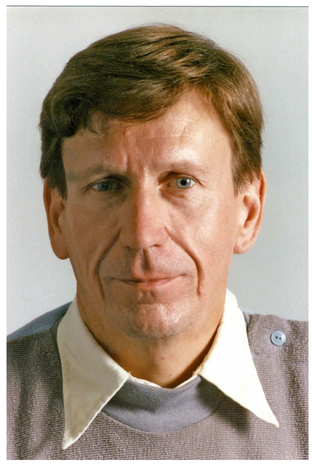
FOUR DECADES OF SUBATOMIC PHYSICS



Essays in Honour of Professor of Theoretical Physics

PENTTI VESA RUUSKANEN

on the Occasion of His Retirement

31 December 2006

Edited by Kari J. Eskola and Kimmo Kainulainen

Professor Pentti Vesa Ruuskanen
– Four Decades of Subatomic Physics

This collection of invited essays is published to celebrate Pentti Vesa Ruuskanen, Professor of Theoretical Physics at the Department of Physics, University of Jyväskylä, JYFL, on the occasion of his retirement on 31 December 2006.

In August–September 2006 we were thinking how to celebrate the retirement of Vesa at the end of the year. Discussions with various people eventually led to the idea of editing this Festschrift, as through this work we could both celebrate Vesas long scientific career in theoretical subatomic physics and document an important part of our Departments history in which Vesa has played a major role.

As indicated by the title, Vesa has been researching in the fields of theoretical and subatomic physics for over four decades. He achieved the degree of MSc from the University of Helsinki at the age of 24 and the degree of PhD there at 30. He became an associate professor at the University of Jyväskylä at 35 and a full professor at 56, from which position he now retires at the age of 66. Vesa has worked on particle and hadron physics, and over the two last decades in particular on the theory and phenomenology of ultrarelativistic heavy ion collisions (URHIC). Indeed, Vesa is one of the pioneers in URHIC theory, especially in the hydrodynamic description and thermal signals of QCD matter and in hard probes. He is truly well liked as a person and recognized for his work in the international scientific community. Vesa is also the one who, together with Professors Keijo Kajantie and Esko Suhonen, initiated the URHIC physics activities in Finland. It is to a large extent thanks to Vesas efforts over the years, that theoretical high energy physics is going on strong in Jyväskylä.

Vesas academic career and selected merits are described on the next pages. In addition to the internationally acknowledged research he has conducted, Vesa has played a major role in planning the teaching and the curriculum reforms at JYFL, a recent example of this being his involvement in the Bologna process. Vesa has always been popular amongst the students, both due to his carefully prepared and insightful lectures and his easily approachable character and friendly attitude – no wonder that he has supervised tens of theses at different levels over the years. Indeed, we are all indebted to Vesa for his contribution in creating and maintaining the collaborative and open working atmosphere we continue to enjoy here at JYFL.

Vesas impact on the growth and development of our Department has been important, as he has been the chairman and the vice-chairman of the department for several years. His leadership skills have also been appreciated in a broader context: he has served as the Chairman of the Finnish Physical Society and as the Director of the Research Institute for Theoretical Physics, which was the predecessor of the Helsinki Institute of Physics. Vesa has also contributed to the development of Nordic physics activities during his years as a member of the Nordita Board. Vesa is well known and respected for his objectivity and fairness. As a trusted member of the physics community he has been elected as a Member of the Research Council for Natural Sciences and Engineering of the Academy of Finland. This committee makes decisions on all grants awarded by the Academy of Finland in natural sciences and so has a strong influence on funding of all basic physics research in Finland. In 2004 Vesa received the Theodor Homén Prize of the Finnish Society of Sciences and Letters for his achievements in the study of strong interactions and in developing physics research and teaching.

In this Festschrift, we have collected essays from Vesas colleagues, collaborators and students, all of them long-time friends. The essays speak for themselves in reflecting Vesas character, achievements and his mental strength. We wish to express our warmest thanks to all authors who, in spite of the very tight publishing schedule caused by our late planning, were immediately ready and enthusiastic to contribute. We also wish to thank all those who provided additional material such as the photographs printed in this book, Soili Leskinen for her help in various practical matters, and eventually JYFL for publishing this Festschrift in its Research Report Series.

We also wish to take this opportunity to personally thank Vesa for the years he has shared with us here at JYFL. We will remember you for your guidance and support, for collaborative efforts, heated debates and for so many interesting coffee table discussions. Needless to say, we are looking forward to seeing you continue these activities for a long time after the date of your retirement.

On behalf of the physicists of Finland and abroad: with this Festschrift we congratulate Professor Pentti Vesa Ruuskanen on the occasion of his retirement on 31 December 2006.

Jyväskylä 11.12.2006

Kari J. Eskola and Kimmo Kainulainen
Department of Physics, University of Jyväskylä

Contents

Professor Pentti Vesa Ruuskanen – Four Decades of Subatomic Physics
Preface by Kari J. Eskola and Kimmo Kainulainen i

Pentti Vesa Ruuskanen – Academic Career iv

Varhaisia ja myöhempiäkin yliopistovuosia
Pertti Lipas 1

Various Phases of Accelerator and Nuclear Physics
Juha Äystö and Rauno Julin 11

Vesa Ruuskanen – Dictator of Teaching
Jukka Maalampi 19

Rendezvous with Vesa during 37 Years
Jussi Timonen 23

Meritullinkatu 8 A, Siltavuorenpenger 20, 1968–69
Keijo Kajantie 29

Vesa Ruuskanen: The Beginning of a Hydrodynamic Description of Ultrarelativistic Heavy Ion Collisions
Larry McLerran 37

The Urbana Connection
Gordon Baym 43

"You wouldn't be interested in hydrodynamics?"
Markku Kataja 49

On the Road to the Hard Probe Cafe
Helmut Satz 63

Ultrarelativistic Heavy Ion Collisions – Two Decades of Interaction with RVesa
Kari J. Eskola 69

Charming Physics with Vesa
Ramona Vogt 89

Hydrodynamiikan kautta opettajaksi
Sami Räsänen 99

Hydroa opiskelemassa
Harri Niemi 105

The Birth of Quark-Gluon Plasma Physics at JYFL
Jouni Suhonen 109

Professor Pentti Vesa Ruuskanen: Academic Career

Education

- MSc, University of Helsinki, 1964
- PhD, University of Helsinki, 1970

Academic Positions

- Research Fellow, Iowa State University, Jan 1967 – July 1968.
- CERN Fellow, Jan 1971 – Aug 1972.
- Lecturer in theoretical physics, University of Jyväskylä, 1972.
- Corresponding Fellow at CERN, 3 months in 1974.
- Associate Professor, University of Jyväskylä, 1975.
- Corresponding Fellow at CERN, 4 months in 1979.
- Senior Scientist, Academy of Finland, 1982.
- Visiting Professor, University of Illinois, Urbana–Champaign, Academic year 1986–87.
- Senior Scientist, Academy of Finland, Academic year 1990–91.
- Professor of theoretical physics, University of Jyväskylä since 1996.

Administration

- Board Member of the Finnish Physical Society, 1975–80.
- Chairman of the Finnish Physical Society, 1978–79.
- Board Member of the Research Institute of Theoretical Physics, University of Helsinki, 1978–82.
- Chairman of the Department of Physics, University of Jyväskylä, 1987–90.
- Acting Director of the Research Institute for Theoretical Physics, University of Helsinki, Aug 1993 – Dec 1995.

- Member of NORDITA subfield committee on Particle Physics, 1993–95.
- Member of the Board of NORDITA, Copenhagen, 1994–98.
- Chairman of the NORDITA Nordic–Baltic Committee for advancement of contacts and collaboration between the Nordic and Baltic countries, 1994–96.
- Member of the Faculty of Mathematics and Science, University of Jyväskylä, several times since 1972.
- Director of the national Graduate School in Particle and Nuclear Physics, 1995–98.
- Member of organizing, advisory and programme committees of several Schools and Conferences, e.g. Quark Matter 93 in Sweden, European Research Conference on Heavy-Ion Physics 1994 (co-chairman), Nordic School on Heavy-Ion Physics in Jyväskylä 1994 and 1995 (chairman); chairman of the programme committee of the Annual Meeting of the Finnish Physical Society, Jyväskylä, 2001.
- Member of The Research Council for Natural Sciences and Engineering of the Academy of Finland 1998–2000.
- Vice chairman of the Department of Physics, University of Jyväskylä, Aug 2002 – July 2005.
- Chairman of the Committee of the Faculty of Mathematics and Science for implementing the changes to the curricula of Biosciences, Chemistry, Mathematics and Physics so that the teaching programmes are commensurate with the outlines of Bologna process 2002–04.

Awards

- The Theodor Homén Prize awarded by The Finnish Society of Sciences and Letters, 2004.

Varhaisia ja myöhempiäkin yliopistovuosia

Pertti Lipas

Aatoksenkatu 6 C, 40720 Jyväskylä

Limeksen mukana Englannissa

Tutustuin fil. yo. Vesa Ruuskaseen keväällä 1962, kun olimme Helsingin yliopiston matemaattisten aineiden opiskelijoiden tiedekuntajärjestön, Limes ry:n, ekskursiolla Englannissa. Vesa oli opiskellut matemaattisia aineita kolme vuotta. Itse olin edellisenä syksynä USA:sta kotiutunut ja siellä tohtoriksi väitellyt, Atomienergianeuvottelukunnan palkkaama tutkija. Siltavuorenpenkereellä Vesa oli fysiikan laitoksessa, minä saman katon alla vinttikerrokseen vasta perustetussa ydinfysiikan laitoksessa. Minut kutsuttiin ekskursion kaitsijaksi. Yhdeksi syyksi arvaan sen, että osasin hyvin englantia, mikä oli siihen aikaan harvinaista.

Ajelimme tilausbussilla ympäri Englantia Lontoosta aina Manchesteriin asti. Kävimme mm. Oxfordissa ja Cambridgessä, Jodrell Bankin radioastronomisella asemalla ja Hinkley Pointin ydinvoimalassa. Tieteellisen ohjelman ohesta mieleeni on erityisesti jäänyt käynti oluella Trout Inn -nimisessä, 700 vuotta vanhassa majatalossa Lechladessa lähellä Oxfordia.

Hinkley Pointin ensimmäistä reaktoria vasta rakennettiin. Esittelypaikalla kutsuttiin kaksi vapaaehtoista menemään reaktorin sisään. Vesa ja minä ilmoittauduimme. Pukeuduimme valkoisiin haalareihin ja muihin suojavaatteisiin ja sitten kuljimme kyykyssä pitkän, kiemurtelevan putken läpi. Se oli jännittävää, kun ajatteli mitä putkessa myöhemmin kulkisi.

Viidestä ekskursion opiskelijaosanottajasta tuli myöhempinä vuosina fysiikan professori.

Tutkijaksi ja maisteriksi

Vesan ensimmäinen tutkimustyö oli kokeellista ydinspektroskopiaa. Työn teettäjiä ja ohjaajia olivat apulaisprofessori Juhani Kantele ja Van de Graaff -kiihdytinlaboratorion johtaja, professori Lennart Simons. Monen laudaturopiskelijan työ kiihdytinlaboratoriossa alkoi kahvinkeittäjänä, ja jatkossakin laskettiin työtunteja eikä pelkästään työn tuloksia. Vaikka tätä vähän irvailtiin, Vesa ja muut ovat jälkeenpäin kiittäneet kiihdytinlaboratoriokokemustaan johdantona tutkimuksen ilmapiiriin.

Kokeellisten laudaturtöidensä päätteeksi Vesa teki teoreettisen pro gradu -tutkielman. Sen aiheena oli Bohrin–Mottelsonin kollektiivinen ydinmalli. Maisterin paperit Vesa sai alkuvuodesta 1964.

Teoreettinen fysiikka marssi voimallisesti Helsingin yliopistoon 1960-luvun alussa. Kehityksen pani alulle K. V. (Kalervo Vihtori) Laurikainen, joka nimitettiin ydinfysiikan professoriksi 1960. Sen lisäksi Siltavuorenpenkereelle tuli teoreettisen fysiikan laitos, jonka ensimmäiseksi professoriksi nimitettiin Pekka Tarjanne 1967, ja vuonna 1964 perustettu Teoreettisen fysiikan tutkimuslaitos (TFT).

TFT:n perustaminen oli K. V. Laurikaisen, Teknillisen korkeakoulun professorin Pekka Jauhon ja matemaatikko-akateemikko Rolf Nevanlinnan suuri saavutus. TFT oli tarkoitettu väli-etapiksi yliopistovirkoja kärkkyville nuorille suomalaisille tohtoreille. Opettajiksi ja ohjaajiksi tavoiteltiin ja määräajaksi kiinnitettiin ulkomaalaisia asiantuntijoita, kotimaisia kun ei juuri ollut. TFT:n toinen ulkomainen tutkimusopettaja oli amerikansuomalainen Kenneth Eino Lassila, joka oli hankkinut kannuksensa maineikkaan Gregory Breitin oppilaana Yalen yliopistossa.

Vesa tutustui Lassilaan Siltavuorenpenkereellä. Sen lisäksi muistan, että molemmat olivat Hangossa elokuussa 1964 pidetyssä ydinfysiikan pohjoismaalais-hollantilaisessa symposiumissa. Sen johtavana järjestäjänä oli prof. Simons, ja se oli ensimmäisiä merkittäviä kansainvälisiä fysiikan kokouksia Suomessa. Osanottajia oli huomattavasti laajemmalta alalta kuin vain ydinrakennefysiikasta. Silloin Kööpenhaminassa vaikuttanut ja jo huomattavaa mainetta niittänyt Gerald E. (Gerry) Brown oli kokouksen tähtiä.

Ken Lassila oli TFT:ssä puolitoista vuotta, siitä puoli vuotta Fulbright-asiantuntijana. Hänestä tuli Vesa Ruuskasen tohtorikouluttaja. Asevelvollisuutensa suoritettuaan Vesa vietti 1967–68 puolitoista vuotta Kenin kutsumana Iowan valtionyliopistossa. Sieltä hän palasi TFT:n tutkijan toimeen ja väitteli tohtoriksi Helsingin yliopistossa 25.11.1970. Pekka Tarjanne oli kustoksena, minä vastaväittäjänä.

Epäeksponentiaalinen hajoaminen

Yleiset teoreettiset kysymykset tulivat Vesan jatko-opintojen pääkohteiksi. Suuntaus oli kohti hiukkasfysiikkaa. Hän kuitenkin haki oppia laajalti fysiikan piiristä, mm. minun ydinfysiikan ja monihiukkasfysiikan luentokursseiltani. Kiinnostus ja menestys olivat korkealla.

Ken Lassilan ohjaamana Vesa teki valmiiksi lisensiaattityönsä 1965 ennen kuin astui varusmiespalveluun alkuvuodesta 1966. Aihe ja tulokset olivat alkusoittoa väitöskirjalle.

Väitöskirjatyön lähtökohta oli varsin yleinen kysymys: ovatko kvanttimekaniikan kuvaamat hajoamisprosessit välttämättä eksponentiaalisia? Toisin sanoen onko hajoamistuotteiden määrä ajan t kuluttua

$$A(t) = A(0)\mathrm{e}^{-t/\tau} \,, \tag{1}$$

kun τ on hajoavan tilan elinaika? Tätä radioaktiivisesta hajoamisesta tuttua kaavaa on harvoin asetettu kyseenalaiseksi. Muita mahdollisuuksia vastaan ei kuitenkaan ole pakottavaa teoreettista tai kokeellista todistusaineistoa, kuten Vesa väitöskirjansa alussa toteaa. Luonteva yleistävä yrite on sisällyttää yhtälön (1) oikeaan puoleen t:n potenssein etenevä polynomi,

$$A(t) = A(0)(1 + at + bt^2 + \ldots)\mathrm{e}^{-t/\tau} \,. \tag{2}$$

Vesan väitöskirja koostuu neljästä julkaisusta. Niistä ensimmäisen otsikko on ”Double poles and nonexponential decays in atomic physics”. Julkaisussa on teoreettisena testiobjektina vetyatomi, jonka kaksi tilaa saatetaan Zeemanin ilmiön avulla päällekkäin. Valon resonanssisironta tällaisesta tilaparista noudattaa kaavan (2) hajontalakia siten, että $a \neq 0$, $b, \ldots = 0$. Kahdessa seuraavassa julkaisussa tutkitaan samaa tilannetta hiukkasfysiikan puitteissa: A_2-mesoni tulkitaan kahden päällekkäisen resonanssin tuottamaksi. Nämä kolme työtä ilmestyivät arvostetussa Physical Review Letters -sarjassa. Väitöskirjan neljäs julkaisu yleistää käsittelyn moneen resonanssiin.

Jyväskylään!

Jyväskylän 1863 perustettu seminaari oli ylennetty kasvatusopilliseksi korkeakouluksi 1934 ja edelleen yliopistoksi 1966. Jo 1965 oli perustettu matemaattis-luonontieteellinen osasto, jonka muodostivat fysiikan, kemian ja matematiikan laitokset. Fysiikan ensimmäiseksi professoriksi nimitettiin Juhani Kantele, joka tuli virkaansa 1968. Hän lähti Jyväskylään elämäntyöhönsä, ilman aikeita hakeutua tilaisuuden tullen muualle. Kanteleen innostus ja omistautuminen viralleen sekä tarjolla olleet toimet vetivät mukanaan innokkaan nuoren fyysikkojoukon Jyväskylään, jossa myös talon omat oppilaat pian tulivat tutkimuksen piiriin.

Kokeellisen fysiikan päästessä vauhtiin 1970-luvun alussa Jyväskylään perustettiin myös teoreettisen fysiikan professuuri, apulaisprofessuuri ja lehtoraatti. Vaikka teoreettinen fysiikka oli oma oppiaineensa, se pidettiin osana yhteistä fysiikkaa ja yhteistä laitosta; muualla maassa tällainen tilanne vakiintui vasta parikymmentä vuotta myöhemmin ulkoisten hallinnollisten päätösten seurauksena. Laitosrakennus oli entinen pesula ja leipomo, jykevä tiilirakennus Nisulankatu 78:ssa. Siitä muutettiin nykyisiin Ylistönrinteen tiloihin vasta tammikuussa 1996.

Tohtoriksi tultuaan Vesa Ruuskanen oli Cernissä 1971–72. Sen jälkeen Jyväskylän innostunut ilmapiiri veti hänetkin puoleensa, ja hänet nimitettiin teoreettisen fysiikan lehtoriksi 1972. Teoreettisen fysiikan ensimmäisenä professorina oli Eero Byckling ja apulaisprofessorina Christofer Cronström. Päivämäärällä 1.9.1974 Vesa nimitettiin Cronströmin seuraajaksi ja minut Bycklingin seuraajaksi. Virkavapauksien ja virkojen täyttövaiheiden aikana Vesa oli jo hoitanut näitä virkoja.

Opetusta ja oppimista

Jyväskylän alkuvuodet olivat pioneerityötä. Fysiikan opetussuunnitelma, johon teoreettinen fysiikka integroitiin, laadittiin omista, uusista lähtökohdista sen sijaan, että olisi kopioitu Helsingin mallia. Vesa oli keskeisesti mukana tässä kehittämistyössä. Alonson ja Finnin kolmiosainen, tuolloin uusi *University Physics* oli opetuksen runko. Kaikille yhteiset kurssit Fysiikka I–VIII sisälsivät nämä kirjat sekä kahtena ensimmäisenä kurssina matemaattisia apuneuvoja. Näiden jälkeen tuli varta vasten teoreettisen fysiikan kursseina mm. analyyttistä mekaniikkaa, sähködynamiikkaa ja kvanttimekaniikkaa.

Laboratoriotyöt olivat kiinteä osa fysiikan peruskursseja. Niiden kehittämiseksi me teoreetikotkin kävimme tekemässä niitä. Kiperä ongelma, jota tuskin vieläkään on saatu täysin ratkaistuksi, oli töiden tahdittaminen luento- ja laskuharjoitusmateriaaliin. Opiskelijat joutuivat tekemään niitä osin keittokirjamaisesti, ymmärtämättä niiden perustana olevaa teoriaa. Vesa ja minä halusimme korostaa töiden arvoa luonnonilmiöiden ja niitä kuvaavien ja selittävien teorioiden demonstraatioina eikä pelkästään kokeellisten menetelmien opetteluna.

Saadaksemme läheisen tuntuman opetukseen sekä Vesa että minä pyrimme varsinkin laitoksen alkuaikoina opettamaan kaikkea, erityisesti koko peruskurssijaksoa. Vesa on jatkanut tätä tyyliä näihin saakka; syyslukukaudella 2006 hän opetti aineopintoihin kuuluvan mekaniikan kurssin. Perustutkintokurssien lisäksi Vesa on vuosien varrella pitänyt mitä moninaisimpia tohtorikoulutuskursseja, yleisistä kvanttimekaniikan ja monihiukkasfysiikan kursseista aina omaa tutkimusta lähellä oleviin erikoiskursseihin. Kun opettaa kaikkea kaikilla tasoilla, erityisesti perustasolla, on hyvässä seurassa: raskaan sarjan nobelisti Richard Feynman piti aikoinaan Kalifornian teknillisessä korkeakoulussa peruskurssisarjan, josta tehty kirja on klassikko. Kun Feynmania vähäisempikin professori opettaa peruskurssia, kurssin arvostus kohenee. Sitä paitsi harva kiistää motoksi sopivaa lausumaa, että opettamalla oppii.

Varsinaista tutkimusyhteistyötä emme Vesa ja minä tehneet, alamme kun eivät oikein kohdanneet. Sen sijaan keskustelimme yleisistä, usein opetuksen yhteydessä esiin tulleista fysiikan kysymyksistä lähes joka päivä. Yhteisenä pyrkimyksenämme oli ymmärtää perin pohjin se fysiikka, mistä puhuimme ja mitä opetimme. Tämän ymmärtämisen vaatimuksen halusimme välittää opiskelijoillemme; emme tyytyneet puolinaiseen ulkolukuun perustuviin oikeisiin vastauksiin.

Jyväskylän fysiikan opetussuunnitelmaa on sittemmin moneen kertaan muutettu ja oppikirjoja on vaihdettu. Vesa on tässä työssä ollut tiiviisti mukana, useimmiten johtavana voimana sekä virallisen aseman että tuloksiin johtaneen työpanoksen puolesta.

Runsaan ja monitahoisen kateederiopetuksen lisäksi Vesa on tehnyt suuren opetustyön opinnäytteiden ohjaajana. Hän on ohjannut kymmeniä pro gradu -töitä fysiikan eri aloilta ja lukuisia ansiokkaita väitöskirjatöitä omalta alaltaan, joka on jo pitkään ollut relativistinen raskasionifysiikka. Tutkimuskohteena on erityisesti ns. kvarkki–gluoni-plasma, aineen olomuoto, jossa ydinten protonien ja neutronien rakenneosaset irtautuvat yhtenäiseksi ”puuroksi”.

Hallintoa ja suunnittelua

Vesa Ruuskasen tutkimustyö on ollut monipuolista, mittavaa ja tuloksekasta. Tätä taustaa vasten on hämmästyttävää ja ihailtavaa, kuinka paljon hän on panostanut opetukseen sekä suunnittelijana että uutterana, tunnontarkkana suorittajana. Mutta ei tässä kylliksi. Sen lisäksi hän on tehnyt täyden rupeaman hallinto- ja järjestötehtäviä paikallisella, valtakunnallisella ja kansainvälisellä tasolla.

Jyväskylän fysiikan laitoksen hyvä perinne on, että kukin professori palvelee vuorollaan laitoksen johtajana. Laitos ei ole edes alussa ollut kenenkään ”oma” sillä tavoin kuin oli vallitseva tapa Suomen yliopistoissa aikaisemmin. Niinpä Vesa on ollut laitoksen johtajana tai varajohtajana kaikkiaan kuusi vuotta.

Matemaattis-luonnontieteellistä tiedekuntaa Vesa on palvellut moneen otteeseen, tiedekuntaneuvoston jäsenenä ensimmäisen kerran jo 1972. Hän on ollut eriasteisten tutkinnonuudistusten suunnittelijana ja toteuttajana. Näistä tärkein, Euroopan tutkintojärjestelmää yhtenäistävä ns. Bolognan prosessi, on nyt toteutusvaiheessa. Vesa on ollut tiedekunnan Bologna-työryhmän puheenjohtajana vuodesta 2002 tähän saakka. Jokavuotiseksi vakiintunut tiedekunnan kansainvälinen kesäkoulu syntyi 1991 Vesan ja tietotekniikan professorin Pekka Neittaanmäen aloitteesta.

Suomen Fyysikkoseuran puheenjohtajana Vesa oli 1978 (silloisen tavan mukaan puheenjohtaja vaihtui vuosittain). TFT:n johtokunnan jäsenenä hän oli jo 1978–82 ja vt. puheenjohtajana 1990-luvulla, vaikeana aikana, jolloin TFT oli uudelleenjärjestelyjen kohteena. Vuosina 1995–98 hän oli hiukkas- ja ydinfysiikan kansallisen tutkijakoulun johtaja ja vuosina 1998–2000 Suomen Akatemian luonnontieteiden ja tekniikan tutkimuksen toimikunnan jäsen.

Vuosina 1993–98 Vesalla oli tärkeitä tehtäviä Kööpenhaminassa toimivan yhteispohjoismaisen teoreettisen fysiikan tutkimuslaitoksen Norditan hallinnossa; hän oli johtokunnan jäsen ja toimi mm. Pohjoismaiden ja Baltian maiden tutkimusyhteistyötä edistävän toimikunnan puheenjohtajana. Näiden yhteyksien lisäksi hän on ollut järjestämässä lukuisia raskasionifysiikan kansainvälisiä kokouksia sekä ulkomailla että Suomessa, mm. Jyväskylässä 1994 ja 1995.

Tieteellisten ansioittensa perusteella Vesa valittiin Suomalaisen Tiedeakatemian jäseneksi 1994. Maamme toinen, ja iältään vanhin, kaikkien tieteenalojen akatemia, Finska Vetenskaps-Societeten – Suomen Tiedeseura, myönsi hänelle Th. Homénin palkinnon 2004.

Teoreettisen fysiikan professori

Menin eläkkeelle 31.12.1995, juuri ennen kuin fysiikan laitos kaikkineen muutti Nisulankadulta Ylistölle (kiihdytinlaboratorio oli muuttanut jo aikaisemmin). Virkani oli pantu avoimeen, kansainväliseen hakuun jo hyvissä ajoin ennen eläköitymistäni. Asiantuntijalausuntojen mukaan parhaat hakijat olivat talon sisältä, mutta laitoksen vuosikymmenisen hyvän hengen mukaisesti hakuprosessi eteni sopuisasti.

Tasavallan presidentti Martti Ahtisaari allekirjoitti 6.9.1996 nimityskirjan, jolla apulaisprofessori Vesa Ruuskanen nimitettiin teoreettisen fysiikan professoriksi 1.10.1996 alkaen. Minulle oli suuri ilo, että ystäväni yli kolmenkymmenen vuoden ajalta tuli seuraajakseni virkaan.

Vesa Ruuskanen avaamassa Suomen Fyysikkoseuran kokousta Siltavuorenpenkereellä marraskuussa 1978. Esitelmöijänä on edellisvuoden nobelisti sir Nevill Mott (etualalla). Finnish Physical Society Chairman Vesa Ruuskanen introducing the speaker, Nobel Laureate Sir Nevill Mott, at Helsinki University in November 1978. Photograph from Pertti Lipas.

Limes ry:n ekskursiolla Englantiin v. 1962. Yläkuva: Timo Alanko ja Vesa Ruuskanen. Alakuva: Vesa (aurinkolaseissa) ystävineen katsomassa Düsseldorfin kellopeliä. On an excursion to England in 1962. Top: Timo Alanko and Vesa Ruuskanen. Bottom: Vesa (with sunglasses) and his friends watching the glockenspiel in Düsseldorf. Photographs from Timo Alanko.

Aune ja Vesa Ruuskanen Pertti Lippaan 60-vuotispäivillä 18.6.1992. Aune and Vesa Ruuskanen celebrating Pertti Lipas's 60th birthday. Photograph from Pertti Lipas.

Ruuskasten pihatalkoot 22.5.1999. Vasemmalta Pertti Lipas, Aune Ruuskanen, Vesa Ruuskanen ja Jussi Timonen. Work party in the Ruuskanens' garden. From the left, PL, AR, VR and JT. Photograph from Pertti Lipas.

Ruuskasten pihatalkoot 22.5.1999. Etualalla vasemmalta Matti Manninen, Vesa Ruuskanen ja Rauno Julin. Work party in the Ruuskanens’ garden. From the left in front, MM, VR and RJ. Photograph from Pertti Lipas.

Ruuskasten pihatalkoot 22.5.1999. Vasemmalta Ari Jokinen, Pertti Lipas, Jussi Timonen ja Shadyar Farhangfar. Work party in the Ruuskanens’ garden. From the left, AJ, PL, JT, SF. Photograph from Pertti Lipas.

Vesa 60-vuotispäivillään juttusilla Riittaliisa ja Pertti Lippaan sekä muiden ystäviensä kanssa.
Vesa chatting with Riittaliisa and Pertti Lipas, and other friends at Vesa's 60th birthday party.
Photograph from Pertti Lipas.

Various Phases of Accelerator and Nuclear Physics at Jyväskylä

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70's at Nisulankatu 78

Vesa Ruuskanen joined the physics department at Jyväskylä in the fall of 1972 when the authors of this article were undergraduate students in the same department. Since the department at that time was quite modest in size, we both were already teaching assistants in the department and were about to start our graduate studies. Juha was supervised by Kalevi Valli and Rauno by Juhani Kantele (Justus). We were on our way to become nuclear physicists.

From the very beginning Vesa Ruuskanen, briefly RVesa, became known to us as an excellent teacher but also as a friend whom we often met in the coffee room of the physics department located at the address Nisulankatu 78. Despite his own research interests in particle physics we remember him being curiously interested in everything in physics and always ready for discussions also with us, junior students. At this time the physics department was preparing for the startup of a small 20 MeV cyclotron that was going to be built in Sweden. Somehow, this cyclotron did not turn out as it was expected, and it had to be basically rebuilt by the staff lead by Esko Liukkonen. We as students were also deeply involved in this process and the basic education we had received in the physics department had to be put into full use. We were still the students of the older generation where ”old-style” physics curriculum was still followed. This changed completely when the overall renovation and restructuring of physics curriculum was done during 1978-79 under the leadership of Vesa who was now an associate professor in theoretical physics. Physics and theoretical physics were under the same roof although separate subjects at that time. This encouraged one of us (Juha) to also study theoretical physics up to the laudatur level. In fact, he prepared a second master thesis under the supervision of Vesa on ”the coherent production of pions in nuclei”.

The JYFL atmosphere

The coffee room of the physics department became famous worldwide. Several visitors to the department often mention ”the good old days” when, no matter what time of the day, one always found someone in the coffee room ready for chatting. In addition to many important physics discussions also sports and politics were often de bated. Particularly, we remember Vesa and Justus being very active on any sports issues. Often bets on winners or losers in big sport events were suggested by them, and the formulations of these bets, were skillfully tailored by these two persons so that you almost always lost if you agreed to join a bet.

Various types of joint sport activities have always played important role at JYFL and Vesa has been the most active participant. Volleyball was important in the 70's. Later, long distance skiing and rowing became highly appreciated. Vesa was qualified to the JYFL top team in the 24 hours skiing relay. Vesa frequently participated in all sorts of work parties, particularly the ones for purchasing a brand new church boat for JYFL. Naturally, Vesa was one of the 14 rowers in the JYFL rowing team, which used to be one of the top teams in the legendary 60 km race at Sulkava.

Also, the social life among the personnel was busy, little Christmas parties being the annual highlight, sometimes even lunch-breaks developed into long lasting social events. The spirit of the physics department was very positive, no walls between different areas of physics existed, and support for the young researchers and students was always there. In nuclear physics, our own area of interest, we were encouraged for trying new ideas and taking risks. This was the time when Jyväskylä was becoming known in nuclear structure physics for its innovations, for example, on IGISOL technique and E0 transitions. An important factor in the excellent spirit was the Friday morning seminars, later called colloquiua. The attendance in these was a must. Vesa we remember already since those days being active participant in these seminars with his penetrating but constructive questions and comments regardless of the topic.

Towards new phases

At the turn of the 1970s and 80s we both left the physics department to become postdocs abroad, in Berkeley and Jülich. At this time, research at the small cyclotron was still flourishing and led in the 1980s to many important results, including the works on discoveries of a large number of new neutron-rich isotopes and nuclear shape coexistence. International visitors were frequent in the department already then. However, it became obvious that more ambition was called for. A new superconducting cyclotron was proposed by the physics department with strong national support in 1983. However, this proposal was considered by the State's Science and Technology Council too expensive on the national scale. Soon after, later in 1986, another proposal based on a room-temperature cyclotron equipped with a modern ion source, based on new electron cyclotron resonance principle, was proposed and approved in the 1987 State budget. This started an important new phase in the department's history. Vesa became the head of the physics department in 1987, and held this position during the construction period of the new cyclotron.

His support was crucial in this process where the main part of the department's resources were directed to this process. The new accelerator laboratory was also accompanied by the start of the planning and construction of the new physics department in the Ylistönrinne Campus area. The cyclotron project was successfully completed in January 1992 with the acceleration of the first beam of 40 MeV alpha particles. Prior to his becoming the department chair Vesa spent an academic year 1986-88 at the University of Illinois. We believe that in these years he became interested in relativistic heavy ion collisions. This was to play an important role later in further developments of the Finnish nuclear physics.

Renewed graduate training

The new accelerator laboratory cranked up its research in 1993-94. It was and still is part of the physics department which has been obviously a good choice. The unity of the physics department, a principle created in the 1970s and 80s, was again found an important asset to successful research and education in this field. Both undergraduate as well as graduate students trained properly have always been an important factor in the successful operation of this facility. Two initiatives of Vesa are important in this connection. The start-up of the Jyväskylä international Summer School in 1991 and the National Graduate School in Particle and Nuclear Physics (GRASPANP) in 1995 have played important roles in our activities. The renewed and organized graduate training in Finland set up in the mid 90s has had a tremendously positive impact on training of young scientists in these fields. In evaluations, the GRASPANP school is regarded as one of the most successful graduate schools in Finland. Its activity covers all the PhD students of the field in Finland independent of the funding source. Eventually, after more than ten years of its operation, the accelerator laboratory has been a true success story. It has become the Finnish Center of Excellence as well as the European large scale research infrastructure under the leadership of both of us. The laboratory has a large user community of the order of 200 scientists from Finland, Europe and overseas.

Creating HIP activities

Vesa was called to Helsinki to become the chair of the Research Institute of Theoretical Physics (TFT) for 1993-95. At these times an idea was put forward by a one man committee (Prof. Jorma Routti) to combine three physics research institutes operating in the Helsinki region into one, to be called later as the Helsinki Institute of Physics (HIP). These institutes were TFT and SEFT (Institute for High Energy Physics) at the University of Helsinki and HTI (Institute for Particle Technology) at the Helsinki University of Technology. This new institute was founded by a special charter on July 27th, 1996 signed by the president of Finland, Martti Ahtisaari. This charter stated as the tasks of the institute to conduct fundamental and applied research as well as graduate training. In addition it was given the task to be the coordinator of the Finnish research activities at CERN and participate in other international particle physics collaborations. The spirit was that this institute were to be a national undertaking, although it was initially founded as a common institute of the University of Helsinki and the Helsinki University of Technology. As the accelerator laboratory representatives we both were consulted in the process of defining the mission for this institute. In practice, the structure of HIP was based on the programme structure which initially included four programmes, theory programme, LEP-programme, LHC-programme and technology programme.

In 1996 one of us (JÄ) was visiting CERN as a research associate. Keijo Kajantie (at that time also at CERN) and Vesa were working hard to convince us, the low energy nuclear physics community, to join the relativistic heavy ion programme at CERN. The Finnish theoretical activity in this field had already been world class for some years and it was natural that some experimental contributions should as well be created. Later, the Jyväskylä group was approached by the first director of HIP Eero Byckling with a request to initiate CERN related nuclear physics research in HIP. Finland was already active in the ISOLDE experiment at CERN and the proposal was that we should also join the ALICE experiment already under planning for the LHC. After preparatory work the teams at Jyväskylä were ready to join HIP with a special project dedicated to Nuclear Matter Physics. This project was started in the beginning of 1998 and it was directed by Vesa under the LHC programme of HIP. In line with this development Finland joined the ISOLDE experiment in 1997 first with the support of the Academy of Finland and later in 1999 as part of the HIP Nuclear Matter project.

Despite the difficult period in his life due to a tragic accident Vesa did not give up. He continued his support for the HIP project. Before the fall 1998 he was instrumental in negotiating the Finnish core funding for both CMS and ALICE experiments. This lead finally to an agreement between three universities (HU, HUT and Jyväskylä University), the Academy of Finland and HIP to cofinance 6 million Swiss francs for the construction of CMS and ALICE experiments of which 1 million was devoted to ALICE. The agreement was signed at the end of 1998. At the same time the Interim memorandum of Understanding was approved by the HIP Board in October 1998. Finland was therefore ready to join fully the heavy ion experiment at CERN. But how and by whom?

At this instance Jyväskylä group was given a charge to organize the Finnish contributions to the ALICE construction. These contributions were finally decided to be on the T0 detector as well as on the construction of a part of the silicon drift detector layers of the inner tracker system (ITS). The final Memorandum of Understanding was then signed by Dan-Olof Riska, the new director of HIP in September 2000. The coordination of T0 was eventually given to Finland and Jyväskylä under the leadership of Wladek Trzaska. Realization of the Finnish contribution to the ITS has been led by Markku Oinonen and the work has been carried out at the detector laboratory of HIP in Helsinki. Both contributions have been carried out very successfully. Starting these major contributions with initially very little experience and their successful realization has been a remarkable achievement. The T0 project is nearing its completion and will be ready by the summer 2007. The completion of the delivery of 715 ITS detector modules was completed in August 2006 in record time: The Finnish group was the first one of three ITS contributors (Helsinki, Trieste, Strasbourg) to complete their delivery.

From the beginning of 2002 the University of Jyväskylä had become the third partner university of HIP. Following this, a new programme was created at HIP, Nuclear Matter Programme, which included both the ALICE as well as the ISOLDE as projects that are currently lead by Markku Oinonen and Ari Jokinen, respectively. Both of them are the Jyväskylä graduates in nuclear physics. The overall programme is currently lead by one of us (JÄ).

While the construction of ALICE was progressing well, it also became evident that the Finnish participation in ALICE physics was becoming an issue. Again here, with the help of Vesa and his theory team a substantial progress has been already made. First of all, Jan Rak from Brookhaven was hired as a chief scientist to lead the Finnish physics team in ALICE. Constant support of the JYFL theory group and intense interaction between experimentalists and theorists in the old physics department spirit was starting to make impact immediately. Eventually, Nuclear Physics in Finland and particularly in Jyväskylä is becoming broad in its nature covering the field from the lowest up to the very highest energies available in the world of accelerators when the LHC machine starts its operation in 2007.

The 25th Anniversary event in Nisulankatu and Ylistönrinne accelerator laboratory. The year must have been 1990. Vesa might have just stepped down as the head of the department. Photographs from Juha Äystö. **Top:** In front row Pertti Lipas and Martti Hämäläinen; from the left in the 2nd row: Juhani Kantele and Rauno Hämäläinen, from the left in the 3rd row: Vesa, Esko Liukkonen, Juhani Korvola. **Middle:** Ahti Pakkanen and Vesa. **Bottom:** Visiting the accelerator laboratory. From the left: Esko Liukkonen, Pertti Lipas, Juha Äystö, Vesa, Jussi Timonen.

One of the 24 hours skiing relay competition in Jyväskylä is over. The team from the left: RVesa, Pauli Heikkinen, Hannu Häkkinen and Rauno Julin. Photograph from Rauno Julin.

Preparations for the 1st Sulkava 60 km rowing race under guidance of Vesa (standing next to the boat in front). Photograph from Rauno Julin.

JYFL personnel in 1976 at Nisulankatu. Vesa is in the 2nd row, 4th from the right. Photograph from JYFL archives.

JYFL personnel in 1996 in the brand-new building at Ylistönrinne. Vesa is in the 1st row, 4th from the left. Photograph from JYFL archives.

Vesa Ruuskanen – Dictator of Teaching

Jukka Maalampi

Head of the Department of Physics, University of Jyväskylä

I met Vesa for the first time in mid-seventies in a winter school held in the Tvärminne Zoological station on a beautiful Hanko Peninsula in the southern-most area of the Finnish mainland. There were two invited lecturers in the school; Finn Ravndal's topic was the Ising model, and Paul Olesen talked about Nielsen-Olesen vortices. Practically everyone working in the field of particle theory in our country attended the School, including graduate students. Vesa was at that time already in associate professor's position in the University of Jyväskylä, I was just about starting my doctoral studies at the University of Helsinki. I had heard Vesa's name often mentioned in our department, most frequently by Keijo Kajantie but also by many other people, and the impression I had got about him this way was most positive: a very solid man in his research and a nice guy in all respects. The week in Tvärminne showed that this impression was correct.

The particle physics research in Finland was in the seventies concentrated in Helsinki. There were a couple of small additional outposts in other places, one in Jyväskylä. Department of Physics in the University of Jyväskylä was founded in 1966, and from the beginning its research activity was concentrated in experimental nuclear physics. In 1968 theoretical physics was defined as a major of its own in the study curriculum, and soon posts of professor and associate professor in theoretical physics where founded. The field of the professorship was defined as low-energy nuclear physics, and the position was held until 1995 by Pertti Lipas. The field of the associate professor's position was not specified, and Vesa was appointed to that position in 1974. Later, in 1996, Vesa followed Pertti Lipas in the professor's position. In spite of a heavy teaching load and the long-lasted lack of any larger local particle physics community Vesa was able to carry on his active and internationally highly recognized research work. The recruitment of Kari J. Eskola to the group in late nineties made it possible to get the graduate schooling in particle physics to a regular basis and the growth of the research group. The Jyväskylä group of ultra-relativistic heavy ion collision physics is highly recognized internationally.

I started working in the Department in 2001, and soon after me Kimmo Kainulainen joined the group. The research fields of Kimmo and myself, electroweak interactions and cosmology, were complementary to the fields of Vesa and Kari, as well as that of a younger senior member of the group Kimmo Tuominen, who all worked on strong interactions and ultra-relativistic heavy ion physics. This made it possible to offer to our students a quite versatile lecture program and study options in high-energy physics. What once was a one man's mission of Vesa is now a group of theoretical high-energy physics with altogether some twenty researchers and graduate students with wide interests and knowledge.

Apart from his research work and lecturing, Vesa has played many other important roles in our Department, one of them was his overall responsibility on the teaching. Sometimes in nineties it became apparent that teaching was becoming a weakness of the Department and called for actions. Teaching was not well organized, the material taught in the courses was not always well thought, and so on. All power in teaching matters was decided to be given to one person, Vesa, and he was given the title "the dictator of teaching". In this position he took care of the development of the curriculum and also watch over the quality of teaching. Vesa himself always takes his own teaching very seriously and puts a lot of effort to make his courses clear and easy to follow but on the other hand demanding enough. He always has time and patiency for the students entering his office with their questions. He made his best to imprint this attitude on the minds of all of us taking part in teaching.

In accordance with the European Union Bologna Declaration, a comprehensive curriculum reform to a two-tier study system was made in our University a couple of years ago. Vesa took responsibility of organizing this reform in our Department and in the Faculty. One must say that this was not a kind of task that people had competed very much for as the relevance of the whole reform was considered questionable. Vesa did without too much ado what was necessary to do trying to get any possible good out of it. This is just one example of Vesa's many services to the Department and the Faculty during his thirty-plus years' career.

What I now know about the dynamics and spirit of the Department I first learned from Vesa. I have benefited a lot from his views and opinions, based on his experience and long perspective, as well as on his common sense attitude, in my present administrative post. The countless discussions with Vesa during the common lunch and coffee breaks of our group have also been most enjoyable. Sometimes these conversations have concerned physics but more often other topics like biology, history, computing, politics, and all between - and of course sports. Vesa used to be until his tragic accident and paralysis a passionate skier himself, which I learned already during the Tvärminne School. There was plenty of snow that winter on the Hanko Peninsula.

We all hope that Vesa will continue after his official retiring his research activities in the Department - and have lunch and coffee with us for many years to come. Actually, in order to guarantee this to happen, we have made an emeritus contract with him covering the next five years. The contract is renewable.

Vesa studies the emeritus contract in his office. Photograph from Jukka Maalampi.

Vesa ja Aune Ruuskanen purjehtimassa Päijänteellä Esko Liukkosen veneessä 21.7.1993. Vesa and Aune Ruuskanen sailing on Lake Päijänne with Esko Liukkonen. Photograph from Esko Liukkonen.

Esko Liukkonen Vesan 60-vuotispäivillä 27.7.2000. Esko Liukkonen at Vesa's 60th birthday party. Photograph from Esko Liukkonen.

Rendezvous with Vesa during 37 Years

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Early Years in Helsinki

I began my studies in theoretical physics at the University of Helsinki in 1968. It was a special study program, approbatur in physics in one semester, initiated by Professor K. V. Laurikainen who also gave the lectures of the course during the first semester. The students selected to this course formed an eager group, and many of them met regularly in the theoretical physics library to work out homework problems. The freshmen of 1968 in Helsinki were the first students in Finland who had the University Physics by Alonso and Finn as their text book. My first exposure to the wonders of modern physics had however happened during the final years at school, when I read the book Modernin fysiikan alkeet (Introduction to modern physics) by K. V. Laurikainen. That was the main inspiration for me to begin my studies in theoretical physics.

As rapid progress in the physics studies was then greatly encouraged, a theme that has remained popular in Finnish Universities ever since, the Mathematical methods I course, part of the approbatur requirements in theoretical physics, was also arranged as a summer course in June 1969. Most students in the intensive study program attended this course. This was the first time I met Vesa: he was one of the teachers on the course. As he had a research position at the Research Institute for Theoretical Physics, he did not take part in teaching on a regular basis. Vesa made then a lasting impression on me even though he was not the teaching assistant in my problem classes. This was in fact the only course in my studies where Vesa took part in teaching. Therefore no personal relationship was formed between us during our time in Helsinki. Pertti Lipas was then the most visible senior teacher to young undergraduate students in theoretical physics.

The second time I was listening to Vesa was in his dissertation in 1970. It took place in the main lecture hall of the then new nuclear physics building at Siltavuorenpenger 20. Pertti Lipas was the examiner (the ’opponent’), and, as perceived by a young undergraduate student, it seemed to proceed very well with Vesa providing thorough answers to the questions posed by Pertti. There was however one incident which made this dissertation memorable, and which I have not personally seen to happen ever since. After Pertti had finished his questioning, Vesa asked as usual those members of the audience, who had something to say about his thesis, to ask for permission to speak from the custodian, Pekka Tarjanne. And there was indeed someone who asked for such a permission and posed an additional question to Vesa. The person who stood up from among the audience was Keijo Kajantie. Unfortunately I cannot recall his question, or whether he seemed to be satisfied with the answer he got. Judged from their continuous and successful collaboration there after, Vesa's answer to that question was obviously quite all right.

Time in Jyväskylä

Very soon after completing his PhD, Vesa left the theoretical physics in Helsinki, first to CERN and then to Jyväskylä. He and Pertti both had a professorial appointment at the University of Jyväskylä in 1974. In the spring 1975, Pertti in particular often visited the Department of Theoretical Physics in Helsinki, trying to persuade younger teaching assistants to apply for assistant positions in the Physics Department at Jyväskylä. At that time all assistant positions in Helsinki were occupied by rather senior scientists who did not have a permanent position, and who in practice never were in the Department as they always had temporary appointments to more senior positions elsewhere. A possible three-year appointment as an assistant in Jyväskylä had therefore some appeal to me, and I was appointed to such a position in theoretical physics starting on the first of August 1975. In practice I arrived in Jyväskylä a year later, having first completed my eleven-month military service. That is how I became a junior colleague of Vesa.

The atmosphere in the Physics Department at Jyväskylä was inspiring. The Department had only existed for a decade, and all staff were relatively young and hungry of success in a positive way. Discussions around the coffee table were frequent and spirited, and more often than not were related to problems in physics. Figure 1 is a photograph taken in the coffee room at Nisulankatu in December 1981.

Vesa played an important role in maintaining and developing this atmosphere of curiosity and interest in science, together with a few other members of staff. It was only many years later, however, that I gave problem classes in a course in which Vesa gave the lectures. It was the course in statistical mechanics. I admired the clarity by which Vesa introduced the basic concepts of statistical physics to the students, who often find this course very difficult. His lectures were the model which I followed when I took over after him the lecturing of this course, exactly twenty years ago. I gave this course again this semester, and used many problems in my problem classes which Vesa had used in his course, and which had not been used for a long time. Many of his problems would now be considered as very demanding by the students.

Very soon after starting to lecture the statistical mechanics course, I decided to give a more advanced course in statistical physics. The audience in this course turned out to be somewhat exceptional. It included in addition to Vesa, Risto Nieminen, now Professor of Physics

Figure 1: The coffee room in the old Physics Department at Nisulankatu. Vesa with his moustache sitting in font, and on his left first Matti Piiparinen and then Jari Laakkonen. JT is trying to find a place at the table. Photograph from Jussi Timonen.

at Helsinki University of Technology, Jouni Suhonen, now Professor of Theoretical Nuclear Physics at Jyväskylä, and some other members of staff at that time. Those who know these three gentlemen can imagine that it was often difficult to stop them talking so that the lectures could have been continued. Basic problems in statistical physics were very thoroughly discussed during that course, and I have never again had an equally active audience in any of my lecture courses. Fortunately there was also one student who attended the course, and passed an exam afterwards. He was Pekka Pakarinen, who later became an important figure in the research and development at Valmet Paper Machines, now Metso Paper.

I have not collaborated with Vesa but with two of his former students. In both cases the motivation for collaboration can be traced down to hydrodynamics. At some point I shared an office with Jouni Suhonen and Markku Kataja, the latter of whom made his PhD for Vesa. We discussed frequently about all matters of interest, including physics. At that time Pekka Pakarinen, who had recently gone over to Valmet, frequently came back to ask advice on various problems related to paper making. Eventually this lead to Markku working full time on modelling wet pressing of a paper web. As described by him elsewhere in the book, a covariant formulation of the problem was derived so that, in principle, any further increase in the speed of paper machines would not necessitate relativistic corrections to the model. Later on Antti Koponen, who did his Master's Thesis for Vesa, became my post-graduate student. He made his

PhD Thesis on a special and then novel lattice formulation of hydrodynamics based on solving a discrete version of the Boltzmann equation, instead of directly solving hydrodynamic equations. This lattice-Boltzmann method has proved to be very powerful, and its development and application has by now lead to half a dozen PhD Theses.

I will not deal in any detail with the numerous extra-curriculum activities in the Physics Department, in which Vesa played such an active and important role, and which were so important to the special atmosphere that prevailed for so long in the Department. Very particular memories are related to the rowing activities, to the many hours spent together in the 'church boat' on various lakes in Finland, to the efforts of collecting money for this boat, and to the hours spent in maintaining it. It is evident from Figs. 2, 3, 4 below that it was great. After his accident, which meant an effective stop for such physical activities, Vesa spent a while in a rehabilitation centre in Helsinki. When I visited him there, the nurse who took me to see him asked if I was his brother. Well, Vesa has always appeared younger than he really is.

Figure 2: Practicing on Lake Päijänne before the race at Sulkava in the summer 1991. The smiling faces from left to right belong to Vesa, Seppo Valkealahti, Seppo Mäkinen and Matti Rinta-Nikkola. Behind Seppo M there is Sari Törmänen, and behind Matti R-N Markku Kataja. Ari Lampinen is standing. Photograph from Jussi Timonen.

Figure 3: Mental and physical preparations before the start at Sulkava in the summer 1991: JT and Vesa. Photograph from Jussi Timonen.

Figure 4: Vesa is relaxed after having finished the 1991 race at Sulkava. Sari Törmänen is having her share of the well-deserved champagne. Photograph from Jussi Timonen.

Meritullinkatu 8 A, Siltavuorenpenger 20, 1968-69

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The following contains some recollections from the end 1960's when Vesa and I were in the beginning of our careers and were working together in an old building in the center of Helsinki. The building was structurally unstable and the floors and staircases were supported by heavy wooden blocks. It was heated by a stove in each room, each morning in winter the janitor carried in firewood and lighted a fire. Very cosy and warm.

First a few words of background. On the personal level, I had spent the years 1966-1967 at CERN working mainly on strong interaction phenomenology. Early sixties was the time of rapidly increasing spectrum of strongly interacting elementary particles, hadrons. Ground states of hadrons ($h = p, \pi, K, ..$), the QCD (to be discovered a decade later in 1972) particles, were well known and high energy beams of them were produced by new accelerators at CERN and Brookhaven. Both 2→2 collisions of type $pp \to pp$, $\pi^\pm N \to \pi^\pm N$, $\pi^- p \to \pi^0 n$, $Kp \to KN$ and inelastic collisions of type $hh \to$ many hadrons were measured, mainly in bubble chambers. In the reaction $\pi^- p \to \pi^- \pi^0 p$ the $\pi^- \pi^0$ were observed to be produced as a ρ^- resonance, which rapidly decayed. Similarly, in $\pi^- p \to \pi^+ \pi^- \pi^0 \pi^- p$ one observed an $\omega^0 \to \pi^+ \pi^- \pi^0$ resonance production and decay. Some systematics was observed in the hadron spectrum, Hagedorn noticed its exponential growth with mass at the end of 1964, its flavour SU($N_f = 3$) symmetry was noted and built in the quark model (Gell-Mann, Zweig, 1965). In searching for the correct reaction dynamics one either tried to get it from general constraints of unitarity, analyticity and crossing symmetry (Chew, 1962) or resorted to the phenomenological Regge pole model. In this the amplitude of a high energy $2 \to 2$ reaction was written in the form

$$A(s,t) = \beta(t) \left(\frac{s}{s_0}\right)^{\alpha(t)}, \quad \alpha(t) = \alpha(0) + \alpha' t \tag{1}$$

where $\beta(t)$ is an unknown residue function, $s \gg -t$ effectively the energy, $s \approx 2m_N E_{\text{beam}}$, t the scattering angle, $t \approx -s/2\cdot(1-\cos\theta)$, s_0 a reference energy scale and $\alpha(t)$ a "Regge trajectory" with the "Regge intercept" $\alpha(0)$ correlated with the quantum numbers of the collision. Chan Hong-Mo at the CERN theory division had in 1965 with great skill and success applied this model to the whole set of $2 \to 2$ reactions. My later career was largely determined when he recruited me to his group trying to extend this to $2 \to 3$ reactions with the help of a double Regge model: in $ab \to 123$ two subenergies s_{12} and s_{23} in the final state can be simultaneously large and a double Regge form for the amplitude can be written down [1]. This was particularly relevant work at CERN where two experimental bubble chamber groups were producing data of type $\pi^- p \to \pi^- \pi^+ n$ and $K^- p \to \bar{K}^0 \pi^- p$. One of the difficulties here was complicated kinematics. I had to work a lot on this, which eventually in 1973 lead to the text book with E. Byckling [2].

While I was at CERN prof. Kalervo Laurikainen had continued his work for building up the resources for theoretical and experimental particle physics. He was a theoretical nuclear physicist but it is he who started experimental particle physics in Finland by collaborating with the Nordic bubble chamber groups. One of his achievements was obtaining an associate professorship for nuclear physics. I had in the usual Finnish academic tradition applied for this already in April 1967 and, after my return to Helsinki in January 1968, took care of this professorship until August 1969 when I returned to CERN for one more year.

The sixties in Finland were a unique period of growth in the university system. In Helsinki Pertti Lipas was a new associate professor of theoretical physics from 1964, Pekka Tarjanne the professor from 1967. I may illustrate the growth by the number of professorships I was able to apply for. They were, ordered according to the date of application (P=professorship, AP=associate professorship):

- AP/theoretical physics in Oulu, 8.4.1967 and AP/physics in Helsinki 25.4.1967. These two I cancelled since I did not want to travel to Finland for the test lecture.

- AP/nuclear physics in Helsinki, 30.4.1967. The experts were Hamilton, Copenhagen and Fogel, Åbo (after many candidates had declined). They ordered Hamilton: 1. Roos 2. Kajantie and Fogel: 1. Kajantie 2. Roos and after many academic meetings the final decision, choosing Roos, was made on 29.5.1969.

- P/theoretical physics in Oulu, 25.6.1967. Final decision 1. Kallio 2. Byckling 3. Kajantie on 23.4.1968

- P/theoretical physics in Turku, 9.11.1967. Final decision 1. Mansikka 2. Byckling 3. Kallio was made on 29.5.1969.

- AP/physics in Helsinki, 16.3.1968. Experts Jauho, Korhonen, Lounasmaa. The faculty first ranked the applicants 1. Inkinen 2. Kajantie 3. Valli on 19.2.1970 but after a round of academic infighting the ordering was reversed by the chancellor; Kajantie was appointed from 1.9.1970.

- P/theoretical physics in Jyväskylä, 16.4.1968. Final decision 1. Byckling 2. Kajantie 3. Lipas on 27.10.1969.

The situation differs in a striking way from the one in 2006. In the sixties the situation was more or less first come first served and those appointed then were in their early thirties and more or less occupied the positions for decades. Many of those who then did not land a job or were not even trained for competition yet, like Vesa Ruuskanen, also succeeded in getting something in the seventies. Now in 2006 there is a large number of good candidates for each position and new professors tend to be in their forties or fifties.

When returning to Helsinki I started, from January 1968, taking care of the duties of Laurikainen's associate professorship in nuclear physics. Pending the construction of a new building at Siltavuorenpenger, Laurikainen's new experimental particle physics group had been assigned office space in an old decrepit building at Meritullinkatu 8, already described above. This building was later torn down and replaced by a modern building, now occupied by the ministry of interior. Some ten years later everyone had wanted to restore it and keep it. I was lecturing and working with younger members of the experimental group, Peter Lindblom, in particular, on Regge physics and kinematics. In the summer of 1968 Gordon Kane from Ann Arbor visited Helsinki and the Liperi Summer School - thanks to his contacts with Pekka Tarjanne - and it was quite natural to team up and work on the process $\pi^+ n \to \omega p$, the outcome is in [3].

In my letter of 7 September 1968 to Jorma Tuominiemi, who then was at CERN working in bubble chamber experiments, I find the sentence "Vesa Ruuskanen sits at the desk left by Peter Lindblom [went also to CERN] and he, of course, is a very nice guy". Indeed, we had a very enjoyable time. Particularly cherished memories are those of sausages warmed up at the end of long sticks for lunch in a fire we put up in the stove in our offices - the janitor's morning fire had already died out. I wonder if there are other particle theorists who have done the same in their university offices. And the alternative also was memorable: going for lunch to Hilkan Baari across the street to have a Hilkan Pannu to the tune of 3 mk 10 penni, some 60 cents in today's Euro currency.

In the middle of sixties accelerators were able to produce pion beams of energies up to almost 10 GeV and big bubble chambers like the CERN 2m BC had been built to observe the reaction products. Large cross section processes like $\pi N \to \pi\pi\pi...N'$ were photographed and laboriously measured and converted to cross sections. In a process like $\pi^+ n \to \pi^+\pi^-\pi^0 p$ the three pions were observed to frequently appear as if they were first produced as a spin 1^- vector meson ω^0 of mass 780 MeV. Understading the physics in the angular distributions and spin properties of the reaction $\pi^+ n \to \omega^0 p$ were then clearly of theoretical interest. This is what I set out to study with Vesa, analysing this process together with a closely related process $\pi^+ p \to \omega^0 \Delta^{++} \to \pi^+\pi^-\pi^0\pi^+ p$, generalising the model I had worked on with Gordon Kane.

What connects these processes is that both are dominated by a ρ exchange in the t-channel. This is obvious for anyone having worked with strong interaction processes at those energies, but what about a young cosmologist in early 2000? Expressed in somewhat physical language, in $\pi^+ n \to \omega^0 p$ the ω^0 prefers to go in the same direction as the beam particle π^+ and this is done by sending a 2-pion state ρ to the nucleon part of the reaction.

The complexity of the problem is reflected in the fact that, for example, the process $\pi^+ p \to \omega^0 \Delta^{++}$ depends altogether on $2 \cdot 3 \cdot 4 = 24$, counting spin states of the particles, complex amplitudes, each a function of energy and scattering angle. Ideally, an ultimate theory of strong interactions would permit one to compute these amplitudes - although nobody probably would care to do this. At that time QCD was still some three years in the future and even now, almost 40 years later, there is not the slightest idea of how one could do the computation. Soft QCD phenomena, those in the confinement domain, are basically incalculable and seem to remain so.

Clearly a large number of assumptions were needed to make the problem tractable in 1968. When reading the paper now they seem quite reasonable and physical and leave finally only one spin amplitude, to leading order. Adding some details to the amplitude (1) this was

$$A(s,t) = \beta(0) \left(\frac{s}{s_0}\right)^{\alpha(0)} (-t) e^{[i\pi(1-\alpha(0))/2 + \alpha'(\log(s/s_0) - i\pi/2)]t}, \tag{2}$$

where $\beta(0)$ gave the overall magnitude (and was undetermined) and $\alpha_\rho(t) = \alpha(0) + \alpha' \approx 0.5 + 0.85t$ is the ρ trajectory (even in 2006 very much alive and well, also in string theory). The second factor gives the energy dependence $\sim |A|^2/s^2 \sim 1/s$ for the cross section and the $-te^{[\ldots]t}$ factor, with the somewhat mysterious phase factors from Regge theory, the angular dependence and forward peaking.

The Regge phase factors in (2) caused us unexpected technical problems. We were evaluating the expressions using an IBM350/60 computer. The fortran code was rather straightforward, but somehow the outcome seemed wrong. It is notoriously easy to be blind to one's programming errors and we worked for months to find one. Ultimately, after recruiting the assistance of a computing specialist, the problem was localised to the fortran compiler. If C was a complex constant and we wrote in the fortran code $A = 1. - C$ (here 1. could be any real number) an erroneous and unsystematic result was produced, although even at that time fortran was expected to handle complex arithmetic. However, if we wrote $B = 1., A = B - C$, correct results were produced. No wonder it took a long time to discover this problem. We wrote a letter to IBM, but I do not know if this had any effect.

However, the leading term (2) was not enough, it had to be corrected by reinteractions, unitarity cuts. How this was to be done was not known and there was and later would be a persistent controversy about how this was to be done. This is typical of phenomenology, no generally accepted theory could be used as a judge. We had our own method involving doing a nice box integral. Ultimately comparison with experiment was made and the paper was sent to Nuclear Physics B in March 1969.

What is the value of that work today - and of all related work on 2→2 processes summarised thoroughly by Gordon Kane in 1976 [5]? There are two problems which limit its impact. For the first, experiments on processes I studied with Vesa are not any more carried out. The most recent ones are from the end of seventies. High energy beams of pions and kaons exist, but they are used for other purposes (for example, as sources of neutrinos). No one cares to measure in great detail exclusive multipion final states, they are deemed too difficult in relation to the outcome. If one had the motivation to do these experiments, one would certainly go back to the old expressions and see how they work today. For the second, we already have a complete and correct theory, QCD. However, we cannot from this correct theory derive the approximate formulas we used to analyse exclusive data. So these phenomenological formulas live an independent life; it is interesting to note that the review [5] in 1976 has not yet a single word about quarks or QCD. Since we believe that we have a theory which is absolutely correct, there is no need to do experiments in a kinematic domain in which nothing new is expected. The path chosen by science to progress was via much simpler concrete analytic formulas for the scattering amplitudes, starting with Veneziano [6]. These ultimately lead to string theory.

Vesa's name caused some problems to other people. The most extended variation was a letter addressed to V. Rimscancu. J. Tuominiemi had a letter mailed to J. Tusminicomi and my best variation was Kiegro Kagantre.

This early work on Regge models for $2 \rightarrow 2$ reactions was just the start of a long association with Vesa working on diffraction theory in the seventies [7] and, in particular, with relativistic heavy ion collisions from early eighties. This was a most harmonious collaboration. Once I felt unjustly treated though. Vesa and Jorma Tuominiemi were the scientific secretaries of the CERN-Dubna summer school organised in Loma-Koli in Finland in June 1970, where I was a lecturer. They did not invite me to the memorable party with the hotel staff celebrating the end of the school. Maybe I was too old?

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In the Nordic meeting in Spåtind, Norway, January 1986. Photograph from Keijo Kajantie.

On the direct route to Reculet on Jura close to CERN on 2 February 1997. Vesa is leading, Kari J. Eskola follows. Bottom part of the slope was easy but higher up it got icier and icier, no steps could be kicked with the tips of shoes. No way to turn back, very frightening. It occurred to us that more jobs for physicists might soon become vacant. Photographs from Keijo Kajantie.

Vesa with Paul and Ulla Hoyer, returning from a rainy hiking trip in 1971. And of course, Vesa's black Ford, a distant dream for many Finns at those times! Photograph from Paul Hoyer.

Paul and Ulla Hoyer with Aune Ruuskanen, admiring the Alps in 1971. Photograph from Paul Hoyer (taken by Vesa).

Vesa Ruuskanen: The Beginning of a Hydrodynamic Description of Ultrarelativistic Heavy Ion Collisions

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Vesa Ruuskanen's pioneering work on the hydrodynamical description of heavy ion collisions is recounted, along with a few personal anecdotes.

Keijo,Vesa and Me

The first time I met Vesa was so long ago I do not remember the date. I was visiting Finland at that time for a couple of months, we were both at a meeting of the Finnish Physical Society. The meeting was in the spring, and in spring it is still very cold in Finland. That winter had been so cold that all the polar bears ran off to Svalberg and all the penguins swam away to Antarctica, whenever that was. People walked on the ice all the way to Hamburg. A long time ago.

Keijo was the one introduced me to Vesa, who was playing billiards and drinking a beer in the lobby of the hotel. We played a few rounds of pool, drank a few beers, and have been good friends ever since.

I had been trying to understand Bjorken's work on hydrodynamics [1]. This paper was where he proposed the $1 + 1$ dimensional perfect fluid hydrodynamics equations for the evolution of matter produced in the central region of ultra-relativistic heavy ion collisions. Keijo and I were discussing how to modify these equations to include the effects of the fragmentation region of the nuclei. This is absolutely necessary for SPS energy collisions, and for the more forward regions of RHIC energy collisions. We had worked out hydrodynamic equations with sources.

Risto Raitio, Keijo and Vesa solved these equations numerically [2]. I studied this paper carefully. It introduced techniques for solving the hydrodynamic equations – and even more importantly, ways of visualizing the collisions and the flow of the matter after the collision. It was the first computation which treated the full longitudinal structure of the matter produced in heavy ion collisions.

Around the time of this work, I bought my first personal computer. I was connected by a 300 baud modem from my home, and could talk with colleagues by e-mail. This all seems trivial now, but it opened up a whole new world of collaboration – in particular, with my Finnish friends. I would receive a message in the morning which was the result of a day's work in

Jyvaskyla or Helsinki. I would then work in the day, and send a message off which would reach their computers before they awoke, and the process would continue. I worked this way with both Vesa and Keijo,

I remember once early in the evening I got a message from Vesa saying, "Send me a joke." It must have been very late at night in Helsinki. I sent him one, probably about fishing or something. Next day I asked him why he had asked. He said he was having a party at his house with a couple of business men, and they wanted to see how e-mail worked. They were apparently very impressed.

I also learned in Helsinki that the US government was reading e-mail. I suspected it, as all foreign e-mail at that time went out through one portal in the US, at Georgetown University in Washington D. C. I had returned from Russia to Helsinki, and had some messages from Russian colleagues concerning how to invite some people to a scientific meeting, and other issues. About 6 months later at a physics meeting in Aspen Colorado, a very senior physicist who was associated with the secret Jason project, which was also meeting in Aspen at the time, made a point of telling me the contents of my e-mail.

Finland has always been in the leader in Europe in internet technology. I remember years ago when I would visit Germany my message would go off at the rate of one letter per minute, yet when I would be using e-mail in Helsinki it was like working in my home. The guest house of the University of Helsinki was one of the first University guest houses in Europe wired for internet.

3D Solutions

In 1983, while Vesa was doing the hydrodynamic computations described above, Baym and collaborators developed an elegant way to solve for the evolution of matter produced in the central region of ultra-relativistic nuclear collisions, which built in the transverse structure of the equations [3]. I had moved to Fermilab by then, and there was serious discussion in the US about building RHIC, so I thought numerically solving these problems would be a fun thing to do.

Well I put it on the computer and began solving and after a few iterations, the code always crashed. It took me months to figure out that I was seeing the formation of a shock front. I was grateful I was able to be talking with Vesa electronically, for I desperately needed his help. He kept telling me about characteristics and other stuff I have never understood. We somehow figured out that we should be using a hydrodynamic code developed by the people at Frankfurt for handling shock front formation, the flux conserving transport algorithm. Our collaboration had also expanded to include two excellent young people, Henrique von Gersdorff and Markku Kataja.

One of the difficulties of our collaboration was that I was running all my computations on a PC, and of course it was very slow and had very little memory by today's standards. I also had to use turbo Pascal as this was the only efficient compiler available at that time. It was clear we had to shift to a bigger machine, and this involved translating everything into Fortran. Henrique made the heroic effort to translate my poorly written code, and Markku turned the code into an efficient algorithm. Vesa for many years afterward would complain bitterly about the sloppy manner in which I wrote code, but, it worked for me.

In spite of Vesa's unhappiness with my skills in numerical computation, we managed to publish three papers on this subject [4]. These papers were important since they were the first to give some understanding of the time scales of transverse expansion. An interestinging feature was that the system did not grow very large in the transverse direction, and a rarefaction shock wave formed near the density of the transition between a quark gluon plasma and a hadronic gas.

Later, Vesa, George Bertsch and I teamed up together with two young people, M. Gong and E. Sarkkinen, to understand what would happen if the nuclei in a heavy ion collisions fragmented into globs of quark gluons plasma, as might happen at a first order phase transition. These globs radiate pions at their surface, and the pions scatter by cascade computation. This was a project with which we had a lot of fun. We especially enjoyed getting to know George, and his deep and clever insights into nuclear physics. I still think there is some truth in the picture we developed together, but it is long since lost in the sands of time.

I think it is fair to say that Vesa was the one who started the serious hydrodynamic computations for ultrarelativistic nuclear collisions. This area has grown into a very successful enterprise. The evidence for thermalization in RHIC collisions comes primarily from comparing computations with data on radial and elliptic flow. The agreement is very impressive. Vesa has produced a number of excellent PhD's working in these areas, most recently Pasi Huovinen and Sami Räsänen and soon Harri Niemi.

The Finnish work on these problems is always reliably and carefully done. It is work marked by a physical insight, done to test a theoretical hypothesis. It is not mindless computation, and it has had a major impact on high energy nuclear physics – both as seminal work and as lasting work of substance.

From the time Vesa and I first worked together, we have remained very good friends. I have visited him in Jyväskylä, and had him rescue me once when I locked myself and Alice out of our room as we were leaving the guest house to catch a train. Unfortunately, our suitcases were still inside. We seemed destined to miss our train, Vesa pulled some sort of rabbit out of his hat, and got the door opened. I no longer remember how he did it, only that at the time it seemed an impossible feat of magic.

We skied together at many meetings. In Hirschegg I went out with Vesa and Helmut Satz on cross country skis, and we went up in hilly terrain. If you have skied with Vesa and Helmut, you know their skill at that sport. If you have skied with me, you can understand how challenged I was. There was no way any thing human, animal or from some other planet could keep up with Vesa on skinny skis, but Helmut was in his league. One would look for the steepest hill, sail on down it, and the other would follow close behind. Much later, by whatever means I could manage, I would join them at the bottom of the hill.

The last time we were skiing together was about ten years ago, I was an organizer of a meeting in Seattle and I invited Vesa and Kris Redlich to my house in Oregon for a few days of skiing. We spent many hours skiing. Mainly I went on the downhill slopes and Vesa went cross country. When I did ski with him on skinny skis once, I found he would go twice around the course while I was going around once. In any case, we would return happily worn out, no longer feeling any need to be thinking before speaking. That relaxed freedom is one of the luxuries of a good friendship.

Vesa is always an optimistic person with an idealistic view of the world. At the last scientific meeting which we both attended, he spoke to me with great heat about US policy in Iraq, which he assumed I supported. As a matter of fact, I share those misgivings. I wish I could match his optimism. But identical opinions are not a requirement for friendship. Friends, like partners in marriage, can with luck have very different views and many heated discussions and still stay close.

Vesa is now retiring from the physics department. Knowing Vesa, I trust the main change in his life is going to be that now he can have more time to do physics.

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The Urbana Connection

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Reminiscences

Little did I imagine when young Vesa and I met at the 1983 Berkeley conference on Relativistic Heavy Ion collisions and discussed, over dinner in the Taiwan Restaurant, his coming to Urbana for a year, that I would one day be writing a contribution to his retirement Festschrift. In the intervening years we often worked and talked together on ultrarelativistic heavy ions collisions. We also became good friends. Our serious collaboration began during the year 1986-87 when Vesa came as a Visiting Professor to the University of Illinois. We spent much of our scientific time thinking about the first rounds of ultrarelativistic heavy ion collisions then being carried out at CERN and the AGS. It was a productive and fun year, with enough snow for us to do some cross-country skiing in the 2+ϵ dimensional landscape around Urbana, and to jog together regularly the rest of the time. In Urbana, Vesa also made a serious attempt to improve my Finnish language ability, which sadly never rose much beyond necessary phrases such as, *Menemmekö yhdessä lounaalle?*

In this period we carried out the first analysis of transverse energy production in the initial 200 GeV proton and ^{16}O runs on Pb targets at the SPS. Notably p-^{16}O collisions produced E_T as large as 40-50 GeV compared with the 200 GeV pp scattering limit of 19.4 GeV. After discussing the HELIOS experiment's results [1] with Peter Braun-Munzinger in Erice in the Fall of 1986, the three of us produced a joint paper, *Transverse energy production in proton-nucleus and nucleus-nucleus collisions* [2], showing the importance of rescattering in the target fragmentation region in the measured E_T spectra. In this analysis Vesa characteristically wrote down a simple model that captured the essential physics, assuming a Poisson distribution of successive proton collisions in the target, with an exponential distribution of transverse energy production per collision. The model predicted a transverse energy distribution,

$$\frac{1}{N}\frac{dN}{dE_T} = \frac{1}{\epsilon_0}e^{-E_T/\epsilon_0}\sum_{n=1}^{\infty}\frac{\bar{n}^n}{n!((n-1)!}e^{-\bar{n}}\left(\frac{E_T}{\epsilon_0}\right)^{n-1},$$

which agreed remarkably well with the data; here the parameter ϵ_0 is the mean transverse energy per collision, and $\bar{n} \simeq 3.8$ is the mean number of collisions the incident proton suffers in the target. In p-Pb, $\epsilon_0 \simeq 1.8$ GeV, in contrast to $\epsilon_0 \simeq 1.2$ GeV for the corresponding Fermilab pp data, pointing immediately to rescattering of excitations in the nuclear targets. A simple estimate of rescattering of produced pions on nucleons led to the required 50% amplification of the transverse energy. We then extended the picture to ^{16}O-Pb, multiplying $\bar{n}$ above by the effective number of nucleons in the projectile, $B_{\rm eff}$, participating in the collision; with $B_{\rm eff}$ growing with increasing E_T, a measure of centrality of the collisions, the results nicely described the early data.

We promised in this paper to report detailed calculations of the effects of rescattering. We then set up, with my graduate student Jerry Friedman, a fuller model (which Jerry named PTHA, after the ancient Egyptian deity, the patron of craftsmen) of the interactions of high energy protons with nuclei, taking into account energy degradation of the projectile through a "multichain" model, nuclear geometry, and rescattering of secondary excitations. We had the ambitious aim of not only calculating rescattering, but understanding the origin of fluctuations and the degree of thermalization in the then ongoing experiments at CERN and BNL energies, as well in future RHIC experiments. Alas, the considerable work we did was never published, owing in part to our becoming dispered around the globe, and other pressures.

Another, smaller, project we tackled in this period, was to understand whether or not matter undergoing hydrodynamic flow in ultrarelativistic heavy-ion collisions would be stable against convection. We wrote two papers, first a preliminary version with Bengt Friman, *Stability of hydrodynamic flow in ultra-relativistic nucleus-nucleus collisions* [3], and finally with Bengt's postdoc at GSI, Wojciech Florkowski, as well, a paper, *Convective stability of hot matter in ultrarelativistic heavy-ion collisions* [4], As we found, for initial conditions corresponding to partial transparency the flow of a quark-gluon plasma (with equation of state calculated in perturbation theory) would be stable, whereas the flow of the later hadron gas would be convectively unstable. However, as we estimated, the timescale for development of instabilities was at least as large as the expected lifetime of the system, suggesting that hadronic flow should be close to neutral convective equilibrium.

From quarks to cold atoms

Over the intervening years we met often at conferences, including Spåtind, Erice, and numerous Quark Matter meetings (photograph), as well as in Finland. While Vesa remained in ultrarelativistic heavy ion physics, I drifted, soon after the creation of cold trapped atomic Bose-Einstein condensates in 1995, towards cold atom physics. In fact, ultrarelativistic heavy ion physics and cold atom physics, despite differing by some 20 orders of magnitude in energy scales – from 100 MeV (10^{12}K) to 1 nK – share an unexpected number of problems in common, which I would like briefly to describe – in hopes of tempting Vesa, now that he will have full time to do physics, to apply his clear and perceptive approach to the many particle cold atom problem as well.

Experiments on ultracold magnetically trapped atomic plasmas [5] present a very intriguing opportunity to use the plasmas as analog systems to gain understanding, under well controlled conditions, of the properties of plasmas relevant to heavy-ion ultrarelativistic heavy ion collisions. These plasmas, produced by photoionizing trapped cold atomic gases such as Xe or Sr, are very strongly interacting, as measured by the ratio of interaction energy to kinetic energy, and thus, unlike normal laboratory plasmas, share many features with the plasmas in ultrarelativistic collisions. Typically they contain about $10^5 - 10^6$ atoms at a density of some 10^9 atoms per cm^3, and with ion temperatures in the millikelvin range and electron temperatures in the Kelvin range. Experiments can, for example, measure the modes of the plasmas, study screening effects, thermal equilibration and expansion (initiated by releasing the magnetic trap), and indeed one can even imagine studying the interaction of the plasmas with fast particles.

Another unexpected intersection of ultrarelativistic collisions and cold atoms is the observation of almost viscosity-free ("perfect fluid") hydrodynamic behavior in both elliptic flow at RHIC [6], and in the expansion of trapped paired fermion gases near unitarity [7], the limit in which interatomic scattering lengths are made large (via a magnetically controlled Feshbach resonance) compared with the interatomic spacing.

There are as well instructive lessons waiting to be drawn the similarities of the hadron-quark transition at finite baryon density and the now observed Bose-Einstein condensation to BCS superfluid crossover in cold paired fermion gases. For example, in a matter with two colors, rather than three, the deconfinement transition would be from Bose-condensed diquarks to BCS paired diquarks [8].

Finally, the question of how color superconductivity in degenerate quark matter works in the presence of unequal populations of light (u,d) and heavy (s) quarks has been the focus of considerable attention [9], since with too large a population imbalance a homogeneous paired system becomes unstable. The stable inhomogeneous states can now be studied in trapped atomic fermion gases, e.g., ^{6}Li, where one can readily control the relative populations of the two hyperfine states that are paired together [10]. These experiments are beginning to allow one to map out the phase diagram as a function of temperature and population imbalance [11], and should shed light on the color superconductivity issues.

I am very much looking forward to a long future of continuing discussions of these and other problems with Vesa in Jyväskyla and elsewhere, and wish him a happy and productive retirement! To Vesa: *Kippis*!

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Vesa at the Heidelberg Quark Matter meeting in 1995. Photograph from Gordon Baym.

“You wouldn’t be interested in hydrodynamics?”

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Prologue: In fall 1983 I was about to graduate in theoretical physics at University of Turku and sought for opportunities to continue my studies, perhaps in another university. A friend of mine, Jussi Markkanen, whom I had met in particle physics summer school somewhere in Sweden told me nice things about the Department of Physics at Jyväskylä and especially about his supervisor Vesa Ruuskanen, associate professor at that time. Encouraged by Jussi, I soon found myself in Vesa’s office at Nisulankatu describing my interests and previous studies, and trying to convince him that I would be a good choice for a doctoral student. He also described his field of research in ultrarelativistic heavy ion physics and then began outlining possible particular topics for my licentiate and doctoral studies. At that point he made the rhetoric question, a free translation of which appears in the title (”Ei sinua tuo hydrodynamiikka kiinnostaisi!”) and added, for motivation or perhaps, for excuse: ”...almost anything *can* be interesting.” Having my limited research background in grand unified theories, I hardly understood the very meaning of the word ’hydrodynamics’ and could not really tell at that point whether I found it interesting or not. I believe the answer to that question, rhetoric or not, is made evident by this short review on the many consequences of that meeting and of those few words by Vesa.

Viscosity and phase transition in the 1-dimensional hydrodynamical model of high energy heavy ion collisions

The idea of producing Quark Gluon Plasma (QGP) by means of nuclear collisions is based on the conjecture that the deep inelastic collision of heavy nuclei can produce a large amount of secondary particles that become thermalized for a short period after the collision. This matter then expands freely and finally breaks up again into individual free particles that, in one form or another, carry information of the thermalized state into detectors. Depending on the initial energy density, the matter may be originally in plasma state or in hadronic state. Provided that the thermalized state is maintained long enough, the expansion/cooling stage may be described as collective motion in terms of (relativistic) hydrodynamics [1].

The starting point of the work reviewed here was the hydrodynamic model of longitudinal expansion in ultrarelativistic collision based on the assumption of complete local equilibrium and consequently, on ideal hydrodynamics [2, 3, 4]. In the work reported in [5] we relax the assumption of complete equilibrium and discuss the effects of finite viscosity on the final state particle distributions. In addition, we study the effects of varying critical temperature on the same observables.

The governing equations for the flow are given by

$$\begin{aligned} \partial_\mu T^{\mu\nu} &= \Sigma^\nu \\ \partial_\mu j^\mu_B &= \sigma, \end{aligned} \tag{1}$$

where

$$\begin{aligned} T^{\mu\nu} &= T^{\mu\nu}_0 + \Delta T^{\mu\nu} && \text{Total energy momentum tensor} \\ T^{\mu\nu}_0 &= (\epsilon + p)u^\mu u^\nu - g^{\mu\nu} p && \text{Ideal fluid energy momentum tensor} \\ \Delta T^{\mu\nu} &= \eta_0 \frac{4}{3} \epsilon^{\frac{3}{4}} \nabla^\mu u^\nu && \text{Dissipative energy momentum tensor} \\ j^\mu_B &= n_B u^\mu && \text{Baryon number current density} \\ \nabla^\mu &= \partial^\mu - u^\mu (u \cdot \partial) \end{aligned} \tag{2}$$

Here, $g^{\mu\nu}$ is the metric tensor and ϵ is the energy density, p is the pressure, u^ν is the four velocity, η_0 is the dimensionless viscosity coefficient and n_B is the baryon number density of the fluid. The quantities Σ^ν and σ are the energy-momentum and baryon number sources of the matter arising from collision [2].

The thermodynamical properties of the matter are given by the equation of state. Here we used the simple 'bag' equation of state that describes first order phase transition to plasma state at critical temperature T_c.

$$p = \begin{cases} \frac{1}{3}\epsilon & ; \ \epsilon < \epsilon_H \\ \frac{1}{3}\epsilon_H & ; \ \epsilon_H \leq \epsilon \leq \epsilon_Q \\ \frac{1}{3}(\epsilon - 4B) & ; \ \epsilon_Q < \epsilon, \end{cases} \tag{3}$$

where $\epsilon_H = 3a_h T_c^4$, $\epsilon_Q = 3a_q T_c^4 + B$ are the lower and higher critical energy densities and $B = (1/4)(\epsilon_Q - \epsilon_H)$ is the bag constant. The constants a_h and a_q depend on the number of degrees of freedom in the hadronic and plasma phases, respectively.

In principle, Eqns. 1 can be solved numerically for given source terms. Two complications arise, however. Firstly, for finite value of viscosity parameter η_0, the equations are implicit in u^ν and must be solved iteratively. Secondly, the bag equation of state implies rarefaction shock in the solution. The shock appears at location where $\epsilon = \epsilon_H$ and can not be specified in advance. The numerical method should thus be able to handle spontaneous shock formation in the solution. The numerical method used in this, and in many of our later works was a specific Flux Corrected Transport (FCT) algorithm, called SHASTA-FCT, originally developed by J.

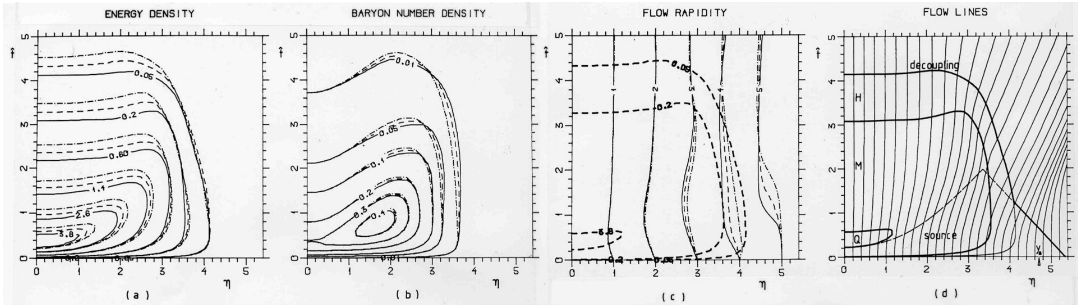

Figure 1: Evolution of various flow quantities for longitudinal expansion of arising from central U-U collision at CM energy 50 GeV/nucl. Thin solid contours are for ideal flow, dashed lines and dash dotted lines correspond to lower and higher limiting values of viscosity parameter $\eta_0 = 1/3$ and $2/3$, respectively. In fig. (d) shown are flow lines (thin solid lines) together with the boundaries of plasma, mixed and hadron phase regions, the decoupling boundary and the source region, all for ideal fluid case.

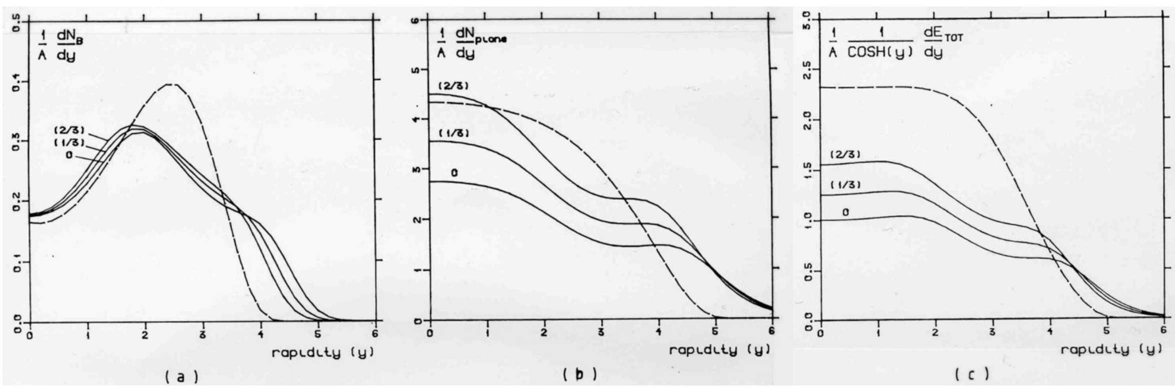

Figure 2: Rapidity distributions of baryon number and number of pions for various values of viscosity parameter η_0 corresponding to flow solution shown in Fig. 1

P. Boris and D. L. Book [6], and later improved by S. T. Zalesak [7]. It is based on a clever balancing between low order and high order methods to allow for resolution of sharp gradients without producing excess oscillations characteristic to high order methods. Figure 1 shows the computed evolution of various flow quantities for longitudinal expansion of arising from central U-U collision at CM energy 50 GeV/nucl, where the initial temperature is sufficiently high to produce QGP in the initial state. The results for the 1+1 dimensional solution are given in terms of scaling variables $\hat{t}$ and η defined by $t = \tau_0 e^{\hat{t}} \cosh \eta$, $z = \tau_0 e^{\hat{t}} \sinh \eta$. Figure 2 shows the resulting particle distributions as calculated using the flow solution and utilizing the decoupling scheme of Cooper and Frye [8, 9]

The results indicate that the effect of finite viscosity on multiplicity and on rapidity distributions is noticeable and should be taken into account. Increasing viscosity and decreasing critical temperature both increase multiplicity of pions and shift the rapidity distributions towards lower rapidity. The distributions show certain characteristic shape that might be useful in identifying collective motion of the matter produced in high-energy heavy-ion collisions.

Further studies of hydrodynamic effects in high-energy nuclear collisions

After completing the study of the effects of viscosity in 1986, the model and the numerical solver based on the FCT method was utilized in a number of studies in an effort to identify possible signatures of collective behavior and of existence of quark-gluon plasma in nuclear collision experiments that were to be carried out in the Super Proton Synchroton (SPS) in CERN, the Relativistic Heavy Ion Collider (RHIC) at Brookhaven (started in 2000) and ultimately for the Large Hadron Collider (LHC) that can reach the energy of the order of 1 TeV/nucleon for heavy ions, and is scheduled to be started in 2007. The hydrodynamical model was extended from its early 1+1 dimensional longitudinal expansion version into boost invariant cylindrical expansion version and to three-dimensional cylindrically symmetric version. The signatures studied included total transverse momentum production, transverse momentum distributions, dilepton production, strangeness evolution and collective effects arising from three-dimensional flow.

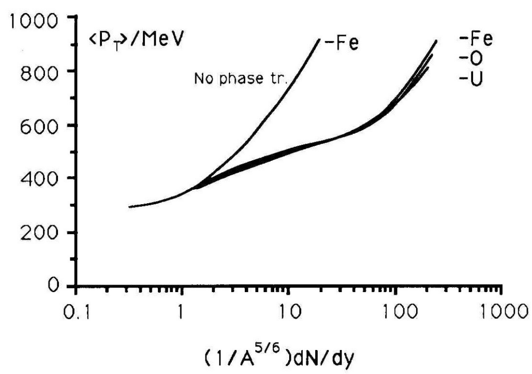

Figure 3: The average transverse momentum of massless pions at decoupling as a function of scaled multiplicity for O+O, Fe+Fe and U+U collisions for $T_c = 200$ MeV. Also shown is the transverse momentum for Fe+Fe without phase transition ($T_c = \infty$).

An example of a proposed signature is shown in Fig. 3 showing the computed mean transverse momentum $< p_T >$ of pions as a function of total multiplicity. The flat region in mid-multiplicity region and the rapid increase of $< p_T >$ at high multiplicities were interpreted as signatures of existence of mixed phase and of plasma phase, respectively. (Notice also the $A^{5/6}$ scaling predicted by the hydrodynamic model.) It was assumed at that time - perhaps with optimism justified by the experiments so far in the future! - that the multiplicity attainable in

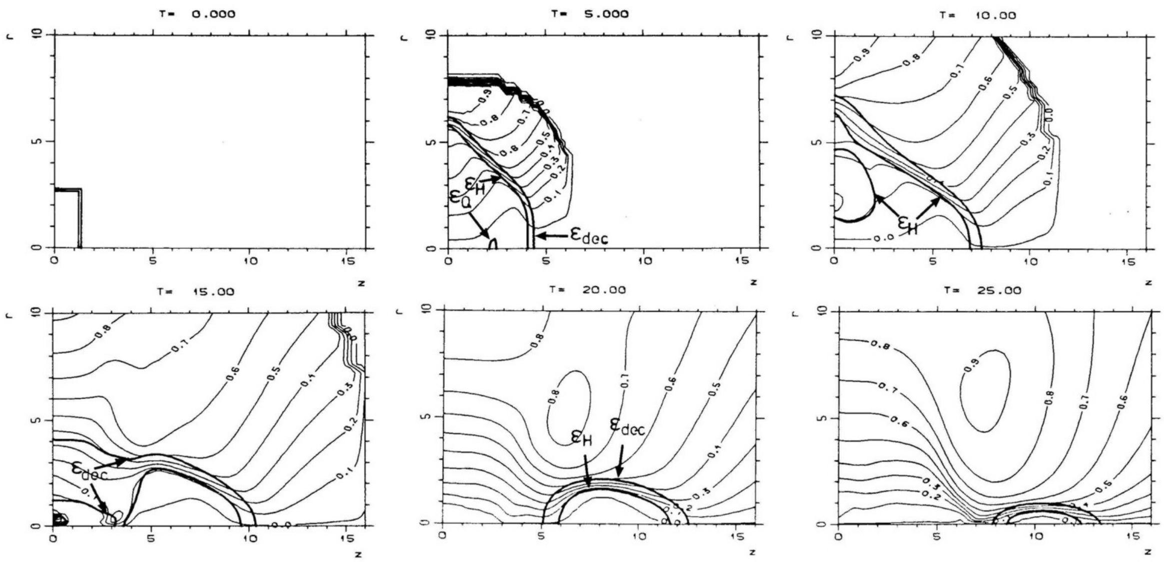

Figure 4: The cylindrically symmetric 3D evolution of the hot tube. Shown are contours of constant transverse velocity (thin lines) and three constant energy density contours at fixed times $t = 0, 5, ..., 25$ fm/c. The initial temperature $T_0 = 260$ MeV and the critical temperature $T_c = 200$ MeV. The flow is reflection symmetric with respect to $z = 0$ plane.

RHIC might be high enough to show this evidence of mixed phase and even of plasma phase. This conjecture might, however, be better founded for the future LHC experiments.

Figure 4 shows the computed three-dimensional evolution of the 'hot tube' arising from nuclear collision at high but finite energy where longitudinal boost invaricance can not be assumed. Here the hydrodynamic model predicts breakage of the initial cylindrical volume (of quark-gluon plasma, in this case) into two 'hot blobs' that move longitudinally apart radiating hadrons from their outer surface. During the long life time of the cigar-shaped blobs, the rarefaction shock formed at the boundary between mixed and hadronic phases effectively converts the flow into radial direction leading to a very high total transverse energy. Also, the complicated shape of the decoupling surface at intermediate times gives rise to a rather peculiar shape of the transverse energy distributions, which I also proposed as a possible 'signature' of collective motion (with not much success, though).

Interlude: The work discussed above was carried out during my licentiate and doctoral studies, completed in 1989 under proficient and tireless supervision by Vesa. In 1987-88, I spent one year at MIT, where I worked under supervision of Vesa's collaborator of that time, Tetsuo Matsui - developing a model for particle production out of the initial color field at the early stages of collision. During the spring 1988 Vesa visited us in Boston, and I still remember that time as one of the most rewarding and happy periods in my scientific life. Figure 5 shows

Figure 5: Breakfast at kitchen table at our temporary home in Belmont in May 1988. Vesa, subject of unreserved admiration by our eldest daughter Loviisa. Notice her precious teddy bear blanket trusted into his possession.

an instant out of Vesas stay at our home in Belmont.

Already before the MIT year and especially after it, I became involved in research apparently quite distinct from heavy ion physics, namely applied research related to paper-making industry. The rest of this paper illustrates problems and research in that field, towards which I gradually diffused in the early 90's.

Application of SHASTA-FCT method in solving two-dimensional flow of fiber suspension on the forming and wet pressing sections of a paper-machine

In a conventional, 'fourdrinier' paper machine, the paper web is formed by injecting a planar slice jet of pulp on a moving wire through which water is removed by gravity and by low pressure suction devices located beneath the wire (see Fig. 6). The pulp is a rather complicated multiphase fluid that includes liquid phase (water and dissolved chemicals) and solid phase (wood fibres and a small particle fraction that contains mainly mineral filler particles and small fractions of fibres). The typical total solids content of the pulp is 1% by weight. The width of a fourdrinier paper machine - and of the planar jet may be up to 10 meters while the thickness of the jet is typically 1-5 cm, depending on the paper grade produced. A typical 'machine speed' *i.e.* the speed of the wire and the jet and is of the order of 10 m/s or higher (approaching 30 m/s in modern gap former machines). The flow and filtration of the pulp on the wire is described using shallow water approach in which all flow quantities are averaged over the vertical z -direction.

The governing continuity and momentum equations for the two-dimensional stationary planar flow on the wire can then be given in the form [10]

$$\begin{aligned} u\frac{\partial v}{\partial x} + \frac{\partial}{\partial y}(gh + \frac{1}{2}v^2) &= -\frac{p\omega v}{h}, \\ \frac{\partial}{\partial x}(gh + \frac{1}{2}u^2) + v\frac{\partial u}{\partial y} &= -\frac{p\omega(u-c)}{h} \\ \frac{\partial hu}{\partial x} + \frac{\partial hv}{\partial y} &= -\omega, \end{aligned} \quad (4)$$

where h is the thickness of the free pulp suspension layer, u and v are the z -averaged velocities in longitudinal x and transverse y directions, c is the machine velocity and p is a parameter that specifies friction between pulp and the filtrated fiber layer. The water removal velocity $\omega = \omega(x, y)$ is given by Darcy's law (not shown here), and depends on acceleration due to gravity g, flow resistance of the wire and of the filtrated fiber layer and on pressure beneath the wire (assumed to be given as a boundary condition).

A practical problem in numerical solution of Eqns. 4 is posed by the boundaries of the jet at $y = 0$ and $y = L$ (=width of the jet). As the jet first hits the wire at $x = 0$, there is a discontinuity in the layer thickness h at the edges. The pulp then starts to flow outwards thus creating a rarefaction wave propagating inwards. From numerical point of view, such a flow condition involving initial discontinuity and subsequent steep gradients is reminiscent of explosion-like behaviour of matter emerging from nuclear collisions. With suitable definitions of various quantities, Eqns. 4 are, however, exactly of the form of Eqns. 1, and amenable of solution using the FCT algorithm! (As a matter of fact, after unsuccessful trials with several other methods, these equations were first solved using the 1+1 dimensional SHASTA-FCT code developed for nuclear collisions without any modifications made in the basic solver.) Figure 7 shows an example of solution obtained for machine speed $c = 8$ m/s and with small sinusoidal variation of jet thickness and velocity components around mean values $h_0 = 5$ cm, $u_0 = 1.03c$ and $v_0 = 0$. The width of the jet $L = 7$ m. The model and the numerical solution has been applied *e.g.* in predicting the length of wire section needed for forming and in estimating the effects of initial jet thickness and velocity variations on fiber orientation of the formed web.

Later, the same numerical code was modified to solve the flow of water and air in compressible porous materials, and applied *e.g.* in modeling drying of wet paper web by mechanical compression, the so-called wet pressing process. Figure, 8 (a) shows a schematic illustration of a wet pressing device, a roll press, that is commonly used in paper-machines after the former section to remove water from the web by compressing it against a porous felt between two pressing rolls. In Fig. 8 (b) shown is an example of a numerically solved compression of the web and the resulting flow of water from web into felt in a roll press nip [11]. Notice that also this kind of flow may involve steep gradients of various flow quantities, especially in the inter-

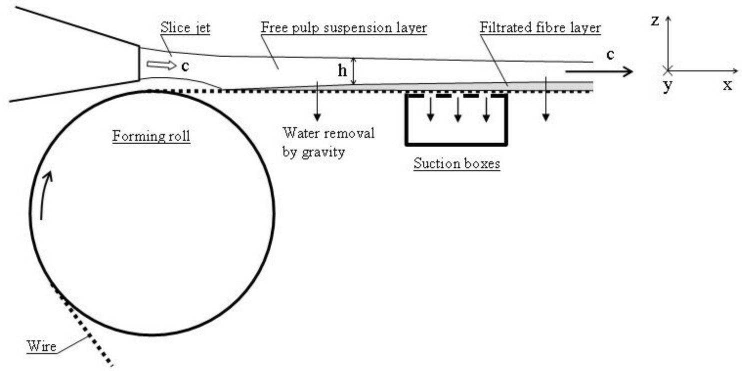

Figure 6: Schematic illustration of a fourdrinier former.

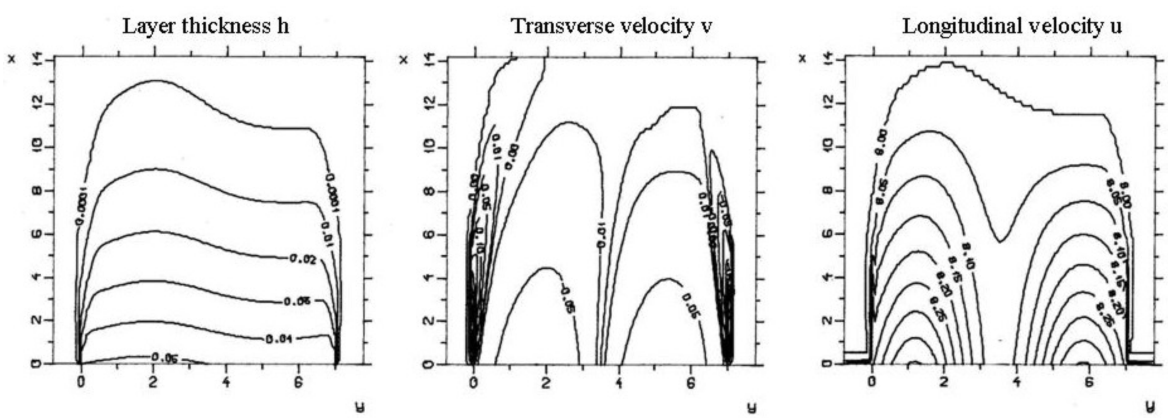

Figure 7: Contours of constant layer thickness h [m], and transverse and longitudinal velocity components v and u [m/s] for two-dimensional flow of pulp on paper machine wire. The slice jet emerges at $x = 0$ with small sinusoidal variation in thickness and velocity profiles in the y direction. Notice the rarefaction waves and steep gradients near $y = 0$ and $y = L = 7$ m.

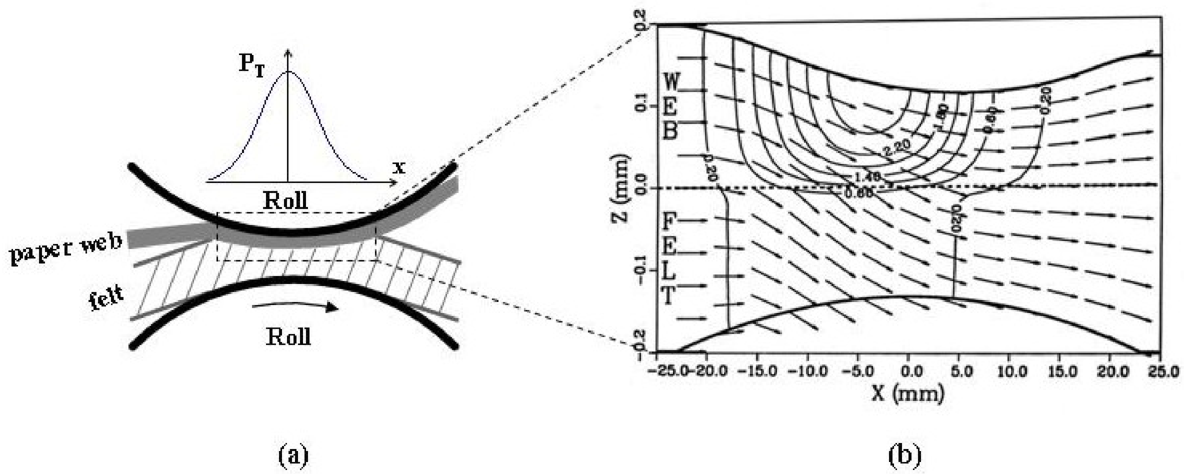

Figure 8: (a) Schematic illustration of a paper machine roll press that applies a mechanical pressure p_T on paper web and felt. (b) A numerical solution for compression of the web and flow of water in a roll press nip. The arrows indicate water velocity field and the solid lines are contours of constant water pressure in the pores.

face between the web and the felt. The same SHASTA-FCT -algorithm used in the previous applications proved effective in solving also this multiphase flow problem.

Probing the equation of state of consolidating fiber network

Within the forming model discussed above, precipitation of fibers on the wire was assumed to take place as a simple filtration process where the filtrated layer, the web, grows at a prescribed constant density (porosity) that is left as a free parameter of the model. Most likely, this assumption is over simplified. The process should rather be described as a gradual formation and consolidation of the soft fiber network. In order to develop improved models for forming process it is thus necessary to know the mechanical material properties, *i.e.* the equation of state of the consolidating fiber network. Unfortunately, this stress-strain relation may include complicated non-linearly elastic, viscoelastic and plastic behavior, and is purely known. The reason for this lies in the experimental difficulty in measuring the local stress state of only the fiber phase in a liquid-fiber suspension during filtration.

We have introduced a novel method for measuring the properties of consolidating wood fibre network in one-dimensional filtration of liquid-fiber suspension [12]. The device consists of a hand-sheet mould equipped with a pulsed ultrasound-Doppler velocimeter (PUDV) capable of measuring the local time-dependent velocity field of the fiber phase during vertical filtration (see Fig. 9). Simultaneously, the fluid surface velocity, giving the total volumetric flux of the

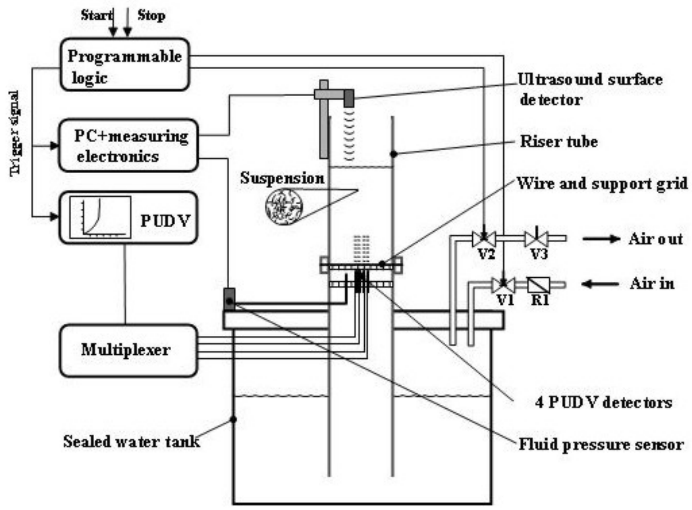

Figure 9: Schematic illustration of the filtration device based on using ultrasound -Doppler velocimetry (PUDV) for measuring the fiber velocity field $\tilde{u}_s$.

suspension, and fluid pressure at the upper surface of the wire are measured. The data analysis is based on the two-phase flow equations that govern the one-dimensional gravity-driven flow in the filtration zone (in the riser tube above the wire), namely

$$\begin{aligned} \frac{\partial}{\partial t}\phi_f + \frac{\partial}{\partial z}(\phi_f u_f) &= 0 \\ \frac{\partial}{\partial t}\phi_s + \frac{\partial}{\partial z}(\phi_s u_s) &= 0 \\ \phi_f \frac{\partial}{\partial z} p_f &= D - \phi_f \rho_f g \\ \phi_f \frac{\partial}{\partial z} p_s &= -D - \phi_f \phi_s (\rho_s - \rho_f) g. \end{aligned} \tag{5}$$

Here, the subscripts f and s refer to the fluid phase (water) and the solid phase (fibers), respectively, ϕ_α is the volume fraction, u_α is the flow velocity in the vertical z -direction, p_α is the pressure and ρ_α is the density for phase $\alpha = f, s$. Utilizing the conventional laws of Darcy and Kozeny-Carman for slow relative flow of fluid in porous medium, the momentum transfer term D between phases can be written in the form

$$D = -\frac{\mu}{k_0} \frac{\phi_s^2}{1 - \phi_s} (u_f - u_s), \tag{6}$$

where μ is the viscosity of the fluid and k_0 is the specific permeability of the fibre network (which can be found by fitting to the measured fluid pressure at the wire).

Utilizing the experimental velocity field information we can integrate Eqns. 6 and find the space-time evolution of also volume fraction and pressure fields separately for the fluid phase

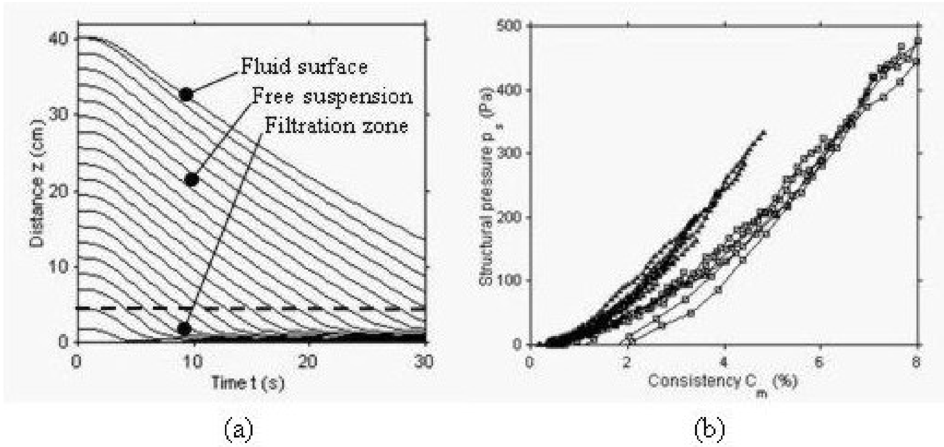

Figure 10: (a) Measured solid phase pathlines in $t-z$ -plane in the riser tube. The uppermost line shows the location of the fluid surface as measured by ultrasound surface detector. The wire is located at $z=0$. (b) Solid pressure p_s (normal stress of fibre network in z direction) as a function of consistency along pathlines for unbeaten pine fibers (triangles) and for unbeaten birch fibers (squares).

and for the fiber phase. This method thus allows us to experimentally study the very details of the consolidation process in dynamic conditions. Especially, we can measure the evolution of solid phase density and stress along flow lines of the solid phase and thereby probe the local stress-strain history of the consolidating fiber layers during the filtration experiment. Figure 10 shows a typical result for measured flow lines of the solid phase and the solid pressure p_s as a function of consistency along a set of pathlines for two different types of wood fibers. Clearly, the data for all the pathlines for each fiber type collapse approximately on the same lines indicating similar stress behavior for all layers of the fiber network. The data can thus be considered to represent a valid material property, namely the experimental stress-strain relation of the fiber network during filtration. These results are presently used in developing more realistic models for modern paper machine formers.

Epilogue: During the ten-year period from spring 1996 until spring 2006 I shared my working time equally between university and VTT (Technical Research Center of Finland). The common field of research of the two groups, including altogether 20 researchers in these two institutes, is fluid mechanics and its applications in industrial processes (see Fig. 11). Mostly, such processes involve multiphase flows where the relevant fluids include more than one constituent. These kinds of flows pose a challenge, not only to industry, but also to basic research. Interactions between the different constituents are usually not known very accurately, and physical modeling and numerical analysis of the flow is required. It is also very difficult to do reliable measurements on multiphase flows, and development of new experimental techniques

Figure 11: Examples of industry-related research facilities at VTT. Part of the paper machine short circulation research environment 'SORTTERI' (upper left). Optical tomograph device developed by University of Jyväskylä during tests at VTT (upper right). Pilot scale flow facility at VTT (lower left). Velocity profile in mixing flows measured using ultrasound Doppler method (lower right).

is an integral part of the field. A primary lesson learned from that period is that drawing distinction between basic and applied research would in this field be difficult and pointless - both warrant a similar research attitude and similar understanding of the underlying physics.

Personally, I owe a large part of the physical understanding that I have to my teacher, supervisor and later, a colleague, Vesa Ruuskanen. Ever since that afternoon in his office in November 1983 I have had great respect for him, not only as a scientist but as a teacher. Furthermore, the lessons given by him are not restricted to science, but cover other fields of life. Many times his teaching is done by example, perhaps not even knowingly, and is ultimately related to that attitude part.

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On the Road to the Hard Probe Cafe

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For me, Finland started in Jyväskylä. In 1960, it was still possible to fly with Karhumäki Air from Stockholm to Jyväskylä, without ever passing Helsinki. I arrived there in winter, and the Jyväskylä airport was a wooden cottage with big wood-burning stove in the middle, surrounded by a crowd of silent Finns warming up. I had come there to visit my friend Aapo Riihimäki, whom I knew from my student days in the US, and we spent the time mainly skiing and in the sauna. Incidentally, in the local high school, Aapo had been a school mate of Pertti Lipas, later Vesa's colleague at the University of Jyväskylä. In those pre-Nokia days, Finland could still afford to spend the winters in a hibernating stage, interrupted only once in a while by outbreaks of letka-jenkka or such. The harbour of Jyväskylä was asleep, the ships boarded up, waiting for the summer.

Twenty years later, in 1980, Aapo, Vesa and I spent a week or two in North Carelia, in a place called Hossa; again, in winter, and for skiing and sauna. There I learned that skiing becomes less pleasurable at temperatures below -30 °C, because of changes in the crystal structure of the snow; my Finnish friends had known about such phase transitions since they were children. The trip from Jyväskylä to Hossa, in a car driven by Aapo, had taken many hours, shortened only by what Vesa called Finnish roulette: every half hour or so, Aapo, driving full speed over ice-covered roads, would bend down to look for a new casette of Mexican mariachi music somewhere on the floor of the car.

I had first met Vesa in Helsinki around 1970 or 1971, when I spent a year there at the TFT. I vividly remember a party, where after some discussion it turned out that almost all of the many eminent Finnish physicists present had graduated from the same high school in Helsinki. All but Vesa. After some reflection, Vesa summarized the situation: it seems that in Finland there are two ways to success in physics; either you were at this high school, or you have talent.

It was already quite clear then that Vesa certainly did have it, and the object of study in those days was what one might call the end of hadron phenomenology - strong interaction physics before partons and before QCD. So one addressed questions like "where do diffractive nucleons go?" - we probably still don't know... But Hagedorn [1] had already signalled the end of hadronic matter, and not long afterwards Cabibbo and Parisi [2] pointed out that this was only the beginning of a new kind of matter, the plasma of deconfined quarks and gluons, which has kept Vesa and many of others busy for many years.

Vesa entered the new state of matter in what from a Finnish point of view (with all the 10000 lakes) must seem a very natural way, through hydrodynamics, and certainly much of the early seminal work in this field was carried out by Finnish theorists and their collaborators [3].

Hydrodynamics has played and continues to play an important, though somewhat curious, role in the search for and the study of the QGP. In a world of equilibrium thermodynamics, each equilibrium stage has forgotten everything about the previous history of the medium. So it seems rather futile to ask hydrodynamic flow of hadronic matter to indicate if the medium in an earlier stage had been a quark-gluon plasma. On the other hand, why should things be in perfect thermodynamic equilibrium, and deviations from this could well survive until later stages.

Nevertheless, Vesa and his colleagues followed the old rural wisdom of not putting all your eggs into one basket. From the very beginning, theorists had noted that an ideal way to study the early, hot, dense stages of matter would be hard probes - hard enough to probe the short scales of such media, and present early enough to test them. An obvious candidate was electromagnetic radiation, real and virtual photons, emitted in the interaction of the constituents making up the early medium [4, 5]. These probes, interacting only electromagnetically, would pass unaffected through all the subsequent phases of the medium and they would therefore be able to carry out their information without further interference. On the other hand, no one could prevent the subsequent hadronic stages from also emitting such radiation. So how could one tell when the observed signal was produced? Is there a window for seeing just what comes from the deconfined stage? Whenever a well-balanced and informative overview of this complex situation was needed, the community turned to Vesa [6]. Things did not become simpler when it was noted that experimentally thermal radiation was hardly seen, whether hadronic or partonic; until today, the issue is still not resolved and remains one of the primary questions in the field. More than perhaps anything else, thermal radiation would establish the formation of a thermalized system in high energy nuclear collisions.

In the early 90's it had become clear, however, that if we had a chance at all to look at the early stages of nuclear collisions, in the hope of finding a quark-gluon plasma, it would be through hard probes. Besides electromagnetic radiation, two other such probes had been proposed. At sufficiently high collision energies, there would be rare but definitely observable jet events, caused by the hadronization of a very energetic transverse quark or gluon, i.e., of a parton emitted orthogonally to the beam axis. If this parton would have to pass through a hot, dense quark-gluon plasma, it would seem likely to lose much of its energy: the QGP should result in jet quenching [7]. Subsequent calculations confirmed this [8], and although jets at SPS energies were only very marginally or not at all accessible, jet quenching became one of the probes to employ when RHIC would become operative.

The other hard probe was based on the fact that a quark-gluon plasma must show the QCD analogue of charge screening: the presence of many unbound colour charges decreases the range of the strong force to shorter and shorter separation distances. The very small and tightly bound quarkonium states, such as the J/ψ or the Υ, which can survive deconfinement up to some temperature above the critical value, will thus eventually also melt, when the force range becomes shorter than their binding radius. It was thus predicted [9] that J/ψ production should be suppressed if nuclear collisions indeed lead to a deconfined medium. In particular, the different binding radii of the different quarkonium states provide a set of different dissociation temperatures and thus a thermometer of the QGP. Finite temperature studies in lattice QCD have addressed this subject in considerable detail and today indeed confirm the basis for such a spectral analysis of the QGP [10].

In order to apply these hard probes, it is of course necessary to have some reference to identify the onset of new behaviour. All of the mentioned phenomena, from photons and dileptons to jets and quarkonia, also show modifications when the process is observed in nuclear targets, i.e., in pA as compared to pp collisions. The effect of normal nuclear matter on each probe thus has to be known, it has to be gauged, before it can be used to look for something "new".

In the early 1990's, shortly after the beginning of the heavy ion program at CERN and BNL, most of the experiments were measuring soft hadrons and soft hadron physics, in the hope of finding some striking new effect there. Hard probes were something rather marginal. Nevertheless, a small group of theorists decided at that time to join forces in a systematic study of hard processes in hadronic interactions, and their first meeting took place at CERN in 1994. Let me list the members of this first hard probe cafe, to show that indeed they were few:

J. Cleymans, K. Eskola, R. V. Gavai, S. Gavin
S. Gupta, D. Kharzeev, E. Quack, K. Redlich
P. V. Ruuskanen, H. Satz, G. Schuler, K. Sridhar
D. K. Srivastava, R. L. Thews, R. Vogt, X.-N. Wang

For moral support, I had recalled the motto of the Tupamaros, the revolutionaries in Argentina who overthrew Spanish rule there: "somos pocos, pero bien montados" (we're few, but on good horses). From the very beginning, Vesa was there, calm, moderate, but also authoritative. The idea was to first study the relevant phenomena in pp collisions, then in pA interactions (to get the effect of normal nuclear matter), and finally go on to AA and the search of the QGP. The idea caught on, with a succession of meetings in a variety of institutions willing to support this effort. And while at the beginning it was an effort of theorists, first Carlos Lourenço and then other experimental colleagues joined in to keep us from straying too far from reality.

Hard Probe
Cafe

CERN, Geneva 1994
LBL, Berkeley 1994
ECT*, Trento 1995
INT, Seattle 1996
CFIF, Lisbon 1997
INT, Seattle 1998
JYFL, Jyväskylä 1999
BNL, New York 2000
NBI, Copenhagen 2001

The first step of our efforts, the pp study, was published in 1995 [11] and has subsequently provided a basic input to much of the planning of RHIC and LHC experiments. The next step, going on to nuclear targets, was clearly much more difficult, but it came in 2003 [12] and also played a major role, particularly in the studies of multiple scattering processes and of nuclear parton distribution functions. The latter is another topic in which Vesa and his school really did much to set the direction of research [13]. Both hard probe volumes thus contain as central contributions the works of Vesa and his collaborators - works which have defined the framework for much of hard probe studies in high energy collisions.

By the time of the last hard probe cafe meeting, in 2001 in Copenhagen, it had become quite clear that hard probes would be a (or the?) main way to address the study of high energy nuclear collisions, and that it would require a larger forum than our little cafe. CERN formed a large working group to study *Hard Probes in Heavy Ion Collisions at the LHC*, and the result [15] provided the basis for much of the planning of nuclear collision studies for the LHC. When the LHC program starts next year, such studies will be carried out not only by the dedicated heavy ion experiment ALICE, but by CMS and ATLAS as well.

It had also become clear that the now much larger hard probe community would need a dedicated forum for the presentation and discussion of experimental results and theoretical developments. So our little cafe grew into *The International Conference on Hard and Electromagnetic Probes of High Energy Nuclear Collisions*, with the first meeting held in Ericeira (Portugal), in November 2004 [14]. The group of sixteen at the first cafe meeting had now become more than 120, and in Ericeira there were as many or more experimentalists as there were theorists. The second Hard Probes Conference came a year and a half later, in Asilomar (California), in June 2006, with still more participants. Number three is at the horizon, in early 2008, and it will be held in Santiago de Compostela (Spain) - for many centuries the end point of pilgrimages from all parts of Europe. So, Vesa, let me already now wish you "hyvää matkaa" and assure you, that no matter where and when, the hard probers are counting on you in the future as much as they did in the past.

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Ultrarelativistic Heavy Ion Collisions
– Two Decades of Interaction with RVesa

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As a longtime collaborator of professor Vesa Ruuskanen, it is an honour to write this essay. In describing our collaboration, I proceed in a chronological order, supplementing the story with some personal impressions. In addition, I will also try to review Vesa's most important works with many others in the field of ultrarelativistic heavy ion collisions (URHIC).

My first encounter with Vesa in the 80s

I became aware of Vesa as a particle physicist in 1981, by reading his article, the first chapter in a new intriguing book *Alkeishiukkasten maailma* [1], aimed for general public. The timing of such introductory book was excellent, shortly before the discovery of the $W^{\pm}$ and Z^0 bosons. The very interesting articles in *Alkeishiukkasten maailma* clearly had a strong impact on my decision to study theoretical particle physics. Some years later, in 1985, when I was a third-year undergraduate student in Helsinki, a good friend of mine was studying at the Department of Physics in the University of Jyväskylä (JYFL). He was struggling with maths at the electrodynamics course given by Vesa. From my friend I learned that ”Professor Ruuskanen is a tough teacher with extremely high requirements!”.

In summer 1986, I first met Vesa's graduate student Markku Kataja, a very nice fellow who was visiting Keijo Kajantie, my MSc thesis supervisor, in Helsinki. Markku was kindly advising me on how I could treat the wave solutions of the $U(1)$ cosmic string fields at the boundary of the universe. I met Vesa himself next winter. There was an exciting world cup ice-hockey game on TV which all of us younger guys at the 4th floor of Siltavuorenpenger 20C were excitedly watching. Keijo and Vesa suddenly stepped into the small office where the TV set was. Vesa first looked a little embarrassed by the horrible noise we were making – then he realized what was going on and looked amused. I knew then I would get along well with this ”professor Ruuskanen, a tough teacher with extremely high requirements” – since in addition to physics, he clearly understood sports, too!

The 80s: Vesa's pioneer work in URHIC phenomenology

From the early 80s on, after his particle physics research activities (see Keijo's article in this book), Vesa has been heavily involved in various pioneering studies on physics of URHIC. Let me briefly describe below the fine collaborative connections Vesa had generated by late 80s.

In 1983, in a well-known paper [2], Keijo, Vesa and Risto Raitio formulated and numerically solved the hydrodynamic equations for hadronic matter produced in URHIC at CERN-SPS energies. By determining the evolution of the energy density, fluid flow velocity and net baryon number density, they discussed the possibility of producing the quark-gluon plasma (QGP) phase, a new phase of matter, in URHIC. In collaboration with Keijo, Vesa had also initiated his studies of electromagnetic signals emitted thermally from strongly interacting matter, photons in particular [3]. He was also collaborating with Bengt Friman on understanding the transition of expanding QCD matter from the mixed phase to the hadronic phase and effects of the relatively long-living mixed phase on the development of transverse flow [4].

Larry McLerran, one of the fathers of the URHIC physics, was visiting Helsinki around 1984 (I was a sophomore and did not meet Larry then, unfortunately), when his collaboration with Vesa started. Vesa also recruited Markku Kataja around that time. As a result of the fruitful collaboration between Larry, Keijo, Vesa, Markku and H. Von Gersdorff, two very well known pioneering papers on hydrodynamic evolution of QCD-matter produced in URHIC (and also in high-multiplicity fluctuations of $p-\bar{p}$ collisions!), were published [5, 6]. Issues related to e.g. multiplicity scalings, initial conditions, importance of transverse flow on hadron spectra, decoupling, and difficulties with the shock waves due to the assumed first-order phase transition, were discussed in detail. In a well-known paper [7] with Keijo, Larry and Markku, Vesa continued the studies on thermal signatures of QCD matter, considering especially the transverse flow effects.

In the latter part of 80s, Vesa extended his URHIC studies also to the evolution of strangeness in the hydrodynamic context [8]. Shock phenomena in QCD matter he studied with P. Danielewicz in [9]. While on a sabbatical in the USA, Vesa collaborated in Urbana with Gordon Baym on transverse energy production in $p+A$ and $A+A$ collisions [10], and in BNL with Helmut Satz on J/Ψ production, in particular the momentum dependence of the J/Ψ suppression [11]. Vesa has also been quite interested in the possibility to develop cascade simulations for multiparticle systems in URHIC [12]. He collaborated with G. Bertsch, Larry et al. on this topic in [13].

Vesa has always been very active in participating in different meetings and schools abroad (even at his own expense when the travel money was scarce). Of the very useful study material for us younger generations from such meetings, let me mention Vesa's lectures given at the 26th Cracow School of Theoretical Physics, at Kazimierz, Poland, in June 1986 [14]. This set of lecture notes has been widely studied on URHIC lecture courses in Finland and elsewhere.

Beginning of my scientific interaction with Vesa in late 80s

In March 1988, having just started my PhD studies in URHIC with Keijo, JYFL was hosting the Annual meeting of the Finnish Physical Society in Jyväskylä, and Keijo and I had a chance to visit Vesa at JYFL. Being used to visit Keijo's small office at Helsinki, my first reaction was that they really have large offices in Jyväskylä – of course I did not realize that Vesa was the head of JYFL at that time. Next, Keijo was already enthusiastically describing our perturbative thermalization problem to Vesa. I hardly dared to comment on anything, I was mainly admiring the ease of the discussion between these experienced scientists. I still recall how impressed I was by Vesa's immediately relevant and constructive comments.

Practically from then on, Vesa and I have been in frequent scientific contact. In 1989, when I was studying secondary production of dilepton pairs with Juha Lindfors, Vesa gave me the thermal dilepton results from his latest calculations [15] for comparison. As the comparison required some further computer runs from Vesa (done over the weekend I believe), and as I knew how busy Vesa was with his other studies and departmental duties, I was very grateful for his kind help to a graduate student. Such friendliness describes Vesa well, I believe.

In 1989 Keijo had recruited Sean Gavin as a postdoc in Helsinki. During his postdoc time there, Sean was also frequently working with Vesa in Jyväskylä, on e.g. the effects of partial thermalization on the pion p_T spectra in URHIC [16]. It was around those time that I more concretely started to realize, still from a graduate student's perspective, how important and productive the Helsinki-Jyväskylä connection actually was. Let me also mention that in the beginning of 90s, Vesa had established also his collaboration with Tanguy Altherr, resulting in Ref. [17], where they studied the QCD corrections to thermal dilepton production at invariant masses much smaller than the QGP temperature. This collaboration ended prematurely due to the shocking climbing accident in which Tanguy died.

In October 1990, right when the two Germanies united, Vesa and I were in Strasbourg, participating in a workshop on QGP signatures. Vesa gave an invited talk on thermal photons and dileptons in URHIC [18]. In addition to the actual meeting, a couple of reminiscences come to my mind: First, in a restaurant there, we both ordered traditional Eisbein without knowing what exactly it was. We were embarrassed of the rather primitive looks of the meal but then equally impressed of its good taste. The other reminiscence is a serious traffic accident we witnessed when taking an evening walk together – luckily help was immediately available for the victim. After Strasbourg, we took a train to Aachen, to a subsequent big ECFA/LHC meeting where LHC physics, including URHIC, was discussed in a larger scale for the first time. We both were impressed of e.g. G. Altarelli's great plenary talk there and have often had good time together in recalling one of his statements, expressed in a charming Italian style: "... and if the *Minimal* Supersymmetric Standard Model fails, we can always take another one!"

In early spring 1991, I finally handed the first version of my PhD thesis to Keijo for his first look. I did know that Vesa was supposed to be one the two referees – but I did not know Keijo, super-efficient as always, was going to send this preliminary version directly to the referees! Well... the amount of corrections Vesa had marked with red in the margin was pretty massive. I felt ashamed and also sorry for Vesa having had to go through my text not properly proof-read! In any case, even after this experience, Vesa did not sign off our scientific relation - instead, it was to grow stronger over the next years.

Early 90s: start of our collaboration and Hard Probes

By the beginning of 90's, with his pioneering works on hydrodynamic description of URHIC and on the predictions for electromagnetic signals, Vesa very clearly had become one of the leaders in the URHIC field. In the *Quark Matter '90* meeting in Menton, France, in May 1990, he was invited to give a plenary talk on emission of thermal dileptons in URHIC [19]. At that time, there were about 300 participants in these largest meetings in the URHIC field. This number has more than doubled now.

Vesa also delivered a big plenary talk at *Quark Matter '91* in Gatlinburg, TN, USA, [19], on the rates of thermal electromagnetic signals of QCD matter in URHIC. In the end of Vesa's talk, there was some trouble with the lights, so Vesa concluded by saying "Dimming the light is a sign of quark-gluon plasma." This got Miklos Gyulassy quite upset, in a friendly Miklos-like manner though. It still makes me laugh to recall how lively these URHIC pioneers were "discussing" in the corridor after Vesa's talk!

In 1991-93, I was a postdoc at LBNL in Miklos Gyulassy's group. During this time, Vesa and I kept in contact more or less frequently and discussed physics in different meetings whenever me met. Our research collaboration, however, started more concretely only in 1994, when I was back to Helsinki and Vesa was the head of the Research Institute for Theoretical Physics there. We were then also both invited to be among the handful of original members of the *Hard Probe Collaboration (HPC)*, initiated by Helmut Satz (see his article in this book) and Xin-Nian Wang at CERN/TH in 1994. The *HPC* has given motivation for many of our studies later, in addition to good friends.

In 1993, the HERA results from deep inelastic scattering revealed the small-x rise of sea quarks and gluons, which obviously also had an impact on our minijet estimates for the LHC: the initially prodcued gluon multiplicities and transverse energies were expected to grow considerably. This is turn would imply denser parton system and longer QGP lifetimes. At that time, we were still thinking about the production of the initial QGP state in terms of hard+soft mechanism rather than in terms of gluon saturation (as we now do). With the gluon multiplicity grown possibly by as much as a factor 3.6 (with the very steepest small-x rise proposed at that time) relative to our old estimates, however, Keijo, Vesa and I made an interesting observation in [21]: the energy per gluon in the perturbatively produced system above $p_T = 2$ GeV seems like in thermal ideal boson gas. This lends support to a rapid thermalization as further multiplication of gluons is not necessary. We were thus able to estimate the lower limits of charged particle multiplicities for central Pb+Pb collisions at the LHC, and especially Keijo and Vesa were enthusiastic about the large numbers $dN_{\rm ch}/dy = 2200 - 3400$ we then obtained.

Around these times, Vesa also started his collaboration with Ramona Vogt. With B. Jacak and P. McGaughey, they launched the well-known studies on rapidity distributions of dileptons, [23, 24] which later continued to include also the very large background from charm decays studied in [25]. For details, see Ramona's article in this book.

In June 1994 in Vuosaari of Helsinki, Vesa organized the meeting *Physics of high energy heavy ion collisions* in the European Physics Conferences series. That June is one of coldest ones I recall in Finland (+8 C). Many of the $>$100 participants were, understandably, horrified by the Finnish "summer". In addition, in connection with this conference, Vesa had also organized an URHIC summer school in Jyväskylä, which took place at the new campus at Ylistönrinne, where the new buildings for the Accelerator Laboratory of JYFL and for the Department of Chemistry had just been completed (the physics building was completed in 1995). I was walking with Vesa and others from Mattilanniemi to Ylistönrinne across the brand-new white pedestrian bridge, and saw for the first time the beautiful white buildings against the green forest raising on the hill. Having a windowless small office in Siltavuorenpenger, I couldn't help gasping: "How can I get to work here...?" Vesa smiled and replied: "We'll see about that..." At least for me, seeing the beautiful new buildings may have been the first seed for the idea that I could come to work in Jyväskylä instead of trying to stay at Helsinki (where the plans for the fine new campus at Kumpula already existed) and collaborate from there with Vesa.

In July 1994, we both also participated in the *HPC* workshop meeting at LBNL. We worked quite hard for the *HPC* write-up [26], Vesa in the groups computing the dilepton and heavy quark cross sections and I for the jet group. In this first *HPC* write-up, our idea was to review the theoretical status of the perturbatively calculable QCD cross sections (to as high order as possible) in $p + p$ collisions which would be used for reference cross sections in the search of QGP signatures such as thermal photons and dileptons, energy losses of hard partons, etc. The work for the write-up continued still after the Berkeley meeting but after this nice but strenuous trip (and the terrible June weather in Finland before that!), we were both convinced that if one works in Finland for the whole year, experiencing the darkness of our long winter, one should at least try to take one's summer vacation during the warm and sunny July here.

Late 90s: our collaboration intensifies

In 1996-7 I was a CERN Fellow at the Theory Division. Also during that time, Vesa and I kept in close scientific contact. We met in different meetings, e.g. in *Quark Matter '96* in Heidelberg, and in the workshop in Bielefeld, organized in honour of Helmut's 60-year birthday, May 1996. We also shared a Travelodge appartment in the *HPC* workshop in Seattle in November 1996, which was very nice and cosy arrangement. "Man's gotta know his limitations!", picked up from a Clint Eastwood movie we watched together one night, is a phrase Vesa sometimes quotes in troublesome situations.

In a Trento ECT* workshop in June 1995, Larry mentioned to me that instead of the $2 \to 2$ minijet processes we had been advocating, the linear limit of his model with Raju Venugopalan (later known as the color-glass condensate model) would suggest a $2 \to 1$ mechanism. This triggered me to initiate a BFKL study [27] with Vesa and his visitor Andrei Leonidov. Vesa and I had intense discussions about this problem, e.g. when we met at Nordita in a workshop in June 1996, organized by Henning Heiselberg. At that time, Vesa was also a member of the Nordita board.

Around these times, Josef Sollfrank was a postdoc in Helsinki, and working also with Vesa in Jyväskylä. Vesa also recruited a new excellent graduate student, Pasi Huovinen, by 1996. In particular, Vesa's group was heavily working on developing a proper treatment for the longitudinal (boost non-invariant) flow and for the initial state in their hydrodynamic codes, and on reproducing the hadron spectra measured at the CERN-SPS [28]. Markku was then already far in shifting from URHIC to physics related to paper manufacturing (see Markku's article), so the very well known paper [28] co-authored by Vesa, Josef, Raju, Pasi and M. Prakash is Markku's last one in URHIC physics (so far!). The fruitful collaboration on the axis Vesa-Pasi-Josef continued also after Josef had left for Bielefeld, in papers [29, 30]. The outcome of this nice series of papers on hydro and thermal signals was clear: the measured hadron spectra at SPS constrain the hydrodynamic spacetime evolution, the initial state and the Equation of State in particular. Based on the computed hydro evolution, the rates of thermal signals could then be predicted.

In the beginning of 1997 Vesa visited Keijo and me at CERN/TH for a week. My wife Riitta and I had the pleasure to accomodate Vesa. Thoughtful as always, Vesa brought a famous children's poem collection by Kirsi Kunnas to our son Otto as a gift. That book has given many enjoyable moments for Otto and later also to his little brother Jussi. Workwise, Vesa's visit was a success. We worked long hours in solving the hydrodynamic space-time evolution of QGP with non-boost-invariant minijet initial conditions [31], a problem Ulrich Heinz described by "somebody should have done this long ago". One of the highlights of Vesa's visit was also the great hiking trip which Keijo, Vesa and I made to Reculet, the highest peak of Jura mountains. Keijo also took some photographs on the hike, see Keijo's article in this book.

It was largely thanks to Vesa's encouragement and the *HPC* meetings that I continued the nuclear parton distributions (nPDFs) studies. Earlier, in 1992 at LBNL, I had carried out one of the first global-like DGLAP analyses for the nPDFs [32]. While nuclear quark distributions were becoming under control, the gluons were in practise not known at all. I was pretty frustrated at the (technical) troubles in the analysis and quite reluctant to carrying it on further, alone at least. In April 1997 Vesa invited me for a visit to Jyväskylä, to describe the nPDF DGLAP analysis to him and his new graduate student Vesa Kolhinen in detail. This was quite late in spring but there was still some snow around, so Vesa lent me skis and took me to the ice of Lake Päijänne. It was fun to ski for the first time in 15 years, the previous time being in the army! While waiting for Vesa to return from a longer ski trip, it was particularly nice to take a rest sitting on a warm stone on the shore bathing in sunshine. It was also very pleasant to enjoy Ruuskanens' hospitality – and of course sauna after the skiing trip.

In any case, after the discussions with the two Vesas, I was willing to restart the DGLAP analysis in collaboration with them. It was also clear that if successful, the analysis would be very useful for the whole URHIC community. Let me also mention that in addition to the discussions with $2\times$Vesa, I had had very useful and motivating discussions with M. Ryskin and Vesa in 1995 at Helsinki, M. Strikman at DESY in May 1996, the *HPC* members, and R. Gavai in the *HPC* workshop in Lisbon in September 1997. All this led to the first global DGLAP analysis of the nPDFs published in [33], and to a longlasting collaboration with Carlos Salgado, from which also the EKS98 parametrization [34] for the nPDFs resulted. I am grateful to Vesa for his encouragement – the papers [33] and [34] now have altogether nearly 500 citations in the QSPIRES data base. This topic, with extensions to nonlinearities and NLO, has been a fruitful one for our graduate student researcher training as well. VesaK's PhD thesis centered around this topic as well, see Fig. 3.

Some years earlier, Vesa had been one of the intiators of the annual International Jyväskylä Summer School activities. In URHIC and particle physics, the standard of the lectures had been set quite high by e.g. Mikko Laine's (now prof. at Bielefeld) excellent finite-T field theory lectures. I gladly accepted Vesa's invitation to give a set of lectures on NLO DIS and Drell-Yan cross sections and PDFs in the Jyväskylä Summer Scool in August 1997. I met two of my future graduate students, Kimmo Tuominen and Heli Honkanen, recruited by Vesa, there. These lectures have later been very useful for me in training graduate students in particular. The other lecturer was Raju Venugopalan, he lectured on light-cone perturbation theory.

In Spring 1997, JYFL was searching for a senior assistant to work in Vesa's URHIC theory group. I had a 5-year assistant position in Helsinki, and had never seriously thought about leaving there but since I had such a positive experience of Vesa as a collaborator and friend, and Jyväskylä as a place and JYFL as a department, I decided to apply - inspite of the fact that it would be harder for my wife to find a job there than in Helsinki. That's how I got to JYFL, in the end of 1997.

With Vesa at JYFL, 1998 –

In the end of 1997 and in the very beginning of 1998, when I still had no apartment in Jyväskylä, Vesa and Aune were very kind in accomodating me until I found something suitable for my whole family. I am grateful for their warm hospitality - it eased up the stress of bringing family with a new-born (2nd) child to a new place. Pretty soon Vesa also took me to buy a pair of skis, and showed how these new types of skis should be properly vaxed. Already earlier I had noticed that we had quite similar curiosity towards all kinds of sports, such as sumo, track&field athletics, skiing, etc. Vesa himself was a serious skier and in a great shape at that time, a member in the department's rowing team as he was, participating in the heroic 4h long rowing competitions at Sulkava annually. This common interest in sports continues: we were e.g. watching together the Finnish T&F championship events at the Harju field in July 2006.

In spring 1998, I was relieved from teaching duties, so I was better able to focus on completing our nPDF analyses [33] and [34], student supervision, and also attend Vesa's successful SuSy lectures. We were also planning how to develop the URHIC activities at JYFL, including the URHIC activities in the Nuclear Matter project of the Helsinki Institute of Physics (HIP), and further research topics, high energy teaching and student supervision. But things did not go as smoothly as Vesa had planned. One day in September 1998, Vesa mentioned in passing that he was going to help his neighbour to assemble the roof support structure. A few days later (after the week-end) Rauno told me that Vesa had had a very bad falling accident. Naturally this was a shock for the whole department. Also the amount of inquiries about Vesa's condition I received from his friends and colleagues world-wide over the next months was overwhelming.

In spite of the all the hardships, Vesa made his way back to work by next summer. After supervising Pasi also from the hospital, it must have been a gratifying moment for Vesa to participate in Pasi's thesis defense in late Spring 1999. Vesa was the custos, the referee between the defendant Pasi and the opponent Dirk Rischke. This defense is one of the best ones I have witnessed, and it also initiated the nice tradition of Dirk's annual visits at JYFL.

By autumn 1999, Vesa was back at office. Everyone was delighted to see Vesa active again, especially in the *HPC* workshop we organized at JYFL in September 1999. Pasi started in LBNL as a postdoc soon after that. In 1999-2000, also the JYFL-ALICE group was founded, an activity that Vesa had been suggesting and supporting for a long time. Juha and Rauno describe this part of JYFL's history in their article in this book. By Spring 2000, Vesa had made a remarkable return back to full-time work with research, teaching, administration and all the other duties. And even traveling abroad: in May 2001, Vesa was already participating in the 9th International Workshop of the *HPC*, organized by Ramona et al in Copenhagen.

But back to physics. After the Torino Quark Matter '99 conference and a discussion with Berndt Müller there, we got the exciting idea of computing the URHIC multiplicities from the perturbative minijet cross sections supplemented with a saturation criterion for the produced gluons. The phenomenon of saturation, gluon fusion instead of splitting at sufficient gluon densities, had been originally introduced by Levin, Ryskin and Gribov already in 1981 for gluons in the wave functions of the colliding hadrons. With Keijo in CERN, we had realized in 1996 that with the infrared cutoff $p_0 = 2$ GeV at the LHC, the *produced* minijet system in Pb+Pb collisions was saturated in the sense that the produced gluons (or the transverse areas occupied by the partonic subcollisions), overlapped. Relating the produced gluon multiplicity to final pion multiplicity through early thermalization and entropy conservation in hydrodynamics, we predicted the RHIC multiplicities in central Au+Au collisions [35] correctly, one year before the first RHIC data. Remarkably, we were finally able to actually *compute* the initial conditions for the produced QGP at RHIC and LHC, including the net-baryon number!

In 2001, with Kimmo Tuominen and our new graduate student Sami Räsänen in [36], we finally managed to combine our expertises in computing the initial state and in solving realistic, transversely expanding, hydrodynamics. In addition to the dramatic transverse energy reduction by a factor ~ 3 between the initial and final states, an important prediction was also the LHC multiplicity of about 2600 charged particles at the central pseudorapidity (η) unit of central Pb+Pb collisions, i.e. only a third of the initial design multiplicity of the ALICE experiment – a prediction soon taken seriously at ALICE, too. Later, in 2002, Vesa, Keijo, Kimmo and I studied also the rapidity dependence of particle production in central Au+Au collisions at RHIC, in the pQCD + saturation + hydrodynamics framework [37], charting the applicability region of our approach. Figure 1, showing our multiplicity predictions against RHIC data, is based on Vesa's invited talk [38] in *International Conference on Statistical QCD* at Bielefeld in May 2001, organized in honour of Prof. Helmut Satz's retirement. In addition to the many interesting physics results presented there, I have one reminiscence related to Vesa: The conference dinner was organized at the local castle. I had promised to give a hand if Vesa needed help in getting to the dining room. The dining hall turned out to be in the second floor of this old castle, and naturally there were no elevators and the floors were quite tall. This is one of the occasions I have been happy about my weight-lifting experience: we made it safely there and back.

Like before, Vesa continued collaboration with others as well. In 2001, Pasi, P. Kolb, Ulrich Heinz, Vesa and S. Voloshin made an important prediction from hydrodynamics for the amount of azimuthal asymmetry in hadron spectra, elliptic flow, in noncentral URHIC at RHIC. This phenomenon is one of the key predictions for the existence of QCD-*matter* in URHIC, as it indicates pressure formation. After the experimental observation, their pioneering paper [39], currently with over 230 citations, is becoming a standard reference in the field. With Ulrich et al, Vesa had studied elliptic flow also earlier, for the SPS, in [40]. In addition, Vesa, Pasi

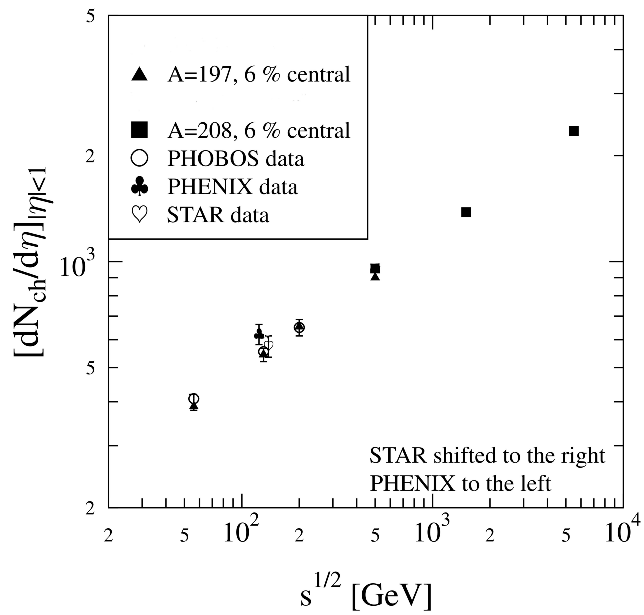

Figure 1: Comparison of our results from pQCD + saturation + hydrodynamics for the charged-particle multiplicities against the first RHIC data from nearly central Au+Au collisions at different cms-energies. Also our prediction for the multiplicity in 6 % most central Pb+Pb collisions at the LHC ($\sqrt{s} = 5500$ GeV) is shown. Figure is based on [38].

and Sami published thermal photon emission rates for the SPS [41]. They also consider the phenomenon for RHIC, the results are summarized in [42]. Currently, one of the hot topics in this field is the promising increase of few-GeV photons (on top of the prompt photons predicted by pQCD) in central Au+Au collisions at RHIC, quite likely yet another sign of the QGP.

An important URHIC theory activity of ours started in 2002. HIP was searching for new projects with young project leaders into its Theory program. With Keijo, we were planning to submit a proposal for an *URHIC* theory project (lead by me), which would combine the research expertises in the URHIC theory group at JYFL and in Keijo's Finite-T field theory group in Helsinki. At first Vesa was slightly reserved about such proposal as in the HIP LHC Program there already was an URHIC project within which the experimental JYFL-ALICE activity had just started (see Juha's and Rauno's article). It was, however, obvious that a separate theory project would be beneficial for everybody, so finally we all agreed to go on and the proposal was accepted. I believe it is fair to say that the *URHIC* theory project has been a success: after an excellent mid-term review, the project is now on its second 3-year period. Thanks to the HIP funding, we have been able to hire postdocs and graduate students in the two groups involved, and also collaborate with Kari Rummukainen in Oulu and CERN/TH. Collaboration with the HIP-ALICE project is concrete, with Jan Rak we now have weekly URHIC theory - ALICE physics meetings, and we also collaborate in recruiting graduate students, and also organize physics meetings together.

Back to physics results again. In 2002, we were able to show that a rather high decoupling temperature, $T_c = 150$ MeV, is needed in the hydrodynamic calculation to reproduce the data on hadron transverse momentum spectra. The results were documented in Ref. [43], prepared in collaboration with Sami and our new graduate student Harri Niemi. Vesa reported the results in the *Quark Matter '02* meeting in Nantes [44], where he also was the convenor of the photon parallel session. In fact, it was surprisingly cumbersome to prepare the paper [43], as there were many intermediate steps to be taken before obtaining the results (see Sami's and Harri's articles). For instance, at first we had too few hadrons in the EoS for the hadron resonance gas, and due to the earlier SPS results where T_c was lower, a high T_c was not obvious to us right away. Thanks to Sami's ideas, and Harri's and Vesa's hydrodynamic efforts as well, we were able to get around these problems. Many quite heated discussions took place in Vesa's office, e.g. on how the results should be interpreted and shown but eventually we got the paper ready. The referee report was pretty positive but the referee (an obvious expert!) wanted to see some checks and comparisons against a pion gas. As we agreed with the referee on the usefulness of such checks, we decided to do them just quickly – this took at least half a year and made us think of the results from yet a new angle! In any case, although the actual results did not change, the paper and our understanding of our hydrodynamic results improved, so all the additional trouble was worth it.

During 2001-2004, one of the major international activities in our URHIC group was the work for the CERN Hard Probes in Heavy Ion Collisions at the LHC. This series of three workshops, invited by Urs Wiedemann, Michelangelo Mangano from CERN/TH and Helmut, was a continuation of the earlier *HPC* activity, only in a larger scope. Vesa was heavily involved with the work for photon subgroup coordinated by Patrick Aurenche. I was one of the convenors of the subgroup for $p + A$ collisions and nPDFs, and also the editor of the subgroup's write-up. The effort was intense indeed, e.g. Vesa and his collaborators in the photon group [45] contributed with 7 articles in the CERN Yellow Report [46]! Let me in particular mention their extensive studies on photon production from nonequilibrium QGP, prepared together with F. Gelis [47].

In August 2003, we were glad to host Ramona Vogt and Jorgen Randrup for a month (see Ramona's article in this book). In terms of papers prepared, this nice visit has been one of our most successful ones: while I was collaborating with Ramona for a paper on heavy quark production with the nonlinearly evolved PDFs, Vesa and Jorgen studied issues related to thermodynamic consistency in the QCD matter EoS, resulting in Ref. [48].

In 2004-5, we launched a more comprehensive study of computing the hadron transverse momentum spectra on one hand from pQCD + saturation + hydrodynamics, and on the other, from pQCD + energy losses + fragmentation which I had been studying Urs, Carlos and my

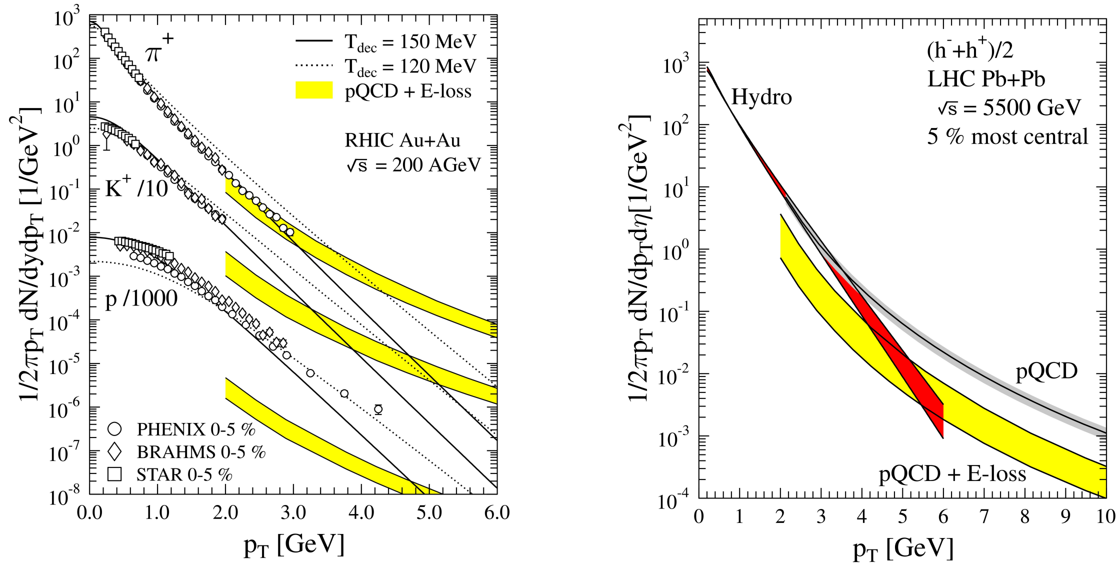

Figure 2: Left: Transverse momentum spectra of positively charged hadrons in 5 % most central Au+Au collisions at RHIC. Right: Our prediction for the charged-particle p_T spectrum at $\eta = 0$ for Pb+Pb collisions at the LHC, corresponding to $dN_{\rm ch}/d\eta = 2600$. Figures are based on [49].

graduate student Heli Honkanen. I believe it was originally Heli who insisted that we should seriously try to combine these two different studies. This proved out to be a very good idea, as the high-p_T pQCD fragmentation component of the spectra turned out to be practically decoupled from the hydrodynamically obtained low-p_T component. We were thus able to check where the interesting cross-over region between these two mechanisms would be. After extensive comparisons with the RHIC data, we were able to make a prediction of the hadron spectra in central Pb+Pb collisions at the LHC, see the results in Figs. 2. Writing the paper [49] was a long but gratifying project, where every one of the authors had a crucial contribution.

Life had yet another unpleasant surprise for Vesa. In Spring 2005, he was diagnosed with cancer. He was immediately treated for the illness. Fig. 5 is taken at Vesa's house at that time. Characteristically, Vesa conquered this setback as well, and after a relatively short sick leave in summer 2005, he again returned back to full-time work in autumn 2005. Soon after this second return, Sami Räsänen reached the degree of PhD (see Sami's article). Then, in 2006, Vesa also finished a review article with Pasi on hydrodynamical results from URHIC, the electromagnetic signals, the hadron spectra and elliptic flow, invited by the Annual Review of Nuclear and Particle Science [50].

Concluding words

It must have been satisfying for Vesa, as one of the pioneers in URHIC physics, to significantly contribute to the birth and growth of the field, and witness the evolution from the fixed-target experiments at CERN-SPS to the collider experiments at BNL-RHIC and now to the very high-energy collider experiments at the CERN-LHC. Locally at JYFL, high-energy physics in general has been growing significantly: while in the 80s and most of the 90s Vesa was the only senior person in high-energy physics at JYFL (with students and postdocs though), we are now altogether five senior physicists working in URHIC, particle physics and cosmology (see Jukka Maalampi's article), we have postdocs and we all supervise students. With the grown number of personnel, high-energy physics teaching at JYFL can nowadays be more easily organized in a systematic manner than before. Vesa himself has been an excellent example of how a good and intuitive teacher, who is actively involved with internationally acknowledged research, also attracts good students at all levels.

URHIC physics activities at JYFL, both theoretical and experimental, have been developing nicely, also thanks to the created HIP projects and obtained external funding from the Academy of Finland. The current URHIC theory group is shown in Fig. 6 below, a photo taken in November 2006. With so many young faces there, this photo demonstrates the continuity of the field at JYFL.

Vesa and Keijo have set the example for my generation of particle physics theorists of how physics should be done, and on a personal level, of how one should stay active. My interaction with Vesa has been quite close over the two decades we have known each other, especially during the last nine years spent together at JYFL. We have discussed practically daily, at office or over lunch and coffee. This has been most enjoyable. It is also fair to say that our collaboration has not always been totally frictionless, we have sometimes had quite frank discussions and disagreements. Most importantly, however, we have always been able to find compromises and solutions for the matters we have not agreed upon. Together with many others, I have also been impressed by Vesa's mental strength in encountering the unexpected in life, in particular by his unique way of maintaining good humour and friendliness.

Vesa, it has been a priviledge to work together with you – I am looking forward to the continuation of this activity in the future, too, now that the first LHC results are soon at hand. Let me wish you relaxed time as a professor emeritus, with many interesting physics problems to study free of other duties!

Acknowledgements. I thank K. Kainulainen for comments. The URHIC theory group gratefully acknowledges the financial support from the Academy of Finland, currently from the project 206024.

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Figure 3: Co-supervisor Vesa, opponent Nestor Armesto, custos and supervisor KJE, with the successful PhD thesis defendant Vesa Kolhinen, in October 2001. Photograph from Vesa Kolhinen.

Figure 4: Vesa congratulating Péter Lévai, the opponent of Heli Honkanen, after the successful PhD thesis defense in January 2005. Kirsi Manninen is congratulating Heli, the relieved PhD to-be. I was the custos in this defense. Photograph from JYFL archives.

Figure 5: Finishing the paper [49] at Vesa's home in June 2005. Photographs from Vesa Ruuskanen (taken by Vesa's son Timo).

Figure 6: The URHIC theory group at JYFL in November 2006. Back row from the left: MSc students Tuomas Karavirta and Matti Heikinheimo, PhD students Hannu Paukkunen and Topi Kähärä, Postdoc Thorsten Renk, Assistant doc. Kimmo Tuominen, MSc student Perttu Mäkinen. Front row from the left: MSc student Jussi Auvinen, Lecturer doc. Kari J. Eskola, Professor Vesa Ruuskanen, PhD student Harri Niemi, and PhD Vesa Kolhinen. Two MSc students are missing in this photograph, borrowed from our homepage, [51].

Charming Physics with Vesa

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I first met Vesa at the $6^{\rm th}$ Winter Workshop on Nuclear Dynamics in Jackson Hole, Wyoming. I was there with Jørgen, my husband to be. Naturally, he wanted to downhill ski, which left me wondering how I was going to occupy my time since the thought of looking down those steep slopes sort of terrified me. After the first morning session, I started talking to a very nice man who turned out to be Vesa. I don't know how the conversation started but it turned out that Sean Gavin, who was a collaborator of mine and currently a postdoc with Keijo Kajantie in Helsinki, had suggested that he meet me. Since Sean was a postdoc at LBNL when he suggested that Jørgen meet "this um-eh student" that he wanted to work with at the Gordon Research Conference two years earlier, I can say that, besides being a good friend himself, Sean also had some small part in introducing me to two of my best friends, Jørgen, my husband for more than 16 years now, and Vesa. Pretty quickly, when he realized that I didn't want to ski downhill, he offered to teach me to cross-country ski and we spent most of the afternoons at the meeting together. I'm afraid I wasn't a very quick study but Vesa was very, very patient with me. I didn't realize how patient until once he said he wanted to go ahead for a while and was off like a rocket, at least as far as I could tell.

At that time, I was a postdoc at Livermore and working in one of Vesa's areas of expertise, hydrodynamic models of heavy ion collisions. I can't say that the part of those big codes I worked with was getting the physics right so I wasn't enjoying that work too much. After returning to Livermore, I soon joined another group that was working on a proposed dimuon spectrometer for RHIC. That group eventually merged with several other proposed experiments to ultimately become the PHENIX Collaboration. The move was a really great one because it got me away from the shaky hydro modeling I was doing and back to hard probes like the J/ψ and open charm, more to my liking. I don't quite remember when next I met Vesa but, by then, I was working with another very nice Finn (I wonder if there are any other kind) from Helsinki, Paul Hoyer. He had been taking sabbatical at SLAC and, together with Stan Brodsky, we made a couple of very nice papers studying the A and x_F dependence of J/ψ [1] and open charm [2] production in the context of their intrinsic charm model [3]. One thing that we remarked on in the charm paper was that the fragmentation functions had to be hard, much harder than the standard Peterson fragmentation function that was the default in most calculations since it could not describe the x_F spectra of D mesons at all, even for nonleading charm [2]. This was a bit of a problem because, according to the QCD factorization theorem, since the Peterson function was fit to e^+e^- data, it was supposed to be universal and thus work for nonleading charm. Later, more modern fits of the fragmentation function have shown this to be true [4] and the Peterson function has been generally replaced by something much harder, more like the delta function we advocated.

It was Paul who first brought me to Finland. By then I was a postdoc at GSI and we were trying to finish the charm paper. Since Paul was back in Helsinki and I was in Germany, it was a question of getting Stan to Helsinki for a visit. I remember very well my arrival at Siltavuorenpenger 20C. I was coming for a couple of weeks and had brought a big suitcase. I'd heard a lot about the city and the department from Sean but I wasn't sure where to find Paul once I got there. After rolling the suitcase for what seemed like an eternity from the train station, uphill too, I pulled it off the elevator with a big thump and Stan said, "There she is!". It turned out that Paul's office was pretty near the elevator so my arrival couldn't be missed. It was May, the days were long and sunny, and I fell in love with Finland.

I know I gave a talk in Helsinki at that time but don't recall whether I went to Jyväskylä then too since if I did, I didn't give a talk. If I had to bet, I would bet that I *did* go because within a year, Vesa and I, along with Barbara Jacak and Pat McGaughey, had written our first paper together [5] in which I thanked the University of Jyväskylä for hospitality – so there. We had combined our mutual interests of charm and dilepton production. There had been several dilepton papers at the time suggesting that thermal dilepton production would be bigger than the Drell-Yan dilepton background in the dilepton mass region, $1 < M < 3$ GeV [6]. It was also at this time that some calculations of the initial conditions using minijet production to thermalize the system [7] projected much higher initial temperatures and shorter formation times than previously proposed which would lead to larger thermal dilepton yields. One thing that had *not* been included at the time was the 'background' from open charm decays. While the charm production cross section at the SPS was pretty small, at RHIC it was expected to be a lot larger and could perhaps swamp the thermal dileptons. In addition, since the initial temperature could be as high as 930 MeV at the energy of the LHC, there was also the possibility that the thermal charm production rate was non-negligible [8]. Indeed, it turned out that the thermal charm contribution was almost as big as the thermal dilepton contribution itself since the dilepton production cross section is of order α^2 while charm production is of order α_s^2. The nascent PHENIX collaboration was quite interested in the charm contribution to the dilepton continuum. However, while I could calculate charm cross sections, I wasn't so good at the decays. Thus Barbara and Pat were involved and Pat did the best he could at taking my $c\overline{c}$ pair mass and rapidity distributions and turning them into correlated lepton pairs. We focused on the rapidity distributions since we were doing a leading order calculation and at leading order the charm pair $p_T \equiv 0$. We found that the widths of the dilepton distributions were strongly source dependent. Thus, while charm decays dominated thermal production at RHIC and LHC, if the level of charm production could be brought under control by precision measurements in pp collisions, the initial production could be reliably subtracted.

Besides my visiting Finland to work on projects with Paul and Vesa, we saw each other pretty regularly at meetings. Fortunately or unfortunately, none of these were real winter meetings so I couldn't work on my minimal ski techniques but I did find out that Vesa also sometimes went running and since I had taken that up, it wasn't hard to convince him to run with me. It turns out that running is ideally suited for me. I am in no way naturally athletic and though I'm not fast, I slowly worked my way up to running over all kinds of terrain for an indeterminate length of time. I remember one meeting in Il Ciocco near Lucca, Italy where we were at a NATO school together. There I convinced him to not only jog up the hill as far as it would go, quite a long way – we decided to turn around when the road sort of ended in a cow pasture, but also to take a walk with me around the city walls of Lucca.

At these various meetings, I also kept bumping into Helmut Satz who, as it turns out, I'd met even before Jørgen and Vesa, and without any intervention from Sean. That was way back in 1986, at Gerry Brown's 60th birthday celebration at Stony Brook when Helmut told me about his interesting new paper with Tetsuo Matsui. At that time, I was working on charmonium mass shifts at finite temperature in potential models. Later on, I worked with Sean on comover breakup of the J/ψ.

Not long after the dilepton paper was finished, Helmut invited Vesa, Sean and I, among others, to CERN to join a small working group called the 'Hard Probes Collaboration'. It was February 1994, the third February in a row that I'd been to CERN. The last time, at an LHC workshop that Sean, Helmut and Vesa were also attending, Jørgen came over from Les Houches to give a talk and then dropped me off at a farm near the Rhone to jog back to CERN along the river. I didn't make it far before the farmer's daughter's dogs came out and bit my leg. They became quite docile and wanted to play afterward but I wanted no more to do with them and heading down to the river had lost its appeal. Jørgen was long gone with the car and I was at least 10 km from CERN and unsure of the way back if I wasn't going past the dogs again. After some time, by jogging, begging rides, getting lost and dumb luck, I showed up again at CERN. I got taken to the Hopital de la Tour, down the road, subjected to the tender mercies of the Swiss health care system. "Where is your insurance card?" Oops. Fortunately for me, Helmut showed up and rescued me, having found out from Sean that I'd gotten myself in trouble. It was not such a bad wound, not bad enough to keep me from going out jogging with Vesa and Jørgen in the vinyards behind CERN a couple of days later. I have been back to CERN many, many times since then and have NEVER gone back to that part of Geneva.

The Hard Probes meeting was the start of a long and fruitful collaborative effort that has led to good friendships, good physics and travel to lots of nice places. The first meeting was really productive if not a little strange in one respect: some psychology student was watching us to study the collaborative efforts of physicists. I don't really know what came out of that or what she made of us since she never talked to me. We formed several working groups on open charm and bottom, quarkonium, jets, Drell-Yan and photons. Since these were all hard processes, calculable with perturbative QCD, and produced at early times, they are 'hard' probes. We started out with pp collisions since they were easier to deal with and we wanted to establish a good baseline for comparison. To do better than previous calculations of the same processes, we wanted to make state-of-the-art calculations using next-to-leading order codes and the most recent parton distribution functions. Vesa and I worked together on the heavy quark report, along with a number of other authors, including Pat who once again provided the lepton decays that the NLO $Q\overline{Q}$ code lacked at that time [9].

The pp part was pretty easy – no arguments over nuclear effects – and by the time we met again in Berkeley a few months later, we were well on the way to getting our first volume of proceedings ready. The Berkeley meeting was in July, a time of rather iffy weather, and one Saturday morning, when Vesa and I went out for a fairly strenuous run in Tilden Park behind our house in the Berkeley hills, the fog was thick and the wind was icy, so it took a while to work up a sweat in those conditions even though we were going fairly fast. But we made it through (including Jørgen who had dropped his yard work after we left and managed to catch up with us after about two hours of even faster running). Afterwards, Vesa (describing himself as being "half dead") was very glad to relax with a Carlsberg in the backyard which had by then turned nice and sunny. During that same weekend, we invited the collaboration over for barbeque and ended up crowded in the living room with the heat on, watching the fog roll up the street like a writhing ghost. Typical summer weather in the Bay Area.

After our Hard Probes pp volume appeared, we realized that we could do much better than before on the dilepton production. The NLO $Q\overline{Q}$ code [10] was capable of calculating the pair p_T and azimuthal angle distributions so that more realistic lepton decay spectra could be obtained. We also had the Drell-Yan p_T distributions available. Along the way, we had come to realize that there were a couple of other important things we hadn't included in the original paper: $b\overline{b}$ production and the possibility of multiple $c\overline{c}$ pairs giving rise to uncorrelated lepton pairs from charm decays (a c and a $\overline{c}$ from two different $c\overline{c}$ pairs decay and these unrelated leptons make the pair). While the b quarks are about three times more massive than the charm quarks, their p_T and pair mass spectra are harder and would eventually take over the continuum from charm, especially if any momentum cut was placed on the decay leptons, as will be the case at the LHC. It wasn't until we'd started working on the NLO calculations that this kind of thing began to sink in.

The uncorrelated charm was something else that hadn't really sunk in before but it should have been obvious. However, when we were working on the Hard Probes report, doing the finger physics calculation of the number of $c\bar{c}$ pairs in a central AA collision given a total $c\bar{c}$ production cross section of a few millibarns in a pp collision at $\sqrt{S} = 5.5$ TeV gave a couple of hundred pairs. Oops. That was about the pion multiplicity in a Pb+Pb collision at the CERN SPS ($\sqrt{S} = 17.3$ GeV). Now *e.g.* muon spectrometers measure both positively and negatively charged muons. Your desired signal comes from pairs of oppositely charged muons (opposite sign pairs) but mixed in with that are also pairs that come from two positive charges or two negative charges (like sign pairs), the combinatorial background. The way you actually observe the J/ψ peak, for example, in the dimuon spectrum is to do a like-sign subtraction of the background to make the peak visible. Since the c and $\bar{c}$ decay to opposite sign muons, they are part of the opposite sign signal as long as only one pair is produced in an event. Once more are produced, the excess has to be gotten rid of, the same as the like-sign background from pions. The charm momentum spectra are harder and appear over a broad rapidity range so that the uncorrelated pairs actually have a higher invariant mass. To subtract them, it is necessary to know the charm yield rather well. Finite detector acceptance works miracles on eliminating many of the uncorrelated decays but that's because most heavy ion collider detectors are not even close to being hermetic. Of course our unexpected background is another person's signal and Bob Thews, another Hard Prober, was one of the first to turn this around and say that if there are so many $c\bar{c}$ pairs with nothing better to do (already even 8-10 $c\bar{c}$ pairs in central Au+Au collisions at $\sqrt{S_{NN}} = 200$ GeV at RHIC), why can't they end up close enough in phase space to make secondary J/ψs? Thus, instead of J/ψ suppression, you will end up with J/ψ enhancement [11]. Since the J/ψ suppression pattern at RHIC is similar to that at the CERN SPS instead of more suppression at the higher energy, some have argued that the observed J/ψs must come from coalescence of uncorrelated c and $\bar{c}$.

This second dilepton paper [12], written in collaboration with Pat and Sean, our Drell-Yan p_T expert, should have followed its predecessor into Physical Review D, especially since the calculational techniques were first advanced in D. However, it ended up in Physical Review C, despite our objections, a victim of policy change that decreed anything even vaguely related to nuclear physics had to go in C. The worst part though was getting our title changed without advance warning. Our rather whimsical title "How to Find Charm in Nuclear Collisions at RHIC and LHC" had been changed in proof to the more lengthy and informative but certainly less entertaining "Lepton Production from Charm Decay in Nuclear Collisions at $\sqrt{S} = 200$ GeV and 5.5 TeV per Nucleon". No amount of pleading could get it changed back again and it was a long time before I wanted to deal with Physical Review C once more.

The only nuclear effect we included in the papers was that on the initial parton distribution functions, nuclear shadowing, using a parameterization by Kari Eskola [13], which can reduce the overall yield in the low mass region at collider energies since shadowing results in a reduction of the low momentum parton densities in nuclei. Of course defenders of the thermal results were all over this result since it wasn't nice to hear that initial charm production would cover all medium effects like a blanket. That's why we called the paper "How to Find Charm..." since it seemed that that was about all you were going to find and not what you might have otherwise wanted. About that time, I gave a talk about our results at Stony Brook and Edward Shuryak started saying that of course the charm would lose energy and stop due to interactions in the dense medium [14] and the thermals would come shining through once more. Of course it wasn't all that simple since the heavy quarks just don't stop dead and would thermalize instead of coming to rest, leading to charm flow. We hadn't included any energy loss effects in that paper but subsequent papers certainly did [15].

In this paper we also pointed out the value of electron-muon correlations. Since a c or b quark could decay to either an electron or a muon with almost the same branching ratio, lepton pairs from heavy flavor decays could be ee, $\mu\mu$ or $e\mu$, while Drell-Yan and thermal dileptons could *only* be ee or $\mu\mu$. Every time I would bring this up in a talk, Mike Tannenbaum seemed to always be in the audience and would jump up and proclaim that nothing was ever learned from $e\mu$ in the ISR experiments and single electrons were the way to go.

Perhaps the main lesson from both of these papers was that the dilepton continuum in heavy-ion colliders was a far from friendly place where the interpretation of your observations is tricky at best. Things were much easier at the lower energy SPS since the charm contribution was small and Drell-Yan production was the main source of observed lepton pairs above the J/ψ mass. Indeed, the RHIC experimentalists have not really tackled disentangling the contributions to the continuum above the J/ψ and the charm-related lepton results at RHIC are all from single leptons. Mike has had his way in the end, after all.

That was the last paper Vesa and I wrote together but we certainly saw each other often at meetings and on my visits to Finland. Once the Hard Probes Collaboration started talking about nuclear effects and how to extend what we did in pp to pA, we found that things were not only 'hard' in the sense of perturbative but just plain hard. We had some great physics discussions in places like Trento, Seattle, Brookhaven (we can't always go somewhere exotic), Lisbon and, later, after Vesa's accident, in Jyväskylä and Copenhagen. We had some great times too. I'll always remember walking on the beach near Lisbon with Helmut and Vesa and going to 'our' restaurant where I had monkfish almost every night and the owner got more and more generous. When Bob came along, the owner decanted the red wine with great ceremony to our enjoyment.

I helped organize the meeting in Copenhagen in May 2001, while Jørgen and I were visiting the Niels Bohr Institute as the main part of a sabbatical year in Europe. That meeting was special in a couple of ways. Although I didn't realize it at the time when making the arrangements, it was the first trip Vesa made out of Finland after his accident. He and Aune stayed at the big SAS hotel, since the 'conference' hotel, the Cab Inn, had only bunk beds. It was a bit far from the institute but they did have a great Thai restaurant in the basement where we had a collaboration dinner one evening. At the Copenhagen meeting, we finally set some deadlines for getting work ready for our next volume which followed a bit over a year later. It was also the last time we met as a more intimate collaboration. Urs Wiedemann came up from CERN for the first time at that meeting and set the stage for a much bigger Hard Probes workshop for LHC physics. Many of our collaboration members were conveners of the working groups. I was on heavy quarks, Kari on pA and Vesa on photons. We had several meetings at CERN over about a year and a half, producing a Yellow Report. Now we've moved on to bigger things, the Hard Probes International Workshops. The first was near Lisbon in 2004, which Vesa attended, followed by a second one in 2006 in California.

Our little collaboration stimulated lots of good work and collaborative efforts among the members. I will always look back on those meetings as some of the best times of my life where I did good science with some of my favorite people.

It has always been a great pleasure to visit Finland and, especially Jyväskylä. My first visits were to the old department. Since it was daylight for so long, I didn't really have any visual cues as to when it was time to go home and so worked until quite late in the evening. I didn't have any hunger cues either since I'd go into the coffee room and eat some of the ubiquitous sugar toasts. That stuff was rock hard since it was supposed to be dipped in coffee. I didn't drink coffee and wouldn't dip it into Coke Light so it's a good thing I have good teeth. (If you ever wondered where that stuff disappeared to during my visits, wonder no more.) The new department across Jyväsjärvi is really in a beautiful setting and I love walking over the bridge to get there but I also liked walking over the hill to the old department. I've been there a couple of times with Helmut, probably one of them when I lectured at a summer school at the then brand new department. That was one of the two times I've been there at the same time as the Neste Rally. I remember the first time, after coming back from dinner with Helmut, being warned not to "get run over by a rally driver". I thought he must be joking but then later heard that someone did get hit the very next day. The next time was in August 2003 when Jørgen and I came with our daughter Kristina for a one-month visit (during which Jørgen made a paper with Vesa). We had an apartment with a view of the rally headquarters and could see (and hear!) the drivers (and helicopters) coming and going. Kristina found it much more exciting to see the Swedish princess on her visit to town. This was just before Kristina started kindergarten and we hoped she'd be inspired to see that the princess wore pants and start doing the same. But no such luck. That was actually the second time that the three of us were there together. The first time was at the Nordic meeting five years earlier when Kristina was still inside me. I was glad they let me back on the plane home since it was less than a month before she was due. That was also the only time I've been in Finland with a car. We drove up to Jyväskylä from Helsinki with a stop in Hämeenlinna and had a nice weekend trip to Savonlinna and Mikkeli.

I was working more with Kari during my later visits but was always glad to have time to discuss with Vesa too. While I like Helsinki and have visited several times, including when Vesa was there for a longer time, it is Jyväskylä that I really appreciate for its serenity. The countryside with the lakes, forests and many paths for walking, biking, skiing and running is absolutely beautiful. I can start running in any direction and find a really scenic place to go. I guess running doesn't sound like a serene activity but the pleasure of the run comes not from endorphins but from the surroundings. Vesa warned me about bears in the forest but I've yet to see one there although I have encountered some in California and Washington. One of my favorite places to go is not far from Vesa's house, near Ristikivi. There is an unpaved road around the peninsula, ski tracks in the forest and around the golf course, and a marked nature trail inside the ski track that I discovered one day, totally unsuited for running but wonderful for eating wild blueberries. It's like a Russian doll where you keep discovering more and more inside. Vesa is like that too. The longer I know him, the the more good qualities I discover. I can only wish him all the best and more, always. Fare you well.

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Hydrodynamiikan kautta opettajaksi

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Ensitapaaminen

Tapasin Vesan ensi kertaa keväällä 1997. Etsin tuolloin ohjaajaa LuK-tutkielmalleni, jonka pusertamisen olin ottanut kesän tavoitteeksi. En vain oikein tiennyt mistä ohjaajaa pitäisi lähteä etsimään, kun siihen mennessä en ollut tutustunut fysiikan laitoksen henkilökuntaan. Niinpä aloin plarata opinto-opasta, jossa törmäsin professoreiden nimilistan kohdalla riviin: ”Ruuskanen, Vesa FT (teor. fysiikka)”. Tämän jälkeen suoristin imaginääristä solmiotani, keräsin rohkeutta ja koputin oveen.

Professorin tapaaminen kaikessa jännittävyydessään sai suorastaan armeijan opit hiipimään selkärangasta, joten Vesan pöydän ääressä tuli istuttua hattu kourassa ja selkä suorassa teititellen. (Muodollisuuden vaivasta toki parannuttiin pikavauhtia.) Asiakseni ilmoitin kandidaatin tutkielman tekemisen EPR-paradoksista ja Bellin epäyhtälöistä, joista olin innostunut lukioaikoinani lukemani populaarifysiikan kirjan pohjalta. Aihe oli myös saanut uutta kutinaa hiljattain suoritetusta kvanttimekaniikan kurssista. Niinpä otin ilolla vastaan Vesan suostumisen LuK-aineeni ohjaajaksi. Itseasiassa Vesa teki joitakin vuosia myöhemmin pienen paljastuksen: hänen edessään istuskeleva pitkätukkainen hippi antoi mielikuvan väärälle laitokselle eksyneestä rentusta filosofista, joka etsii olevaisuuden tarkoitusta fysiikan terminologiasta. Valmistunut LuK-tutkielmani oli onneksi ollut positiivinen yllätys sen sisältäessä enemmän fysiikkaa kuin filosofiaa. LuK-aineessa sain myös oppia, ettei ”EPR-poppoo” ole sovelias ilmaisu tieteellisessä tekstissä.

LuK-tutkielman kirjoittaminen tietyssä määrin osoittautui merkittäväksi murrosvaiheeksi minun jatkoni kannalta. Olin tullut yliopistoon lukeakseni fysiikan ja matematiikan opettajaksi. Populaarikirjallisuuden kautta olin toki sangen kiinnostunut fysiikan tutkimuksesta, mutta sen tekeminen ei vielä siinä vaiheessa koskaan ollut aktiivisesti mielessä. Vesa kuitenkin näki minussa potentiaalia, joten tässä vaiheessa nuori mies mies pääsi ”fyysikkouskokeeseen”. LuK-aineen palautuksen yhteydessä Vesa ohimennen mainitsi luennoivansa kvanttimekaniikka II kurssin tulevana syksynä ja aprikoi kiinnostustani osallistua kurssille. Minä taas olin syttynyt pyrkimyksestä oppia lisää kvanttimekaniikasta, joten tapasimme uudelleen syyskuussa. Tässä kohden otin ensiaskeleet kohti jatko-opintoja, sillä tämän jälkeen erikoisalan kurssit valtasivat paikan opettajan opinnoilta.

Opetus fysiikan tekemisestä

Tuiki tavallisena päivänä porisimme Vesan kanssa Feynmanista kahvikupposen ääressä. Keskustelu kirvoitti hänet kertomaan tärkeimmän opetuksensa minulle fysiikan tekemisestä. (Ellei nyt muut opetukset ole harvenneet päästäni samaan tahtiin hiuksieni kanssa.) Vesan mukaan Feynmanilla — kuten monella muullakin urallaan menestyneellä fyysikolla — oli eräs erityispiirre: Feynman oli aina kiinnostunut sananmukaisesti kaikesta fysiikasta. Mikään ongelma ei ollut liian vähäpätöinen tai mikään fysiikan ala toisen yläpuolella. Feynman halusi ymmärtää (ja tunnetun kunnianhimoisesti ratkaista) kaikki fysiikan ongelmat joihin hän törmäsi. Kaikista meistä ei tule uusia Feynmaneja, mutta silti jokaisen fyysikon tulisi pyrkiä parhaimpansa mukaan seuraamaan ja ymmärtämään fysiikkaa mahdollisimman laajasti sen sijasta, että hautautuisi oman erikoisalan syövereihin niin, ettei enää näe sen ulkopuolella mitään. Monipuolisuus on osa fysiikan yleissivistystä. Selkeinä käytännön toimenpiteinä Vesa mainitsi monipuolisen fysiikan opiskelun, laitoksen kollokvioissa käynnin sekä aktiivisen osallistumisen konferensseissa sen sijasta, että jyrkästi rajaisin esitelmät, oman työn välittömästä näkökulmasta, kiinnostaviin ja turhanpäiväisiin. Lisäksi jokaisen teoreetikon tulisi arvostaa kokeellista fysiikkaa ja sen tekemisen haasteita. Kokeellinen fysiikkahan on mitä suuremmassa määrin erilaisten fysikaalisten vuorovaikutusten ymmärtämistä. Vaikka yksityiskohtien hallinta monesti rajoittuukin oman erikoisalan piiriin, niin silti voi oppia paljon fysiikaalisen ongelman muotoilusta sekä sen ratkaisun periaatteista. Vesa on myös itse elänyt kuten opettaa. Hänen kiinnostusta eri fysiikan osa-alueisiin ja vankan kokemuksen suomaa rautaista fysikaalista ajattelua on ollut opettavaa seurata ja helppo arvostaa.

Minun tullessani ryhmän toimintaan mukaan Pasi oli tekemässä lähtöä ensimmäiseen post doc -paikkaansa ja Kari juuri saapunut taloon. Karin saapuminen osaltaan loi pohjaa minun väitöskirjani aihealueisiin: Pasi oli keskittynyt SPS:ään, nyt oli RHIC:n ja LHC:n vuoro. Ensin hadronispektrit, perään fotonit. Harmittavasti myöhästyimme spektrien ennustamisessa hieman. Ne päätyivät graduun ennen ensimmäisiä spektrejä RHIC:stä, mutteivat julkaisuun. Fotoneiden osalta onneksi kävi toisin CERN:n Yellow Reportin yhteydessä. Kokeelliset spektrit osoittautuivat selkeästi jyrkemmiksi verrattuna meidän laskuihimme. Samaan aikaan Pasi oli vierailemassa ja esitteli Kolbin ja Heinzin tuloksia kokeellisen datan rinnalla. Yhdessä ihmettelimme täydellistä yhteensopivuutta sekä muodon että protonien suuren multiplisiteetin suhteen heidän tuloksissaan. Pasi tiesi kertoa heidän soveltamastaan ”efektiivisestä” irtikytkennästä, jossa multiplisiteetit laskettiin suuremmassa lämpötilassa kun taas spektrien muoto matalammassa lämpötilassa. Tällöin heitin ilmaan kysymyksen siitä, mitä tapahtuisi mikäli irtikytkentä tehtäisiin kokonaisuudessaan suuressa lämpötilassa. Arvailujen päälle tehtiin lasku, jonka tuloksista ensialkuun kaikki olivat hämmästyneitä. Spektrien jyrkkeneminen meidän laskuissamme täytyi selittää, mikä johtikin sinänsä mielenkiintoiseen prosessiin. Lähes painoon lähtenyt paperi sai jäädä hautumaan, Harri tuli ryhmän työhön mukaan ja aiheen tiimoilta syntyi kokonaan toinen paperi. Paperin referee esitti yksinkertaisen kysymyksen: ”Mikä on faasitranstion osuus spektrien käyttäytymisessä?”. Kysymyksen jälkeen tehtiin lisää laskuja ja saatiin myös erinomaisen paljon lisää päänvaivaa. Mehän emme ottaneet huomioon erillistä kemiallista ja kineettistä irtikytkeytymistä puhumattakaan eri hiukkasten sirontareittien yksityiskohdista, mikä on tätä kirjoitettaessakin auki oleva kysymys. Siispä emme ehkä löytäneet täsmällistä selitystä oikealle irtikytkeytymisen fysiikalle. Siitä huolimatta koko prosessi opetti paljon siitä, että hyvinkin paljon tutkitusta mallista voi löytää paljon sellaisia yksityiskohtia, jotka eivät ole alan pioneereillekaan itsestäänselvyyksiä.

Kiitokset

Pedagogisten opintojen kautta olen itse päätynyt insinöörejä opettamaan ja tarjoamaan osaamistani teknisiin projekteihin. Nyt minulta kysytään oppieni soveltamista kokonaan uudenlaisiin tilanteisiin, joten Vesan ohjeet fysiikan laaja-alaisesta ymmärtämisestä auttavat edelleen eteenpäin.

Omasta puolestani kiitän kaikesta ja toivotan Sinulle hyvää vointia ja antoisia vuosia niin fysiikan parissa kuin muutenkin.

Sami Räsänen's PhD thesis defense in December 2005. Top: opponent Horst Stöcker, custos Vesa, defendant Sami. Bottom: Horst and Vesa. Photographs from Sami Räsänen.

Top: Horst, Vesa and Sami after the successful defense. Bottom: Harri Niemi congratulating Sami. Photographs from Sami Räsänen.

Vesa congratulating Sami Räsänen for his successful PhD thesis defense. Photograph from Sami Räsänen.

Hydroa opiskelemassa

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Fysiikan laitos, PL 35, 40014 Jyväskylän yliopisto

Minä aloitin opiskeluni Jyväskylässä vuonna 1997, vakaana aikomuksenani ryhtyä hiukkasfyysikoksi. Ensimmäisen kosketukseni hiukkasfysiikan ryhmään sain, kun osallistuin kvanttimekaniikan peruskurssille. Kyseistä kurssia luennoi silloin Kari J. Eskola. Silloin minulla ei kylläkään ollut tarkkaa käsitystä minkälaista hiukkasfysiikan tutkimusta Jyväskylässä harrastetaan. Noin vuotta myöhemmin hain fysiikan laitokselle työharjoittelupaikkaa. Minulla oli tuolloin kaksi mahdollista vaihtoehtoa sekä suhteellisuusteorian että hiukkasfysiikan paikka. Ensisijaisena hakemuksessa taisi olla suhteellisuusteoria, kun hiukkasfysiikasta ei juuri muuta käsitystä ollut kuin se, mitä olin populaarikirjallisuudesta lukenut, ja että siihen liittyi jollakin tavalla kvanttimekaniikkaa. Eikä kvanttimekaniikan kurssi tuota sinänsä oikeaa käsitystä muuttanut. Suhteellisuusteoriasta olin kuitenkin kurssin käynyt, enkä uskonut omaavani juurikaan mahdollisuuksia työharjoittelupaikkaan hiukkasfysiikan ryhmässä. Ilmeisesti Kari oli kuitenkin riittävän vakuuttunut kyvyistä hyvin suoritetun kvanttimekaniikan kurssin jälkeen, niin että kuitenkin tuon paikan sain. Onneksi niin.

Ruuskasen Vesasta en oikeastaan ollut tiennyt mitään muuta, kuin mitä joskus fysiikan peruskurssilla luennoitsija pohjusti jotakin välikoetta kertomalla, että laitoksen opetusdiktaattori Vesa Ruuskanen on päättänyt, että sopiva leikkuri välikokeisiin on 7 pistettä. Näillä odotuksilla menin tapaamaan ”opetusdiktaattoria”, ja kyselemään työtehtäviä tulevaa harjoittelupaikkaa varten. Kuten aiemmin tuli ilmi käsitykseni hiukkasfysiikasta oli, että se liittyy jollakin tavalla kvanttimekaniikkaan. Tämä käsitys kyllä laajeni melkoisesti tämän tapaamisen jälkeen, kun ystävällinen, mukava vanhempi herrasmies alkoi kertoa hydrodynamiikasta, paineesta, tilanyhtälöstä ja virtauksesta eli kaikesta siitä millä en uskonut olevan mitään tekemistä hiukkasfysiikan kanssa. Vesa puhui hydrodynaamisista malleista, joita Jyväskylässä käytettiin ultrarelativististen raskasionitörmäyksien mallintamisessa. Tuohon aikaan Vesa ja graduaan viimeistelevä Sami Räsänen olivat mallintamassa RHIC:llä ja LHC:llä tehtäviä raskasionikokeita, käyttäen Karin ja Kimmo Tuomisen ns. EKRT-mallin avulla laskemia tuloksia hydrodynaamisten laskujen alkuehtoina. Minun tehtäväkseni tuli Vesan ja Samin tekemien simulaatioiden havainnollistaminen ja visualisointi. Jos olin joskus uskonut, että opinnäytetöiden ohjaus on sitä, että opiskelija näkee ensimmäisen kerran ohjaajan, kun työn aihe annetaan, ja toisen kerran kun työ palautetaan, niin tämä käsitys kyllä muuttui kertaheitolla. Vesan työhuoneen kynnys oli heti alusta alkaen matala, jos kynnystä nyt oli ollenkaan. Ohjausta sai välillä päivittäin tai useamminkin, ja Vesan kanssa käydyissä keskusteluissa oppii aina paljon. Työilmapiiri oli leppoisa ja kannustava, minkä johdosta tulostakin oli helppo saada aikaan. Hydrosimulaatioiden visualisointi onnistui lopulta yli ainakin minun odotusten, ja syntyneitä kuvia ja animaatioita käytetään yhä mm. tutkimusryhmän webbisivulla ja esitelmissä. Vaikka työ ei edellyttänytkään yksityiskohtaista hydrodynaamisten mallien tuntemusta, niin suhteellisen mittavan ohjelmointiprojektin toteuttaminen oli erittäin hyödyllistä tulevien projektien kannalta, ja tulihan siinä Minkowskin avaruuden valokartiot ja puskuinvarianteissa malleissa esiintyvät aikadilataatioefektit konkreettisesti tutuiksi. Samoihin aikoihin kävin sekä Vesan että Karin luennoimilla kursseilla. Tutkimusryhmä ja -ala alkoivat tulla yhä paremmin tutuiksi.

Figure 1: Kahden ytimen törmäyksen jälkeen muodostunut systeemi. Taustalla oleva kuva perustuu todellisiin hydrodynaamisiin laskuihin, kun taas emittoituvat fotonit ovat ”taiteilijan” näkemys. Kuva HN.

Sen jälkeen kun kvanttimekaniikan, hiukkasfysiikan ja muita vastaavia kursseja oli suoritettu, Vesan kanssa aloimme puhua erilaisista jatkomahdollisuuksista. Vesa ehdotti projektiksi ns. elliptisen hydrokoodin rakentamista. Tähän mennessä Vesan ja Samin yhdessä tekemät hydrolaskut oli tehty keskseisille ydin-ydintörmäyksille. Epäkeskeisten törmäyksien mallintaminen vaatisi uuden ohjelman kirjoittamista, ja tällaisen koodin rakentaminen olisi kuitenkin vähintään gradun laajuinen työ. Toisaalta Jukka Maalampi oli myös tulossa Jyväskylään, ja toisena vaihtoehtona Vesa esittikin mahdollisuutta tehdä Jukan ohjauksessa hieman perinteisempää hiukkasfysiikkaa. Päätin kuitenkin tarttua Vesan tarjoukseen osaksi sen takia, että virtausmekaniikka oli mielestäni jo sellaisenaan kiehtovaa, ja sen yhdistäminen hiukkasfysiikkaan vielä lisäsi mielenkiintoa. Toinen tärkeä seikka, joka kallisti vaakaa selvästi Vesa tarjouksen puolelle oli se, että aiemman kokemuksen perusteella Vesan ohjauksessa ja muun tutkimusryhmän kanssa oli miellyttävä työskennellä. Jos näin ei olisi ollut, olisin varmasti harkinnut vakavammin myös toista vaihtoehtoa. Eipä ole tarvinnut tähän päivään mennessä katua sitä päätöstä.

Ryhdyin siis Vesan ohjauksessa rakentamaan hydrokoodia epäkeskeisille ydintörmäyksille, ja työ etenikin enemmän tai vähemmän joutuisasti. Alkuperäinen tarkoitus oli, että sen valmistuttua siitä saisi gradun kirjoitettua suhteellisen nopeassa aikataulussa. Vertasin tietysti omia laskuja Samin ja Vesan aikaisemmin tekemiin keskeisiä ydintörmäyksiä mallintaviin laskuihin, ja jossakin vaiheessa totesin, että pitkällä aikavälillä on helpompaa rakentaa oma keskeistörmäyksiä mallintava koodi. Kun yhden hydrokoodin on rakentanut, siitä on suhteellisen helppo generoida lisää hydrokoodia, varsinkin kun mennään epäkeskeisiä törmäyksiä kuvaavasta mallista numeerisesti vähemmän työlääseen sylinterisymmetriseen tapaukseen. Samalla vaivalla malliin oli helppo lisätä baryoniluvun säilymistä kuvaava yhtälö, joka Vesan ja Samin koodeista puuttui. Osoittautui, että tämä lisäys muutti kaikki opiskeluun ja gradun valmistumiseen liittyvät aikataulut täysin päälaelleen. Vesa, Kari ja Sami olivat aiemmin julkaiseet ennusteet RHIC:ssä ja LHC:ssä tehtävissä ydin-ydinkokeissa tuotetulle hiukkasten lukumäärälle eri törmäyksissä. Nyt he olivat laskemassa hiukkasspektrejä samoille törmäyksille, ja vertaamassa niitä vastajulkaistuun dataan RHIC:stä. Näihin laskuihin baryoniluvun huomioon ottaminen toi mukavan lisän. Minun osuuteni piti olla tässä tutkimuksessa suhteellisen pieni. Muistan kun olin tehnyt uudet hydrodynaamiset simulaatiot baryoniluvulla terästettynä ja lähettänyt ne Vesalle ja Samille hiukkasspektrien laskemista varten. Vesa sanoikin sen jälkeen ruokapöytäkeskustelussa, että Harri on tehnyt ensimmäiset ja myöskin viimeiset laskunsa tähän paperiin. Samassa paperissa kuitenkin tarkasteltiin myös irtikytkeytymislämpötilan vaikutusta spektrien muotoihin, ja Vesa osasi selittää spektrien voimakkaan riippuvuuden tästä parametrista hadronien massaspektrin ja hadronien lukumäärätiheyksien lämpötilariippuvuuden ominaisuutena. Osoittautui kuitenkin, että faasitransitio kvarkki-gluoniplasmasta hadronikaasuun aiheutti samankaltaisia efektejä, eikä ollut helppo sanoa oliko Vesan alkuperäinen ajatus oikea. Tätä varten tutkittiin varmasti kymmeniä erilaisia hydroajoja, joista suuri osa myös minun rakentamallani hydrokoodilla. Se oli kuitenkin aika paljon ajoja ensimmäisen ja viimeisen laskun jälkeen. Lopulta kuitenkin saimme myös Vesan vakuuttuneeksi, että hän oli ollut oikeassa alunperinkin. Minulle tämä oli ensimmäinen tutkimusprojekti, ja vaikka se veikin aikaa pois kurssien suorittamisesta ja gradun tekemisestä, niin opin varmasti monissa keskusteluissa Vesan kanssa enemmän fysiikkaa, kuin luentoja kuuntelemalla. Monessa tilanteessa tuli ilmi Vesan hyvin laaja ja monipuolinen eri fysiikan alojen tuntemus. Niin luennoilla kuin keskusteluissa Vesa on aina esimerkillisen huolellinen ja tarkka sanomisissaan, ja monesti opiskelijan epämääräiset ajatukset selkenivät täsmälliseksi tapaamisen jälkeen. Tosin tämä huolellisuus ei aina näy nimien muistamisessa. Monessa keskustelussa Vesan ja Samin kanssa saattoi käydä niin, että Sami oli muuttunut Pasiksi, joka oli Vesan edellinen oppilas ennen Samia, ja minä puolestani Samiksi. Asiayhteydestä kuitenkin aina ilmeni ketä tarkoitetaan, eikä se koskaan ole mitään haitannut.

Ensimäisen paperin julkaisemisen jälkeen jatkoin gradun tekemistä, ja samalla yritin kiriä kiinni hieman jälkeenjäänyttä opiskeluaikataulua. Samaan aikaan Vesa ja Sami olivat laskemassa fotoniemissiota termisestä aineesta RHIC:n ja LHC:n ydin-ydintörmäyskokeita varten. Vesa ehdottikin projektia, jossa ottaisimme hydrodynaamisessa mallissa myös kvarkkien ja gluonien lukumääriä muuttavat reaktiot mukaan laskuihin, ja laskisimme kemiallisen epätasapainon vaikutusta fotoniemissioon. Projektin piti olla suhteellisen pienitöinen, eikä sen pitänyt häiritä muuta opiskelua merkittävästi. Kemiallisia reaktioita oli tarkasteltu hydrodynaamisissa malleissa aiemminkin, mutta kaikki edelliset laskut tarkastelivat laskuissaan pelkkää kvarkki-gluoniplasmaa, eikä faasitransitiota hadroneiksi käsitelty lainkaan. Meillä oli kuitenkin kaikki työkalut erilaisten tilanyhtälöiden laskemiseksi, emmekä uskoneet faasitransition lisäämisen olevan suurikaan ongelma. Kaikki olikin suhteellisen helppoa niin kauan kun laskuja tehtiin ilman faasitransitiota, kun taas faasitransition kanssa laskuja ei tahtonut millään saada läpi. Tämän ongelman kanssa painittiinkin sitten pitkään ja hartaasti. Taisi Vesakin jo välillä tuskastua, kun kävin lähes päivittäin esittelemässä uusinta nollatulosta. Lopulta kuitenkin olin vakuuttunut, että olin tehnyt laskut oikein, ja että meidän malli faasitransitiolle epätasapainotilanteessa oli väärin. Sain lopulta tarkasti selvitettyä missä ja millä tavalla malli pettää, ja lähdin tapaamaan Vesaa tarkoituksenani vakuuttaa viimein, että jotakin muuta oli tehtävä. Vesa oli kuitenkin askeleen edellä. Ilmeisesti hänkin oli lopulta vakuuttunut, että vika ei ollut opiskelijan ohjelmointitaidoissa, ja tarttunut ongelman selvittämiseen. En varmasti ehtinyt sanoa kuin faasitransitio, ja Vesa alkoi selittää yksityiskohtaisesti faasitransition dynamiikkaa, ja sitä millä tavalla se oikeasti pitäisi mallissamme ottaa huomioon. Siihen analyysiin minulla ei ollut mitään lisättävää. Tämä tutkimus opetti kyllä kuinka tärkeää on ihan oikeasti ymmärtää mitä on tekemässä, ja myös sen että Vesa ymmärtää.

Lopulta tutkimuksen lomasta löytyi aikaa myös gradun ja maisteriopintojen viimeistelyyn, ja maisteriksi valmistumisen jälkeen olen jatkanut jatko-opiskelijana edelleen Vesan ja Karin ohjauksessa, joten työ jatkuu. Omasta puolestani voin sanoa, että on ollut kunnia ja etuoikeus olla niin hienon ihmisen ja fyysikon ohjauksessa kuin mitä Vesa on. Minä kiitän ja toivotan miellyttäviä eläkepäiviä.

The Birth of Quark Gluon Plasma Physics at JYFL

A cartoon by **Jouni Suhonen**, inspired by H.I. Miettinen's visit at JYFL in the early 80s.