JiAPS

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Central Office Report

The ICPS'97 Is Coming Up!

Interview with James Bjorken

Summary

Credits

JiAPS is the Journal of IAPS
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The Editor Speaks

I know you couldn't wait to see the second issue of JiAPS (I certainly couldn't wait to get it over with) and, since *«what is not forbidden is compulsory»* (Feynman), it had to happen: here it is again, for your rejoicing and my rest.

Everybody seemed very enthusiastic about JiAPS when they read the first issue, and my mailbox was flooded with compliments (I have a very small mailbox, you know), which I really appreciate. However, that enthusiasm soon waned (in fact, as soon as I started asking for contributions for the second issue) and we have less articles in this issue than we did in the first. I can't say I appreciate this, though.

It seems we will be trying to include some Physics in the Journal (just to prove a point) and the little Physics you will find in this issue is in the form of problems (and a very *Dutch* science quiz, but I'll let you see that for yourselves). Please, try to solve the problems and send in the solutions. If you don't like my problems, send in problems you like! If you *do* like my problems, well, send in your own! It would be very interesting to publish articles about physics as well. They don't have to be too long or too advanced (just good Physics). I know a lot of you are capable of producing a paper for Conference Proceedings --you've done it before--, so don't say you can't write an informal introduction to some topic you're interested in. If you can write a Nobel-Prize-winning paper, then I'd be more than glad to publish it as well! Just so that you don't have the excuse available, it's not absolutely necessary to be interested in a topic to write about it --just look at your lecturers, how many are interested in what they're lecturing about?

In this issue you will find another Central Office Report; an account of IAPS' participation in the UN World Youth Forum; a superb interview with James Bjorken (yes, the famous particle Physicist --I'm afraid his picture is not big enough for a poster, but you can carry it in your wallet) sent by our friends in Lisbon, and the Dutch Science Quiz (you can't expect anything serious from the Dutch in IAPS, just downright *fun* stuff). Am I leaving something out? I'm afraid not. By the way, I am particularly thankful to the guys at Physis (prospective member of IAPS and soon to be Portugal NC). A fair share of this issue could not have been done without their help.

The cover picture is the Logo of the UNWYF (boy, do they like acronyms at the United Nations). and it's there because I already had lots of other pictures to put with the article and it's a nice picture to be on a cover anyway. As the joke goes, «My son, one day all this will belong to Bill Gates».

Read and Enjoy,

Miguel Carrión
JiAPS Editor

Editor's Note: *This article was written by Bente Hansen in the name of the IAPS representantives at the UNWYF:*
Ms. Bente Hansen (4π)
Mr. Oscar Pleguezuelos (President of IAPS)
Mr. Stephan Witoszynskyj (ICPS 97 Organizer and the man in chargue of the IAPS stand

Future Leaders of the World?

The United Nations World Youth Forum (UNWYF)
(25-29 November 1996, Vienna, Austria)
By Bente Hansen, 4π (Past Past Past President)

***Future leaders of the world to gather in Vienna.* This was the title of the press announcement the World Youth Forum <*http://www.blackbox.net/wyf*> of the United Nations System had released before the event 25th to 29th november 1996. With these brilliant prospects for my future career as almost the only knowledge in advance about the World Youth Forum (WYF), I was really curious to meet the other 349 coming world leaders.**

It is not every day that you get such a chance. My luck seems to be that I have the right sex. The organizers of the WYF had decided on a policy of positive discrimination of women: The selection of invited youth organisations should participate with both a male and a *female* higher representative, and there aren't really that many female members of the executive commitee of IAPS (none at the moment). I was president of IAPS 1993-'94, and remember having inscribed IAPS into a book called *Who's who.* The book was an international guidebook of youth and student organizations, and I think our presence there is the reason for the WYF to invite IAPS to participate. Other invited youth organisations were: a lot of National Boy & Girl Scouts, a lot of National Youth and Student Councils, Young Revolutionary from Benin, Creative and Gifted of Serbia, Young Women's Christian Association of Zimbabwe, Ukrainian Liberal Youth, World Organization of Young Esperantists, etc... I met only four other international student associations: Pharmacy, History, Medicine and Leisure-Time Studies (the last one is my favorite after Physics!!).

***Óscar at the Forum Session,* Photo by Bente Hansen**

The goal of the WYF was to promote the implementation of the World Programme of Action for Youth to the Year 2000 and Beyond (a document of youth politics adopted by the UN General Assembly in 1995) and to promote improved and new channels of communication between the youth organizations and the UN. The Forum was displayed at the United Nations buildings in Vienna. Discussions were held there in 12 working groups <*http://www.blackbox.net/wyf/programme.html*>. Oscar and I had chosen working group no. 5: "Youth, Environment and Sustainable Development" <http://www.blackbox.net/wyf/wg/5.html>, which came up with 3 concrete project proposals. Oscar was made responsible for one of them: an interactive homepage called "Youth of the United Nations Eco-Awareness Network" (YUNEAN).

***The Stand of IAPS,* photo by Óscar Pleguezuelos**

One thing I found a bit bizarre for a youth forum was the fact that more than half of the participants seemed to be professional young people aged over 30, travelling all over the earth representing the youth and promoting their own career. «Did you really think anything would come out of this conference?», was a standard sentence during the WYF. As before the WYF I was only a naïve young person (young is by the UN definition until the age of 30) not yet familiar with UN conferences, I actually thought there was a genuine interest on the part of the UN agencies - or at least the Youth Unit - to listen to the voice of the youth organisations. I thought the WYF would be a process

***Main Entrance of the UN Vienna Site*, photo by Óscar Pleguezuelos**

of two-way communication, but my impression of the Forum is that the main goal was to teach the youth organizations about the UN agencies, which in turn were hermetically closed towards new ideas and interfered with the youth organizations. My judgement may be to negative, as representatives from some of the UN agencies supported the voices of criticism during the WYF. But especially the Youth Unit insisted on overlooking the suggestions for improved communication: there were a European and an African document which can be found at the WYF homepage. Also Oscar, 5-10 other participants and I spent most of a night making a document of recommendations *<http://www.blackbox.net/wyf/rec/rec.html>* and translating it into the three languages of the WYF. The main problems mentioned here were insuffient translation, untrained and biased chairs of some of the working groups and lack of preparatory information. 110 participants signed the document, but the Youth Unit didn't recognize it. The UN Youth Unit invited 350 people from all parts of the world to listen to speaches (53 speaches the first day!!), having symbolic discussions in working groups, and finally as a result of the Forum making a statement of support for the World Programme of Action for the Youth to the Year 2000 and Beyond, thereby rubberstamping this document as youth policy.

I am afraid of being too negative, but the above description was the general impression I got from sitting in the plenary hall. I would like to illustrate this with an example from the final session. All participants' attempts of a two-way communication were firmly rejected by the chairperson of the UN Youth Unit:

* One member of the Environment Working Group: Question of why the UN Youth Unit were unwilling to symbolically support one of the projects of the working group. Chairman: Not willing to answer the question or comment on it.
* Oscar (yes, our President): Presented the signed recommendation document, wich was given in advance to Bill Angel from the Youth Unit. Chairman: Not willing to accept the document or any suggestions or comments from the participants in general.
* Oscar: Question for translation of the final result-document of the WYF into all the languages of the Forum. Reaction: No translation and closing down of all interpretation in the midle of the meeting, because of working hours...

It seems odd to invite people from all parts of the world to participate in a conference without really being interested in listening to them. Personally it made me wonder what the intentions of hosting the Forum were on the UN's part. The problems and questions regarding the procedures are in line with the general criticism of the UN coming from some nations, observers and participants from both earlier and later conferences (for example after the WTO conference in December). If the UN wants to survive its present political and economic crisis, and avoid disatrous incidents (e.g. the lack of intervention in Rwanda), which arises because of ineffectivity and supressing of information, the beginning is a discussion of whether it would be better to replace the present consensus policy in UN with more democratic and open procedures.

In spite of my rather tough criticism, my overall impression of the WYF is that all in all I met some very talented and wonderful persons at the WYF. Their main concerns did not seem to be whether they will be the future leaders of the world. Instead they had a living interest in their organisation, in the United Nations system and in the important issues which vere discussed during the week in Vienna. That is why an international conference (see *Hail ICPS* on the following page) for youth is a great idea.

***The members of the #5 Workshop*, photo by Óscar Pleguezuelos**

Where is IAPS going?

Central Office Report

by Óscar Pleguezuelos, President of IAPS

Being part of the Central Office of any organization requires a certain degree of commitment to the ideals the organization represents, as well as a clear knowledge of the real dimensions of the association, its domain of action and its members' capabilities, its goals and its possibilities, and above all how to implement in practice the always beautiful goals inscribed in the Charter.

Thus, among the objectives Daniel, Franjo and I set for ourselves more than six months ago, when during the superb ICPS'96 in Szeged we were appointed IAPS Central Office, were to study its actual state, as well as to assess its possibilities and, based on them, set the guidelines of our activity. So we started off checking e-mail daily, and answering it withing 72 hours, actualizing the existing information on IAPS members, as well as trying to involve all of them in IAPS work, though the distribution of information bulletins and the creation of Workshops.

However, we realized that limiting ourselves to these activities would mean perpetuating continuism during our time as Central Office, so we developed an action plan aimed at increasing the international presence of IAPS (both through new members and active participation in international events) and spreading our name and word of our activities in the field of Physics. Notwithstanding, we were soon aware that IAPS' instability (due its Central Office moving on a yearly basis) might be a big handicap when trying to further our work in the years to follow, so we started to take the necessary steps to get a permanent postal address (and a place to keep the archives), as well as the funding required to redirect IAPS mail to future Central Offices.

As to the expansion of IAPS, thanks to our participation in the World Youth Forum of the United Nations System we had a chance to introduce IAPS to youth from 140 different countries and 200 different organizations. We also had the chance to establish personal contacts with the leaders of other international organizations and to set new goals for the future of IAPS. Among these, there is the project of this Central Office to cooperate with the EMSPS (European Mobility Scheme for Physics Students) in student exchanges, systematically providing assistance (though our members and contact persons) to the students when they reach their host university, in order to ease their first steps in the new university and make their cultural integration faster and more pleasant.

Finally, I would like to congratulate Mr. Miguel Carrión in the name of the Central Office and myself for the magnificent job done by him in editing the first issue of JiAPS.

In sum, there is much left to do and not many hands to work with, so I would like to remind all of you once again from the IAPS Central Office, though I risk being a pain, that:

IAPS is more than a Central Office
All of us are IAPS, and ours is the future.

Hail ICPS!

by Óscar Pleguezuelos, IAPS President

There was one organisational matter of the Forum which Stephan Witoszynskyj, the ICPS'97 organizer, were shocked to see. The WYF opening party was non-alcoholic! I must admit that it was a very quiet party, and that people didn't mingle much. During the whole WYF the evenings and social gatherings didn't play as important a role as they do during an ICPS. My experience from several ICPS events is that the informal getting together is essential for creating mutual understanding and cooperation.

Dutch National Science Quiz 1996

courtesy of Remco Hammen and Olav Frijns

Olav Frijns and I are very happy to send you the English translation of the Dutch National Science Quiz 1996. As an introduction to the quiz, let me give you some background:

The quiz was organized by the Dutch Organisation for Scientific Research (NWO) and a Dutch scientific television program called *Noorderlicht.* The questions were published in 15 Dutch national and regional newspapers, and over 10.000 people participated. The quiz was intended for the general public, so not specifically for science students, but when answering the questions, some scientific background can really help. Of the 10.000 participants, 11 persons made no mistake (Ramón de Vries was one of them). People aged 19-24 performed best, with an average of 62 % of the questions answered right. I'm curious what the average of the IAPS participants will be ;-)

And the Winner Is...

Ramón de Vries, our Past president, actually won the Dutch National Science Quiz! A lottery decided that he, of the 11 persons who answered all questions, would win the 1st prize: a trip to China! Of course, you are all invited to congratulate him [Editor's Note: and crash his mailbox] at <ramon@ph.tn.tudelft.nl>.

In the original quiz 3 possible answers were given, but while Olav and I translated the quiz, we decided to add some extra possible (sometimes not so serious) answers to the questions...

Okay, the most important part were the questions, so ....

Question 1:
What is the easiest way to pull a cork out of a bottle of wine with a corkscrew?
A. By pulling the cork perpendicularly out of the bottle.
B. By shaking the bottle to increase the pressure on the inner side.
C. By pulling slowly at a small angle.
D. By pulling hard and turning at the same time.

Question 2:
In a cellar you find a bottle of cognac VSOP ('Very Superior Old Pale') dating from the beginning of this century. Assuming that cognac is still produced in the same way, what is the difference in taste between this very old cognac and a bottle of VSOP made nowadays?
A. The old one is probably contaminated.
B. The old one has a much more mature and deeper taste.
C. After a few glasses you won't ba able to tell the difference anymore.
D. There is no difference in taste.

[**Editor's Note:** How did *Mr. Nur Limonade* come up with this one?]

Question 3:
Which flying object is energetically the most efficient during a routine flight at cruise speed?
A. An UFO.
B. A Boeing 747.
C. A carrier pigeon.
D. A Concorde.

Question 4:
What is in principle the most reliable tool to measure time?
A. A pulsar.
B. A quartz crystal.
C. A laser.
D. A sundial.

Question 5:
You hold a funnel upside-down and push a ping-pong ball in the funnel against the opening. When you let go of the ball, it will fall. What will happen in you release the ball while blowing hard through the funnel from the upside?
A. The ball will fall faster.
B. You will swallow the ball.
C. The ball will fall just as fast.
D. The ball will float in the funnel.

Question 6:
What caused the Standard model to lose credibility for a short time last year?
A. The opening speech of the IAPS President at the ICPS'96 in Szeged.
B. A publication about life on Mars.
C. A publication about proteins which are able to multiply themselves.
D. A publication about the substructure of quarks.

Question 7:
If you assume the chance of giving birth to a boy or a girl is the same for both (50%), what is the chance that

a couple with four children has two boys and two girls?

A. 0.3750
B. 0.2500
C. 0.5000
D. 0.3125

Question 8:
An oblong aquarium is resting with both ends at two scales. Both scales show the same weight. At one of the two ends, a ball is pushed partly into the water. What will happen?

A. At the end where the ball is pushed into the water, the scale shows less weight than at the other end.
B. At the end where the ball is pushed into the water, the scale shows more weight than at the other end.
C. Both scales show the same weight.
D. That depends on the position of the moon in the sky.

Question 9:
Where on the circular area below a conical pile of sand will you find the highest pressure?

A. Right below the top.
B. The pressure is the same everywhere.
C. At a ring at the edge of the pile.
D. At a ring near the centre of the circle.

Question 10:
Why does the Australian Bowerbird (Latin: Ptilonorhynchidae; in Dutch, literally: 'Summerhouse bird') build a summerhouse?

A. To enjoy his holidays.
B. To impress the females.
C. To lay eggs in it.
D. To sleep in at night.

Question 11:
When you stand in front of a mirror and raise your left arm, your reflection will raise its right arm; the top and bottom will remain unchanged in the mirror. What is the reason?

A. The reason is the position of the human eyes.
B. There is some other person behind the mirror.
C. Because a mirror only changes front and back.
D. Because the symmetry-axis of a mirror is vertical.

Question 12:
A book worm wants to eat itself along the shortest path through a encyclopaedia with three volumes. The three volumes, each with and inside of 8 centimetres and a cardboard cover of 1 centimetre at both sides, are standing in the usual order at a bookshelf. The worm will start nibbling at the front cover of volume I. How many centimetres of paper and cardboard has the worm devoured when he reaches the back cover of part III?

A. 0 centimetres.
B. 10 centimetres.
C. 20 centimetres.
D. 30 centimetres.

Question 13:
Why is the clear sky a blue sky?

A. The carbon-dioxide in the air absorbs more infrared light than blue light.
B. Because the human eye is more sensitive to blue light.
C. The oceans colour the sky blue by reflection at the troposphere.
D. Especially blue light will be scattered by the air.
E. IBM (Big Blue) sponsors the sky.

Question 14:
What can be said about the moon during a solar eclipse?

A. The moon is invisible because Venus is blocking the light.
B. It is Full moon.
C. It is New moon.
D. It is Half moon.

Question 15:
What are the characteristics of a species? Species are classes of natural populations the members of which:

A. Are able to breed and produce fertile offspring.
B. Think it is fun to have sex with each other.
C. Share the same hereditary information in the form of DNA.
D. Find their origin in the same phenotype.

Question 16:
When you sit down on a bike with well inflated tires, the tires will dent just a little bit. Why do they dent only a little bit?

A. Because of the increased elasticity of the outer-tire.
B. Because of the larger area of contact with the ground.
C. Because otherwise they would rip open.
D. Because of the decrease of volume.

Question 17:
What is cognitive dissonance?

A. The difference in transport of information between the right and left hemispheres of the brain.

B. Knowing that you know something but being unable to recall it from your memory.
C. Ignoring of facts that are incompatible with an already present idea or prejudice.
D. Going to the toilet even though it is not necessary.

Question 18:
Imagine you want to know how many children per family are attending school. Therefore you take a large random test among schoolchildren, and ask them how many brothers and sisters they have who attend school. With these data the average number of school attending children per family is determined. Is this a good approach?
A. Yes, you will get a good estimate of the average number of children per family.
B. No, children are not allowed to talk to strangers.
C. No, your estimate is too low.
D. No, your estimate is too high.

Question 19:
A rich businessman dies. He has got two relatives: a daughter and a nephew, the son of his brother. A man shows up, who claims to be a child of the businessman; his mother was his mistress. To make sure, a DNA test is done. Who has to be tested, besides the illegitimate son?
A. Bill Clinton.
B. The mistress.
C. The businessman's daughter.
D. The businessman's brother's son.
E. The mistress's butler.

Question 20:
If you are being turned around, your eyes jump from one corner of the eye to the other. When the experiment is repeated with a blindfolded person, this phenomenon appears at first, but disappears after some time. What is the explanation?
A. The eye muscles are getting tired.
B. The brain is getting used to the constant turning and does not control the movement of the eyes anymore.
C. The balance organ does not detect the constant turning after some time.
D. The volunteer will have fainted.

Question 21:
A sphere-shaped bread is cut into seven slices of the same thickness. How much crust is there on the outer slice (heel) compared to a slice out of the middle?
A. More than twice as much.
B. Twice as much.
C. Less.
D. Just as much.

Question 22:
Is it true that a slice of bread with peanut butter which is pushed from a table and falls on the ground, usually falls on the side with the peanut butter?
A. No, this is a fairy tale.
B. Yes, the peanut butter displaces the centre of gravity of the slice of bread.
C. No, the peanut butter causes a lift force which makes the slice float down slowly.
D. Yes, the electrical charge of the peanut butter (heavy stuff!) attracts it to the ground.
E. Yes, this is caused by the height of the table.

The answers to the questions can be e-mailed to <R.Hammen@wbmt.tudelft.nl>. the deadline for sending in the answers is 15 April. This way everybody will have about one month to answer the questions. We think it’s best not to accept any participants from Holland, as they already may have seen the right answers, or can look them up very easily.

Editor's Note: *The following interview was originally published in PULSAR, the Journal of the Technological Engineering Physics Students, Instituto Superior Técnico, Lisbon (issue no. 7 - October 1996).*

Interview with James Bjorken

by Filipe Moura, Pedro Castelo Ferreira, and Yasser Omar

JAMES BJORKEN is known by most physicists mainly for two reasons: being co-author (with Sidney Drell) of two books which have been for many years the main reference to a generation of particle physicists, and discovering the property of electron-proton inelastic scattering which carries his name: *Bjorken scaling.* Last September we had the opportunity to talk with this huge (about 2 meters tall) scientist during the XXVI International Symposium on *Multiparticle Dynamics,* at the University of Algarve, in Portugal. "BJ" (as he is known among is colleagues) is not only a physicist, but also a surfer and an inspiring person to talk with.

PULSAR: We would like to know what your path through physics was.

BJORKEN: Actually, I'm a laboratory person, a "laboratory animal", not a university type. I went through MIT as an undergraduate and then went to Stanford in California as a graduate student. The main reason was that I wanted to be out in the west, in the mountains. That was a very *in* location! I've essentially stayed with Stanford ever since. I started out in the university physics department for 2 or 3 years after I got my PhD. Then SLAC was proposed, and I soon moved over to it (this was the early 1960's). When I joined up SLAC was just being designed, there was absolutely nothing on the site where the laboratory is now. All there was was a warehouse on the Stanford campus with a small collection of offices. I stayed at SLAC until 1979; then I went to FermiLab for 10 years. After that I came back to SLAC, where I am now. So, other than those first few years with the Stanford Physics Department, I've been with accelerator laboratories. I've done very little teaching; it's been almost all research.

James Bjorken, **photo by Ana Margarida Teixeira**

P: What made you choose particle physics?

BJ: In high school I was interested in mathematics and chemistry. After one year of chemistry and one year of physics at MIT, I changed to physics —especially because the professor who taught freshman physics was superb—very inspirational. Physics was also a fashionable subject at that time, just as it is now, and I was caught up in the fashion. Actually I did not like physics in high school; it came across as an uninteresting subject. At MIT, particle physics quickly became for me the most interesting part of the physics menu because it was so fundamental. And MIT had very good people there. Sid Drell was there at that time, and already as a second or third year student I was taking courses with him. It just so happened that when I left MIT he did too, with both of us choosing Stanford. We kept together in that way, and I became one of his many thesis students.

P: You said that you are a laboratory person, not a university one. But why, as a laboratory person, is your work is mainly theoretical, not experimental?

BJ: The good laboratories have very good theory groups. A good laboratory will realize that it is important to have top quality theorists in the middle of their program. The first director of SLAC, Panofsky, a very wise man, realized that especially clearly. Drell also moved over to SLAC about the same time I did. He has been a superb leader of the theory group. He is an inspirational person to work with, and quickly established a very special intellectual climate in the theory group, informal but with high standards, which persists to this day. I had a choice of staying with the university department or going to SLAC. In those days it was much better than it is now. It was usually the question of not whether you could find a job, but rather what job you chose. It is hard for me to relate to the problems nowadays for students getting their degrees and looking for jobs. For not just me but for most theorists who got their degrees, there was a choice of places to go. The field was expanding, back then. Anyway, I had the choice of staying on the Stanford campus or going over to SLAC. And the problem, of course, in going to SLAC was just what you said: it is a laboratory, and would it have the right kind of theoretical environment. So I asked Panofsky in the interview stage, «Supposing I get interested in General Relativity, nothing to do with Particle Physics at all? Would that be ok?» And he said, with no hesitation at all, «Yes». We have the freedom to do the research we want to do. There are two categories of theorists at SLAC: theorists who actually have a formal faculty connection with the university, and another class of theorists at SLAC called staff. Before Fermilab I was in the first category; now I am in the second. For all practical purposes, we all can do what we want to do. But in principle, staff people can be asked to do tasks for the laboratory program—for example to calculate certain things. One of our staff people is Paul Tsai, whose responsibility in early times was calculating radiative corrections, especially in electron-scattering, for example the effects of the initial-state and final-state radiation. It is a very important correction for deep-inelastic scattering, and had to be done very carefully. Tsai is a real expert on that. But through the years he has done his own independent research and is now an expert in the theory of the tau lepton. And Stanley Brodsky, who is now one of our faculty members, was for many years a staff person. So the climate in the SLAC theory group... and I think in almost any large laboratory theory group that I know of... is very good. The CERN theory group is enormous; they do everything there, Superstrings, Supergravity, Quantum Gravity, you name it! At DESY, Brookhaven, FermiLab, Berkeley it is the same. So all of the big laboratories come with very good theory groups. The intellectual climate, in almost all laboratories, is very good.

'In those days it was not a question of whether you could find a job, but rather of what job you chose'

P: What do you think is better? To have research in university or in the laboratories ?

BJ: You need both! You must have theorists in the universities. I mean, that is most important!

P: But, what kind of research do you like more?

BJ: Both. Physicists in the universities, I think, will have more intellectual contact with a much broader range of experts than people in laboratories. People in the laboratories have, on the other hand, special advantages in being connected more closely within the field, with acccelerator experts and all kinds of technical people—not to mention with the data itself. So there is a different kind of depth and variety in the laboratories. But in terms of getting really new ideas, new directions for the field, and so on, the university is still the most essential element. And their teaching role is extremely important.

P: You are mostly known for your two famous books *Relativistic Quantum Mechanics* and *Relativistic Quantum Fields* and, of course, for Bjorken Scaling. How old or how young were you when you wrote those two books?

BJ: That was around the time I made my decision to go to SLAC. I hadn't had my Ph.D. degree for more than a couple of years. Sid Drell, the senior author of course, had wanted to write this book for some time (it began as one book), and he asked me whether I wanted to join in. That was not an easy question, because those were supposed to be research years. To get involved in writing a book at such an early age wasn't obviously the best thing to do. In fact I asked some friends, wise people, for advice. It was mixed; some people said that it was a mistake. There were two main reasons I decided to do it. One was that working with Sid represented a great opportunity and a lot of pleasure. The other was that I didn't know field theory very well, so this was a way of learning it. Sid had somewhat the same attitude, actually. And it especially shows in the last chapter (in the second book) on renormalization. Almost any reader nowadays will regard it as very awkward and clumsy... pretty terrible from the present point of view. There are things in that last chapter I still like, but on the whole it is clumsy. What is in there could be rewritten in about one third or one fourth of the space. But at that time, the renormalization theory itself wasn't anywhere near as well developed as it is now. The idea of running coupling constant wasn't there, dimensional regularisation wasn't done, and lots of technical things weren't yet developed. All the basic ideas were there, all the essence of it, of course was there! If you go back to the great Gell-Mann and Low paper, you find most everything about the renormalisation group. But you will notice that it is not expressed at all in the way it is expressed nowadays. Anyway, we were learning this stuff as we wrote it. We re-edited it many times and it just turned into a big monster by the time we stopped and said, «All right, enough is enough, let's get rid of it!»

P: Nevertheless it is a classic...

BJ: I'm still very happy with the first book. We wrote that with the attitude that nothing should go in which would become out of date. In fact, in both books, we tried to write that way. I think the first one still has some longevity. Most of the second one is obsolete. In fact, I happily use this opportunity to advertise what I think is a very, very good successor, It is a book that has just come out by Michael Peskin and Dan Schroeder. Peskin is a colleague at SLAC. It is a very good pedagogical book, called *An Introduction to Quantum Field Theory*. It is an inch and a half thick, one book, not too heavy, and it does everything beautifully. I've been in Oxford this year where I had an opportunity to do same teaching. I carried it along as a reference book. When I needed to look up something, I went to it to see what was there, and, yes, everything I needed was there! This happened on several occasions, and it always had the important issue expressed in a good way. If you haven't seen it, I really recommend that you find one and take a look. I was asked to provide a quote for the back cover and

I said, «This book is so good that I fear for our royalties.»

P: But in those days, when your books were published, they were very necessary, weren't they?

BJ: It wasn't that there were no textbooks at that time. I don't want to say anything either bad or good about the standard books of that time. But they were not to our style —in some places they covered things too heavily and in other places not heavily enough--. Of course we felt there was a need for what we did, that is why we wrote them after all. It was not for any commercial reason. I do not recommend writing a book at the graduate student level for the purpose of making money.

P: But, were you expecting them to become classics for a generation of physicists?

BJ: I never thought they'd last as long as they have.

P: Just a curiosity: it's commonly said that your books have no mistakes. Have you ever found one?

BJ: We have an errata sheet, but I don't know if we'll ever get around to putting it out. It is a page or two of mostly minor typographical mistakes. The worst mistake is a real matter of substance in the second book about renormalization. There's an argument that $Z_1=Z_2$. Z_1 is the constant that renormalizes the vertex, and Z_2 is the renormalisation of the wave function of the electron. We had a formal argument why these two were equal, and the argument is mathematically wrong. We just did it wrong. We were dividing zero by zero to get one. This was pointed out to us by a very good Polish theorist, who has also written a nice book on quantum electrodynamics, Iwo Bialynicki-Birula. There are other errors of clumsiness and so on, but that one hurts the most because it is just plain wrong.

P: About Bjorken scaling, could you explain it to undergraduate students?

BJ: The idea behind all of that is very simple. SLAC is a big electron-microscope. The particle accelerator produces a beam of very short-wavelength electrons which illuminates the subject, typically a bucket of hydrogen molecules. The analog of the ordinary microscope is a large spectrometer built of magnetic lenses and prisms. The eye is represented by an array of electron detectors, typically pieces of plastic which emit light when an electron passes through, the light being detected by photomultiplier tubes. Instead of the optic nerve, there are bundles of cables transporting the photomultiplier signals to the computers ("brain"). All this technology was designed to look at the interior of a proton. From the beginning one could see that the SLAC energy scale was high enough that the resolution of this "microscope" was good enough to see the interior of the proton. There are two kinds of experiments that one can do: one is elastic scattering, where the incident electron plus proton goes to scattered electron plus a solitary proton. The other is inelastic scattering, where the proton gets broken up. The first kind of experiment had been done extensively at Stanford, in a smaller machine which was the precursor to SLAC. The target particles were both protons and nuclei. Robert Hofstadter led that program, which was extremely successful in measuring the mean charge distribution within nuclei. That was one big attraction for Drell and me to go to Stanford from MIT. Drell was interested in inelastic electron scattering from nuclei, because if you break up the nucleus, the electron scatters from the individual constituents of the nucleus and you could see the properties of protons and neutrons as they are in the middle of nuclei. If you scatter elastically, you only see the average charge of the nucleus. Inelastic scattering at SLAC attracted us both, especially me. It was something that was, for the first time, powerful enough for looking at the inside of the proton. Therefore one could see whether there was, in, principle, structure inside. So it was clearly an interesting program right from the start, at least it was for some of us theorists from the start. Experimentalists saw it less clearly at that time, I think. Leonard Schiff, the chairman of the physics department (who wrote a good book on Quantum Mechanics!), who worked actively with Drell on inelastic electron scattering from nuclei, saw it especially clearly and so stated it when the project was first announced to the Stanford physics community. But it was not easy to do something about it. The new question was not the basic idea of finding out about the insides of a proton via inelastic scattering, but how to do it in detail. Unlike the case of atoms or nuclei, this was a problem involving in an essential way Special Relativity: for the first time, you had to really deal with the fact that the things inside of the object of interest were not slowly moving. The time it takes for an electron to go through a proton is the same as the time

From left to right: Derrick, Bjorken and Halzen. **Photo by A. M. Teixeira** for the constituents inside to move across it. So the old techniques were, technically, no good. So that was the real problem: trying to figure out the framework for interpreting data. I can't here really go into the details of that very well, I am afraid. The techniques which Drell, Schiff et. al. had been using for nuclei were developed long ago in atomic physics: they are the various "sum rules" of atomic physics. Heisenberg used them when he invented Quantum Mechanics. The sum over all the probabilities of the electromagnetic transitions to excited atomic levels is a simple quantity. The same is true for the sum of all transitions to excited nuclear levels induced by electron scattering. One looked for something analogous for scattering of electrons from protons, when all final inelastic states were summed over. Murray Gell-Mann and others did find relativistic sum rules for other processes involving protons, although their starting point was somewhat different. But their ideas could be adapted for electron-proton scattering. When all of this kind of machinery was put together, the results were very suggestive of history repeating itself, that inside the proton there could be hard point-like things from which the electron could scatter. That is the simple idea that underlies the scaling hypothesis. From the quantities that the experimentalists measured, one could construct a dimensionless parameter (the x with which I get associated) which chararacterizes all the data. The dependence of data on only a dimensionless parameter is essentially the reflection of the idea that the objects from which the electron is scattering have no intrinsic size parameter.

P: Is there any special story about this discovery ?

BJ: Well, there is a story, which you can read about it in various semipopular accounts. The way the scaling variable x was created was, like I said, rather indirect and depended on history, on sum rules, on a whole collection of ideas which is not the way you learn about it now. The ideas of the parton model, the way you learn it, you can learn in kindergarten. Some of those simple ideas I knew about, and I used them as a guide. But I didn't trust them at all, it simply wasn't very respectable thinking. The more sophisticated ideas common now (such as operator-product expansion) I knew in an imperfect form, and not the form as is used now. And point-like constituents was only one of a dozen ways of looking at the problem. Anyway the bottom line is that I made a suggestion to the experimentalists on how to plot their data, the experimentalists then made the plots, and the scaling hypothesis seemed to work. At about this time Feynman got involved. He stopped by SLAC by chance; he had a sister who lived nearby. It was essentially a social visit. He had been working on his parton model, but for collisions of protons with protons, a much dirtier kind of physics. I was out of town, and when people told him about scaling, he immediately realized he was worrying about the wrong problem; electron proton scattering was much simpler. But nobody really explained to him why I was using this funny variable x. The story at that time was that he went off to a nearby girlie bar, and worked things out there. Feynman said that it was not true, and that he worked it out in his motel room. In any case he worked it out overnight, and figured out what I was doing, what it was about. Next day he told SLAC people about it, and naturally everybody got very excited. I came back just before Feynman left town. We got together in a little room, and Feynman said, «Of course you know about this, of course you know about that...». His language wasn't quite the same as mine, but some of the things he said I could easily understand. But some of the ways he expressed things were new, which I didn't appreciate right away; in fact it took a long time before I appreciated fully the depth of his understanding. Also, many of the things that I knew, he didn't then know. Then he went away and thereafter we worked in parallel but quite independently. Of course with Feynman getting into the problem, the subject then took off and never came down again. It is amazing how big an industry it is even to this day!

'personal satisfaction often doesn't correlate with what the world of Physics recognizes'

P: Looking at all your entire work, do you think it is fair for you to be recognized mostly for the Bjorken scaling and for your books?

BJ: Oh, some of the things that I've done that I am happiest about weren't recognized at all. Most of them don't deserve recognition, but they gave me (and still do give) satisfaction anyway. I think the actual sense of personal satisfaction often doesn't correlate with what the outside world of Physics chooses to recognize. With respect to the books, we really worked hard on them, and that effort was well worth it. Scaling was a matter of being in the right place at the right time— it was good luck. I'm very fortunate. The notoriety for me which came out of scaling is out of proportion to my contribution. There are a lot of other people who worked hard on that problem, and whose contributions are of at least a comparable magnitude. Steve Adler is one of them, Sergio Fubini is another, just to mention two who were very important. Murray Gell-Mann started the whole development and deserves a great deal of credit —but, well, he has his share of notoriety--. But I think Adler, in particular, deserves much more recognition for his work. Anyway, in that area I'm lucky. There is another related area which I've worked on which is much less noticed, but which for me was especially satisfying. For some years I worried about the final states in deep inelastic scattering. The electron scatters off a quark in the proton, and breaks it up, but no quark comes out. The problem was: What controls the final state evolution? Those were very enjoyable, exciting years figuring it out. And at the end of the line, the prediction was that, if you looked at things the right way, the final state in this extraordinary, apparently violent process wouldn't look any different from an ordinary process, say pion-nucleon scattering at a comparable energy. That was a serious conclusion because it wasn't obvious at all. This turned out to be not only the convincing answer to us, but is what came out experimentally as well. The answer is pretty boring —not a very exciting conclusion. What we see is just what we've seen before. So these ideas didn't have much impact one way or the other. Also, most of the data arrived about the time that the psi was discovered. So of course there wasn't much attention paid, including myself, to that kind of stuff. But again, I don't think there is necessarily a strong correlation between what can be very satisfying personally and what the outside world regards as important. But it is important, I think, not only to get satisfaction from doing well the little things that make you happy, but also to have good taste in the choice of what you do. Taste is one of the qualities that I consider most important when evaluating job candidates. *Taste* means knowing what to work out, as well as being able to do a good job on what you do. You can do a very good job on a problem, but it doesn't matter too much if it is one that has nothing to do with anything. But if you do even a half-decent job working on a problem which you sense is very important and other people just don't realize is important, it can be relatively easy to make an impact. So I myself do try, most of the time, to make my choices of problem tasteful. But even sometimes, when they are not, I can get the satisfaction anyway.

P: You were for the last three years doing some experimental work. How was it? Good for you or hard?

BJ: Both good and hard. When I started this latest experimental business, I already had my union card as experimentalist. I served "as a graduate student" on a small experiment in the early 1980's at SLAC (while I was employed at Fermilab), looking for things called *axions*, and I learned then what experimental physics was about. This time around it was at Fermilab where we worked (after I had returned to SLAC, which was kind of stupid). And this time I was a co-spokesman of the experiment. From the beginning I knew what I was in for. For 3 years, life was very intense; I couldn't do much of anything else but the experiment. I was half time at Fermilab, half time at SLAC, and essentially canceled (maybe *postponed* is a better way of putting it) the rest of life. It was nuts, extremely demanding, but it was also enormous fun. I can be rightfully accused of being totally crazy, having terrible taste, because the experimental goals were very small and speculative. But ever since the day I walked into SLAC, I have enjoyed working with experimentalists to the point of going through the imagining and designing of experiments. So this was fulfillment of those fantasies. There were only a few of us were doing the Fermilab experiment. Most of the time, all of our running time was opportunistic. Nothing was officially scheduled for us; we ran when the conditions for the others was poor. We always had to go in every morning at 9 o'clock for the daily checkout meeting with the accelerator people, and I remember walking in there one morning when they greeted me with «How would you like some running time?», and I said, «Sure, when do we get it?» «In a half hour». I was by myself, no one else was in town. I said, «Good-bye», ran over to the experiment, turned the high voltage on, got the gas flowing through the chambers, etc., set up the data acquisition, and called our computer expert, who works at Fermilab, to come over in case something crashed. In the half hour we were ready, and up and running — just the two of us--. And we got good data. Similar things happened in the middle of the night, when I was completely alone, just me and the Tevatron and the data from our home-built apparatus coming in. So, that kind of experience is a real kick, and worth a lot of struggle to experience. And the general experience of experimental physics is very rich compared to theory. You have to deal with all kinds of people: experts on building accelerators, the operators who run them, the people who run the safety divisions, all kinds of engineers, technicians, surveyors, etc., and the various laboratory bureaucracies. There are many, many more ranges of experience that one gets than in theory. I think that was, in many ways, the most enjoyable side. Everybody at FermiLab really works hard to help out the experimental groups get the physics. It was especially satisfying that that was the case for such a small group, like ours. So I enjoyed it a lot. But I'm glad it was for a limited period. It started three years ago, and is ending now; now I am returning to theory. To do experimental physics as a career is a hard life, and a rewarding one. But basically I am a theorist and probably I shouldn't do experiments all the time.

'the experience of experimental physics is very rich compared to theory'

P: Are you preparing another project in particles?

BJ: No, but that doesn't mean I am completely decoupled from experimental activity. Before I came here to Faro I was in Geneva. There is an initiative there, called FELIX, to build an LHC detector to do QCD. It is not optimized for discoveries beyond the standard model, the Higgs Boson, SuperSymmetry or any of that. But it is intended to really understand the strong interactions instead. That kind of thing has to be done as well. I believe strongly in diversity in the program. There is nothing I believe more strongly than the idea that you have to let a wide variety of ideas go ahead together. Even in my own personal career, I've seen that progress can come from some unexpected place that is not in the main stream. When proposed, SLAC was thought by many as an unpromising accelerator. I remember when I decided to go to SLAC, one of my theory friends, who later got a Nobel prize, asked: «Why do you want to go to SLAC, that is going to be a white elephant. There is nothing going to be discovered at SLAC! You're wasting your time, you should be at a proton machine.» That was a common view at the time. Later on, the electron-positron ring at SLAC (SPEAR), where the psi was discovered, was not a community-blessed project. None of the *committees of wise people*recommended that it be built. It was built right out of the laboratory's own budget, sacrificing other parts of the program. Still later, the Cornell machine CESR, which does all of the beautiful B physics, was recommended by the *wise-men committees*not to be built. I was in fact on a committee which recommended it not be built. Mistakes like this can happen any time. So it is really important to have a very broad range of possibilities. This detector at CERN, called FELIX, is a project which I want to stay involved with, to help to lay out the physics menu, and right now also to work on the initial conceptual design of the detector.

'you have to let a wide variety of ideas go ahead together'

P: What do you think of the costs of Experimental Particle Physics? Do you think it is worthwhile to spend such amount of money?

BJ: It is a very valid question. I think it is something of a miracle that governments support Particle Physics at all. I have felt this way for a long time, and that was one of the reasons why I went to SLAC. Even in those rich times, I felt that it was a privilege that we got these machines, and therefore it was very important that we did everything we possibly could to exploit them to the fullest. But I also think that, compared to the way all kinds of public funds are spent, the question is very easy. It is not a question that what we do is a waste of money, but simply that the public has to be very enlightened to notice that there is something out there called Particle Physics. If they are enlightened, they may come to the conclusion that it is a good deal because the research does benefit society a great deal. For example, the World Wide Web came out of CERN. If they had demanded royalties from it, maybe they could have built the LHC from WWW proceeds. Down through history of particle physics, the practical spinoffs have justified the investments. So I don't think we have to be embarassed about asking for resources. I think that what has been spent has generated a good return, except for the supercollider in Texas, which is a disaster. But it still is really something of a miracle that something so abstract as particle physics does get so much support.

P: What are your perspectives for the future of Particle Physics?

BJ: It is a lot harder now than when I started out. I began in a really golden age, shortly after the war. The war effort and atomic weaponry and all that gave the physicists a special status with the government and the public, and so it was rather easy to get support. Additionally, in the USA there were a great expansion in education, the Sputnik Era, because the U.S. couldn't be behind the Russians. I was not of the generation which was involved in the World War, only the growth period which followed it, and at the same time earlier than the difficulties which came later. Now, with the end of the Cold War, the support is harder to get. Also, the field moves much more slowly and the experiments are big centralized enterprises. Even the theory gets more centralized. These are negatives. But on the other hand the opportunities are still pretty good. It is not that there is no excitement in the field or that there is nothing to do, but just that it is harder. To go into this field I think you should have the feeling that you have to do it. If you have the training as a particle physicist, you can do very well in the outside world, quite often much better. If you stay inside the field, best you do it just because you feel you have to.

P: Is there any thing else you would like to recommend to a young student interested in Particle Physics.

BJ: I already said it. If you really have the feeling you have to do it and you love doing it, you will find a way to do it. When I was at FermiLab I was for a while a part of the laboratory administration, associate director for physics (this basically meant I was an assistant to the director). One of the things I did was to interview all of the incoming Post-Doctoral candidates. FermiLab had then a very wide variety of experiments, from very small to the the big ones like CDF. The postdocs could choose what experiment they were to work on; they weren't assigned to a particular one. I would always ask the incoming applicants whether they had any particular preference, large versus small, and tried to

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The ICPS'97

by Stephan Witoszynskyj, Chairman of the Organizing Committee

Can you imagine anything more difficult than writing an article about ICPS '97 after a long skiing day? But the deadline for JiAPS is approaching and it seems as if the Conference Organizer has some duties. :-) I guess most of you already know what the ICPS is and that the next will be in Vienna/Austria. But for those of you who have never heard anything about the ICPS I'll write a short introduction.

First of all an ICPS is really something great. Anyone who has ever been to one will confirm this. The ICPS (International Conference for Physics Students) is the annual conference of IAPS. Every year it is organized by the members of IAPS in a different country. The conference gives physics students from all over the world the opportunity to meet each other, to establish contacts with colleagues from different countries, to present their work to an international audience, to get experiences in giving lectures in a foreign language... And maybe as important as the other aims: to have a good time in a foreign country, to enjoy life and to see different cultures.

Since the first ICPS in Hungary in 1986 the conference has been organized in Hungary (several times), Turkey, Portugal, Russia, Denmark, the Netherlands and Austria. ICPS '97 is already the 12th ICPS, and will take place at the Vienna University of Technology (Technische Universität Wien) in Vienna (Austria) from the August 10th to August 17th. All physics students are invited to participate in the conference. If you want you can present your work in a lecture or poster (but you are not required to do it). Most of the programme of ICPS'97 will be similar to the previous conferences. That means most of the time will be devoted to the participants' lectures.

Every participant has the opportunity to give a lecture. The subject can be chosen freely within the boundaries of physics. Each talk is limited to 25 minutes plus five minutes for questions. There is also the possibility to present posters. During the poster session the posters will be displayed for the other participants. Of course the programme will also include parties, sight-seeing and excursions. During the ICPS in Copenhagen and in Szeged the very successful idea of a "National Party" evolved. The participants have a chance to introduce their country and culture to the others by singing or presenting special food. This event was so successful that we will organize something similar this year. Another very important event for IAPS and its members is the General Meeting, which will be held during the conference.

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let them to express a personal view on this. I remember one student in particular, who had worked with only one or two physicists on atomic parity violation as a thesis topic. Because of that, this person got interested in particle-physics experimentation. I asked, «Wouldn't you like to be in a small experiment?» «No, I want to go to a big one like CDF because that is what the physics is!» There was no general rule on whether a postdoc would choose small or large. I find also now, when I talk to young members of large collaborations, that despite the large size they generally find enough personal freedom and ability to express themselves creatively. It surprises me because I think that the small experiments have special opportunities, and I try to promote that side of the field. During our experiment our neighbors CDF and D0 were looking for the Top quark. It was essentially the one thing happening, everything else was subordinated, and the top search became quite a corporate, not individualistic enterprise. I would tease the CDF and D0 people, that they were so obsessed about finding the Top quark that they were missing the real new physics— they were putting it aside, in favor of that boring Top quark coming along. But when the top was discovered it was impossible not to share in the genuine excitement, right along with them. So, I think the opportunity of having a satisfying career within big collaborations is certainly there. And it is a safer way to go, obviously, than being involved with a small experiment. But I do hope the variety of enterprises in our field can somehow be preserved in the presence of this big concentration of physics within a few detectors and a small handful of major scientific goals.

'it is something of a miracle that governments support Particle Physics at all'

P: About new Physics, for instance the substructure of quark or the chaotic behavior in QCD. What is your opinion on these subjects?

BJ: The evidence for "quark substructure" is very thin. I also dislike this language. What is really meant is that quarks have interactions, new scattering mechanisms, beyond what is known from the standard model. It doesn't necessarily mean substructure per se; that is just bad language. That there is chaoticity in QCD, fractal structure and all of that, of that I am sure! But it is a question of how clearly it is manifested in observational data, not that it is there in the underlying mechanisms.

To take part in the conference

First of all you have to send in your pre-registration before March 31st, 1997. Please send us your family name, first name, sex, address, country, citizenship, university and e-mail address. Further, we would like to know if you intend to give a lecture or to present a poster and if so, its title and contents. If you have access to the World Wide Web, you can use the automatic form on our homepage. Of course you can also send your pre-registration by e-mail, normal mail or fax using the pre-registration form to be found on the next page. The next step for participating in the conference is to register. Therefore you have to transfer the conference fee to our bank account. Before you do this we ask you to send us a notice to make sure that there are still places left. The deadline for registering (sending the conference fee) is May 31st, 1997. Please note that the full amount of the fee has to arrive in Vienna, otherwise you will have to pay the rest during the conference.

I already mentioned the conference fee several time, so I'm going to explain it a little bit. Every participant has to pay a fee of 190 DM (German mark). The fee helps to cover the costs of the conference. It includes lodging, two meals per day (breakfast and lunch), the proceedings and the programme book. US$6 (about 10 DM) of this 190 DM are membership fee for IAPS. For those participants who are already individual members of IAPS, or members of a Local or National Committee, this amount will be returned during the conference.

If you plan to give a lecture or to present a poster you have to send the *abstract* of your poster/lecture before June 31st, 1997. This abstract will be printed in the programme book of the conference. Please remember that an abstract should just be a short summary of your lecture/poster and should give the other participants an idea what you are talking about. For the proceedings of the conference you have to send a paper about your lecture/poster. The deadline is also June 31st, 1997. Both, the abstract and the paper can be sent in as LaTeX (preferred) or as text file.

To find more information

The Organizing Committee has set up a Web page. If you need more information you are welcome to send an e-mail, a fax, a letter or even to give us a phone-call. So finally here is our address:

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PRE-REGISTRATION FORM

Family name:
First name:
Address:

University:
Country:
Citizenship:
Sex: **( )** male
( ) female
E-mail address:

Are you going to give a lecture? **( )** yes **( )** no

Are you going to present a poster? **( )** yes **( )** no

Title of your lecture/poster:
Contents of your lecture/poster:

Houston, we have a problem!

by Miguel Carrión

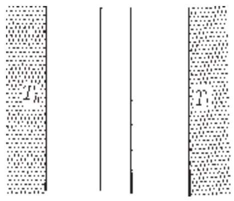
This is the first (and let's hope it's not the last) selection of physics problems we will be publishing in JiAPS. The problems range from the almost trivial to the next-to-impossible, or at least I've tried to make it so. A problem section is for the readers to send in solutions and more problems, so your participation is essential to make this a great section. Before we start, a word about the nature of the problems. Your typical homework problem is too easily reduced to mathematics, and I have tried to avoid that. Some of the problems are *deliberately* stated in vague terms so that part of the solution involves using physical intuition to add some additional assumptions that fully specify the problem. It would be very interesting to see different people solving slightly different problems as an answer to the same question!

Problem #1: A given volume of 'dry water' (as John von Neumann called 'perfect, incompressible' fluids) in a cylindrical vessel is rotated about the cylinder's axis at constant angular velocity. The whole system is immersed in a uniform gravity field. Assuming the rotation axis is parallel to the direction of gravity, deduce the shape of the free surface of the fluid.

Problem #2: Based only on the fact that the antineutrino has positive helicity (its spin and momentum are parallel), deduce the angular dependence of the amplitude of beta decay: n -> $p^+ + e^- + \nu_e$. [Hint: assume the 'outgoing' proton is at rest in the centre-of-mass frame]

Problem #3: A disc of radius R moves in a perfect, incompressible, irrotational fluid with velocity $\mathbf{v_0}$. Calculate the lift force perpendicular to the direction of motion. This is a two-dimensional problem; if you wish, you can picture a very long cylinder moving in the fluid perpendicularly to its axis, and calculate the lift force per unit length. [Hint: that the fluid is irrotational does not mean that the circulation of the fluid around the disc vanishes]

Problem #4: A black plane surface at a constant high temperature T_h is parallel to another black plane surface at a constant lower temperature T_l. Between the plates is vacuum. In order to reduce the heat flow due to radiation, a heat shield consisting of two thin black plates, thermally isolated from each other, is placed between the warm and the cold surfaces and parallel to these. After some time stationary conditions are obtained. By what factor is the stationary heat flow reduced due to the presence of the heat shield? Neglect end effects due to the finite size of the surfaces. (From the 1996 International Physics Olympiad)



Problem #5: It is well known that an electric dipole in a uniform electric field experiences a torque tending to orient the dipole parallel to the field. For a neutral object without a permanent dipole moment, a non-uniform field gives rise to a net force. Prove or disprove the conjecture that a neutral *conducting* object in a uniform electric field will in general experience a torque. [Hint: a spherical object obviously experiences no torque]

Problem #6: Christiaan Huygens, the inventor of the pendulum clock, first observed the phenomenon known as 'entrainment' or 'phase locking', which can be described as follows. Two different clocks, having minute differences in length and mass of their bobs, would oscillate freely at slightly different frequencies and therefore develop a phase difference even if they started oscillating in phase. However, if those two clocks were mounted on the same wall, they would end up oscillating synchronously despite the difference in natural frequencies. modelling the penduli by harmonic oscillators, though, this behaviour is not recovered. There are two fundamental modes and the general motion shows *beats* in the amplitude of oscillation but no phase locking. It seems that nonlinearity in the pendulum equation (and possibly damping as well) is essential to Huygens' discovery. What may or may not be essential is the fact that a clock is maintained by an escapement, i.e. that it is a *forced* oscillator. After all, the escapement is only necessary to compensate for damping.

Now for the problem: write a simple model of two weakly-coupled nonlinear oscillators (e.g. obeying the simple pendulum equation) and explain how Huygens' 'phase locking' arises, possibly giving conditions the natural frequencies must satisfy for the phenomenon to arise. [Note: this should be an exceedingly difficult problem to solve with more than 'handwaving' arguments, but that's precisely the point]

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