is becoming a crucial tool for investigating Biological Complexity and, following this approach, crucial questions, ranging from intra-cellular investigations (as for instance in metabolic or protein networks[1,12,16,17]), to extra-cellular ones (as for instance in neural networks[4,9,20,22]) have already been satisfactorily addressed.

According to empirical evidence, biological networks typically exhibit scale-free and/or hierarchical topologies[18,23,24] with a high number (with respect to a random reference) of "motifs", namely recurrent and strongly-connected sub-graphs or patterns.[19] Further, the interaction strength (e.g., based on lock-and-key mechanisms[4]) between the elements making up the network usually varies over several orders of magnitude, in accordance with a log-normal or power-law distribution for link's magnitudes. This has stimulated a renewed interest for the Dyson model:[14] indeed the latter, originally developed as a model to overcome mean-field limitations in the statistical mechanical description of ferromagnetism, is exactly a hierarchical network, where spins are pasted on its nodes and their couplings follow a power-law distribution.[8] In this abstract model spins may play as neurons (thus dealing with a hierarchical neural networks[3]) and one is interested in controlling a clique of firing neurons in a sea of quiescent ones, or may be thought of as lymphocytes (i.e. clones of B or T cells), thus dealing with hierarchical immune networks,[9] and one may question the activation of a cluster of clones while all the others are silenced and so on.

In these notes we aim to analyze the meta-stability of key motifs highly occurring in the Dyson model, as their existence has been found only recently.[2,3] In particular, we consider the *dimer*, i.e., the prototype of a loop-less reticular animal, and the *square*, i.e., the prototype of a loopy reticular animal, and we check whether magnetic configurations where spins associated to these motifs are misaligned with respect to the bulk -but aligned among themselves- are stable. Not surprisingly, while the former is found to be always unstable (i.e. there is no value of the tuneable parameters defining the model that allows its stability), the latter has a range of stability. It is worth noting, however, that -as these motifs are by definition not-extensive (i.e. their sizes do not scale with the system size)- nor they contribute to the model's free energy in the thermodynamic limit, neither they are expected to be stable whenever a finite-amount of fast noise is applied on the system.

As a last remark, we note that the Dyson model has a power law distribution for the link's magnitude[8] as well as a modular architecture of the graph hosting the spins:[3] remarkably, the reason for the stability of its loopy motifs lies exactly in these intrinsic features of the model, that -in turn- play a major role even in real biological networks,[15] exactly those where the presence of motifs is expected.[18,19]

1. Definition of the model

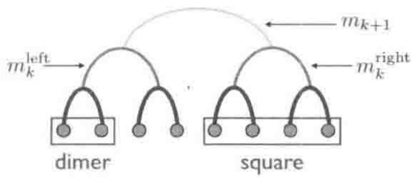

Fig. 1. Schematic representation of the hierarchical network defined by the Dyson model of size 4. Each node hosts an Ising spin and interactions among nodes are stronger (here denoted by thicker links) for closer nodes as depicted in the Hamiltonian (eq.(1)) defining the model.

The aim of this section is to give a microscopical description of the Dyson Hierarchical Model (DHM), which is composed by 2^{k+1} Ising spins S_i, for $i = 1, \cdots, 2^{k+1}$, that are embedded in a hierarchical topology. The model is represented by the Hamiltonian introduced in the following definition:

Definition 1. The Hamiltonian of Dyson's Hierarchical Model (DHM) is defined by

$$H_{k+1}(\vec{S}|J,\sigma) = H_k(\vec{S_1}) + H_k(\vec{S_2}) - \frac{J}{2^{2\sigma(k+1)}} \sum_{i<j=1}^{2^{k+1}} S_i S_j, \qquad (1)$$

where $J > 0$ and $\sigma \in (1/2, 1]$ are numbers tuning the interaction strength. Clearly $\vec{S_1} \equiv \{S_i\}_{1 \le i \le 2^k}$, $\vec{S_2} \equiv \{S_j\}_{2^k+1 \le j \le 2^{k+1}}$ and $H_0[S] = 0$.

Thus, in this model, σ triggers the decay of the interaction with the distance among spins, while J uniformly rules the overall intensity of the couplings. Note further that the coupling distribution $P(J)$ is scale free as it follows the power-law relation:[8] $P(J) \propto J^{-\frac{1}{2\sigma}}$. We can introduce the partition function $Z_{k+1}(\beta, J, \sigma)$ at finite volume $k+1$ as

$$Z_{k+1}(\beta, J, \sigma) = \sum_{\sigma}^{2^{k+1}} \exp\left[-\beta H_{k+1}(\vec{S}|J,\sigma)\right], \qquad (2)$$

and the related free energy $f_{k+1}(\beta, J, \sigma)$, namely the intensive logarithm of the partition function, as

$$f_{k+1}(\beta, J, \sigma) = \frac{1}{2^{k+1}} \log \sum_{\vec{S}} \exp \left[-\beta H_{k+1}(\vec{S}) + h \sum_{i=1}^{2^{k+1}} S_i \right]. \qquad (3)$$

We introduce also the global magnetization $m = \lim_{k\to\infty} m_{k+1}$ where

$$m_{k+1} = \frac{1}{2^{k+1}} \sum_{i=1}^{2^{k+1}} S_i, \qquad (4)$$

that can be defined recursively, level by level (see Figure 1). Finally, we denote the thermodynamical average as

$$\langle m_{k+1}(\beta, J, \sigma) \rangle = \frac{\sum_{\vec{S}} m_{k+1} e^{-\beta H_{k+1}(\vec{S}|J,\sigma)}}{Z_{k+1}(\beta, J, \sigma)}, \qquad (5)$$

and $\lim_{k\to\infty} \langle m_{k+1}(\beta, J, \sigma) \rangle = \langle m(\beta, J, \sigma) \rangle$.

We are interested in understanding the conditions to be applied on σ such that different configurations remain stable in noiseless conditions. We will start with some simple cases, and we will try to apply the results to a general structure composed by 2^n elements, with $n < k + 1$.

2. Loop-less case: Stability analysis of the dimer

The goal of this section is to study the existence of possible values of σ such that the dimer (i.e. a spin-configuration where $S_i = +1$ for $i = 1, 2$ and $S_j = -1$ for $j = 3, \cdots, k + 1$) remains stable, clearly in the noiseless limit. To reach our conclusions, we adapt to the case an interpolative strategy -firstly developed in[13]- that has been recently applied to hierarchical networks:[2]

Definition 2. Once considered a real scalar parameter $t \in [0, 1]$, we introduce the following interpolating Hamiltonian

$$H_{k+1,t}(\vec{S}) = -\frac{Jt}{2^{2\sigma(k+1)}} \sum_{i>j=1}^{2^{k+1}} S_i S_j - \frac{Jm(1-t)}{2^{(2\sigma-1)(k+1)}} \sum_{i=1}^{2^{k+1}} S_i + +H_k(\vec{S_1}) + H_k(\vec{S_2}), \qquad (6)$$

such that for $t = 1$ the original system is recovered, while at $t = 0$ the two-body interaction is replaced by an effective, tractable one-body term. The possible presence of an external magnetic field can be accounted simply by adding to the Hamiltonian a term $\propto h \sum_i^{2^{k+1}} \sigma_i$, with $h \in \mathbb{R}$.

This prescription allows defining an extended partition function as

$$Z_{k+1,t}(h,\beta,J,\sigma)=\sum_{\vec{S}}\exp\{-\beta[H_{k+1,t}(\vec{S})+h\sum_{i=1}^{2^{k+1}}S_i]\}, \tag{7}$$

where the subscript t stresses its interpolative nature, and, analogously,

$$\Phi_{k+1,t}(h,\beta,J,\sigma)=\frac{1}{2^{k+1}}\log Z_{k+1,t}(h,\beta,J,\sigma). \tag{8}$$

It is easy to show that

$$\Phi_{k+1,0}(h,\beta,J,\sigma)=\Phi_{k,1}(h+mJ2^{(k+1)(1-2\sigma)},\beta,J,\sigma), \tag{9}$$

so that we can write

$$\Phi_{k+1,1}(h,\beta,J,\sigma)=\Phi_{k+1,0}(h,\beta,J,\sigma)+\int_0^1\frac{d\Phi}{dt}dt\Longrightarrow$$
$$\Phi_{k+1,1}(h,\beta,J,\sigma)=\Phi_{k,1}(h+mJ2^{(k+1)(1-2\sigma)},\beta,J,\sigma)+\int_0^1\frac{d\Phi}{dt}dt. \tag{10}$$

Using the identity (10), with the appropriate computations, we obtain

$$\begin{aligned}\Phi_{k+1,1}(h)&=\Phi_{k+1,0}(h)-\frac{\beta J}{2}(2^{(k+1)(1-2\sigma)}m^2+2^{-2(k+1)\sigma})\\&+\frac{\beta J}{2}2^{(k+1)(1-2\sigma)}\left\langle(m_{k+1}(\vec{S})-m)^2\right\rangle_t\\&\geq\Phi_{k,1}(h+Jm2^{(k+1)(1-2\sigma)})-\frac{\beta J}{2}(2^{(k+1)(1-2\sigma)}m^2\\&+2^{-2(k+1)\sigma}).\end{aligned} \tag{11}$$

We already know that we can study non-standard stabilities where the system undergoes the influence of two different contributions m_1 and m_2, with the same absolute value, but opposite in sign.[2] Now we want to analyze the case in which the subsystems have different cardinality: in this case, the first one is constituted only by two spins, and the other one is constituted by all the others; we can write the two sub-magnetizations m_1 and m_2, more rigorously, as:

$$m_1=\frac{2}{2^{k+1}}\sum_{i=1}^{2}S_i,\qquad m_2=\frac{2^{k+1}-2}{2^{k+1}}\sum_{i=3}^{2^{k+1}}S_i, \tag{12}$$

where m_1 has an opposite sign with respect to m_2. This means that the system preserves the same general magnetization m up to the last level, where one has 2^k blocks formed by 2 spins: at this point, one of the blocks, say the one formed by S_1 and S_2, is not affected by the rest of the system,