HADRONIC YIELDS, PHYSICAL REALITY AND THE OBJECTIVE EXTERNAL WORLD

B. Lukács

CRIP RMKI, H-1525 Bp. 114. Pf. 49., Budapest, Hungary

ABSTRACT

Our knowledge about Quark Plasma is collected through the filter of rehadronisation process/models. In reality Rehadronisation strongly influences the observed data, in the process of cognition we cannot simply reconstruct the states before Rehadronisation, since rehadronisation models are various and diverse. The situation illustrates some theorems of Logic, and calls for a methodical work to synchronously determine the properties of Rehadronisation. Maybe the necessary experimental data will be at reach in the near future.

1. INTRODUCTION

Hadronic yields are the main tool of Heavy Ion Physics (HIP) to find out the data and properties of the heavy ion collisions (HIC) called loosely Quark Plasma (QP).

QP is thought to be a deconfined phase where, therefore, quarks have more than 2 neighbours to interact with via QCD forces. Such states are not observed in "everyday life"; we conclude about their existence by indirect and theoretical ways, and the laws of this phase have to be discovered (except that QCD theory gives some guesses and yields some structure).

HIC's are almost the only way to observe QP, and surely the only one in laboratories. QP is told to have existed in the early Universe before 15 microseconds [1], but the traces of this state are remote. QP may or may not exist in cores of neutron stars, but evidences are rather weak; and it must exist in quark stars but we do not know if quark stars themselves do exist in Nature. In addition, neutron stars are far objects, observations are not easy and experiments with them are impossible. So we may tell that HIC is the main tool to investigate the properties of QP.

However, then, the investigation is rather indirect. We prepare the initial state in everyday, hadronic phase. We do not know for surety if QP will be formed at all. At the end we detect something; but again in the hadronic phase, after rehadronisation. From them, we *conclude* about the situation in QP. (A few, leptonic, signals may have come directly from QP, but we canot see directly that they came directly.)

The old uncertainty if we already see or not QP in itself is a proof that ad absurdum the story can be told even without QP. After all, superdense, hot hadronic matter is exotic enough and rich in degrees of freedom. If we create such states, almost anything may happen; we cannot know a priori, and only experiments can tell what will happen.

A lot of people are looking for *QP signals*, decisive experimental evidences that QP had been formed for a while but of course has again vanished before detection. Opinions are no unequivocal which are the best QP signals. Here I concentrate on hadronic yields; but even those leave a wide choice for optimum. However I would like to call attention to a rarely emphasized consequence of (mathematical or formal) logic, according to which *in itself* no statement can be either proven or disproven [2].

Sect. 2 demonstrates that the problem is not unexpected for Logic; which, however, formulated its answers in lofty generality. Sect. 3 is a brief overview of the rehadronisation. Sect. 4 deals with a theoretical thermodynamic problem of shadow particle components of questionable existence. We compare 9 rehadronisation models in Chap. 5, with the result that indeed the same Quark Plasma could end in very various hadronic states. Sect. 6 is the discussion.

2. TRUTH AND REALITY

In the followings for logic I will refer [2]. However I remember an article of the author of [2] too which I read some 35 years ago and which I cannot find just now; I am going to use the arguments of that article too in this Chapter.

Ref. [2] is explicit. In the Introduction it states that statements about physical objects *cannot be verified or falsified by means of direct comparison to experience*. Namely, with a few exceptions of favourite examples of episthemologists (Quine uses the example of seeing a green spot and simultaneously stating "This is a green spot"; but wait a moment and even this will not be trivial) our experiences are not direct. The body of hypotheses, "laws" &c. can (almost?) *always* be rearranged to prove *any* individual statement; of course at the cost of maybe getting very complicated theories and then it is not practical to be done. As Ref. [2] tells, when something happens, the system of hypotheses must be revised. The more fundamental a law *in our system*, the less chance to revise it; but in extremal cases even "direct experience" ("I hold a pen in my hand"; maybe even "I see a green spot?") may be refuted as a delusion.

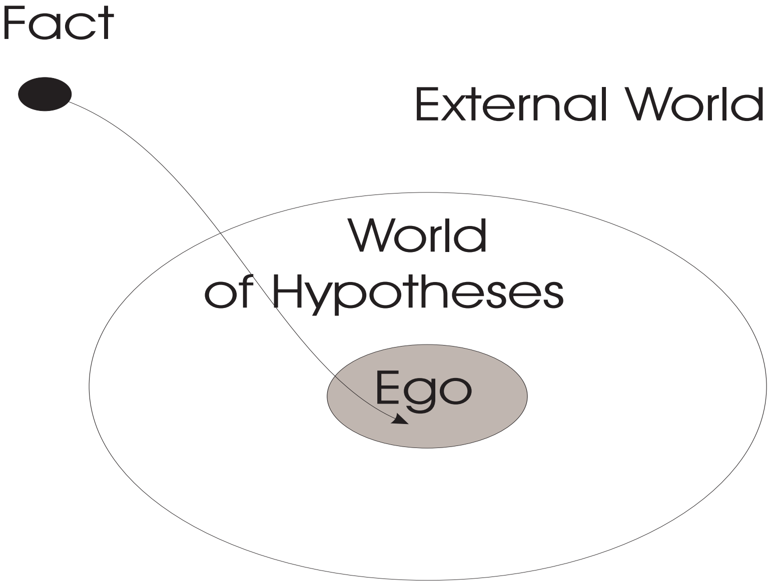


Fig. 1: Quine's scheme about external world, hypotheses and cognition.

Quine argued that "I see a green spot" or "I see the grass" or "the grass is green" still involves hypotheses about light propagation, spectral composition of the illuminating light &c. E.g. surfaces illuminated by monochromatic green cannot seem anything else than green. As if we were in the center of a circle, the hypotheses within the circle and "facts" at the periphery or outside. We see the external world through our hypotheses. This is not philosophy but Logic.

Recent years emphasize one more uncertainty about the elementary factuality of "being/seeming green". In a minority of female retinas *four* types of cones can be found in contrast of the male three, via the split of either the red or the green paint [3] into, say, two variants of Green. Such an observer can distinguish colours within my one and indivisible "true and pure green"; and while she may call all of them "green" while speaking with me, disabled, for her the differences may be as between green and blue for me. Therefore "I see a green spot" means different things for half of the tetrachromat females than for other tetrachromats and all trichromats. The truth of the above direct enough statement depends on the alleles on Chromosome X. There is no such objective thing than "green". So much about "direct experiences".

As Ref. [2] states, most of our statements concern experiences only indirectly. The *system* of statements does have experimental meaning, but predicting experiences is possible only via the *relations* of statements can be used.

This is told by Logic. Now we are going to see what is told by (Heavy Ion) Physics.

3. REHADRONISATION BETWEEN QUARK PLASMA AND US

While HIC quarks are microscopic both in duration and in size, in the diagram of Logic where we, or rather, the Ego, in singular, is at the center, they definitely belong to External World. The Ego cannot go to them to observe them directly; he can observe them only via hypotheses and such. Namely, the observation goes as follows. I assume here that everything is known in the hadronic phase, including reliable 3 dimensional simulations; if it is not true, the problem is still only technical.

1) We prepare the experiment by determining the collision energy and nucleus sizes; also, by a posteriori selection, we specialize the impact factor. In ideal case, then, the maximal density & temperature are unique, *only we do not yet know the unique values because they depend on compressibility & such in the QP*.

2) Hadronic matter is compressing because of the initial conditions. In principle we know what is happening; but nothing interesting.

3) If theory is good, at some density/temperature QP is formed, through a coexistence domain if the transition is of first order. We cannot observe the QP.

4) The firecloud reaches its maximal compression. Since the matter is momentarily stationary, a lot of reactions are expected around maximal compression. We are most interested in the material characteristics in these stages.

5) Expansion starts, still in QP.

6) At the phase boundary all quarks rehadronise. Henceforth again solely hadrons are the actors, quarks are microscopically confined.

7) Much later, more or less freely flying hadrons are detected. However, their *numbers* (obviously energy distributions as well) keep the memory of QP. Reaction rates were *different* in QP than in hadronic matter. Some products were thus created more abundantly, others less so. If a *reliable* simulation shows that particle Q (anything it be) is much more abundant with a story containing a transient QP than in one without phase transition, then hadron Q is a *signature* of QP, although it cannot live in QP.

But: how to select good signatures? There are always some superstitions. E.g. in 1990 we guessed that K^+/π^+ might be a good signal [4]. The reasoning was sound. A vstrange hadron may be formed in pure hadronic reactions, but then the extra mass is some 350 MeV. It is, maybe, only 150 MeV in QP, and reactions are faster as well.

In this paper I concentrate on this problem of selecting good QP signatures, which is far from being trivial.

But first, let us state the goal of HIP. Obviously the ultimate goal is *not* to reproduce hadronic yields by calculations. Nobody is really interested in hadronic yields *in themselves*; that abstract knowledge would be hard to utilize in other parts of Physics. A theory with rules yielding the always the experimental yields, independently of other circumstances would simply mean that everything is good, but hadronic yields do not give any clue anymore for any problem. Rather, we *hope* that some combinations of yields

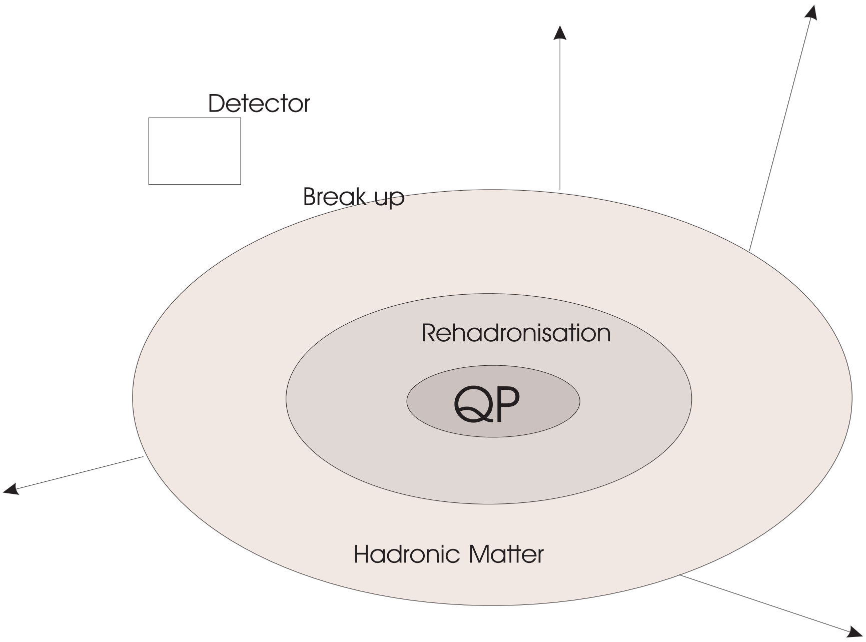
$$y^{\Gamma} = y^{\Gamma}(N_{detect}^{I(hadronic)}) \tag{3.1}$$

where I runs over all hadronic degrees of freedom would be sensitive on the fact if Q was present at maximal compression or not, while other combinations would depend instead on other properties of particle physical theories. We may hope this, but this is not sure. And it is sure that a methodical work would be needed to select good hadronic QP signatures. But before going to that problem, let us see examples that it is not trivial to tell even what *does exist* in a phase.

4. WHAT IS AN EXISTING COMPONENT?

The chapter title is intentionally strange. Our first example is a problem still in hadronic phase. But the hadronic phase of the firecloud is *separated* from detection by a transition called breakup (see the scheme of Fig. 2). During that transition from an interacting continuum to a free streaming Knudsen gas many things may happen.

Since 1979 a work was done to simulate Pb+Pb collisions at Berkeley energies [5]. For a long time the simulation was unsuccessful. The hadrochemical equations included Δ resonances, with all observed data of theirs. Then the simulation told that it was cca. as many Δ's at breakup as π's. So half of the detected π's must come from free streaming Δ's decaying between breakup and detector. Then this component would be centered around cca. 100 MeV energy. But no such component was ever seen; the distribution of π's was roughly conform with purely thermal ones.



Then we looked at the problem from another angle. Thermodynamics tells us at the very first to postulate the set of independent extensives [6]. This step must precede the determination of the thermodynamic potential. Now, the obvious choice is a set

$$X^I = \{V,E,N^N,N^\pi,N^\Delta\};\ S=S(X^I) \qquad (4.1)$$

An approximately ideal gas entropy function seemed well defendable and still the final result was quite contrary to the experiment. But maybe our postulate for the dimensionality of the state space was inappropriate. Instead, we may try with

$$X^I = \{V,E,N^N,N^\pi\};\ S=S(X^I) \qquad (4.2)$$

Again using an ideal gas approximation the simulation is good enough. So the measurement gave the result that the Δ component did not form an independent *thermodynamic* component in the firecloud!

No doubt, some resonances with Δ quantum numbers existed transiently there as correlations. But their numbers were always determined by the N and π numbers. If we cannot prepare N^3 independently of N^1 and N^2, then there is no third particle component *in the thermodynamic description* anything be told by the detectors; but in this case even the detectors did not recognise any ex-Δ.

To speak less axiomatically, we *may* call N-π correlations Δ's. But at not too high temperatures the avarage "mass" of these correlations will be well below 1236 MeV, the accelerator average, since the average thermal π momentum is well below the corresponding 230 MeV/c center-of-mass momentum. So, when the average N-π correlation "decays", the π momentum will be moderate. If the correlation breaks up in the breakup of the firecloud, the resulting π will be hardly distinguishable from the average thermal π. Questions of kind "Were there *really* Δ's in the matter just before breakup?" belong for us to scholastics about how much angels can occupy the point of a needle. The existence or nonexistence of underdeveloped Δ's does not have clear signatures, so it does belong to the easily rearrangeable part of World of Hypotheses on the scheme of

Indeed, long time ago we looked for the kinds of particle components in the firecloud which may or may not be hypothetised at will [7]. Without completeness, I only tell that you can, for example, introduce a new particle component whose particle number density is proportional to entropy density. Then, of course, the entropy of the firecloud is increased by a constant; but, as we know, the entropy (of all systems, simultaneously, at least) may be multiplied by a strict constant keeping the thermodynamic description intact [8]. Therefore, if we are cautious, the shadowy new particle does not change anything in any phase. Of course, we have to talk away why it is not detected in the final Knudsen gas.

Detectable particles, of course, should not belong to the easily rearrangeable part of the scheme of Fig. 1, at least if we did not operate with delusia of the detector.

5. ON DEPENDENCES ON REHADRONISATION MODELS

Let us return to the scheme of Fig. 2. *Symbolically* we can translate the Figure as

$$\{\text{Hadronic Yields } N_{\text{detect}}{}^{I}\} = \{\text{Quark Data}\} * \{\text{Hadronisation}\} \qquad (5.1)$$

and I am sure, the detailed calculation of the rehadronisation processes *from first principles* would not be easier than calculations in pure QP.

However, something must be done. First, one may elaborate a "thermodynamic" approach, when microscopic variables are substituted with a few macroscopic ones. Such an approach has proven successful in many cases; maybe it will not be too bad now. Then final hadronic yields, which are extensives, can be expected a functions of 3 sets of extensives:

$$N^I = N^I(\mathbf{X_Q},\mathbf{X_H},\mathbf{X_{reh}}) \qquad (5.2)$$

Here the first two sets are data of the two phases, while $\mathbf{X_{reh}}$ is some characteristics of the rehadronisation, say

$$\mathbf{X_{reh}} = \approx \mathbf{X_Q} - \mathbf{X_H} \qquad (5.3)$$

Now here I utilize 8+1 different rehadronisation *models* and compare their "predictions". Model 1 is pure combinbatorical, while Model 9 is not rehadronisation at all, but in it the matter is always in the hadronic phase, moreover, in chemical equilibrium. The other 7 models are "in between".

The scheme can be found in Refs. [9], [10] & [11]. The transition is imagined at a prescribed line in the (T,μ) plane; the curve is taken from Ref. [12]. Now a three digit binary number is formed. Third digit: is a pressure equilibrium maintained during transition? No (0) or Yes (1; and the pressure is corrected by the simplest quadratic way, with hadronic compressibility). Second digit: are final (hadronic state) statistical weights of macrostates felt? No (0) or Yes (1; and the weights are given according to Appendix)? And: does gluon fragmentation happens during rehadronisation? No (0) or Yes
(1; and 0.85 of gluons go into q pairs, 0.15 into s pairs)?

Then we wet a 3 digit binary number and the number of the model is this + 1. Number 9 is the hadronic chemical equilibrium and Model 10 would be sequential fission [13], a par excellence QP model. However earlier we saw that the predictions of Sequential Fission are rather similar to Model 9 at $S/N_B > 10$. Now we calculate for specific entropies $10<S/N_B<50$.

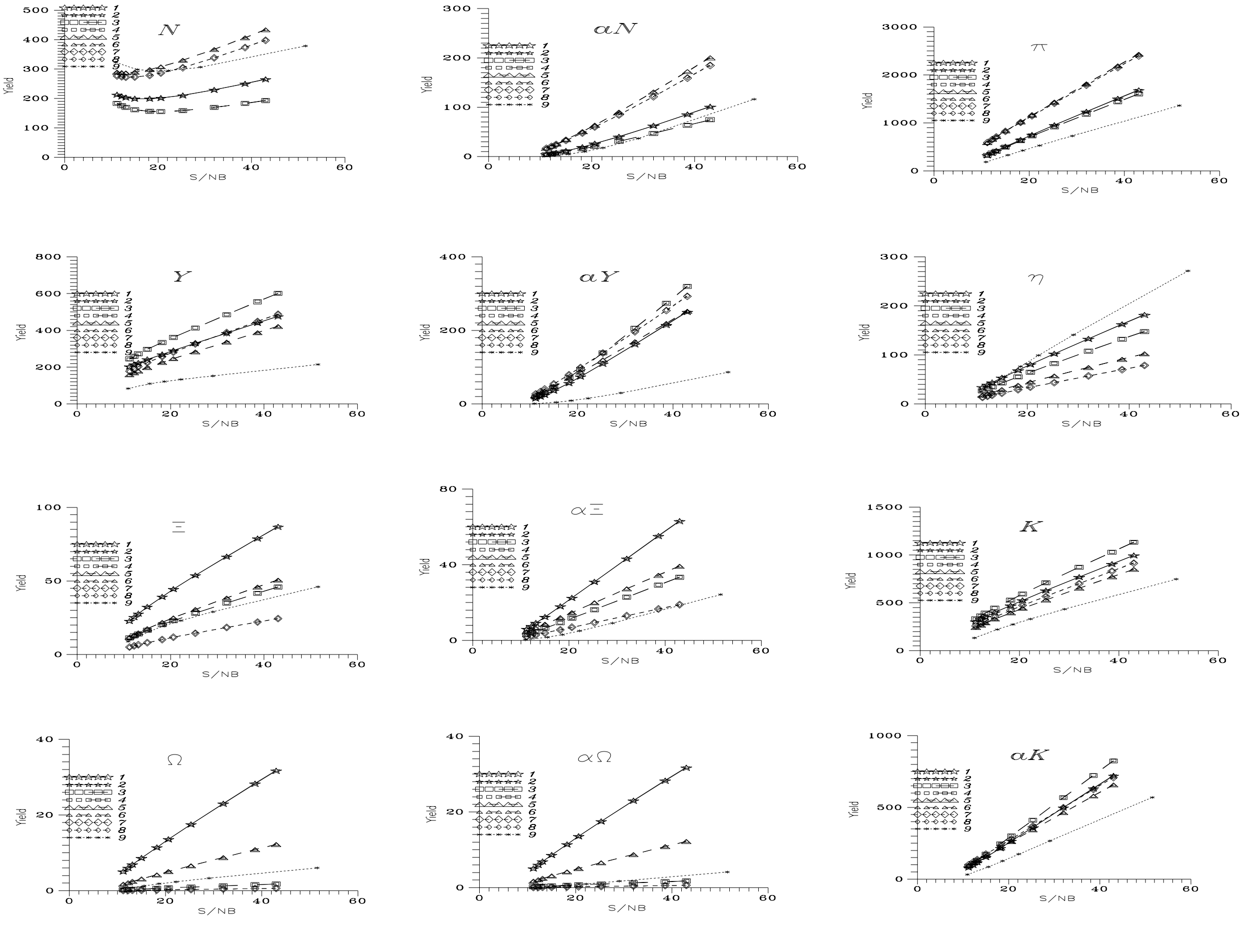
A result is that for $S/N_B>20$ the third digit is irrelevant, Models 1 &2, 3& 4 &c. practically coincide. However the predictions of Models 1, 3, 5, 7 & 9 clearly differ there. That specific entropy range seems to correspond to cca. hundred GeV on hundred GeV collision eergy, although we cannot measure entropy directly, so this conclusion is drawn mainly from yields themselves.

The following 12 Figures give the specific entropy dependence of 12 yields from a central collision of total baryon number 414 (so Pb+Pb). The applied approximations (which are serious, indeed) have been discussed in [9], [10], [11] and [14]. Even for first inspection one can see that

1) in the same rehadronisation model & entropy (energy) different yields differ orders of magnitude (of course);

2) the same yield strongly depends on entropy;

3) most yields strongly depend on the rehadronisation model used.



Figs. 3-14: Hadronic yields. Numbers for rehadronisation models explained in text; Model 9 is complete hadronic chemical equilibrium. Symbol "a" stands for "anti". Details: [8-10] & [14].

So *in principle* an ambitious programme is not hopeless. Namely, if there is a curve which gives all yields according to the experiment at one specific entropy then we have measured the specific entropy produced in that collision, and the way of rehadronisation as well. Then at not too different collision energies we may assume the same rehadronisation mechanism.

By doing so, our hypotheses do not belong anymore to the *easily rearrangeable* part of the World of Hypotheses on Scheme 1, since they were singled out by measurements; of cours a change of rehadronisation process between two measurements is not a easy choice. We shall see that until the evaluations are not ready, it is rather dangerous to be stack to specific models.

Also, it is obvious that here I will not conclude from the Figures about specific models; technical problems are tremendous. However, in the next Chapter I would like to draw some conclusions about the trends.

6. DISCUSSIONS

First we see that the lines of hadronic chemical equilibrium clearly differ from those of any rehadronisation model. This is not too surprising; but now we have seen it.

Maybe the "combinatoric" Model 1 is easiest to visualize. There hadrons are produced proportionally to the meeting probabilities of their constituent quarks. One can interpret this as "love for first sight", or that a hadron is produced always if the needed number of quarks meet in a "coalescence volume". The fundament of such an assumption can be argued without any definite result; but ratios of particle yields can show if the assumption is true. The simplest consequences of Model 1 are: high Ξ and Ω, moderate Y (Λ and Σ), rather low N; and roughly the same for antihadrons.

To see the reasons let us switch final state weights in. Then Ξ and Ω drop drastically, Y goes up, and, perhaps surprisingly, N goes down too.

Switching in the gluon fragmentation, but not statistical weights, Ω's are still quite abundant, and Y's are particularly low. With weights *and* gluon fragmentation Ξ's and Ω's drop particularly deep.

Now, while it would be difficult to guess all the trends without specific calculations, something can be understood. Take first Model 1. Initial baryon number cca. determines the number of net q's, and temperature at rehadronisation (cca. 160 MeV) those of the ss^* and qq^* pairs. Then

$$\Omega/N \sim (s/q)^3 \tag{6.1}$$

Now, if we switch on the statistical weights, the ratio (6.1) drops by the factor $\exp(M_\Omega - M_N)/T \approx 1/20$. Gluon fragmentation does not change the net q number but increase the pairs, so one would expect more π's and antibaryons. While this is not exactly true, at least N^*, Y^* and π unequivocally increase with gluon fragmentation. Obviously competition of 12 hadronic channels for 6 kinds of quarks make the story complicated.

The next Figure shows the K/π ratio. As told, some years ago K^+/π^+ was believed a good QP signal on the grounds that it is easier to produce lot of s quarks in QP than strange hadrons in hadronic phase. Now we can calculate the ratio in any model (to be sure, in these models there is no charge distinction), and the results are seen in Fig. 15. Now, the trends are as follows.

Since both the K and π curves are roughly parallel with each other, the relative positions of the ratio curves do not depend on S/N_B. At S/N_B=12 the ratios group around 3 values. Without gluon fragmentation $K/\pi \approx 1$. With gluon fragmentation it goes down to 0.4. And for hadronic chemical equilibrium it is 0.7.

Then it cannot be told that K/π would be higher if the system has a QP past than otherwise. It is still true that K/π is higher without gluon fragmentation than without QP.

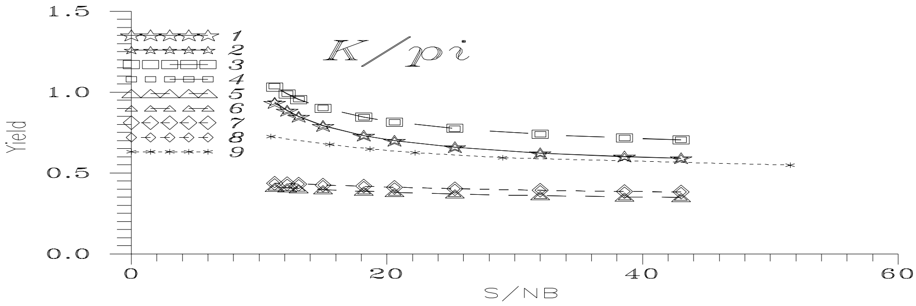


Fig. 15: Ratio K^+/π^+ in different rehadronisation models

Often people believe that they can choose amongst the rehadronisation models beforehand, e.g. on the grounds of physical analogies. I do not oppose anybody's intuition, but *analogies* may misdirect. Let us see here, to close, 2 cases analogous with final state statistical weights (Models 3, 4, 7 & 8), in astrophysics. (For the details see [15].)

There was a primordial hadronisation in the Universe, closing about 15 microsecond age [1]. Now, the general belief is that it led mainly to non-strange hadrons, around us. However there is a disturbing fact and a suggestion. The disturbing fact is that cosmologists would prefer more mass than seen and galaxy clusters would need at least 5 times their observed masses to be stable; with the observed masses they would have evaporated 10 billion years ago. (For arguments see Ref. [1] and citations therein.) The suggestion of some QCD people is that "symmetric" N*(uds) droplets would be the real ground state below (N*(uud) + N'*(udd)) "nuclear matter".

If the suggestion is correct (at least Fermi energy is indeed minimal then), the "hidden mass" may be in N*(uds) strange nuggets [16] somewhere in Universe. But this happens only if primordial hadronisation ended in a final state preferred by final state statistical weights because the symmetric state (uds) is not preferred in QP (and not even by 3-quark hadrons; Λ is heavier than N). I do not claim that primordial hadronisation were to proceed according to Model 4; we know almost nothing about the details. I only tell that Model 1 seems to contradict to the elegant strangelet explanation of hidden mass.

The another analogy is the condensation of the protosolar nebula. From an average interstellar gas, in a condensation, roughly phase transition, it has formed the primordial crystalls of Solar System material, which is preserved in chodritic meteorites up to now. Molecule formation was rather needed, because even most homoatomic molecules, as H_2, O_2 and such, cannot condensate at any reasonable temperature. Metals, oxygen, carbon, sulphur and hydrogen are expected to condensate in compounds. Then we may ask if chemical affinities (chemical binding energies) were felt in the condensation process ("Models 3 & 4") or compounds were formed on combinatorical grounds "just when meeting" ("Models 1 & 2").

Now, observations vote for Models 3 or 4. Namely, most oxygen was catched by aggressive metals as Fe, Mg, Al (and Si), and only a small amount went into H_2O, although H dominated O by a factor 1000 and any metal by 1,000,000 [17]. In a combinatoric model oxygen would have been used up almost exclusively by H, and then there would be no planet inside Jupiter.

However, in C chondrites of low petrologic classes a clear anticorrelation is seen between H_2O and Na_2O contents [18]. So meeting probabilities do influence the "molecular yields", and that is qualitatively "Model 3". Now, hence one could jump to a conclusion by analogy; and indeed Model 3 seems to be wily/involved, taking two processes into consideration in one time. But then observe that around S/N_B=50 Model 1 seems to be conform with CERN HIC experiments, while Model 3 seems not to be [14].

This is the reason I would rather be cautious with preconceptions and rather I suggest a methodical evaluation of yields. For this the whole 10-200 GeV range could be covered not only the upper end. It seems such experiments are planned now.

ACKNOWLEDGEMENTS

Illuminating discussions with Ágnes Holba are acknowledged. Earlier discussions with Drs. K. Martinás & A. Rácz were hepful. Supported by OTKA T/032796.

APPENDIX: ON FINAL STATE STATISTICAL WEIGHTS

For the details see Refs. [1] and [19]. Number of microstates belonging to a given macrostate is proportional to e^S. However in phase transitions situations are rather isothermal (not exactly). If temperature is prescribed, the proper potential is S-E/T = - F/T. So

$$N(\text{microstates}) \sim e^{-F/T} \qquad (A.1)$$

Now, using an ideal gas equation of state with moderate temperature, we get that the leading term is $e^{-M(\text{hadron})/T}$. Indeed, transition temperature is cca. 160 MeV, negligible compared to at least baryon masses. Of course this is a crude, first order approximation and if methodical work starts, better ones will be needed.

REFERENCES

[1] Kämpfer B., Lukács B. & Paál G.: Cosmic Phase Transitions. Teubner, Stuttgart, 1994

[2] Van Orman Quine W.: Methods of Logic. Holt, Rinehart and Winston, New York, 1963

[3] Mollon J.: Nature **356**, 378 (1992)

[4] Lévai P., Lukács B. & Zimányi J.: J. Phys. **G16**, 1019 (1990)

[5] Barz H-W. & al.: Z. Phys. **A302**, 73 (1981)

[6] Landsberg P. T.: Thermodynamics. Interscience, New York, 1961

[7] Lukács B. & Martinás K.: Acta Phys. Slov. **36**, 81 (1986)

[8] Lukács B. & Martinás K: Phys. Lett. **114A**, 306 (1986)

[9] Lukács B., Rácz A. & Zimányi J.: in Proc. Workshop on RHIC at Present and Future Accelerators, eds. Csörgő T. & al., Budapest, 1991, KFKI-1991-28, p. 144

[10] Zimányi J. & al.: in Proc. Budapest Workshop on RHIC, eds. Csörgő T. & al., Budapest, 1993, KFKI-1993-11, p. 15

[11] Zimányi J. & al.: in Particle Production in Highly Excited Matter, eds. Gutbrod H. H. & Rafelsky J., Plenum Press, New York, 1993, p. 243

[12] Koch P., Müller B. & Rafelsky J.: Phys. Rep. **142**, 167 (1986)

[13] Árvai Z., Csörgő T. & Zimányi J.: in Proc. Workshop on RHIC at Present and Future Accelerators, eds. Csörgő T. & al., Budapest, 1991, KFKI-1991-28, p. 95

[14] Lukács B. & Rácz A.: Heavy Ion Phys. **4**, 435 (1996)

[15] Holba Ágnes & Lukács B.: Heavy Ion Phys., to appear

[16] Farhi E. & Jaffe R. L.: Phys. Rev. **D30**, 2374 (1984)

[17] Novotny Eva: Introduction to Stellar Interiors and Atmospheres. Oxford Univ. Press, New York, 1973

[18] Solt P. & al.: in Terrestrial and Cosmic Spherules, ed. Detre Cs. H., Akadémiai Kiadó, Budapest, 2000, p. 155

[19] Kämpfer B. & al.: Acta Phys. Pol. **17B**, 685 (1986)_