MY ADVENTURES WITH MAREK EVENT-BY-EVENT[*]

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On the occasion of the 60th birthday of Marek Gaździcki, I present some personal recollections and briefly discuss a few research projects we worked together.

1. Prologue

This year my friend and collaborator Marek Gaździcki turns 60. Among his numerous achievements and contributions, Marek also initiated *Critical Point and Onset of Deconfinement* as series of regular meetings. So, CPOD 2016 is the right place and time to say a few words about Marek, our friendship and a long-term collaboration which has always been interesting and stimulating, and at a few instances appeared even successful.

2. The first events

I met Marek for the first time in autumn 1975 when we became students of the Faculty of Physics of the University of Warsaw. At the beginning, our relations were rather loose but things changed at the fourth year of our studies when we two had to perform a laboratory exercise. At that time, there was operating in Warsaw the Van de Graaff accelerator 'Lech' which accelerated protons, deuterons and helium 3 and 4 up to the energy of about 3 MeV. We were supposed to measure a cross section of elastic scattering. I do not remember what was the projectile and target but I do remember very well what was the idea behind the exercise. The measurements were to be done at two collision energies. At the lower one, we were supposed to observe the pure Rutherford cross section, while at the higher collision energy, which was close to the Coulomb barrier of colliding nuclei, a deviation was to be

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seen. After performing the measurements and analyzing the collected data, we found the expected results. It took us some time and effort but the experience was very positive and played a role in our choice of a subject of diploma thesis. We both decided to work with collisions of relativistic ions — the field of research which was just born at that time. The Bevelac and Synchrophasotron programs at Berkley and Dubna, respectively, just started.

Marek joint the Dubna group SKM-200, which was using the streamer chamber as a main detector, and begun analyzing data. I could not decide whether I should be an experimentalist or theorist. I tried both and, in particular, I did some calculations for the SKM-200 experiment which were included in the publication [1]. This was my very first research paper and Marek was obviously a coauthor.

Some time after receiving our diplomas, we both appeared in the Joint Institute for Nuclear Research in Dubna. Marek continued his work with the SKM-200 experiment and I was a member of another experimental group. We were spending a lot of time together and in Russia at the Volga river our relations were really tightened. In Dubna, we were also shaped as physicists and I finally decided to be a theorist. Because none of us had a real boss, we became independent and self-reliant which appeared very useful in our future carriers.

Some time after our returns from Dubna to Warsaw, we moved to Germany. Marek worked for the NA35 experiment in Heidelberg and then in Darmstadt and I was a postdoc with Uli Heinz in Regensburg. We could not meet on a daily basis but we still managed to publish something together. The CERN SPS program of relativistic-ion collisions was in a full swing and we were lucky to take part in it.

At the beginning of the 1990s, we were both again in Warsaw. The communist system collapsed and political life was very turbulent in Poland. We mostly discussed politics but physics was also an important subject of our conversations. At that time, we realized that fluctuations of various physical observables can be an important source of dynamical information on relativistic heavy-ion collisions. The question was how to disentangle interesting fluctuations from boring ones.

3. Φ measure of event-by-event fluctuations

When nuclei collide, the impact parameter varies and so does the number of participants. This is typically the main source of fluctuations in relativistic heavy-ion collisions. We repeatedly discussed about a possibility to eliminate this trivial source of fluctuations by means of a statistical analysis and we formulated a well-defined problem to be solved.

Imagine a single source of particles. A value of variable x — we usually thought about the particle's transverse momentum — is assigned to every particle. The process of emission of N particles with x_1, x_2, ... x_N is characterized by the probability distribution of $P_N(x_1, x_2, \dots x_N)$. We are interested in the variance of the variable $X \equiv \sum_{i=1}^{N} x_i$. And now imagine k sources which are identical and independent from each other. The k source distribution is a superposition of k single source distributions with the number of sources k given by the distribution p_k. The question we posed is the following: is it possible to reconstruct the variance of X of the single source distribution knowing the first two moments of the k source distribution with k being a random variable.

I doubted whether the question had a positive answer but Marek guided by his great intuition tried to find a solution performing various numerical experiments. After some time, it seemed he found a solution and my duty was to prove analytically that the solution was right. It took me some efforts but, finally, we managed to define the fluctuation measure [2] which later on was called Φ. It gives exactly the same value for the single and k source distributions independently of the form of p_k. Φ is also normalized in such a way that $\Phi = 0$ in an absence of correlations. Those who got interested in the problem I sent to my review article [3] where properties of Φ are explained and discussed in detail.

The event-by-event fluctuations kept us busy for several years. I considered various models of fluctuations and Marek was mostly pushing the experimental studies which, to my disappointment, have not provided any spectacular result. The observed fluctuations are dominated by a statistical noise and dynamical effects are rather small. I gave up the event-by-event fluctuations but Marek is more persistent. He worked hard to improve methods of experimental analysis. He was also looking for fluctuation measures better than Φ. With Mark Gorenstein, they introduced a whole set of measures which, as Φ, are blind to fluctuations of number of particle sources [4]. Referring to the language of thermodynamics, where the quantities independent of system's volume are known as *intensive*, they called the measures *strongly intensive* as these quantities are independent not only of the system's volume but of the fluctuations of the volume as well.

The NA61/SHINE experiment, that Marek is currently a spokesperson, is well-equipped to measure the event-by-event fluctuations which are still believed to be an important signal of phase transitions and critical points in relativistic heavy-ion collisions. Hopefully, the improved experimental capability and better methods of statistical analysis will ultimately allow one to observe an interesting fluctuation signal. Marek's persistence will then pay off.

4. Physics, philosophy and quarrels

In 1992, Marek moved to Frankfurt am Main for good, I stayed in Poland and thus the parallelism of lives ended. However, we still collaborate in various ways and often discuss physics. Many times we started doing something together but there are not many projects we managed to complete. It appears we have developed rather different tastes in physics. With time passing I became rather skeptical or even conservative but Marek remains rebellious. Our disagreements often start already when we try to formulate a problem: Marek says 'Let us assume that ...' and I oppose 'The assumption makes no sense'. Our discussions are usually hot and sometimes are changed into quarrels. It happened twice — once in Spain and once in Austria — that traveling by train we missed the right station because of a very vigorous debate.

Even so my father was a philosopher and I am rather accustomed to philosophical speculations, this is Marek who often invokes philosophical arguments into our conversations about physics. As a follower of Karl Popper, he likes to claim that a given theoretical model is unfalsifiable and thus non-scientific because the set of model assumptions and parameters can be extended. On other occasions, he refers to Thomas Kuhn and defends an idea, which I find as unjustified, by saying that the idea requires a 'paradigm shifts'. I am not sure about usefulness of our debates but they are certainly interesting and stimulating, and I got an evident profit — Marek gifted me *The Structure of Scientific Revolutions* — the Khun's classical book.

The subject we have frequently discussed over the years is the thermodynamical model of high-energy collisions and, in particular, the meaning of thermodynamical equilibrium in this context. In line with my conservative attitude, I insist that the equilibrium results from secondary interactions of particles produced in the collisions, as in many other physical systems, but Marek tends to see it, if I correctly report, as a law of nature that the particles are just produced according to the maximum entropy principle. We have not arrived to a conclusion acceptable to both of us, but in 2013 we coorganized the workshop *Unreasonable effectiveness of statistical approaches to high-energy collisions* — the title refers to the famous article by Eugene Wigner — which certainly was a success.

In spite of all disagreements and quarrels, we manage to effectively collaborate and from time to time we even complete a project. The most recent one I find as particularly interesting and useful.

5. Identity method

Particle's identification in high-energy experiments is usually far from perfect and typically particles are uniquely identified only in a small part

of detector acceptance. This is not an obstacle for one-particle inclusive measurements as long as the *statistical* identification is reliable. 'Statistical' means that you do not not know whether a given particle is, say, a proton, but you know that, say, 17% of positively charged particles in the phase-space region are protons. This is perfectly sufficient to get a proton inclusive distribution with a high accuracy.

Fluctuation or correlation measurements are much more difficult. Imagine you want to study a correlation between protons and negative pions. Then, you compute a correlator of p and π^- and you must know that a given pair really consists of p and π^-. Therefore, such measurements are typically performed in small acceptance regions where the particle's identification is reliable. Consequently, these measurements loose sensitivity as they are dominated by a statistical noise. The reason is that any particle multiplicity distribution becomes Poissonian if a large fraction of particles is not observed.

The NA49 Collaboration — the predecessor of the NA61/SHINE Collaboration — studied fluctuations of chemical composition of final sates of heavy-ion collisions through the measurement of event-by-event fluctuations of the particle yield ratios like $(K^+ + K^-)/(\pi^+ + \pi^-)$ [5]. The measurement was performed in a rather small acceptance window where the particle's identification was sufficiently reliable. Marek wanted to extend the acceptance arguing that the unique identification can be replaced by the statistical one. Following his excellent intuition, he essentially guessed how the fluctuation measure, which assumes the ideal identification of particles, can be extracted from the fluctuation measure obtained within the statistical identification. Then, Marek contacted me asking to prove that his method (further on called the *identity method*) is right. With my typical skepticism, I initially thought that the method cannot be in general correct that it is either approximate or some extra assumptions are needed. After a hard and long work, I proved to my dismay that Marek was right. The identity method, which I find a really great invention, was described and discussed in the paper [6]. The method was further developed by Anar Rustamov and Mark Gorenstein [7] and subsequently it was successfully applied to experimental data of the NA61/SHINE and ALICE experiments [8, 9].

6. Epilogue

There is no epilogue of this story yet. We still collaborate, discuss physics and sometimes quarrel. So, I wish you, Marek, many, many great ideas that I will try my best to prove or disprove. There must be some adventures ahead.

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