PHYSICS
THE CHINESE UNIVERSITY OF HONG KONG
中大物理
從基本粒子到宇宙 明萬物之理
中大物理通訊
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系況速遞

- 根據大學統計數字，2011年物理本科畢業生有*42%*選擇繼續升學，當中包括美國芝加哥大學（University of Chicago) 和美國布朗大學（Brown University）；另有39%選擇就業，當中投身教育界和工商界的分別佔17%和21%。

- 2011-12年度暑期本科生研究交流計劃（SURE）共有7位同學獲選，他們已於6月至8月期間前往美國及瑞士的著名學府進行研究工作。其中兩位同學在歐洲核子研究組織 (CERN) 參與暑期實習計劃。此外，共有13位同學獲選參加物理系和理學院的交換生計劃，分別前往中國、法國、美國、瑞典、加拿大及澳洲等地的著名學府修課一至兩個學期。

- 2011-12年度暑期教師學徒計劃（STAR）共有8位同學獲選。是次參與計劃的學校包括基督教宣道會宣基中學、聖公會聖本德中學、聖公會林護紀念中學、沙田基督書院、德望學校、聖公會李炳中學、路德會呂祥光中學、聖母無玷聖心書院。此外，各有*2*位同學分別獲天文台及太空館頒贈獎學金，於暑期到天文台及太空館參與研究工作。

- 今年本系共有9個項目獲得研究資助局的優配研究金（RGC General Research Fund）撥款，款項總和超過港幣700萬元。研究課題包括湍流、量子技術和納米科學等。

中大物理系與歐洲核子研究組織簽署合作協議 推動學生及科研人員參與粒子物理研究

香港中文大學物理系與歐洲核子研究組織（CERN）的主要研究團隊之一 - 緊湊渺子線圈實驗（CMS）於2012年7月13日簽署合作協議，為本系研究生及科研人員提供參與粒子物理實驗的平台，研究領域包括儀器物理學、數據分析及資訊科技。此平台將促進兩地學者交流，合作開發雙方有興趣的研究項目。是次合作協議由中大前理學院院長伍灼耀教授及 CMS 發言人 Joe Incandela 教授簽署。而 Incandela 教授及 CMS 的希格斯粒子小組召集人 Albert de Roeck 教授亦在簽署儀式後主持講座及座談會，向本系師生分享 CMS 實驗的成果，包括尋找希格斯粒子的最新進展。

2011年的暑假，兩名本科生郭家希及梁乘宙便參加了 CERN 的暑期實習計劃。郭同學主要分析由 CMS 探測器取得的大量數據，而梁同學則製作 CMS 探測器上名為陰極條感應室 (Cathode Stripe Chambers) 的部件。他們現於本系繼續攻讀研究課程並繼續從事 CMS 的研究工作。而剛過去的暑假，我們另外兩位本科生李浚彥及譚振南亦前往了 CERN，進行 CMS 探測器的數據分析，改善尋找粒子軌跡的方法。他們於今期的實習及交流天地內，跟大家分享經歷。

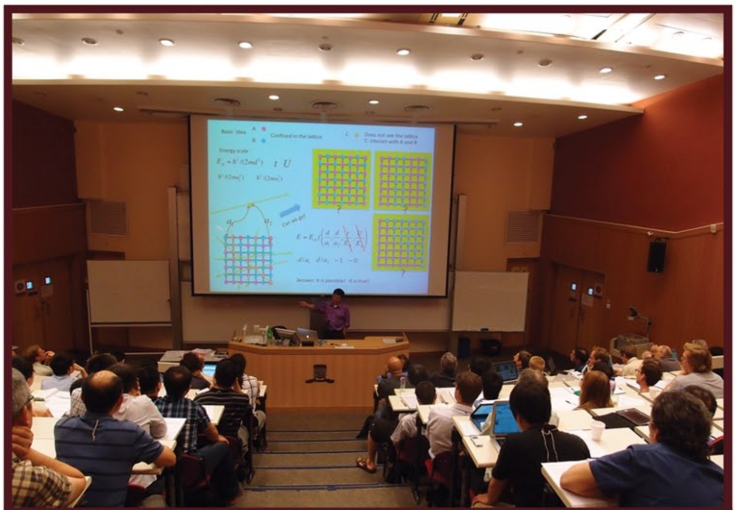
此外，亦有本系校友正在 CERN 參與的研究計劃，包括張承亮先生（*2001*年本科畢業，*2003*年碩士畢業）及鄭學全先生（*2012*年本科畢業），他們均是 ATLAS 團隊的成員，參與尋找希格斯粒子；蘇卓文先生（*2008*年本科畢業）則是 ALPHA 的成員，該組於*2011*年成功將反氫原子保存超過*16*分鐘，轟動科學界。

國際學術會議

香港中文大學和香港科技大學聯合主辦的「冷原子前沿和相關課題」國際學術會議已於2012年5月14日至17日舉行。

27位來自世界各地的傑出物理學家於會議中作了邀請報告，當中包括諾貝爾物理學獎得主Anthony J. Leggett教授。會中回顧了當今冷原子物理研究中最激動人心的問題，學者們亦就相關課題的未來進展彼此交換意見，分享經驗和知識。會議進一步促進了亞洲地區冷原子物理領域的新興研究組和世界其他地區的主要研究組之間的合作和交流。

會議的晚宴於5月15日舉行，同時為冷原子物理專家何天倫教授(本系校友、1972年本科畢業)60歲生日祝壽。



校友何天倫教授亦是嘉賓講者之一。

物理學公開講座

本系分別於2012年5月25日、6月21日及6月29日舉辦了三場物理學公開講座，每場都座無虛席。我們先由王福俊教授打頭陣，以新穎凡題目〈變形金剛不是夢〉來介紹現今世界熱門話題納米技術的發展。當中王教授還讓觀眾親身試玩一些簡單的實驗，令大家印象難忘。

於6月份，練立明博士及陳文豪博士則以上下集的形式講述天文物理學的超新星及黑暗物質，吸引一班天文愛好者出席，於講座後更圍著講者討論更多的天文學話題。

王福俊教授在〈變形金剛不是夢〉的講座中，加插了讓聽眾參與的互動實驗。

練立明博士於〈超新星〉講座完結後，仍然與很多天文愛好者在會場內討論其他天文話題。

陳文豪博士主講的〈黑暗物質〉，吸引很多天文愛好者，當天現場人數多得坐滿樓梯級及地上。

接上頁

楊振寧教授九十華誕慶祝活動

今年為香港中文大學博文講座教授兼理論物理研究所所長楊振寧教授的九十華誕，本系於9月15日舉行了科普講座及學術研討會，以表達對楊教授的敬意。當晚本系師生共同出席由校方舉行的祝壽晚宴，學生們獻上音樂表演及精心製作的祝賀短片，向楊教授送上祝賀。

該日的慶祝活動由中大物理系楊綱凱教授主講的科普講座揭開序幕，主題為「希斯玻色子與楊米理論」，吸引逾450名高中生及公眾人士聽講。楊振寧教授與美國物理學家米爾斯合作研究的「楊米理論」，是研究「希斯玻色子」之重要基礎。楊綱凱教授指出，沒有「楊米理論」，便沒有「希斯玻色子」。該日下午，物理系舉行學術研討會，由五位物理學學者 — 中大校友、美國俄亥俄州立大學物理系何天倫教授、中大物理系夏克青教授、朱明中教授、劉仁保教授及中國科學院院士葛墨林教授(以視像)分別從不同角度介紹楊振寧教授的學術成就，以及所衍生的最新科研發展，向楊教授致敬。

1999年，楊教授將他的論文、手稿、相片、影音資料及獎牌等慷慨捐贈予中大，促成「楊振寧學術資料館」的成立。資料館位於中大田家炳樓，收藏約22,000份珍貴資料，中大已將館藏內容數碼化，建立了「楊振寧學術資料在線系統」http://cnyangarchive.cuhk.edu.hk/，現已開放予公眾、學者及研究人員瀏覽。而本刊誠邀到楊振寧教授答應，於這期轉載他於International Journal of Modern Physics A(2012)刊登的<MY EXPERIENCE AS STUDENT AND RESEARCHER>[1] 一文，分享他的學習與研究經歷。

科普講座

學術研討會

祝壽晚宴

[1]C. N. Yang, "My Experience as Student and Research", reprinted with permission from International Journal of Modern Physics A 27, No. 9, (2012) 123009. Copyright ©World Scientific Publishing Company.

與粒子一起穿梭宇宙 一起探索物理

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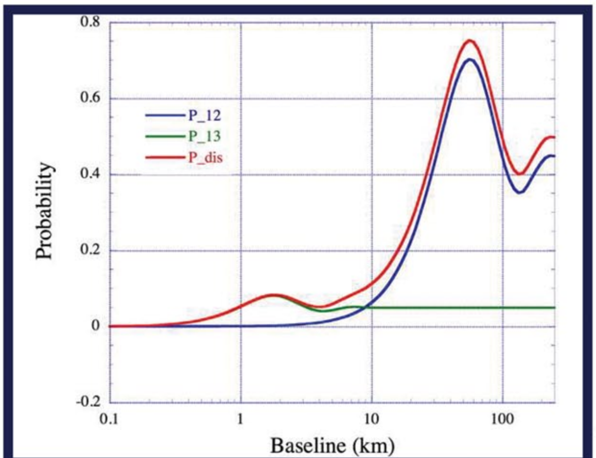
粒子，通過相互作用形成我們現在看到的世界。「相互作用」可以是指粒子之間的結合，形成微細的結構，以至萬物。透過相互作用，不同粒子便知道對方的存在了。然而粒子的種類很多，有些粒子個性較為「高傲」，與其他粒子只作很微弱的相互作用，人類要到很後期才發現到它們的存在。即便知道，也很難捕捉它們去了解它們的特性，在這裡我們將會探討其中一種「高傲」的粒子－「中微子」。

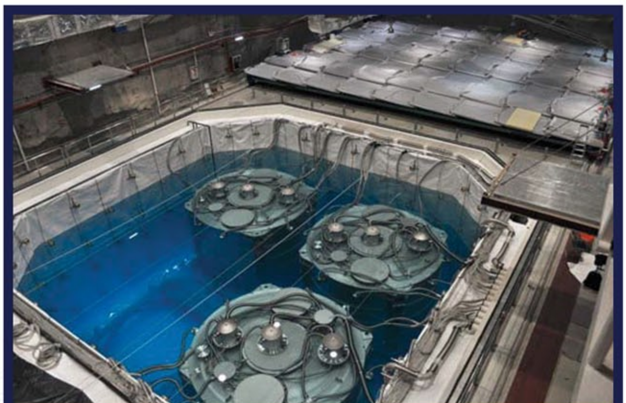
中微子，分別有三種稱為「味」(Flavour)的狀態：e、μ及τ中微子。其反粒子亦相應的有三種形態（$\bar{e}$、$\bar{\mu}$及$\bar{\tau}$），所以我們平常說中微子共有六種「味」，並可以通過不同的核子反應產生出來，但因為這類粒子的質量非常接近零，通常遠低於其能量，於是便以接近光的速度向四方八面飛散。太陽系中也有一個巨型的核融合爐，就是太陽。它會產生很多e中微子。在二十世紀六十年代，美國有一項名為「Homestake」的實驗，製造出e中微子的探測器，並探測出從太陽飛來的e中微子數量比計算得出的少了三分之二。之後在八十年代的日本實驗「神岡」亦得到出差不多的結果，究竟那三分之二去了哪？中微子幾乎與其他物質沒有相互作用，即是它具有極高的穿透力，所以消失的三分之二絕不能以「被阻隔」來解釋，這個謎案就是當時很著名的「太陽中微子問題」。（按：二零零二年的諾貝爾物理學獎便是頒發給Homestake實驗及神岡實驗的負責人）

物理學家為此提出了一個基於量子力學上的理論：「中微子振盪」(Neutrino Oscillation)理論。在這個理論的框架下，中微子自產生後自由傳播時便不斷在三個「味」中互相轉換，例如當我們在一米的距離觀察它時，它以e中微子的狀態出現，走到三米的距離再觀察它時，它卻以μ中微子呈現眼前，然而可能它一出生的時候，人們看到它卻是處於τ狀態的。這是量子世界中的一個很有趣現象，粒子的狀態絕不單一，而且隨時地更替著，引用理論的述語，我們形容中微子在e、μ及τ三種狀態間「振盪」著，為了定量描述不同「味」之間的振盪發生機會率，我們需要定義最少三個參數，在中微子振盪理論中，我們稱它們為「混合角」(mixing angles)，分別是θ_{12}、θ_{23}及θ_{13}。

「混合」這個詞具有「混和」、「融合」的意思，而這種意思在理解這三個角時更別具意義，當我們運算中微子由一種「味」轉化成另一種「味」的發生率時，其實必須要同時知道三個混合角的值。為此，科學家們在這幾十年間致力進行實驗並嘗試找出它們的大小，前兩者的數值均較大，因而較容易被找出，相反最後的θ_{13}卻相當小，令科學家一直很難得到充份的數據來總結這個參數的大小值，科學家亦只能為這個數值設置一個上限值。若θ_{13}等於它的下限值一零，便導致有一種振盪的發生機會率也等於零，即該種振盪方式不存在，這種不確定性成為了找出整幅中微子圖畫中的一個缺口。



圖一:綠線及藍線分別描述電子類中微子在不同距離時「振盪」至其餘兩類中微子的機會率，紅色線是兩線的總和。這裡的距離是指中微子由被產生（即位於發電機組）至其被探測時的位置的長度，由此我們可以判斷怎樣揀選「振盪」發生機會率最高的地方來放置探測器。

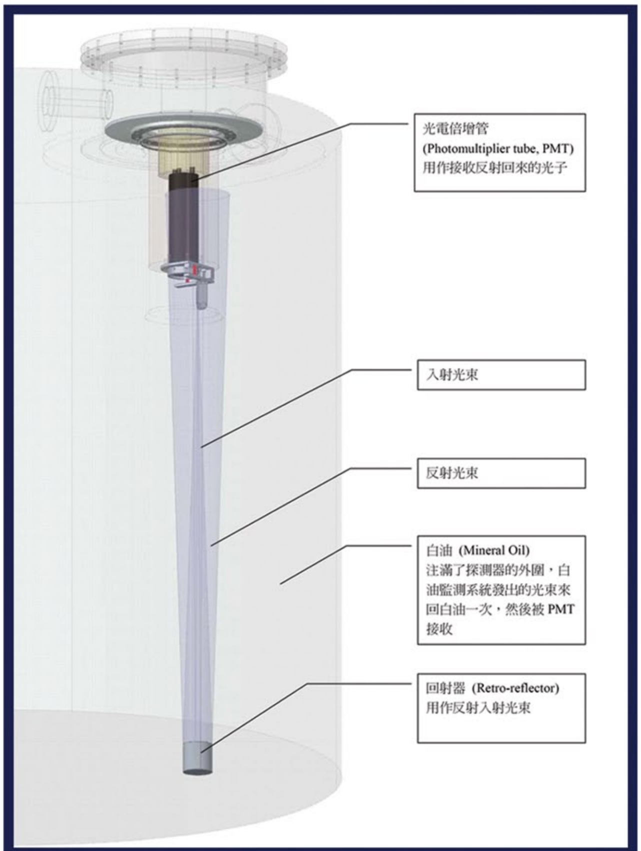


圖二:在首期實驗中，三個巨形探驗器被放置在位於大「振盪」機率的實驗廳內，最後更會增至四個來采集更多的中微子數據。

接上頁

從以前的實驗中我們知道這種振盪發生的機率很低，再加上中微子很難被捕獲到，於是吸引來自世界各地的物理學家共同創立一個大型的粒子物理實驗「大亞灣中微子振盪實驗」，並選址於距離香港約五十公里的大亞灣核電站。由中文大學物理系朱明中教授領導的研究小組一直參與這項目中，由於核電站在發電過程中會產生大量的e中微子，因此那裡成為了我們觀察中微子振盪的極佳場所。而且核電機組旁邊的山脈群，讓我們可以把精密的中微子探測儀器收藏其內，以山體把影響到實驗數據的宇宙線輻射隔走。以中微子振盪理論為基礎，再參考從其他實驗而來的數據，我們推定出一個與發電機組的距離，在那裡e中微子因為「振盪」而減低的幅度應該相對大（圖一）。於是我們在那個距離放置了四個巨型的探測器，並在接近發電機組的位置額外設置兩個探測器點，然後便可以通過比較三個點所收集到的中微子數目，來計算出θ_{13}。整個過程中，掌握及維持探測器的穩定性非常重要，因為小小的誤差及污染均會令到這個需要高精確度的實驗結果出現嚴重的錯誤，所以我們的研究小組主要負責兩個系統 – 白油監察系統及探測器的氮氣系統。

中微子探測器內最外層注滿白油。當e中微子穿過探測器時有機會引發連串的放光反應，這些光需先通過白油才能被感光儀器（光電倍增管，Photomultiplier Tube，或PMT）所吸收並形成訊號。由於光通過任何液體都會有一定程度的衰減（attenuation），因此白油若然變質的話，便會令到我們偵測到的中微子訊號產生變化。為此我們的小組在每個探測器上均安置了兩種發光源及一枝PMT，並在探測器的底部裝置了一個特殊的反光鏡(回射器，Retro-reflector)，讓我們可以長期監察已知光源放出的光束通過白油後，折返的光量有沒有變化，並根據結果考慮是否需要對探測器的數據進行修正。而氮氣系統則是由我們的小組與美國威斯康辛大學麥迪遜分校(University of Wisconsin–Madison)共同發展的，是為了無間斷向探測器頂部的空間灌入氮氣，從而將有機會滲進採測器的水氣、氧氣、具放射性的氡氣等雜質的濃度降低，避免它們污染探測器內的液體及降低探測器對中微子的敏感度，於是確保一整套氣密而具監測作用的氮氣系統便成為了我們的另一項挑戰了。



圖三:白油監察系統裝置圖。光源需要穿過白油，然後被反射鏡反射，沿路再穿過白油一次才回到感光的光電倍增管。反射鏡令光在白油之中行進的路程加倍，因而令白油對光的衰減效果也大大增加，變相提升監察系統的敏感度。

這個實驗在二零一一年十二月正式進入首期運作，在連續取數達三個月後我們根據數據得到了一個準確度非常高的$\sin^2(2\theta_{13})$的值，並在二零一二年的三月八日正式公佈，其大小為0.092±0.017。其高準確度令我們排除了θ_{13}為零的可能性，亦說明了我們證明了這種振盪模式的存在。在公佈結果後，實驗的第二期安裝及測試正進行得如火如荼，希望盡快重啟並使用其他分析方法試驗首期得到的結果，並進一步提高其準確度。

對基本粒子的研究同時可以追溯至更基本更有趣的物理問題，這令我們更鍥而不捨地追尋這些來去無踪的粒子。其中一個問題是為何現今我們看到的世界是由「正」物質來主宰的呢？假如宇宙初開時正、反物質數量相等而且平均分佈，則正、反物質最終會因為互相煙滅，無法不斷凝聚成為恒星及太陽系。為何物理現象會「偏心」於「正」物質的呢？對中微子的研究有望讓我們得到答案。在中微子振盪理論中出現了一個參數，稱為CP破壞因子（CP Violation factor），意思是若這個數值不等於零，那便有可能用來解釋正、反物質的對稱性為何會遭到破壞，而這個因子在θ13等於零的時候亦等於零，因此得到一個準確的θ13數值，除了增加了我們對粒子世界的知識外，還為我們打開了另一道解釋正反物質不對稱的大門。

人事動態

這兩年本系招攬了很多新老師，在這裡小編會為大家一一介紹。

陳文豪博士/中大物理系講師

我於2011年9月開始重返物理系的大家庭，是以教師身份回歸母系。我2002年在中大物理本科畢業後，留在系裡深造並完成碩士及博士課程，師承朱明中教授。我的研究範疇主要是天文物理，除了物理外，我也在中大修畢基督教研究文學碩士和教育文憑，故此我也喜歡研究教育理論和宗教科學對談。

閒時喜愛閱讀、打籃球和踢足球，也喜歡彈琴、彈結他、打鼓和唱歌。我喜歡跟學生分享生活軼事，歡迎各位同學閒時到來我的辦公室（科學館北座104室）暢談。

陳濤教授/中大物理系研究助理教授

2010畢業於新加坡南洋理工大學博士學位。在進行為期一年的博士後研究工作之後，陳濤博士於2011年10月加入香港中文大學物理系，任職研究助理教授。他一直致力於材料科學的研究，包括金屬，半導體和聚合物的合成，性質與應用，以及納米材料的自組裝。他近期的研究主要集中基於納米材料的光電轉換器件（太陽能電池）的開發和利用。

梁寶建博士/中大物理系講師

我於2000年在中大物理系本科畢業後，在2002年完成碩士課程。及後便遠赴美國伊利諾大學香檳分校深造，於2010獲得博士學位。沒想不到2012年8月會回來這裡教物理學。

現在要從新適應香港的生活，也要適應新的人際關係：從前的老師成了今天的同事，而現在的學生算來亦是我的師弟妹。盼望能在物理系的運作和發展方面付出一分力，也能在各位老師和同學身上學習更多。

路新慧教授/中大物理系研究助理教授

2004年獲得南京大學物理學士學位後，赴美國耶魯大學攻讀物理博士，於2010年獲得博士學位。隨後在美國布魯克海文國家實驗室從事兩年博士後研究工作，2012年6月加入中大物理系。研究方向涉及軟物質結構與動力學，新能源以及納米科學等。現在主要研究有機及無機薄膜太陽能電池，利用各種同步輻射技術研究物質結構與動態。

平時喜愛打球，登山等各項體育運動。

王一教授/中大物理系助理教授

大家好，我是今年新加入物理系的助理教授。我2003年畢業於浙江大學，其後在美國伊利諾大學香檳分校繼續深造，並於2008年獲得博士學位。主要的研究興趣是生物物理當中的分子動力類比—這是一個相對較新的方向，結合統計物理的定律和計算的手段模擬生物大分子的運動軌跡，從而解答相關的生物問題。目前我們的課題包括研究抗菌多肽的選擇性，構建結核桿菌細胞壁模型，和開發快速計算小分子活性的軟體。歡迎對理論生物物理有興趣的本科和研究生同學加入我們的研究，或者來同我討論你感興趣的生物物理課題。

接上頁

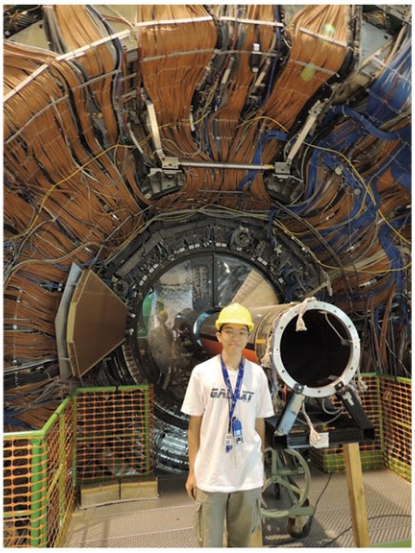
吳藝林教授/中大物理系助理教授

我於2004年本科畢業於中國科學技術大學，之後2009年獲得美國聖母大學博士學位。隨後便到美國哈佛大學作博士後研究工作至今年有幸成為香港中文大學物理系的一員。去年底新年前夕，曾到訪中文大學，感受到同事之間的熱情友善，看到同學們求知若渴，即喜歡上了這裏。記得那天中午途經文物館，有社團揮寫春聯，許多同學排隊領取，我心裏湧起一陣感動，一種價值觀和文化的共鳴，讓我堅信在這所大學教學相長將是一件美事。

我的研究興趣是生物物理，在我看來生命是宇宙間最為奇妙的物質形態。現時研究的問題是細菌的集體運動和對環境的感知。

實習及交流天地

2011-12年度本系共有13位同學獲選參加物理系和理學院的交換生計劃、及7位同學參與了暑期本科生研究交流計劃（SURE），小編今期先邀請了兩位本科生李浚彥及譚振南分享他們在CERN的經歷及體會。



歐洲核子研究組織的地下研究設施

剛過去的暑期，我透過SURE到了蘇黎世歐洲核子研究組織，參與了他們的研究。這次的經驗非常豐富及寶貴，讓我體會到研究人員的日常生活，以及他們是如何互相發揮所長去配合工作 - 如果找出不同方面的專家，互相合作才能成事。

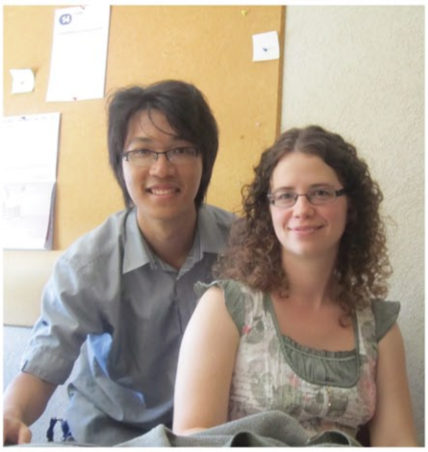
在他們發表關於發現"新粒子"時，我幸運地剛好也身在其中，與他們一起分享這份快樂。

譚振南

我在歐洲核子研究組織與一班大約六人的研究團隊合作，當中包括帶領的教授、訪問學人、博士後研究者及博士生。在這8星期間，最深刻體會是學生與教授以及研究人員的緊密關係，比起中大物理更強。在午飯時間，我們常會討論研究項目，還會分享彼此的生活趣事。這些交流，讓我知道一位研究人員除了研究，還有生活。

除非工作緊急，不然他們也盡量不加班，週末更會安排很多戶外活動或派對。他們的觀念是"盡情工作，享受生活"，以最佳的心理質素投入工作。

李浚彥



這位是帶領我這研究團隊的教授

國際學術會議

由裘槎基金會、中大理論物理研究所和崇基學院贊助，物理系主辦的Rayleigh-Bénard湍流國際學術會議（International Conference on Rayleigh-Bénard Turbulence），將於2012年12月10日至14日於香港中文大學舉行。Rayleigh-Bénard湍流熱對流是當今最熱門的課題之一，可應用於天文物理學和地球物理學，以及工程學中的核反應堆的熱傳遞。會議為從事湍流研究的學者提供平台，共同探討這個課題當前的最新進展；超過60位來自世界各地的科學家將會於會議中發表和交流他們的研究工作。我們期望透過此會議進一步促進湍流這個領域的發展，並加強學者之間的相互合作交流。

會議詳情請瀏覽網址：
http://www.phy.cuhk.edu.hk/events/rb-conf/。

物理學公開講座

另有：
高中物理精研講座系列 – 天文篇
日期：2012年11月23日
時間：下午4:45-6:15
地點：康本國際學術園LT3
*詳情請瀏覽本系網頁

中大物理校友會

物 Café

今年的"中大校友會"將於2012年12月2日舉行。

一如以往，物理系校友會將會為物理系及材料科學的校友、學生及教授於當日下午預備茶點小食，藉此讓大家於校友日有一個聚腳點，回到物理系大家庭相聚。

日期：2012年12月2(日)
時間：16:30-18:00
地點：科學館北座一樓126室 楊振寧閱覽室
費用：全免
聯絡人：許棉斯 Fiona (fionahui@cupaa.org)

每年都會有 40 多名不同屆別的同學和校友以及教授出席，

希望今年可以見到你！

本會網頁：http://www.cupaa.org

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MY EXPERIENCE AS STUDENT AND RESEARCHER

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I was a student in Chongde (崇德) Middle School for four years 1933–1937, from Grade 7 to Grade 10. The school had about 300 students. It had a small library in which I developed the habit of browsing around. It was in that library that I had a first glimpse of modern physics through reading a Chinese translation of James Jeans' *The Mysterious Universe* (Fig. 1).

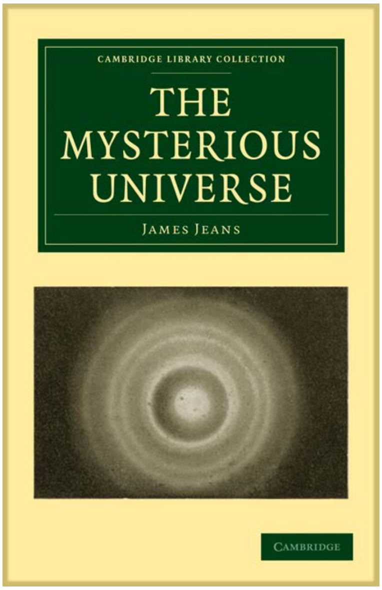


Fig. 1. Cover of the book *The Mysterious Universe* (Cambridge University Press).

In the book Jeans used daily language to describe the development of special relativity in 1905, of general relativity in 1915 and quantum mechanics in 1925. I was fascinated.

In 1937 the war against Japanese invasion broke out. My family had to move through a long and difficult journey, arriving finally in the Spring of 1938 at Kunming (昆明). I still vividly remember how the train in which we were riding between Wuhan (武汉) and Guangzhou (广州) was bombed midway in Hunan Province (湖南). In Kunming I skipped Grade 12 and took the entrance examination to the Southwest Associated University (Lianda 联大).

I had not studied high school physics, so to prepare for the entrance examination I borrowed a copy of a standard high school physics textbook and read it through in several weeks. At the end of this reading I concluded that physics was the subject that I liked. Thus when I registered at Lianda I enrolled in the Department of Physics.

I still remember, that textbook stated that the acceleration in a uniform circular motion is centripetal and not along the tangential direction. I felt at first that this contradicted my intuition. But after thinking about it for a couple of days I finally realized that *velocity is a vector*. It has not only magnitude but also direction. This story taught me an important lesson: We all have intuitions. Most of them are correct. But some need revision. Thus

On the one hand our intuitions are extremely important. But on the other hand one must constantly absorb new concepts to revise one's intuitions. (1)

In the Spring of 1942, to write a B.Sc. thesis required by the University, I went to see Professor T. Y. Wu (吴大猷) (Fig. 2), asking him to be my advisor. Forty years later I thus described how Professor Wu gave me guidance.[1]

Fig. 2. Professor T. Y. Wu (1907–2000) and I (taken in Stony Brook in 1982).

> He gave me a copy of an article by J. E. Rosenthal and G. M. Murphy in the 1936 volume of *Reviews of Modern Physics*. It was a review paper on group theory and molecular spectra. I was thus introduced to group theory in physics. In retrospect I am deeply grateful to Wu for this introduction, since it had a profound effect on my subsequent development as a physicist.

Group theory's applications in physics later became my principal area of work, comprising approximately two thirds of my research efforts.

In the fall of 1942 I enrolled as a graduate student in the Physics Department of Lianda. My M.Sc. thesis advisor was Professor J. S. Wang (王竹溪) (Fig. 3). His specialty was statistical mechanics and he guided me into this area of research. Approximately one third of my later research work was in this area.

Fig. 3. Professor J. S. Wang (1911–1983) (taken by H. T. Nieh in 1980).

Many years later I made the following description of my graduate days in Lianda:[2]

> During the academic year 1941–42 I was a senior in the Physics Department at the National Southwest Associated University in Kunming. The Department was quite small, with about ten faculty members, ten instructors, a few graduate students and not more than 20 students in each undergraduate class. When the academic year started in the fall of 1941, a new face appeared, auditing many of the senior and graduate courses and participating in all discussions. That was Huang Kun. He had already received his bachelor's degree in physics from Yenching University in Beiping, and had come to Kunming to join the Southwest Associated University as an instructor. Soon we got to know each other well, and that was the beginning of half of a century of warm friendship.

Fig. 4. Huang Kun (黄昆), Sheldon Chang (張守廉), and I (taken at the birthday party for celebrating Zhou Pei-Yuan's birthday at Peking University on June 1st, 1992).

Two of the courses we took that year were Professor Ta-You Wu's (吴大猷) classical mechanics and quantum mechanics. Quantum mechanics was a revolutionary development in physics which had started in Germany, Switzerland, England and Denmark in the years 1925 to 1927. Professor Wu was the physicist in China who trained during the 1930's and early 1940's the greatest number of students in this important new fundamental branch of physics. I remember many discussions after Professor Wu's lectures. It was through these discussions that I first got to know Huang Kun well, as a physicist and as a person.

A year later, in the summer of 1942, Huang Kun and I both enrolled as graduate students at the Southwest Associated University. He worked on his thesis with Professor Wu on atomic and molecular problems in astrophysics, while I worked on mine with Professor Wang Zhu-Xi (J. S. Wang) (王竹溪) on statistics mechanics. The graduate students' stipend was meager and we all looked for teaching positions to supplement our incomes. My father was a friend of President Xu Ji-Zu (徐继祖) of Kun Hua Middle School (昆华中学) who arranged to have Huang Kun, me and Zhang Shou-Lien (张守廉 Sheldon Chang), a fellow physics graduate student, split a teaching position at the School. As part of the package deal, the School gave us a corner room in one of their new buildings for the three of us to live in.

The School was situated approximately 3 km from the campus of the Southwest Associated University. Huang, Chang and I would spend our day on the University campus, taking our meals in the mess hall on campus, returning to our room at the School in the evening to sleep. There was no facility for producing potable water on the University campus, so we developed the habit of spending an hour or two everyday after supper, drinking tea in one of the tea houses clustered along the three streets near the campus before returning to the School. During these hours and hours of tea drinking, we really got to know each other well. We discussed and argued about everything under the sun: from ancient history to contemporary politics, from large cultural patterns in the world to small details in some movies we had recently seen. In all of these, I remember Huang Kun as a fair debator, not given to tricking his opponents. I also remember that he had a tendency to push his arguments to the extreme. Many years later when I reflected on this, I found it interesting how this tendency of his had seemed to be totally absent in his physics.

Our fellow tea drinkers formed a colorful and curious group. There were many students like us. But the majority of the tea drinkers were towns folks, horse carriage drivers, traders from distant counties and the like. Everyone was noisy. We especially. Oftentimes during a heated debate we would suddenly become aware that we were by far the noisiest and that everyone was watching us. (That awareness may or may not terminate our argument.) However, there was rarely any animosity between us and the nonstudents in the tea houses.

We experienced, during these hours of tea drinking, scenes and events that I was never to forget. Several times, sitting in the tea houses along Fung Du Street (凤翥街) which led northward away from the city into small hills dotted with simple tombs, we saw processions of soldiers marching in the northward direction with one or several prisoners in their midst. Each prisoner had a white picket-like cardboard tied to his back with his name and crime written on it. Most of the prisoners marched in silence. A few would yell something which sounded like: "Twenty years later, again a brave fellow!" Every time such a procession passed, the noise level in the tea house would drop perceptibly. Then the expected distant gun shots, and we would sit silently, waiting to watch the soldiers marching back.

Against such background happenings, we argued endlessly about physics. I remember once our topic of discussion was about the meaning of measurements according to the Copenhagen interpretation of quantum mechanics. That is a very subtle subject, and our arguments started at tea, lasted throughout the evening, and continued back in our room at the Kun Hua Middle School. After the electric lights were turned off and we were all in bed, the arguments did not stop.

It is no longer clear to me today what precise point we were arguing about that evening, nor who among us took which side. But I vividly remember that all three of us eventually got up from our beds, lit candles and examined in detail a few paragraphs of Heisenberg's *The Physical Principle of the Quantum Theory* to settle our argument.

Huang was an avid reader of English novels. It was he who introduced me to Joseph Conrad, Rudyard Kipling, John Galsworthy and other authors. Many of the novels of these authors we borrowed from the University libraries. Others we bought from the stalls that sold K-rations, army boots, cans of cheese as well as used pocket books with which the U.S. military personnel flooded the flea markets of Kunming.

It was a simple life we led in those years, as I have described elsewhere. A dish of peanuts to go with the tea was a great treat, which we could not indulge in very often. It was not an austere life, for we did not expect and did not desire more material things. It was also not a frustrated life, for we found plenty of intellectual stimulation and satisfaction. It was just a very simple life which, without our realizing it at the time, had shaped our tastes and styles in physics in ways that were to have profound effects on our later careers.

My discussions/arguments with Huang Kun and Sheldon Chang and my later experience as a professor of physics taught me:

Discussions with classmates offer opportunities for deep understanding. (2)

Sheldon Chang later moved to Electrical Engineering and Control Theory, obtaining his Ph.D. in the U.S. He is now a Professor Emeritus of Stony Brook University. Huang Kun (1919–2005) later received his Ph.D. degree in Great Britain, specializing in solid state physics in which he made very important contributions. He was also the father of semiconductor research in China. In 2001 he received the highest Science Award of China.

A few weeks after the end of World War II in 1945 I flew on a DC3 to Calcutta, where I had to wait for a berth on ships bound for the U.S. for several months, finally arriving in New York on November 24th after a long voyage through the Red Sea, Suez Canal and the Mediterranean. In January 1946 I enrolled as a graduate student in the Physics Department of the University of Chicago. My aim was to write an experimental Ph.D. thesis with Professor Enrico Fermi (1901–1954, Fig. 5).

In 2001 there were celebrations in Chicago and in Rome for Fermi's 100th birthday. In the paper I read at these celebrations was the following passage:[3]

> Enrico Fermi was, of all the great physicists of the 20th century, among the most respected and admired. He was respected and admired because of his contributions to both theoretical and experimental physics, because of his leadership in discovering for mankind a powerful new source of energy, and above all, because of his personal character. He was always reliable and trustworthy. He had both of his feet on the ground all the time. He had great strength, but never threw his weight around. He did not play to the gallery. He did not practise one-up-manship. He exemplified, I always believe, the perfect Confucian gentleman.

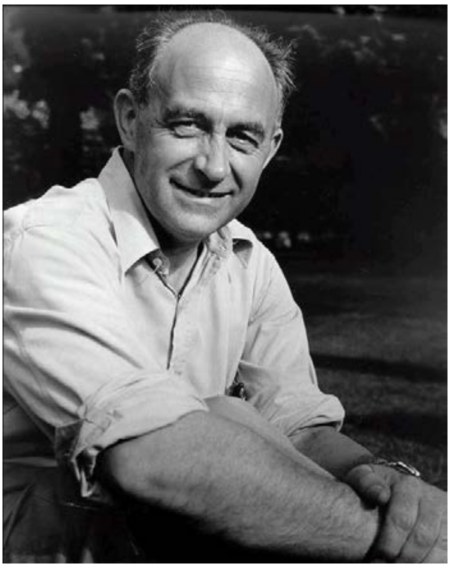


Fig. 5. Enrico Fermi (taken in 1940's).

Fig. 6. Edward Teller and I (taken in 1982).

At the time the Physics Department of the University of Chicago was ranked No. 1 or 2 in the world. Besides Fermi, another important theoretical professor was Edward Teller (1908–2003, Fig. 6). He was a brilliant scientist who had made important contributions in physics and in chemistry. Later in the early 1950's he discovered the method of constructing a hydrogen bomb and became world famous.

My plan to write an experimental thesis with Fermi did not materialize because his laboratory at the time was in the Argonne National Laboratory which was not open to me. So Fermi recommended that I first work with Teller on theory.

During the first half of 1946 I was Teller's graduate student. The first research problem he gave me was about the lifetimes of K-capture in Be and in BeO. He proposed that I use the Thomas–Fermi–Dirac and the Wigner–Seitz approximations to do the calculation. Several weeks later I showed him my results and he happily arranged for me to give a seminar. That was my first seminar report in America. In the small audience of less than twenty people there were several very important scientists: Fermi, Urey, the Mayers, etc. My report received universal approval, and Teller asked me to write it up for publication. I then spent approximately a whole week to do this but without success, since I could not adequately estimate the reliability of my results which involved several different kinds of approximations. Fortunately Teller was not offended and he gave me another problem on nuclear physics.

Teller had six or seven graduate students at that time. We met to discuss physics once or twice a week, oftentimes also to have lunch with him. Teller had lots of new ideas concerning nuclear physics, solid state physics, cosmic rays, etc. Gradually I found his style of research was different from my own. Thus while I continued to participate in his discussion group, I began to look for theoretical problems myself.

In the fall of 1946 Fermi recommended me to work in the laboratory of Professor Allison. Allison was an experimental nuclear physicist. At the time he was constructing a 400 keV Cockroft–Walton accelerator. He had five or six graduate students and accepted me as an additional one. In the meantime I continued to participate in Teller's discussion group.

At the time I was one of the most prominent graduate students in the Physics Department because what I had learned in Lianda of fundamental theoretical physics had already reached the frontiers of the field at the time. But I was very clumsy in the laboratory. Laboratory mates respected my theoretical knowledge, oftentimes asking me to help them to solve theoretical problems. But they laughed at my blunders in the laboratory. "Where there is a Bang, there is Yang!"

1947 was a year of unhappiness for me. In a letter I wrote to Huang Kun who was then a graduate student in England, I had used the word "disillusioned" to describe my feelings at the time. Why? Because although I was trying hard but I did not have the talents to do experiments. On the other hand in theoretical physics all the topics that I had concentrated on myself were leading to pure frustration.

It is very common for a graduate student to feel discouraged in looking for a good problem for his/her thesis work. (3)

The theoretical problems that I worked on myself in the year 1947 included the following items:

(1) Onsager's 1944 paper on Ising Model.
(2) Bethe's 1931 paper on Spin Waves.
(3) Pauli's 1941 review paper on Field Theory.
(4) Many papers after 1943 on angular distributions in nuclear reactions.

Of these four topics the first two were in statistical mechanics. I was interested in them because of the influence of Professor J. S. Wang. The other two problems were related to the concept of symmetry. I was interested in them because of the influence of Professor T. Y. Wu.

In the Physics Department in the University of Chicago at that time nobody was interested in the first three of these problems. I worked on them by myself in the department library, trying to understand the papers on these problems, and hoping to make further advances. The end result of several weeks of hard work on each of these three topics was total frustration.

Fortunately Teller was very much interested in the fourth problem. At that time there were many theoretical papers on this topic. I found all of them were intuitive and lacked rigour. So I spent several weeks applying the concept of "Invariance of

Physical Laws under Space Rotation" to substantiate the intuitive ideas floating in the literature on the subject. The effort was successful and I wrote a short paper which Teller very much liked. At that time everybody in the department knew that Frank Yang's work in Allison's laboratory was unsuccessful. One day in the spring of 1948 Teller came to Allison's laboratory[4] and suggested that I *abandon* my plan to write an experimental thesis. He would sponsor my short paper as my Ph.D. thesis after I had extended it to include relativistic cases. I was at first disheartened by Teller's proposal, but after a few days recovered and accepted his suggestion with relief.

Altogether I spent about 18 to 20 months in Allison's laboratory. Was that a complete waste of time? No, absolutely not! Through that experience I learned that the value judgement of an experimental physicist was very different from that of a theoretical physicist. This understantding influnenced many of my later research work, including the 1956 work on the possibility of parity nonconservation and my 1964 paper with T. T. Wu on phenomenological analysis of CP invariance.[5]

My Ph.D. thesis was the first paper I published concerning invariance and non-invariance of physical laws. Soon after I published a second paper in this field, about the spin of π^0 meson through a careful analysis of the group theoretical representation of invariance in field theory. These two papers established me as one of the foremost theorist on the use of group theory in analysing symmetry properties. It was then that this field was beginning and I was very fortunate to have gotten into it at its infancy.

It is best to enter a research area when it is new and developing. (4)

After I got my Ph.D. degree in the summer of 1948, the University of Chicago appointed me as an instructor. At that time the hottest theoretical area of research was renormalization theory. The three theory professors in Chicago: Fermi, Teller and Wentzel were not working in this field. Therefore after one year 1948–1949 I moved to the Institute for Advanced Study (IAS) in Princeton. Before leaving Chicago Fermi told me it was good to spend some time at the IAS, but the work there was too academic. It was a bit like a medieval cloister, he said, and was not a place for long stay. So he suggested that I go to the IAS for one year and then return to Chicago. I was of course totally in agreement with this advice of his. But[6] because of the convenience of dating Miss Tu (later my wife) in New York City, I did not return to Chicago after one year and instead remained at the IAS for altogether seventeen years, 1949–1966.

During these seventeen years the four topics that I had worked on by myself in Chicago, described above, all became successful areas of research for me. The breakthrough for the first item, Onsager's work on the Ising Model, had come about accidentally:[7]

> One day in early November, 1949, in a ride in the station wagon that the Institute ran from Palmer Square opposite Princeton Univeristy to the

> Institute, J. M. Luttinger and I happened to talk about the Ising model. Luttinger said that Bruria Kaufman had simplified Onsager's method so that the solution could be understood in terms of the representation of a system of $2n$ anticommuting Hermitian matrices. I knew such representations well and understood quite readily the main points of the Onsager–Kaufman method. After arriving at the Institute, I worked out the essential steps of this approach and was very happy at finally understanding Onsager's solution . . .
>
> I kept thinking about it, and realized that Onsager and Kaufman had obtained much more information than just the partition function , . . .
>
> I was thus led to a long calculation, the longest in my career. Full of local, tactical tricks, the calculation proceeded by twists and turns. There were many obstrctions. But always, after a few days, a new trick was somehow found that pointed to a new path. The trouble was that I soon felt I was in a maze and was not sure whether in fact, after so many turns, I was anywhere nearer the goal than when I began. This kind of strategic overview was very depressing, and several times I almost gave up. But each time something drew me back, usually a new tactical trick that brightened the scene, even though only locally.
>
> Finally, after about six months of work off and on, all the pieces suddenly fitted together, producing miraculous cancellation, and I was staring at the amazingly simple final result.

Why was I able to "understood quite readily the main points of the Onsager–Kaufman method"? One: I had *thoroughly* studied the representation of $2n$ anticommuting Hermitian matrices when I studied Dirac's equation in Kunming. Two: I had spent several weeks in 1947 in Chicago trying to understand Onsager's 1944 paper, without success. But that experience had *thoroughly familiarized me* with Onsager's paper. Combining these two earlier efforts I was able to rapidly understand what Luttinger had said about the real strategy of Onsager's solution.

What I had learned from Professor J. S. Wang in Kunming made me deeply *interested* in statistical mechanics, leading to my *preparatory efforts* in Chicago on Onsager's work. The final *breakthrough* came from a short conversation with Luttinger. This process:

$$\textbf{Interest} \rightarrow \textbf{Preparation} \rightarrow \textbf{Breakthrough,} \tag{5}$$

I think, is the necessary three-step process for any research work. In the experience described above the breakthrough came from an external stimulation (conversation with Luttinger). But in most cases the breakthrough is a sudden *inspiration* without external stimulation: After long preparation, when one is not thinking about the problem, apparently new conceptual combinations were still being formed subconsciously in one's brain. When the right combination occurs a sudden *inspiration* would result. A century earlier Poincaré[8] had given a very interesting analysis of this process.

The third problem I had unsuccessfully worked on in Chicago was about the concept of gauge invariance in electromagnetism in Pauli's 1941 review paper. Gauge invariance was invented by Hermann Weyl (1885–1955, Fig. 7) in the years 1918–1929. I was deeply interested in this beautiful concept and wanted in 1947 to generalize it. (Why had my fellow graduate students at the time not launched into similar efforts? I think it is because I was especially interested in group theory and in the concept of invariance in physics, subjects in which most of my fellow graduate students at the time were not interested.)

Fig. 7. Hermann Weyl (1885–1955).

I started with an important formula in electromagnetism

$$F_{\mu\nu} = A_{\mu,\nu} - A_{\nu,\mu} \tag{A}$$

and generalized it to

$$F_{\mu\nu} = B_{\mu,\nu} - B_{\nu,\mu} \,, \tag{B}$$

in which B_μ is a 2×2 square matrix, unlike A_μ, which is a simple 1×1 matrix. This very natural generalization however led to increasingly complicated calculations. Thus I had to give up. That was in 1947. My aim was to use this generalized gauge invariance to construct the mutual interactions between the many particles, Λ, K, etc., which were newly discovered at the time. Figure 8 are reproductions from three pages of my notes of 1947.

More and more new particles were discovered in the succeeding few years. So several times I returned to this attempt at generalizing gauge invariance to formulate interactions between them. But each time the formulae became *increasingly complex*, increasingly ugly, and I had to give up. Finally in 1953 to 1954 I was

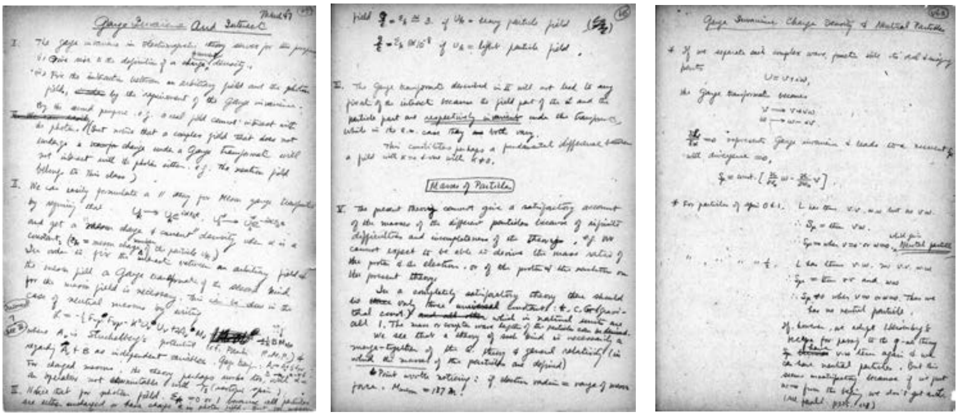


Fig. 8. Reproductions from three pages of the notes of 1947.

visiting the Brookhaven National Laboratory (BNL) for one year. I had two young officemates at BNL that year. One was Robert Mills (1927–1999, Fig. 9) who was a student of Norman Kroll (1922–2004) and was just about to get his Ph.D. degree. Another one was an experimental physics graduate student Burton Richter (1931–) who later shared the Nobel Prize in physics in 1976 with Samuel Ting.

Fig. 9. Robert Mills and I (taken in Stony Brook on May 22nd, 1999).

Naturally in my discussions with Mills I mentioned my unsuccessful attempts to generalize gauge invariance. One day somehow we said although formula (B) was quite natural but maybe it should be amended to become

$$F_{\mu\nu} = B_{\mu,\nu} - B_{\nu,\mu} + (B_\mu \text{ and } B_\nu \text{ polynomial})\,, \tag{C}$$

in order to possibly cancel out the "increasingly complex" terms. We decided to first try a quadratic polynomial. If that did not work we would try a cubic one, etc., etc. Fortunately we rapidly found that if we replaced (B) with

$$F_{\mu\nu} = B_{\mu,\nu} - B_{\nu,\mu} + B_\mu B_\nu - B_\nu B_\mu \,, \tag{D}$$

the subsequent calculation became *increasingly simple*. Thus we knew we had uncovered a great treasure!!! [We did not know then that from a geometrical viewpoint it was *natural* to include the quadratic terms in (D).]

With this breakthrough we followed the theory of Maxwell (1831–1879) and quickly wrote down a set of equations for generalized gauge theory. It was truly beautiful. But it had a problem: It seemed to imply that there should be massless charged particles which had never been seen. Besides it was difficult to understand how a charged particle could be massless. This problem caused us several months of complicated and fruitless research. There was also a famous episode of unpleasant criticism from Pauli.[9] Finally we decided although we did not have a good solution to this problem, the whole idea was too beautiful not to be published. Our paper was sent to the *Physical Review* in June, 1954. Fortunately it was immediately accepted and was published in October.

This paper is my most important contribution to physics. It led to the principle that *symmetry dictates interactions* which in a sense is one part of Einstein's *geometrization of physics.*

The paper did not answer the massless particle question mentioned above, but our decision to publish, in retrospect, was absolutely correct. I learned from this:

It is often not possible to solve at once all aspects of a difficult problem. (6)

About the problem of massless particles, the idea of *symmetry breaking* was introduced around 1970. With this additional idea there later was developed a very successful "standard model." Since at that time I did not like to introduce symmetry breaking into a fundamental theory,[10] I lost the opportunity to make contributions in this area of research.

About the collaboration between Mills and me, Mr. Wang Zhi (王志) of CCTV interviewd me on January 26th, 2005 in Beijing. He asked why my research work was often done in collaboration with other physicists. The following was my answer:[11]

> There are many advantages in scientific collaboration. When you are working on a problem, sometimes you get stuck, you get discouraged. If at that time another person discusses the problem with you, asking you questions, suggesting a new direction for consideration, etc., your interest might be rekindled.

Thus I believe:

Discussions with colleagues is oftentimes a very fruitful method of research. (7)

Between 1954 to 1956 many new particles were found. Particularly interesting were two particles θ and τ, which decay into two or three π mesons:

$$\theta \to \pi + \pi\,,$$

$$\tau \to \pi + \pi + \pi\,.$$

Increasingly more accurate experiments showed that θ and τ have approximately the same masses and the same lifetimes, indicating they might in fact be the same particle which had two different modes of decay. There is nothing strange about this EXCEPT IT WOULD VIOLATE THE LAW OF PARITY CONSERVATION: According to this law the "parity" of two π's is $+1$ and of three π's is -1. If θ and τ are the same particle then it can decay into $+1$ parity and also into -1 parity. Parity is thus not conserved. That is absolutely impossible! This dilemma was called the θ–τ puzzle and was the most puzzling problem in fundamental physics in the years 1954 to 1956. In a 1957 paper I said:[12]

> The situation that the physicist found himself in at that time has been likened to a man in a dark room groping for an outlet. He is aware of the fact that in some direction there must be a door which would lead him out of his predicament. But in which direction?

In the summer of 1956 T. D. Lee and I (Fig. 10) were trying to find this door. After carefully examined the five types of experiments which had been considered to have established parity conservation in β-decay, we found to our surprise that in fact *none of them had proved parity conservation*: all of them were *not complicated enough*. We also pointed out several kinds of *more complicated* experiments which could decide whether parity was conserved in weak interactions such as in β-decay.[a]

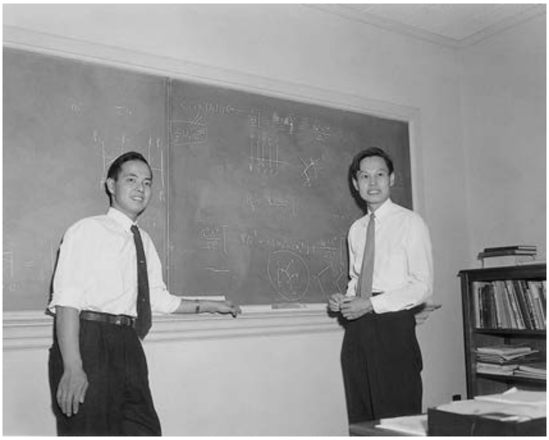


Fig. 10. T. D. Lee and I (taken at IAS in 1957).

[a]Weak interactions include the decay of θ, τ, etc. and also β-decay.

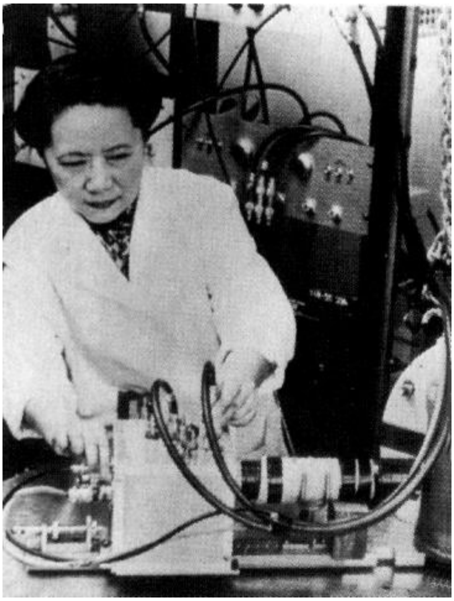


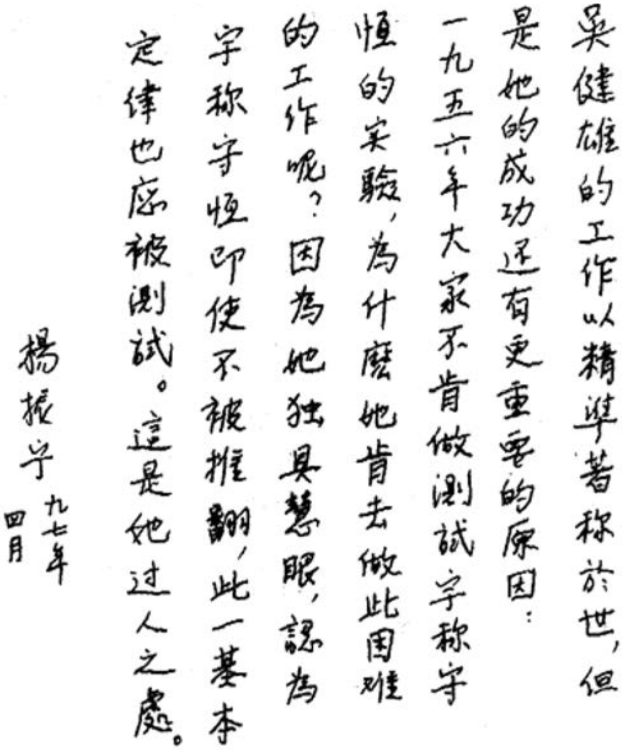
Fig. 11. C. S. Wu (1912–1997).

In June 1956 we wrote these results up and submitted it for publication in the *Physical Review*. We also sent copies of this preprint to many colleagues. Rapidly and uniformly came the response: Parity is definitely conserved. The experiments suggested by Yang and Lee were unnecessary and would only waste human and material resources. Fortunately C. S. Wu (1912–1997, Fig. 11) thought otherwise. She was influenced by Pauli and did not believe that parity could be nonconserved. But she thought since no past experiments had verified that parity was conserved in β-decay, it was important to do *an experiment to test this fundamental law of nature*. After six months of efforts she announced at the beginning of 1957 that in fact in β-decay *parity was not conserved*. Furthermore it was maximally nonconserved. Her announcement shocked the whole world of physics including: particle physics, nuclear physics, atomic and molecular physics. As to why the physical universe satisfies, on the one hand very accurate left–right symmetry (parity conservation) in most interactions, but on the other hand left–right asymmetry (parity nonconservation) in weak interactions, is a deep mystery still unsolved today.

That epoch-making success of Wu's experiment taught her:[13]

> **Never believe in laws which are considered self-evident requiring no experimental proof.** (8)

The second problem I had tackled unsuccessfully in 1947, about Bethe's 1931 paper, became important for me later in the 1960's through a round about way. In the summer of 1961 I visited Stanford University. It happened that Fairbank and Deaver were doing their experiments on flux quantization in superconducting


吳健雄的工作以精準著称於世，但是她的成功还有更重要的原因：一九五六年大家不肯做測試字称守恒的实驗，為什麼她肯去做此困难的工作呢？因為她独具慧眼，認為字称守恒即使不被推翻，此一基本定律也应被測試。這是她过人之處。

楊振宁

九七年四月

Fig. 12. A paragraph I wrote after Wu passed away in 1997.

rings. Their experiments induced me to start working on superconductivity, which led later to the concept of ODLRO, which I have always been very fond of.

A couple of years later, in order to find a mathematical model which rigorously exhibit ODLRO, T. T. Wu, C. P. Yang and I tried many different models. In these considerations we returned to Bethe's 1931 paper. But this time we started to investigate a *generalized* Bethe's problem, thereby introducing the concept of *analytic continuation* to the model. With this generalization the complicated equations in Bethe's paper became controllable. Thus we were able to gain better understanding of their solutions, and between 1966–1969 C. P. Yang and I published several good papers based on this concept. They turned out to be my first research papers in Stony Brook.

In retrospect this experience also followed the three steps:

$$\textbf{Interest} \rightarrow \textbf{Preparation} \rightarrow \textbf{Breakthrough,}$$

but this time the breakthrough came about because of a new factor: To study the problem from a generalized angle. Thus:

Putting a problem in a generalized context is often a good strategy. (9)

As a matter of fact, my 1954 work with Mills was another example of the usefulness of generalization: We generalized the gauge invariance of electromagnetism to non-Abelian gauge invariance.

I moved from the IAS in Princeton to Stony Brook University in 1966. At Stony Brook I began to regularly teach undergraduate and graduate courses, which I did not do in the IAS. One day in 1967 or 1968 I was teaching a course on general relativity. As I wrote down on the blackboard the famous curvature equation of Riemann:

$$R^l_{ijk} = \frac{\partial}{\partial x^j}\left\{\frac{l}{ik}\right\} - \frac{\partial}{\partial x^k}\left\{\frac{l}{ij}\right\} + \left\{\frac{m}{ik}\right\}\left\{\frac{l}{mj}\right\} - \left\{\frac{m}{ij}\right\}\left\{\frac{l}{mk}\right\}, \tag{E}$$

I noticed its similarity to the equation of gauge theory exhibited above as (D). After the lecture I examined these two equations in detail and realized they were in fact both special cases of a general type of equation. In great excitement I went to Jim Simons, who was then the Chair of Mathematics Department in Stony Brook. He told me they were both equations in *fiber bundle* theory and gave me a copy of a standard monograph on the subject by Steenrod. The book turned out to be too dry and too formal for me and I did not learn anything from it. A few years later I asked Simons to give us physicists informal lectures on the basic ideas of fiber bundle theory. These lectures were extremely helpful to us, enabling us to realize that the basic *physical* concepts of all gauge theory (including electromagnetism), such as *potentials*, *field strengths*, etc., are in fact identical to beautiful basic *geometrical* concepts in fiber bundle theory. *I was deeply awed and inspired by this realization.*

Two lessons I learned from this experience:

Fundamental physics is based on beautiful mathematics. (10)

But not all beautiful mathematics find their way into physics. (11)

In Stony Brook I began to have Ph.D. students. My style of guiding students was such that I rarely had more than one graduate student at any time. So altogether I graduated only about ten Ph.D. students. But I am proud to have influenced the career of several graduate students some of whom were in fact not my own: They had come to Stony Brook aiming at specializing in high energy physics. I told them high energy physics did make spectacular progress in the years before 1980, but after that it was difficult for a young person to make good contributions in that field. The big accelerations were becoming increasingly expensive, and the collaborative experiments were becoming too too big. Unfortunately many young people did not realize these and continue to enter the field, causing overcrowding, and the number of good ideas/person/year in the field became very small.

Several Stony Brook graduate students did take my advice seriously and I am happy today that they are now very successful in their respective fields different from high energy physics.

A graduate student had better not choose a field which is becoming overcrowded. (12)

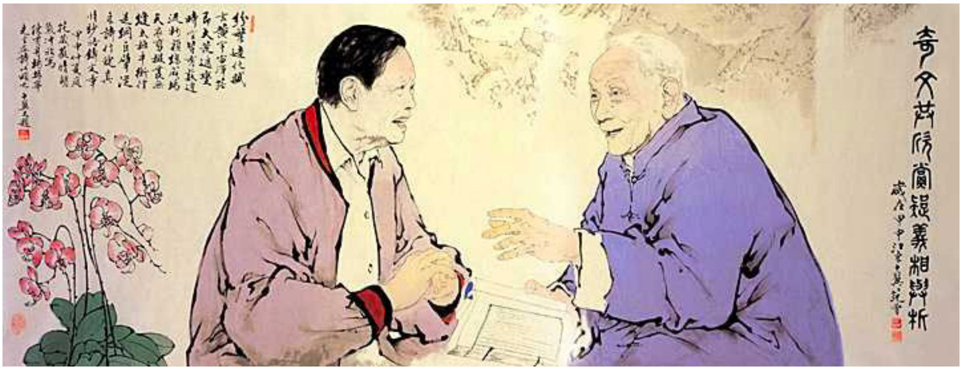


Fig. 13. Painting by Fan Zeng in 2004.

I have listed above, in (1) to (12), personal experiences which might be helpful to a graduate student of physics. Of these, perhaps the one most worthy of attention is

$$\textbf{Interest} \rightarrow \textbf{Preparation} \rightarrow \textbf{Breakthrough.} \tag{13}$$

Two comments about this process:

(a) My father was a professor of mathematics. When I was a little boy he taught me traditional Chinese mathematical problems like "Chicken and Rabbits in a Cage" (鸡兔同笼), "Han Xin Counting Soldiers" (韩信点兵), etc. I learned quickly and he was pleased. Later I had three children in the U.S. When they were little I also taught them these problems. They all learned quickly and I was pleased. But there was a difference between me and my children: One year after learning how to solve these problems, I still remember them, while one year after learning these solutions, my children had completely forgotten having ever heard of them.

It seems that the SAVE and RETRIEVE systems in one's brain are highly selective. And these systems are structured differently for different individuals. If for one individual these systems happen to be *partial* to one kind of information, that could be a seedling. With nourishment and sunlight and care the seedling could *slowly grow* and eventually may *flower*, completing the three-step process (5).

(b) Fan Zeng (范曾), the poet and painter, had presented in 2004 a big painting (Fig. 13) to the Chern Institute of Mathematics of Nankai University. He added to the painting a beautiful poem. Its last line describes how profound undauntable *love*, plus long *efforts*, plus *inspiration* produce literature. I do not remember Fan had ever discussed the creative process in science with either Professor Chern or with me. His poem seems to indicate that the process of creation for an artist follows also the same three steps as for us scientists.

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