

Presented at the Symposium on Relativistic
Heavy Ion Research G.S.I., Darmstadt,
Germany, March 10, 1978

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SUMMARY TALK AT THE
SYMPOSIUM ON RELATIVISTIC HEAVY ION RESEARCH
G.S.I., DARMSTADT, GERMANY

Hermann A. Grunder

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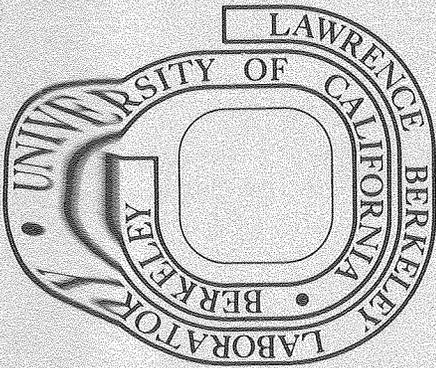
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SUMMARY TALK AT THE
SYMPOSIUM ON RELATIVISTIC HEAVY ION RESEARCH
G.S.I., Darmstadt, Germany
March 10, 1978

Hermann A. Grunder,
Lawrence Berkeley Laboratory

Madame le Présidente, dear colleagues, ladies & gentlemen:

It is a great honor and a pleasure for me to give the summary talk at the final hour of this most stimulating symposium. We have covered a wealth of scientific information and will leave this symposium enriched in knowledge.

I am very impressed that representatives of the German government, Drs. Rembser and Schött, as well as representatives of U.S. agencies, Drs. Erskine and Wildenthal, were able to experience the excitement which relativistic heavy ions have created at this symposium.

Since I know how much our Director for Nuclear Physics in the U.S. Department of Energy would have wanted to be at this symposium, let me share with you a few of his well-chosen sentences, which summarize the scope of the symposium so well. This is a memo addressed to Dr. Tony Upchurch, Budget Fiscal Officer, from George Rogosa, Acting Director for Nuclear Physics, DOE (Fig. 1).

The next summary I would like to make is my own, and of course cannot compete with George Rogosa's.

We all recognize that Quarkmatter physics--or whatever one's preference for the name of this field happens to be--has of course been the central topic of this conference. But we must not overlook that there are very important related fields from which investigators are most welcome as colleagues. They must be assured of a generous, genuine and continuing collaboration. Standing together in this scientific endeavor will make all of us richer for it.



Department of Energy
Washington, D.C. 20545

February 24, 1978

MEMORANDUM FOR: Tony C. Upchurch, Budget Fiscal Officer
Program Support Office, IA

FROM: George L. Rogosa, Acting Director
Division of Nuclear Physics, HENP

SUBJECT: SYMPOSIUM ON RELATIVISTIC HEAVY ION RESEARCH

I offer a few comments on the scope of the planned Symposium, its importance and justifiable extent of U. S. participation.

First, the Symposium covers three areas requiring different types of scientists. Relativistic heavy-ion research is new and exciting. It is a frontier area of nuclear research. There is only one place on planet earth where it can be carried out at an accelerator facility. That is at the Bevalac at the Lawrence Berkeley Laboratory.

Second, there is substantial interest in the use of heavy ions for cancer therapy. This possible biomedical application is being pioneered at LBL. The Symposium will also cover this field.

Third, a-few people including _____ have stirred up U.S. interest in heavy ion inertial fusion to the extent that DOE is now spending in the several millions annually on this most interesting prospect of a new energy source. -The-Symposium will address this-topic as well.

Therefore, we have three symposia all wrapped up in one because of interests in the use of relativistic heavy ions.

• • • • •

Do you believe in pion condensation or abnormal states of nuclear matter or shock waves in nuclear matter? _____ could likely tell us.

Please let me know if you need any additional information.

Enclosure:
Symposium Program

Original signed by
G.L. Rogosa

Director Division of Nuclear Physics DOE

I would like to make a few points on the involvement of Biology & Medicine. Radiobiology, in-vivo and in-vitro, is a very important aspect of heavy-ion work. In this conference we have heard about the oxygen enhancement ratio (OER); we have heard about depth-dose distribution; we have heard about computer-assisted transaxial tomography with heavy ions. These fields are now being actively investigated. At LBL we will next undertake pre-clinical patient treatment. However, as Dr. Alpen has pointed out, it is evident that at some point the medical community will want its own accelerators--hospital-based, reliable, under the control of the radiotherapists. Until that time, a fruitful collaboration is not only responsible, it is essential if we are to make good use of our resources.

Biology & Medicine has greatly profited from the instrumentation and from the beams which physics has provided. But physics has also profited a great deal. For example, through radiobiology and through medicine some efforts in physics have become understandable to the public at large--and this point should not be underestimated.

A lot remains to be done in radiobiology and medicine, but we have started. I am happy to be part of a laboratory where a number of radiobiological investigations with heavy ions are being conducted, but I am eagerly looking forward to the time when heavy-ion radiobiology and radiation physics are under investigation throughout the world.

Another neighboring field of relativistic heavy ion physics, which we have discussed, is heavy-ion fusion. HIF needs to be put in focus. There are no easy solutions to the enormous problem of adequate energy supply, and it is in this perspective that we must understand the efforts to explore any avenue which has a possibility of success. Therefore, the question is not, "will it work?"--the question is, "does it have a chance?" And if the answer, as we undoubtedly heard at this conference, is "Yes, at this time," then it has to be pursued to its logical

conclusion. This means enough data has to be accumulated in order to compare it with other energy-producing sources. The figures of merit are: complexity, cost, environmental and social impact.

We know of energy sources today which would be readily available, but they are either environmentally or socially unacceptable. Whatever we personally may think about it, this is reality. Finding ourselves in this situation, it becomes the responsibility of this community to do our best to reach a conclusion as soon as possible--a responsibility which has been emphasized by many speakers at this symposium. If this requires a large effort in manpower and expenditure, then that has to be acceptable. This is a problem of crucial significance to our future.

I will say little about pellets. We heard an excellent talk on this subject. Present-day pellets are designed mainly for lasers. Not much work has been done yet on pellets specifically designed for heavy ion fusion, except for some calculations.

I would like to remind you of what Lee Teng mentioned in his paper--the specifications we should be aiming for in Heavy Ion Fusion (Table 1). In this field there is ample room for bright, new, hot ideas. For the last two years accelerator physicists and pellet designers were somewhat lost in the vast parameter space, searching for guidelines and landmarks. By and large it will simply be plain, hard work which will get us there. The charged heavy particle with its high-mass, low charge-to-mass ratio and definite range certainly seems to have the edge for pellet fusion, particularly in combination with the high-repetition rate technology of accelerators. We have not progressed enough with ion sources and low-beta structures to produce optimum designs.

The problem would be a great deal easier if we could trade energy for current. As Kjell Johnsen observed at the recent Brookhaven Conference, and again at this seminar, "the energy we can buy; the current is given by nature, and we have to take what we can get;" so that should be an encouragement to pellet designers to allow us the highest possible energy.

TABLE I

BIOLOGY & MEDICINE

RADIOBIOLOGY (IN-VIVO AND IN-VITRO)

RELATIVE BIOLOGICAL EFFECTIVENESS (RBE)

OXYGEN ENHANCEMENT RATIO (OER)

DEPTH DOSE DISTRIBUTION

COMPUTER-ASSISTED TRANSAXIAL RADIOGRAPHY (CATR)

PRECLINICAL TREATMENTS

TREATMENT-PLANNING FOR CHARGED PARTICLES.

The R & D on heavy-ion fusion is not a scientific endeavor in its usual sense, where we scientists are free to do our own thing. It is R & D of ultimately commercial application. I hope that the next phase will be assignments to capable groups of subsystems and/or problems. Then we as a community can proceed in parallel with the multitude of problems facing us and find out sooner where this is leading us. I encourage you to support this effort to the fullest.

For many of us, our current principal interest is relativistic heavy ion research as a tool of intellectual adventure. We are keenly aware that relativistic heavy ion research has been an illegitimate child of the nuclear physics community. Linger- ing evidence is the rather inadequate instrumentation thus far available to experimenters with relativistic heavy ions. As a consequence, we recognize that our present experimental efforts are not commensurate with the difficulty of the task. There is much to be done experimentally, and it turns out to be a little more difficult than some of us expected it to be.

At the beginning it seemed apparently clear to most of the physics community that nothing useful could come out of relativistic heavy-ion collisions. The fact that this conference is taking place is evidence of a changing attitude. But let me hasten to add--so far we haven't really discovered anything spectacular. There are many good reasons for this; there are also excuses, but we know science is made of facts, not of excuses. At this point I would like to mention the old proverb: "An ihren Taten nicht an ihren Worten sollt ihr sie erkennen," which translated means, "Get on with the job."

As a practical matter, what have we learned about relativistic heavy ions so far?

We have learned to cope with kinematics; we have found temperature but not yet density; we were enamoured with single particle inclusive spectra--a nice love affair; it has given us some good insights, but it has to end. (Table 2)

TABLE 2

HEAVY ION FUSION SPECIFICATIONS

ENERGY \geq 1 MJ

POWER \geq 100 TW

ENERGY DENSITY \geq 40 MJ/GM

SOME PULSE SHAPING

We have also learned that conventional 4π detectors such as emulsions, silver chloride, plastics and streamer chambers are good survey instruments; they are tedious in the analysis, but they have contributed to the imagination and to the vision of what the physics could be in relativistic heavy-ion collisions.

We have particularly learned something which I barely have the courage to write down--that a handful of part-time physicists just cannot do justice to the field of relativistic heavy ions. Something has to be done here--and this is true on both the experimental and the theoretical side.

There are plenty of tantalizing speculations and expectations, which are of course the work of many a dedicated theoretician; and in that aspect, we are greatly thankful to them. I think we can truly say that the majority of theoreticians have recognized the potential that nuclear-nuclear interactions at high energies can't just be treated as an extension of nuclear physics.

The collaboration between theoreticians and experimenters in the last two years has been too loose. You no doubt recall this discussion: A theory fitting within a factor of 2 or 3, missing some major features; finally a renormalization questioned the whole theory; we renormalized the theory, and so on... But we really have to go after very specific features; and here we experimenters scream: "Help!--theoreticians, give us something real; give us some signatures which we can detect." We are indebted to George Rogosa (DOE), who recognized this several years ago, and initiated the augmentation of theoretical capabilities at a number of laboratories.

Any new field lives to some extent on speculations. This keeps the enthusiasm up, but there must be an underlying breadth of significant physics. We are just now coming into the region where we will have a good opportunity to find physics of lasting value.

We know of one recipe theoreticians impress on us--and that is, in part, addressed to accelerator physicists: Measure

excitation functions! All these very speculative phenomena may be found as a bump on a very carefully measured excitation function. Hence we have to promise you that in the future any accelerator will work like a charm if it comes to executing energy changes for excitation function experiments.

I foresee a certain danger--and perhaps it's more local than international. We have experienced at this conference an enormous momentum which will follow many of us home, fired up for better and greater experiments, for more theories and fabulous ideas. But one cannot maintain this momentum in too small a group with inadequate tools. This is of course known as being below threshold.

I occasionally have this terrible dream--I wake up at 4 o'clock in the morning, and I clearly see that density isomers exist, but we haven't gone about it the proper way to discover them. And twenty years later, somebody else finds them! That would be a tragedy. However, if we have given relativistic heavy ion physics our best effort, and there happens to be only nucleon-nucleon scattering and kinematics, then we can rest in peace.

Having shown you in Table 3 what we learned about relativistic heavy ions--and there should obviously be a couple of etceteras because this list is by no means complete--let me summarize what I understand should be the next step (Table 4). Singling out central collisions with a good trigger has become a central problem. It is currently accepted that multiplicity is a signature of central collisions. Some of us have lived through the struggle of convincing ourselves that this is so--I still hope it's true.

Excitation function experiments should look for pi-multiplicity, large transverse momenta, and other threshold effects. Two-particle correlation experiments are an absolute must in the next round of data.

One participating physicist said: "I can't believe it--these guys have this opportunity and haven't done any two-particle correlation measurements!" Why haven't we? Well, today we don't

TABLE 3

WHAT HAVE WE LEARNED ABOUT

RELATIVISTIC HEAVY IONS

1. TO COPE WITH KINEMATICS
 2. WE FOUND HIGH TEMPERATURE
BUT NOT HIGH DENSITY
 3. WE WERE ENAMoured WITH SINGLE PARTICLE
INCLUSIVE SPECTRAS.
 4. SIMPLE ~~4 π~~ DETECTORS SUCH AS EMULSION, AgCl,
PLASTICS AND STREAMER-CHAMBERS - ARE TEDIOUS
IN THE ANALYSIS BUT VERY STIMULATING.
-
5. CENTRAL COLLISIONS ARE RELATED TO HIGH
MULTIPLICITY.
 6. EXPERIMENTS LACK THE FEATURES OF A DISCRIMINATING
THEORY.

TABLE 4
WHAT TO DO NEXT :

1. SINGLE OUT CENTRAL COLLISIONS
MULTIPLICITY TRIGGER
 2. EXCITATION FUNCTION EXPERIMENTS
 - π - MULTIPLICITY & $\sum |p_T|$
 - ANY OTHER THRESHOLD EFFECT
 3. TWO PARTICLE CORRELATION EXPERIMENTS
 4. FIND MESSENGERS OF EARLY STAGE OF COMPRESSION
(α, γ, π, K ETC. ETC.)
 5. DETERMINE DEGREE OF EQUILIBRATION (10^{-22} SEC)
 6. SEMIPERIPHERAL COLLISIONS - ASSYMETRY - HOT SPOTS
-
7. ATTEMPT EVENT BY EVENT ANALYSIS OF HIGH MULTIPLICITY INTERACTIONS
 8. THEORETICIANS: HELP US FIND SIGNATURES!

talk about money--we talk about ideas. One of the fundamental difficulties of relativistic heavy ion physics is the high-energy physics and the nuclear physics aspect of the field.

A distinguished colleague recently said to me--and I paraphrase him: "This field is not quite nuclear physics, but it isn't high-energy physics either. So, although we must use their tools, we must not simply imitate the high-energy physicists. In the combination of the two disciplines we will find exciting physics."

Returning once more to the theoreticians and their importance to this and other new fields. Theorists are very important; they stimulate and excite our interest by predictions of fabulous phenomena to be found. Many of these predictions may be wrong, but that's not important; they give the experimenter time to go about his business and to find equally exciting events that do occur.

This conference was dedicated to the unusual. We came here ~~because we wanted to hear about new and exciting things. Hence~~ it was proper that we address the unusual. But let's also remember that there is physics in other energy regions of heavy ions--physics in a more classical sense, which should not be neglected. These areas of physics must and can complement each other. One should not study one field to the exclusion of the other.

At this conference it has been most rewarding to see biologists, physicists, theoreticians, and other concerned professionals communicating with each other.

Allow me this personal comment--What have I learned about the field of relativistic heavy ion physics? 1) There are plenty of theoretical speculations. 2) An extended experimental program is sorely needed. 3) An eager and capable experimental community must continue, and 4) A (hopefully) benevolent funding agency is essential. These four factors constitute a recipe for success.

Let me finish with a few remarks about accelerators. It is significant that the relatively modest experimental output from the Bevalac has spawned so much activity in other places. We now have Dubna with a heavy ion beam; we will have Saclay with a heavy ion beam within a short time. Also, we have two serious studies in progress--one here at GSI and one in Japan (the Numatron).

As some of the speakers have already made clear, a lot of work goes into these studies, because many possibilities must be explored. It is essential to study all of the problems, and go into a great deal of detail, but one should still be willing to change a proposed machine, if time or circumstances so demand. We have done much fundamental work on these accelerators, and a number of problems has come into focus.

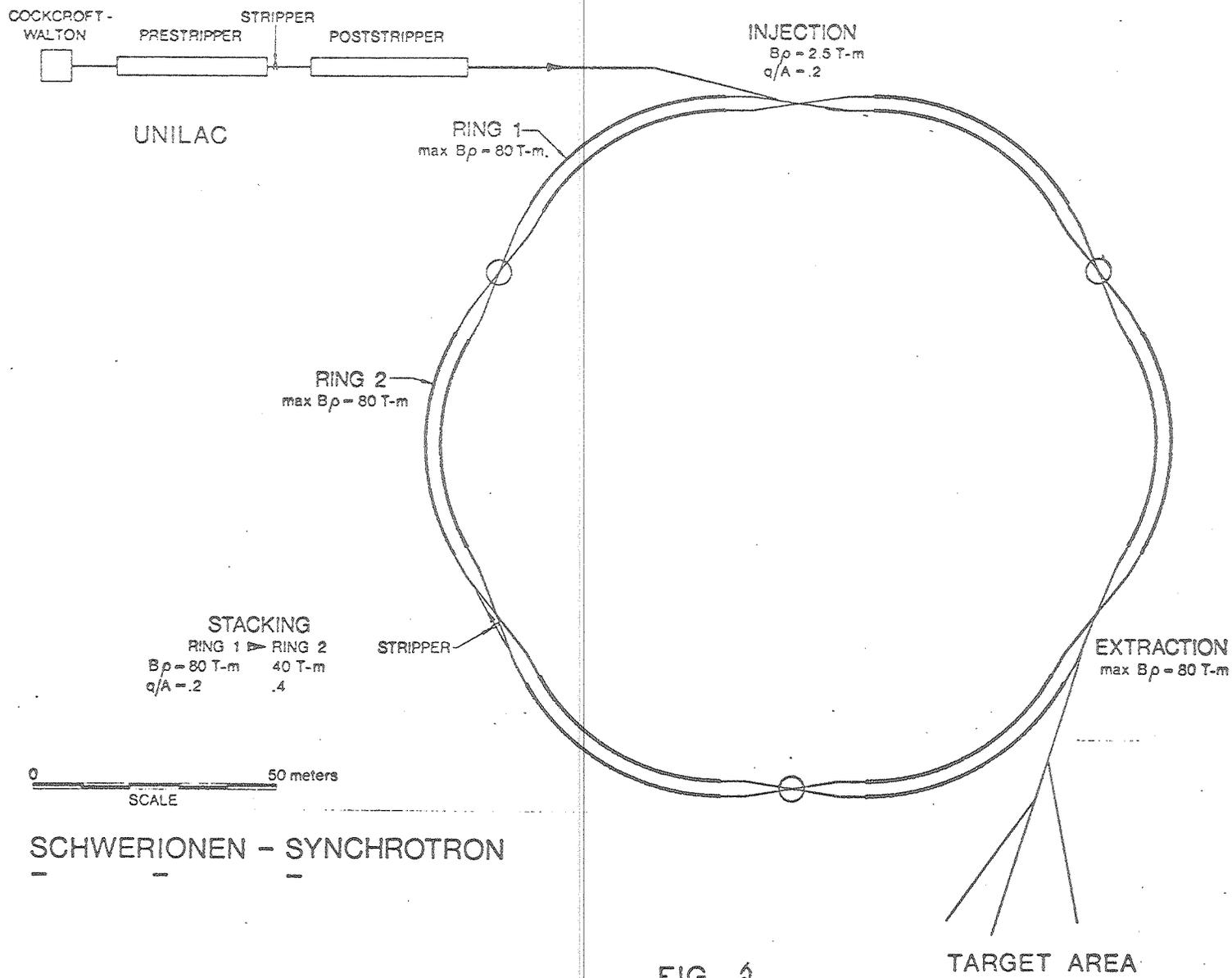
Let me show you a picture of an accelerator in the 30 MeV/u to 10 GeV/u range which we have been considering at LBL (Fig. 2). This would be a satisfactory increase (5 x) over top Bevalac energies with excellent duty cycle, and would satisfy those experimenters who want to extend the Bevalac capabilities, but without giving anything up.

Now, I would like to show you an accelerator to satisfy those people who really want to explore the unusual--quark bags and the like, who want to see collisions of nuclear matter, say tubes 400 fermi diameter, at very high center-of-mass energies (Fig. 3). It is, we think, a very fortunate circumstance that the two pictures are so similar.

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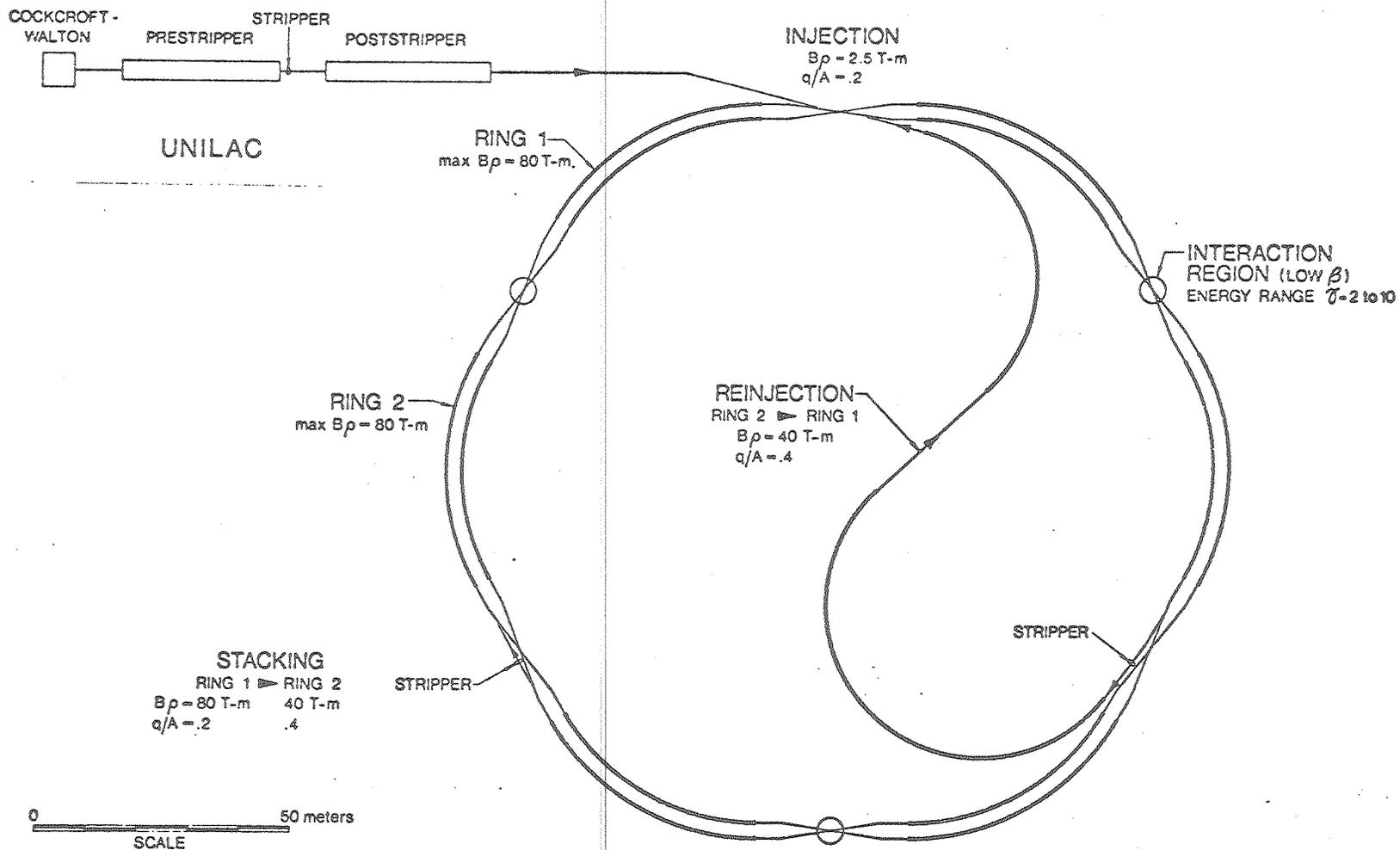
I am sure I speak for all the guest scientists in expressing my sincere gratitude to Professor Ch. Schmelzer--our gracious host--who has done so much for the physical sciences, and particularly for the field of heavy ions.

I would like to congratulate the Program Committee for the courage of including in this symposium the entire field of



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FIG. 2



SUPRALEITENDE SPEICHERRINGANLAGE

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FIG. 3

relativistic heavy ions with the very important applications in Biology & Medicine, and in heavy-ion fusion.

Professors R. Bock and R. Stock, and their helpers, deserve our gratitude for the enormous task of organizing such a symposium, and then keeping it on track.

Last, but not least, let us thank the charming ladies--Mrs. Eishold and Mrs. Fenzlau--for their helpfulness throughout the conference.

I acknowledge with pleasure the many helpful discussions with Professors Bock and Stock during the difficult period of selecting the scope of this talk.

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UNIVERSITY OF CALIFORNIA
BERKELEY, CALIFORNIA 94720