Facing the tantalizing amount of experimental and simulated or computational data which were shown us at this meeting I was tempted to say that now the task of theorists would be to describe (these) data. Then I started thinking. And realized: no! I am objecting this view. Our task is not to describe, but to understand data.

This effort to understand, whereas understanding necessarily involves reduction (not throwing, however, really important parts away), implies further efforts. Here is a to-do list of the necessary prerequisites of understanding:

- understand theory (this might sound trivial first, but I assure it is not),
- verify assumptions (assumptions are always there, explicit or implicit, when applying abstract theoretical concepts in models of reality)
- falsify assumptions (equally important as verification, otherwise different theories would interpret the same data)
- weave a network of non-contradicting statements (which is still a process internal to the theorist’s work), and finally
- make a new theory only if it is unavoidable (otherwise we would be led too much by our phantasy and mathematical invention instead of experimental facts).

Ladies and gentlemen, this is going to be the theoretical summary talk of the Strange Quark Matter 2003 conference. When I was alerted by the e-mail we all got, “prepare your transparencies”, I took this home-work exercise seriously. I have prepared quite a few pages before this conference. What can one know in advance, before listening to the talks?‡

First of all there is a general outline which a summary talk should follow. On the level of the basic theory one is supposed to conclude about the present status of the underlying theoretical concepts, one ought to emphasize important news, the novel aspects we are encountering, and finally it is useful to formulate in a possibly definite way, what our perspectives for further development are.

A summary of the phenomenology should also be given, not necessarily in the order of their original presentation, but restructured, following the natural clusters and

‡ I attended to almost all of them, and I listened to all I attended
network of themes and applied methods which is formed by the contemporary research. Alas, this dichotomic splitting to basic theory and phenomenology is basically false: reality is rather described by a continuous spectrum than by two peaks, there are even cases of mixed states.

A separate effort should be devoted to selecting out highlights, new predictions and explanations, if any occurred. Some colleagues used to apply the defaming expression “post-dictions” to explanations. This attitude – I think – is mistaken. Really good explanations of already known experimental facts help us towards understanding, especially when, if they are selective, clearly separating alternative approaches. The key requirement is to contribute to our understanding.

Finally, in order to complete the summary, I have written up a list of issues “we did not talk about”, meaning to mention theoretical works and concepts not taken explicitly into the official program, although implicitly used and often referred to by speakers in their presentations.

After attending to the lectures, listening to the talks and a seemingly uncountable number of follow-up discussions I was able to formulate a second summary outline. This I would like to present in a form of questions. The main question of this and several previous meetings remained:

Have we seen (strange) quark matter?

This question implies further questions. What is actually quark matter? Is there any quark-level collectivity? The concept of collectivity can be split to the following main subtopics:

- the question of flow and \(T\) (meaning a common slope of transverse momentum spectra of different particle species, not temperature),
- quark level recombination and its effects on the finally observed hadronic states,
- suppression and/or enhancement of any hadronic or leptonic signal known from \(pp\) and \(e^+e^-\) collisions, and finally
- the very question whether all what we see is just a maximum – entropy state, washing out all memory of that, whatever may have existed before.

Can we trace back the pre-hadron state? Do we understand already what quark matter is or are we misled by preconception and wishful thinking of simplicity by doing the theorists’ homework?

In order to present an overview of the theoretical topics of this conference I would like to confront you with an “artist’s view” of the theory landscape of SQM 2003 (Fig.1). This landscape includes a lake with a big, fat duck swimming in it at the front, a small stream rushing into the lake, a few big trees, green fields in the background and far-away hills and mountains. I have learned from the talk given by Barbara Jacak – and I think she was citing Nu Xu – that the quark gluon plasma (QGP) “looks like a
duck and talks like a duck” (the experimentalists just cannot say whether what they see is a duck). The duck first has been spotted in the Lattice Lake which receives some fresh water from pQCD (perturbative QCD) stream through the Resummation Falls.

There is a tree behind with ramified roots near to the water, symbolizing the parton model and in particular the quark level recombination. On the other hand a unique, plain-textured stem dives high from this, the “standard thermal model”. Its main and only advantage lies in simplicity. It may dart some shadow onto the duck, but can hardly help to learn what the duck looks like. The branches of the tree carry uncountable leaves representing fluctuation and correlation studies, and a big, reddish fruit is starting to reach the state of maturity, the apple of hadronization.

The big vast field in the background stands for the big micro-dynamical codes. At a distance they all show the same green color, only by laying down can one start to see separate grass pieces. It requires close inspection. Beyond all those smooth hills of hydro and the Mt.EoS, symbolizing the far leading problem of the equation of state, occur. The top of this mount, where all climbers must be heading to, may – by the way – be reflected in the water of the Lattice Lake quite accurately. Above, mostly in cloudy skies, reside astrophysics and cosmology. Finally to our left some strange looking pine trees hint to a forest of exotic theories.

A further prepared transparency shows you a clustering of the theoretical themes presented at this conference, deciphered from the preliminary program. Up to a few name changes it reflects now the real program well. The themes in a rather reverse order of their presentation, were: lattice QCD, (Steven Gottlieb, Péter Petreczky, Owe Philipsen) astrophysics, (Mark Alford, Fred Walter, Markus Thoma, Madappa Prakash, James Lattimer) strange and exotic, (Johann Rafelski, Volker Koch, Jack Sandweiss, Ariel Zhintnitsky, Jeffrey Bowers, Sarmista Banik, Jürgen Schaffner-
Now let me reconsider – in the original time order – what we have learned, what I, as a test-person, have learned from the theoretical talks of this meeting. In the opening review talk given by Ian Rafelski we were reminded of the basic facts of strange quark matter physics:

(i) **Strangeness is produced.** On the quark level by $s\bar{s}$ pair production, on the hadronic level by associated hyperon and strange meson production – so actually net strangeness is **never** produced in strong interaction.

(ii) **Strangeness is equilibrated.** This fact can be really understood only on the quark level, where the gluon fusion ($gg \rightarrow s\bar{s}$) cross section is large and dominant leading to equilibration in fractions of 1 fm/c. On the hadronic level strangeness is **not equilibrated**. It is practically impossible due to much longer, in the order of tens of fm/c hadrochemical time scales. A caveat to this point: it was tacitly assumed that that gluons and gluon fusion is not suppressed by some nontrivial, non-perturbative phenomenon.

The central role of chemistry and the usefulness of thinking in terms of chemistry was also emphasized by the speaker. We learned that the equilibrium chemical potentials ($\mu_a$) do not fix all particle species numbers ($N_i, i > a$). Conventional chemical potentials are related to conserved charges, to a few selected linear combinations of particle types ($Q_a = \sum_i q_{a,i} N_i$). There are other combinations, e.g. those related to pairs, which are linearly independent from these constraints. They can be characterized by fugacities ($\gamma_b$). Whenever such a quantity is not one, the system is **not in chemical equilibrium**.

It was made plain by him with a definite statement: a $\chi^2$/degree of freedom fit is much better using $\gamma_{\text{strangepairs}} < 1$, than without this possibility taken into account (Fig.2). Later (by others) an equally firm counterstatement was made: $\gamma \approx 0.75$ does not matter (does not quark matter). Last but not least to this point I would like to emphasize that contrary to Johanna Stachel’s remark fugacity is not a “fudge factor”. **It is as fundamental as the chemical potential itself**, no less, no more.

We have two principal possibilities of dealing with conservation laws: i) take them literally and then end up with a (micro)canonical description, or ii) take them on the mean (hoping that a reservoir is there) and use the grand canonical approach. Where it matters, is the case when the available phase space is small. This phenomenon was named – strangely enough to me – “canonical suppression” and was promoted
by Redlich, Cleymans, Koch, Tounsi and others. The next figure (Fig.3) borrowed from Johanna Stachel’s talk shows how effective this factor really is: from about ten participants on it becomes unimportant. Jean Cleymans has shown us in his talk yesterday trends of statistical model parameters as a function of the participant number. The $\gamma_s$ rises from 0.5 to 0.75 at SPS and from 0.65 to 0.9 at RHIC, showing a clear trend (Fig.4). The temperature parameter, fitted to spectral slopes, is much less trendy, it behaves differently at SPS than at RHIC energies. The most characteristic behavior is shown by the baryon chemical potential, $\mu_B$: it is namely constant, albeit a different constant, at both accelerator energies (it shows no centrality dependence at all).
Many of us were shocked by the thermal model, more so, that it has been applied and seems to work not only for heavy ion collisions, but also for $pp$ and $e^+e^-$ data (Becattini et. al.). In my opinion we who have been shocked might have been misled by our gut feelings and have been misjudging the minimal size which is necessary for the thermal limit to make sense. How far can the thermal limit be? This question was also addressed by Fuming Liu in a micro-canonical analysis of $pp$ data and was shown that for pions the thermal model works well (not for heavy hyperons, however).

I would like to present here a simple case study which might help to change our attitude towards what is big and what is small with respect to thermal models. Take a random variable $x$ distributed in the interval $(-1, 1)$ uniformly (Fig.5). The distribution of the sum of two such variables, $P_2(x) = \int dx_1 \int dx_2 \delta(x - (x_1 + x_2))$, shows a triangular shape. The distribution with three variables is made of parabolic (second order) pieces, it already shows the main qualitative features (symmetric maximum, two inflection points) of the Gauss distribution

$$P_\infty(x) = \lim_{n \to \infty} \int \prod_{i=1}^n dx_i \delta(x - \sum_{i=1}^n x_i).$$

Already $P_3$ is not easily distinguishable from the Gaussian (thermal) limit.

Talking about finite systems the main classification is done according to the value of mean occupancy (number of quanta) $\langle N \rangle$. If it is much bigger than one, we approach $\langle N^2 \rangle \approx \langle N \rangle^2$ and everything looks like a Gaussian distribution. In the opposite case, when $\langle N \rangle \ll 1$, we have $\langle N^2 \rangle \approx \langle N \rangle$ and the generic distribution is Poissonian. From Volker Koch’s review talk on the topic we could learn that the normalized second cumulative moment of charge fluctuations, $F_2$, most probably varies in time, so whenever one is led to infer $F_2 < 1$ from experiments, he/she would witness a memory of a short time scale process. Several other talks, e.g. those given by Chihio...
Nonaka, Sangyong Jeon and Gary Westfall elaborated on this point. A main message of these studies is that the finite size scaling of cumulative moments is important: equilibrium can be excluded (although not positively proven) by analysis of scaling powers.

In general we would like to understand both the mechanism and the timescale of the formation of the observed hadronic final state in high energy heavy ion collisions. On an abstract map with qualitative scales (Fig.6), the horizontal axis of time constants distinguishes between 0, 1 and infinity, the vertical axis represents...
the recombination and fragmentation mechanisms by 0 and 1, respectively. Here pQCD draws the upper line border of nowadays theoretical approaches, while micro-dynamical codes cover rather the lower edge. The effective quark recombination model by Zimányi et.al. (ALCOR) resides near to the origin, its philosophical counterpart, the “standard thermal model” occupies the right side line at infinite time scales (assuming equilibrium, so the hadronization mechanism does not matter in this case). We do not yet know where the real process of hadronization of a quark-gluon plasma should be located on this plot, it has a big question mark.

As a flavor of success of the thermal model strongly logarithmic ratios of particle numbers for many selected types were shown at different accelerator energies by Johanna Stachel. Fig.7, plotting RHIC data is a good representative for the overall quality of these fits, used to infer bulk parameters such as temperature, baryon chemical potential, etc. I was very pleased to hear the speaker’s conclusion at the end of her talk: “It works perfectly, but it can’t be.” I rather agree with the second half of this sentence and I would leave the word “perfect” certainly out from this evaluation, but otherwise we can witness a novel convergence of opinions here. That this simple model “works” on the other hand imposes on us a real task to try to understand even the relative success of such simplifications.

It is clear that we are dealing with highly non-equilibrium dynamical processes, which lead to a (closely) statistical final state. This is not simply “equilibrium”. Such a result can occur via at least three different underlying mechanisms with different physical background.

(i) gain and loss term rates are nearly equal leading to a “steady state”; this is
Figure 8. A sketch of the parton recombination and fragmentation mechanisms in the $p_T$ spectra. (After Rainer Fries’ talk.

the textbook case for equilibrium (however, this equilibrium is sometimes partial only),

(ii) all rates approach zero fast enough resulting in a state in which the pre-history of the mixture is frozen in (this process is called “freeze out”),

(iii) a repetitive and self similar dynamical process, e.g. multiple fragmentation, leads to a pattern reflecting self organized criticality (s.o.c.).

Giorgio Torrieri pointed out nicely in his talk where the main uncertainties of statistical models (which assume a state of maximum entropy, or thinking of entropy as lack of information, a state of maximal ignorance) lie. He showed the general formula

$$dN = \int d\Sigma \mu p^\mu f(T, p^\mu, \mu_f) + \text{resonances}.$$ 

The form, evolution and the very nature of a freeze-out hypersurface, $\Sigma$, may pose restrictions, e.g. causality limits on the statistical interpretation. This question was copiously studied in HBT concentrated works on which Roy Lacey, Fabrice Retiere and Ziwei Lin presented talks. The latter called our attention to non-trivial $x-t$ correlations which may influence the interpretation of experimental data. Furthermore the value of chemical potentials and the neglect of further fugacity factors is correct only as far as chemical equilibrium has been established. Finally the presence and different time evolution of resonances may add new terms to this simple picture.

These experiences push us back to the roots. The roots of statistical and bulk models grasp into the parton dynamics which in turn should have its backup from field theory.

Here is quite new the application of the idea of quark recombination (Fig.8). Following Rainer Fries I repeat here the main idea in its possibly simplest form. Assume that a
meson with transverse momentum \( p \) is a recombination of two partons, a quark and an anti-quark, each with a momentum of \( p/2 \). With the same logic baryons are composed of 3 quarks each with a momentum of \( p/3 \).

\[
\text{meson}(p) = \text{quark}^2(p/2),
\]

\[
\text{baryon}(p) = \text{quark}^3(p/3).
\]

Interpreting this as a functional equation it gives us the hint that the stationary solution of hadronization rate equations is proportional to \( \exp(-p/T) \). On the other hand pQCD calculations using fragmentation functions always connect a parton with momentum \( p \) to hadrons with less momentum \( zp(z < 1) \) and – due to the very nature of free propagators – always lead to a power-law behavior of transverse spectra \( (p^{-a}) \).

Experimental data show a nice interpolation between these two mechanisms as passing from low to high \( p_T \) at around 4 GeV/c (Fig.9). Here we deal with absolutely non-equilibrium processes.

In order to understand better this interplay we need to gain information on the process and on the achieved degree of equilibration. This is the main goal of microscopic computer simulation codes, reviewed and reported about by Sven Soff, Marcus Bleicher, Klaus Werner and Christoph Hartnack. This field looks like a huge jungle, at least for non-experts. It is not really transparent for us and looks like the same green color everywhere. Efforts to incorporate QCD color dynamics, if only on the semiclassical level, are rare. We have listened for a while to the sounds from this jungle. A clear tenor has emerged: all calculations conclude that simple string dynamics would not do a good job in reproducing experimental data. Either the string constant has to be changed or the hadronization mechanism without deeper justification. A further negativum is the presence of far too many internal code parameters, a positivum is the self-equilibrating property inherent in this approach. Fig.10 shows elastic and inelastic rates, related to the maintenance of thermal equilibrium and to the evolution
Figure 10. Elastic and inelastic cross section averages during the time evolution in a micro-dynamical simulation of a heavy ion collision. (From Marcus Bleicher’s talk.)

of chemical composition, respectively. A cross over of the respective dominance of these two rates is clearly seen at times of the order of 10 fm/c.

There are two main problems microscopic codes suffer from:

(i) the initial state is unknown,

(ii) the final state, in particular the dynamics of hadronization, is oversimplified.

New progress has been presented by Joe Kapusta, who calculated thermal baryon – antibaryon production rates in a QGP by using effective field theory (a quite realistic one). His result, stating a characteristic time of about 10 fm/c at the usually inferred temperature gives some interesting perspective to improve microcodes in the future (Fig.11).

Studying the dynamics of hadron formation is not simply a selfish entertainment of pQCD adversed colleagues. Dénes Molnár has pointed out that parton recombination may resolve the so called opacity puzzle. Studying the flow asymmetry via the Fourier expansion coefficient $v_2$ as a function of the transverse momentum $p_T$ baryons, mesons and partons can well be separated into three distinct curves (Fig.12). This explains why baryon and meson (proton and pion) flows are different, and on the other hand also reduces the need for assuming too many gluons in the initial state used for the calculation. Very important data are in this respect the flow asymmetry, $v_2$, the collectivity ratio $R_{AA}$, and their flavor dependence. This is a “húp” (pronounced: kha up) from the QGP duck – at least this is what Hungarian ducks say...
Studying the very low $p_T$ flow asymmetry data in the framework of parton hydrodynamics Ulrich Heinz made a remarkable conclusion related to this issue §. The $v_2 - p_T$ data for protons and pions from RHIC simultaneously “cannot be described by hadronic equations of state” (EoS). One has to use at least mixed phase EoS to fit (Fig. 13). The inferred initial state has an enormously high initial energy density (24 GeV/fm$^3$) which lasts at least 5 fm/c in time. Another “háp” from the duck.

§ Since his talk was going on by an express speed I hardly realized first what the matter is, but thank to talking him after his presentation he reinforced me in that, what I am going to say now.
These achievements bring me to mention upcoming efforts to describe hadronization dynamics on the elementary level. Ralf Rapp has given a review talk on this and many others, e.g. Thomas Mehen, were presenting QCD based studies of heavy meson formation. Here an expansion in terms of $\alpha_s m_\Lambda QCD / m_T^2$ has been established. However it is still unresolved that what is the real process of hadronization. It is in particular unclear, what the correct hadronic wave function in the final state is, and what is the leading soft process. A clean-cut theory approach would here explain the very fragmentation function applied and would recombine partons to hadrons not just statistically, but by first-principle, non-perturbative QCD dynamics.

So we have arrived at lattice QCD. This is our only hope in the moment to learn about these questions. Now we are dashing in the water of Lattice Lake and are closer to the duck than ever before. We just should proceed carefully, the QGP-duck may be a shy creature, easy to whisk away.

The main new lessons we have learned from lattice QCD at this conference are as follows:

(i) The $q\overline{q}$ potential at temperatures moderately over $T_c$ does not resemble a picture of Debye screening (contrary to former expectations), but rather justifies a gradual string break picture.

(ii) Studies of mesonic spectral functions reveal that up to $T = 3T_c$ temperatures the deconfined phase is far from being a plasma of free quarks and gluons; plasmons alike pre-hadrons are formed. I would like to show here characteristic results presented by Péter Petreczky on Friday morning (cf. Fig.14).

(iii) Finally, as we could have learned from the review talk of Steven Gottlieb, there are finite $\mu_B$ calculations by Zoltán Fodor and Sándor Katz, and also other upcoming results at $\mu_B = 0$ which all confirm that in the most realistic 2+1 flavor QCD with dynamical quarks there is no first order phase transition at color deconfinement just a crossover (cf. Fig.15).
These important new results of lattice QCD should be taken seriously also in our community. No naked quarks can be used for recombination (only effective ones, and plasmons) and very probably there are less gluons present at this process, as previously believed. Nobody is entitled to take bag models as input for the phenomenological calculation any more, rather the string picture should be revitalized.

We do not need to be plainly passive in this field. We should try to communicate our needs and wishes to lattice people. What would we, what would I like to ask from them? The main message for lattice Santa Claus has to be: please, help to deal with hadron formation from the quark gluon plasma. This wish can be expanded to a longer list:

- Clarify the QGP structure near $T_c$. (Lattice QCD is already going into this direction).
- Carry out finite $\mu_B$ and not less finite $\mu_S$ simulations. (The latter would be important also for strangelet and strange star search, for the main idea there is still built on the bag model. Now it is within reach that we can do better.)
• Give hints about the real time hadron formation. (Although this is usually considered to be hopeless, it is not impossible. I shall elaborate on this remark a little bit later.)
• Inform about hadron properties near $T < T_c$ in the confining phase. (Mass, width, hadronic sizes are of interest.)
• Help to clarify string dynamics. (String melting to color rope and string breaking in case of non-trivial, higher color multiplets at the ends.)

Coming back to the question of squeezing real time information out from lattice QCD, let me outline, what the problem blocking the way here is. The main difference between real and imaginary time treatment can be comprised into two versions of the path integral formula. In the Euclidean version,

$$\langle \mathcal{O} \rangle_{\text{Euk}} = \frac{1}{Z} \int \mathcal{D}U e^{-S[U]} \mathcal{O}(U),$$

$\mathcal{O}(U)$ is the observable (any operator with physical meaning) and $\mathcal{D}U \exp(-S[U])$ is the weighted integral measure in the field-configuration space allowing for Monte Carlo type simulation techniques to converge in an acceptable time to the expectation values we are seeking for.

The real (Minkowski) time version of this formula,

$$\langle \mathcal{O} \rangle_{\text{Min}} = \frac{1}{Z} \int \mathcal{D}U e^{iS[U]} \mathcal{O}(U),$$

would lead us to treat $e^{iS[U]} \mathcal{O}(U)$ as an observable (because the complex $\exp(iS)$ factor is oscillating now and is therefore useless for Monte Carlo). It would burden a hopeless task upon us: to integrate over all possible configurations (with the measure $\mathcal{D}U$ only). On a lattice with $N$ elements (sites, links, etc.) even in the case of a most simple-minded two-state (Ising) model this means an order of $2^N$ contributions to the wanted integral. This is exponentially prohibitive, akin to algorithmic NP problems, which cannot be solved in a time polynomial in $N$.

In principle there can be two ways out from this problem, one of them may become viable already in the near future. At finite temperature and short enough real-time problems the re-weighting technique – already applied in finite $\mu$ calculations – can be of use. The other way today still sounds like science fiction, being hopeless yet not impossible: using quantum computers and corresponding q-bit manipulating algorithms which would solve the NP problem in general. We should watch the development in this field in the future.

Last but not least our QGP-duck may be already flying. Flying in the high skies of astrophysics and cosmology. Let me try to reconcile the main message here. The basic idea is that if and whenever a star according to some observable properties like size and brightness occurs to be abnormal, two possible reasons could hide behind:

(i) the core or the whole of that (neutron) star consists of an exotic material with non-nuclear equation of state, or
(ii) a misjudgment of either observable led us to a wrong conclusion.

For such an exotic material we have heard quite a few suggestions. Ariel Zhintinsky talked about Q-balls tightened together by axion domain walls (unfortunately no sign of any axion has been observed yet), Jeffrey Bowers about color crystals, Madappa Prakash about strange quark matter, several others about color-flavor locking or kaon condensate. Markus Thoma has shown us nice photos of the Chandra satellite and of the star RXJ1856. This star was a candidate for being extraordinarily compact with a radius of $R < 6$ km, and still too bright in the X-ray spectrum. Several speakers concluded, however, that in this case the size of this star has been misjudged. The following two figures borrowed from Markus' talk show that present astronomical data would exclude pure strange quark matter stars, but not hybrid stars with quark or mixed phase core (Fig.16). Using another equation of state promoted by Prakash and Lattimer there even could be a version of strange quark star compatible with the allowed range.

Following this review of main themes and rhymes of this conference I would now like to summarize the main achievements. My third prepared transparency had two words on it: “Explanations” and “Predictions”. Two nice, exclusively non-hadronic explanations of the difference in proton and pion flow asymmetry at RHIC were given by Dénes Molnár (based on parton recombination) and by Ulrich Heinz (based on non-hadronic EoS in hydrodynamics).

Also a few predictions has been made. Sean Gavin suggested a dcc signal observable in charge fluctuations, Joe Kapusta predicted thermal baryon - antibaryon pair creation rates from QGP. An important prediction of lattice QCD was presented by Péter Petreczky. He has shown that with $2\sigma$ confidence the $J/\psi$ ground state does not disappear above $T_c$. Even a quantitative value for the width of this resonance has been given ($210$ MeV at $T = 1.08T_c$). Finally Prakash has made a prediction pointing out much smaller time scales for strange quark stars in a neutron-star black hole binary system than for normal neutron star partners, $\tau_{SQM} \ll \tau_{NS}$, in the pre-merging stage. This should, in principle, be seen in the modified spectra of gravitational waves.

I am now almost at the end of this review, so – as a summary of the summary – let me put on a list of dualisms. These are dual views which have to be resolved in order to achieve further progress in the theory of our field. In general dualisms can be resolved in two ways: either by fusion, merging to a more complex view, or by decision, which view is correct.

- **Thermal vs. combinatorial** models of hadronic final states. We hope to be able to decide here some day.
- **Parton vs. constituent quark** picture. Here an interpolating or unifying model should be searched for.

|| I swear it was otherwise empty. Now I can fill this form.
¶ according to himself just last week
+ as the particle – wave duality had been resolved by working out quantum mechanics
Real time vs. imaginary time problems treated in lattice QCD, related to the question of statistical weights and observables. Here a case by case decision seems to be favorable.

A very important dualism existed between first order and higher order phase transition or crossover at color deconfinement. This question now seems to approach clarification quickly.

Last but not least a shortlist of topics which were not on the official program, although many of them have been mentioned or referred to. Finite $\mu$ lattice calculations should have been worth for a self-contained review. The quark recombination model ALCOR, pioneered by József Zimányi as well as ideas like colored molecular dynamics, gluon saturation and colored glass condensate were only indirectly talked about.
Now we have arrived at the conclusion. The conclusion of the theory part of the SQM2003 conference is straight and simple. This conference has been a success because

a) now we understand more than before, and
b) we have got some idea what needs to be done.

A side remark to the discussion about the future of this conference series: please, do not change the name! We are already fond of it. The phrase “Strange Quark Matter” does not exclude any new, good physics ideas related to heavier flavors or other aspects. Finally let me thank the organizers, first of all Berndt Müller and Steffen Bass for providing us this inspiring atmosphere and to you all for making the meeting to a success by your contributions as speakers and listeners. Thank you!

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