Scientific Autobiography

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Biographical facts

I was born in Chicago in 1946, grew up in northern Illinois, and studied physics at Massachusetts Institute of Technology (B.A. 1968) and at the University of California at Berkeley (M.A., 1969; Ph.D., 1972). Following a postdoc at Rockefeller University I went to the University of Pennsylvania in 1974, first as a postdoc and then as a faculty member, becoming a Full Professor in 1985 and serving as Chair of the department of Physics and Astronomy from 1996-2001. I retired from Penn in 2006 and went to the Institute for Advanced Study (IAS) in Princeton as a member and then long-term visitor until my retirement in 2019. I also had Senior Scientist and Lecturer appointments at Princeton University during 2011-2018, and several times taught a graduate course in particle physics there.

I was married for the first time right after graduation from MIT in 1968, but the marriage ended after nine or ten years. Irmgard Mueller (née Sieker) and I were married in 1983 and have been happily together ever since. I acquired a stepdaughter from the second marriage, and later three (technically step) grandchildren, although one tragically died of leukemia at age four.

Research areas

I have worked extensively in theoretical elementary particle physics, especially in making contact between theory and experiment. Specific subfields include precision electroweak physics, neutrino physics, extended gauge structures, grand unification, chiral perturbation theory, cosmology, supersymmetry, and superstring phenomenology. I have been fortunate in that my professional lifetime closely coincided with the development and testing of the standard model.

Professional activities

In addition to research I have authored two editions of an advanced text and reference book on particle physics, *The Standard Model and Beyond* [1], a colloquium-level monograph *Can the Laws of Physics Be Unified?* [2], and have edited or co-edited several books and proceedings, including *Precision Tests of the Standard Electroweak Model* [3], *Testing the Standard Model* [4], and *Neutrinos in Physics and Astrophysics: From* 10^{-33} *to* 10^{+28} *cm* [5]. I have also written a number of major review articles[1]; have lectured at numerous advanced schools all over the world; have given a number of major review/summary lectures at international conferences [7, 8, 9, 10, 11, 12]; have twice been the scientific director of the Theoretical Advanced Study Institute (TASI) [4, 5]; have served on the High Energy Physics Advisory Panel (HEPAP) and on the Executive Committee of the Division of Particles and Fields of the American Physical Society; and have held editorial positions with Physical Review D, Physical Review Letters, Reviews of Modern Physics, and Annual Reviews of Nuclear and Particle Science. For many years I authored or coauthored the review article on electroweak physics for the Particle Data Group. I am a Fellow of the American Physical Society and of the American Association for the Advancement of Science; and have been the Keck Distinguished Visiting Professor (IAS), the William Smith Term Professor of Physics (Penn), a Fermilab Frontier Fellow; and received the Alexander von Humboldt-Stiftung Senior U.S. Scientist Award.

The early years

I studied physics and math at MIT, graduating in 1968. I was always most interested in elementary particle physics, and my Senior thesis, under the direction of Robert Hulsizer, involved the phenomenological analysis of bubble chamber data on $\bar{p}p$ elastic scattering. I also became familiar with computing through part-time and summer employment as a computer programmer for

[1] See below and also [6].

a company involved in computer typesetting, and spent one summer working on radioactive counting at an applied physics lab in Holland.

My graduate work was done at Berkeley from 1968 to 1972. I worked for one summer in Owen Chamberlain's experimental group, and then began theoretical research under the direction of Mahiko Suzuki, who had recently arrived at Berkeley. The theoretical work at Berkeley at that time was heavily dominated by S-matrix and bootstrap theory, with efforts led by Geoffrey Chew and Stanley Mandelstam. I learned all about that subject from lecture and reading courses, but most of my thesis work with Suzuki was instead oriented towards current algebra and electroweak interactions. I was also thoroughly exposed to axiomatic field theory (by Eyvind Wichmann), and learned my first weak interactions in a course by Eugene Commins (using a draft version of his well-known textbook).

My Berkeley years were very stimulating intellectually. However, it was often difficult to stay focussed due to the nationwide disturbances due to the Vietnam War and the frequent campus unrest associated with the war and other topics. I came close to being drafted myself, but received a last minute reprieve when a draft lottery was instituted (my birthday was number 331). Nevertheless, I managed to complete my dissertation in four years. It must have been at least marginally satisfactory because Ronald Reagan (then the governor of California) signed my PhD certificate.

Rockefeller University

From Berkeley I went as a postdoc to Rockefeller University in New York, where I carried out my first research projects that had much lasting significance. Rockefeller was and is mainly known for its research in biological and medical fields, but in the early 70's it was expanding into other areas such as physics and mathematics. (It later backed off of this.) I mainly worked with Heinz Pagels, who was an originator of chiral perturbation theory (CPT). As originally formulated, CPT exploited the fact that the formally leading contributions in an expansion around the chiral limit of the strong interactions are non-analytic and can often be calculated exactly. This is due to infrared singularities associated with the pseudoscalar mesons, which become massless Goldstone bosons in the limit. Pagels and I showed that the leading corrections to vector current form factors at zero momentum transfer are actually linear in chiral breaking [13], rather than the quadratic behavior expected for symmetries that are not spontaneously broken (the Ademollo-Gatto theorem). We subsequently carried out a general analysis of the leading log and other singularities expected in matrix elements and scattering amplitudes [14], and (in later years) used techniques of current algebra and CPT to extract the mass ratios of the light quarks, including non-electromagnetic isospin breaking from the u and d quark masses [15, 16].

At Rockefeller I also participated in a global Regge fit and analysis of total hadronic cross section data [17], which were dominated by a recent experiment at Fermilab in which the Rockefeller experimenters were involved. This was my first experience with global theoretical analyses of experimental data, which were to play a major role in my later research. Other recollections of the Rockefeller years include a summer-long visit to the Department of Mathematics and Theoretical Physics at Cambridge University hosted by John Polkinghorne (who I had met when he visited Berkeley), and meeting my future collaborator Alberto Sirlin, who frequently visited from New York University.

During the time that I was at Berkeley, Rockefeller, and the early years at Penn, there were many exciting developments in the field that led to the subsequent development and confirmation of the standard $SU(2) \times U(1)$ model and QCD, including observations of the weak neutral current, scaling, and charm, and the ideas of asymptotic freedom, color, etc. I eagerly observed and learned about these developments even though I was not directly involved in them until later.

Penn

I went to the University of Pennsylvania as a postdoc in 1974 and was promoted to Assistant Professor the following year. I was initially rejected for tenure, but following a productive sabbatical at SLAC; Berkeley; Madison, Wisconsin; and the Institute for Advanced Study (IAS) the department reconsidered. I remained at Penn until 2006 except for subsequent sabbaticals at DESY, the IAS, Madison, and Fermilab.

Most of my teaching duties at Penn were either in the introductory courses or at the graduate level. (The advanced undergraduate courses were usually taught by experimenters). In the early years I often taught a lab section in addition to a lecture course. I actually enjoyed teaching the introductory lab courses, in part because I had never taken such courses myself (MIT had temporarily eliminated them when I was there). They involved a number of very clever analysis techniques that I probably appreciated much more than the students, such as obtaining a very accurate measurement of the period of a pendulum by a series of successive approximations. Also, I found that when something went wrong on an E & M experiment it could almost always be fixed by checking that the apparatus was plugged in and turned on. In addition to the standard calculus and non-calculus introductory courses, I also devised a "physics and astronomy for poets"-type course that I taught a number of times and in which I learned a lot about astronomy. My favorite graduate course was the two-semester sequence in quantum mechanics, which I taught perhaps ten times in several non-consecutive groups over 30 years. I also taught graduate courses in mathematical methods, field theory, and particle physics, with the latter providing a basis for my later review articles, books, and lecture series at advanced summer schools. This came full circle years later when I used my own text several times in teaching the graduate particle physics course at Princeton.

Towards the end of my Penn years I taught introductory (non-major) courses in astronomy and in cosmology several times. (This was because we had been building an astrophysics program, and our young faculty members in that field were understandably anxious to sometimes be relieved from always having to teach introductory astronomy.) This was scary at first because I had never studied astronomy as an undergraduate. However, I had acquired a good background in stars and in cosmology from my research projects and from the "poets" course. I had to come up to speed on the Solar System and on galaxies by reading the textbook, but as far as I could tell the students were not aware of my weak background. I always lived in fear that one of these classes would include an astronomy buff who would ask detailed questions about the night sky, but fortunately that never happened.

I mentored five PhD students at Penn, S. Uma Sankar, Mingxing Luo, Nir Polonsky, Naoya Hata, and Junhai Kang; co-advised Lisa Everett, Tao Liu, and Jing Wang; and worked with many postdocs at Penn, the IAS, and Princeton.

I served as Chair of the Department of Physics and Astronomy for $5\frac{1}{2}$ years, from 1995 to 2000. That was totally unlike anything else I experienced before or after, and of course I had never had any kind of training in administration. The job involved the routine running of a department consisting of over 35 faculty and 200 students and staff. I accepted the position because I felt that the department was aging and needed to be rejuvenated with regard to faculty, teaching, research directions, and diversity. During my term I oversaw either the hiring or promotion of a significant fraction of the faculty, with generally outstanding individuals. My other priorities were to build a new astrophysics program (the Physics and Astronomy departments had recently merged, with the last two members of the latter retiring soon thereafter), and to increase the representation of women on the faculty. I had at best limited success in these two areas, though I think I can claim credit for laying the groundwork for the successes that came later. I also established a successful summer program for high school students. All of this is described in more detail in my AIP oral history interview [18].

After my term as department Chair and subsequent sabbatical at the IAS and at Madison, I became increasingly frustrated by the lack of support for my program in connecting theory and experiment (i.e., particle theory or "phenomenology"), even by the particle experimentalists. I therefore took advantage of Penn's generous early retirement program when I turned 60 in 2006.

The IAS and Princeton

Prior to my retirement from Penn I arranged to move to the Institute for Advanced Study in Princeton, first as a member and then as a long-term visitor. I had previously spent three sabbaticals and a number of visits at the IAS, which I had always found to be highly-productive and stimulating. Furthermore, the IAS is located close enough to my home near Philadelphia that I could commute on a daily or multi-day basis.

The IAS is a research institution, although its faculty does supervise graduate students from nearby Princeton University. When I first arranged to go there the particle physics group mainly worked in formal and mathematical topics such as string theory, but it was nevertheless very supportive of and interested in phenomenological theory and particle experiment. The IAS typically had a couple of postdocs in that area, was welcoming to visitors such as myself, and at the time had a thriving phenomenology seminar organized by Steve Adler. (The phenomenology effort expanded a few years later with the arrival of Nima Arkani-Hamed). Closely related was a neutrino physics subgroup of the Astrophysics program, led by John Bahcall. One of my major interests at the time was in neutrino physics. I was very much looking forward to interacting with John, but he tragically died prior to my arrival.

I spent 13 wonderful years at the IAS, and it was the most productive period of my career. I worked on numerous research projects with many collaborators, some from Princeton/IAS but mainly elsewhere; wrote my books [1, 2] and two major review articles [19, 20]; and lectured at many advanced international schools. I was also invited to teach the graduate course in particle physics at Princeton University for three years (which I based on my own textbook) and held a Senior Scientist position there for a number of years.

I slowed down my activities the last couple of years, and retired completely in 2019. I had been working in particle physics for some 50 years, during which most of the standard model was developed and established[2]. There remained many important open questions. There are very strong ongoing and anticipated research programs in particle physics, cosmology, and theory to address these issues, and many promising new ideas and techniques. However, there were no major breakthroughs in those final (for me) years. Also, experimental, theoretical, and even phenomenological techniques were becoming ever more sophisticated, e.g., involving large theory collaborations for data analyses, artificial intelligence, etc. I felt that it was time to depart and leave the future progress in the hands of the new generations of brilliant young physicists such as those at the IAS.

Research at Penn and the IAS

Most of my research work was carried out with a wide range of collaborators. These are detailed in the bibliography, but I would like to especially emphasize the roles of three longtime and frequent coauthors: Mirjam Cvetič, Jens Erler, and Vernon Barger.

I worked on a variety of research topics during my early years at Penn. In particular, I became very interested in a number of (interrelated) areas that I continued to pursue for the rest of my professional career. These included electroweak physics; tests of the $SU(2) \times U(1)$ model, especially via analyses of weak neutral current and other precision data; possible extensions of the model, such as extended gauge, Higgs, or fermion sectors; the relation

[2] My last talk was the summary/outlook lecture for the *2018 International Conference on High Energy Physics* in Seoul [12], and my last publication was a brief history of the theoretical development of the standard model, written with Mary K. Gaillard, in *Physics Today* [21]. I felt that these were suitable concluding chapters to my career.

between theory and experiment; global analyses of many experiments; grand unification; and neutrino physics. Later areas included cosmology, supersymmetry, and possible implications of concrete superstring constructions.

Precision electroweak physics

My interest in weak interactions was largely motivated by attempts (e.g., [22, 23]) to extend or modify the quark and $SU(2) \times U(1)$ models to account for reported experimental anomalies (which, however, were later were found to be erroneous). The initial experiments supporting the extension from the Fermi theory to $SU(2) \times U(1)$ involved the weak neutral current (WNC). The early experiments were analyzed independently, often with differing theoretical assumptions even for similar experiments, such as the treatment of quark distributions in deep inelastic scattering. Usually, the published results focussed only on the extraction of the weak angle, $\sin^2 \theta_W$. To test the model as precisely as possible, probe for small perturbations, and distinguish it from completely different theories required a global analysis of all of the WNC experiments, and (later) of related experiments, such as high precision studies at CERN, SLAC, and Fermilab on the W and Z properties and couplings. Such analyses can utilize a common theoretical treatment of radiative corrections, QCD corrections, and correlated systematic uncertainties. Studies with various collaborators as well as by others carried out from the early days until the present have shown that $SU(2) \times U(1)$ is the uniquely correct theory to first approximation, yielded precise tests of the theory at the level of radiative corrections, allowed an accurate determination of parameters (needed for tests of gauge coupling unification), set limits on many extensions of the model, and eventually allowed indirect predictions of the t-quark and Higgs masses. My own efforts in the field included [24, 25, 26, 27, 28, 29]. I also wrote the *Electroweak* review for the Particle Data Group for many years, eventually in collaboration with Jens Erler (e.g., [30]), edited a reference volume [3], and gave numerous lectures at conferences and schools (e.g., [8, 9, 10, 31]). Some of the history is elaborated in [21, 32, 33].

My efforts were initially resented by some of the experimenters, especially at CERN, who felt that I was somehow stealing their results. This attitude gradually changed as they realized that there was much more information content in the global analysis than in individual experiments. I am happy that many years later many collaborations were or are carrying out global analyses in many areas of particle physics, such as the electroweak and other joint working groups of the four LEP experiments, or groups in flavor physics. I was especially honored when I was invited to present the talk on *The Electroweak Sector* at the *LEP Fest 2000* celebration of LEP at CERN.

The precision electroweak program, often combined with results or limits from weak charged current experiments (WCC), flavor-changing neutral currents (FCNC), hadron collider searches, and cosmology, allowed constraints or search strategies on many types of "beyond the standard model" (BSM) physics. Some of the areas I have been involved with include heavy fermions (such as from a hypothetical fourth family, or from exotics with nonstandard $SU(2) \times U(1)$ assignments) [34, 35, 36, 37, 38, 39], extended Higgs or dynamical symmetry breaking sectors [30, 35, 40, 41, 42, 43, 44], and heavy W' gauge bosons[3] (such as those coupling to right-handed currents as found in $SU(2)_L \times SU(2)_R \times U(1)$ models) [45, 46, 47, 48]. Miscellaneous BSM studies included those on μ decay [49] and the strong CP problem [50, 51].

Additional Z' gauge bosons

Over most of my career (both at Penn and the IAS) I was interested in the possibility of additional Z' gauge bosons, associated especially with new $U(1)'$ gauge factors. These frequently remain unbroken when a larger gauge symmetry is broken at a high scale. In that case the $U(1)'$ breaking can occur at the TeV scale or lower, where it is usually tied to $SU(2) \times U(1)$ breaking. I have worked on many different aspects of Z' physics. These include Z' limits and implications from precision electroweak physics [24, 25, 27, 52, 53, 54]; collider signatures and diagnostics of the couplings and decay modes if a Z' is observed [45, 55, 56]; possible FCNC expected for family-nonuniversal $U(1)'$ charges [57, 58]; implications for extended Higgs [59] and neutralino [60] sectors and for exotic fermions [34, 36, 39]; implications for light Majorana, Dirac, and pseudo-Dirac neutrino masses and for heavy neutrinos [61]; implications for supersymmetry breaking and mediation [62, 63], for superpartners [60, 64], and as a simple solution to the supersymmetry μ problem [65, 66]; $U(1)'$ expectations from grand unification [67], supersymme-

[3] In [45] we pointed out that there could be a forward-backward asymmetry in the dileptons produced in $pp \to V \to \ell_1 \bar{\ell}_2$, where V is a charged or neutral gauge boson, even for a pp collider, provided that the V is produced with nonzero rapidity. This is because of the roles of the valence versus sea quarks in the protons. This seems obvious now, but was not generally appreciated at the time. This effect is closely related to a dilepton charge asymmetry.

try [68], and string theory [39, 65, 69]; and implications for cosmology [70, 71]. Much of this was reviewed in [19].

Grand unification

I was not involved in any of the original or major developments in grand unification (GUTs), but became interested after working with Gino Segrè and Arthur Weldon on a short-lived alternative model [72]. Over the years I worked on various implications of GUTs, such as gauge unification, proton stability, exotic fermions, magnetic monopoles, and supersymmetry, as well as on the relation to superstring theories.

One of the difficulties with the original $SU(5)$ model was that it predicted the existence of superheavy magnetic monopoles that would greatly overclose the universe. So-Young Pi and I showed that this problem could be resolved in a variation on the model involving a "backward phase transition"[4], in which the $U(1)$ of electric charge is conserved at ordinary temperatures but broken at extremely high temperatures [73]. However, Alan Guth subsequently rendered this solution unlikely by the much better proposal of inflationary cosmology, which not only solved the monopole problem but other cosmological problems as well.

I worked extensively, especially with Mingxing Luo and with Nir Polonsky, on the predictions of gauge coupling unification (and also for the bottom quark mass) in grand unified theories (e.g., [74, 75, 76]), emphasizing two-loop effects, low- and high-scale threshold corrections, and various uncertainties. We and our competitors found that the gauge coupling predictions worked better in the supersymmetric extension of the standard model (MSSM) than in the standard model itself (assuming that the underlying GUT breaks directly to the SM or MSSM)[5]. The predictions are even better if the supersymmetry breaking occurs at several TeV rather than at the TeV scale.

Gino Segrè, Matt Strassler, and I argued that possible time variations of the fine structure constant (which had at the time been hinted at in cosmological observations) might also imply variations in the other gauge couplings (and possibly other parameters) in the context of gauge unification, with

[4]Such backward phase transitions are known in condensed matter physics, and had been previously discussed by Steve Weinberg in relativistic quantum field theory.

[5]An earlier indication of this had been discussed in [25], mainly emphasized by Bill Marciano.

cosmological and particle physics implications [77].

Grand unification usually refers to the possibility that there is some scale of physics in which there is a "free-standing" GUT in four space-time dimensions, and at which gravity can be neglected. Superstring theories often contain some form of unification of the gauge interactions as well as gravity in the underlying ten- or eleven-dimensional theory. However, most concrete superstring constructions (or at least the ones that were usually considered when I was active) break directly to the standard model plus other interactions in four dimensions, without an intermediate free-standing GUT (or even if they do they likely do not contain the large adjoint representations needed to break the GUT symmetry). I therefore later largely lost interest in the full implications of GUTs, although some aspects, such as some form of gauge unification (e.g., [78]), might survive.

Although I was not an originator of grand unification, my work on implications eventually led to my giving many talks and authoring or co-authoring various review articles [7, 9, 79, 80, 81]. Although the main topic of the book-length review article [80] was grand unification, it included an extensive introduction to the standard model that was used as a graduate text on particle physics at several universities and by individuals[6]. My later textbook *The Standard Model and Beyond* [1] was very loosely based on [80] as well as on a graduate course that I taught at Penn and on various lectures at advanced schools. [80] also led to an invitation to give the review talk on *Grand Unified Theories* at the *International Symposium on Lepton and Photon Interactions at High Energy* [7] in 1981, which was my first large plenary talk at a major international conference.

Neutrinos

My attraction to neutrino physics derived in part from my general interest in weak interactions. Another motivation was that the experimental and

[6] There was an amusing episode concerning the publication of [80]. Some time after I had returned the galley proofs I received a call from Steven Weinberg, who had studied the article carefully. He requested a long list of minor changes, mainly involving things like the ordering of references. Although these were reasonable, I explained that the article was about to be published, and it was too late to make any changes. Steve then told me that he had already conveyed the changes to North Holland (the publisher), and that all I had to do was to telephone Mrs *so and so* in the Netherlands at *such and such* a number within the next few hours to give my permission! Needless to say, I complied. It is hard to argue with a Nobel laureate.

observational results on neutrino mass and mixing eventually lended themselves to global analyses similar to those for precision electroweak physics. Yet another was that Ray Davis came to Penn following his retirement from Brookhaven and continued to run his chlorine experiment from there.

There were two major thrusts to my work: the global analyses, and "nonstandard" neutrino properties and mass origins (i.e., not the simple seesaw model for Majorana mass).

The Davis radiochemical Solar neutrino experiment on chlorine established that the flux of ν_e is not consistent with the Standard Solar Model (SSM), implying a difficulty with our understanding of the Sun or with neutrinos (or both). This was confirmed by the subsequent gallium radiochemical experiments and the Kamiokande water Cherenkov ($\nu_e e^-$) experiment. Since these all disagreed significantly from the SSM, I felt that a more general analysis framework was called for. In [82] Naoya Hata, Sidney Bludman, and I utilized the fact that the different types of experiments were sensitive to different neutrino energies to obtain a rough Solar spectrum. That is, the experiments could be combined in a "model independent" way (i.e., with pp, ^7Be, CNO, and ^8B fluxes constrained only by the total Solar luminosity) to show that the middle of the spectrum (the ^7Be lines) is more strongly suppressed than the higher energy ^8B or low-energy pp neutrinos. This was incompatible with any plausible astrophysical explanation, but could be accounted for by neutrino oscillations or MSW conversions.

I was involved in a number of subsequent global analyses of the Solar neutrino observations, mainly with Hata, including model independent ones and those assuming the SSM. These incorporated uncertainties (where appropriate) from the nuclear cross sections, Earth effects, and the Solar temperature, including proper correlations between experiments and between flux components. The results were that the data at the time[7] were consistent with both large- and small-angle MSW conversions. See, e.g., [83, 84, 85].

One of my most important contributions to neutrino physics was the correction of a critical sign error in the MSW formalism[8]. The story began earlier when Jacques Leveille, Jon Sheiman, and I had written a paper [86] debunking claims that the cosmological relic neutrinos could be detected by observing $O(G_F)$ forces on a macroscopic object due to total external

[7]Later experiments eliminated the small-angle solution.

[8]See [18] for a fuller description of the story. John Bahcall also recounts the story fairly accurately in his book on *Neutrino Astrophysics*, even though I never discussed it with him.

reflection. The details of the erroneous effect depended on the index of refraction for neutrino propagation in matter, so I had carefully calculated the needed forward scattering amplitude and found that there was a sign error in the existing literature. Thus, when the MSW papers came out, I quickly verified that they were also using the wrong sign. I chose (probably unwisely) to inform everyone involved privately rather than publish a paper[9]. For some time, Ray Davis, who was travelling extensively at the time, would return from trips and inform me that such and such person had tried to reproduce my results and disagreed with me. However, I stuck to my guns and basically staked my reputation on the result[10]. Fortunately, everyone eventually came to agree with me, even with Hans Bethe correcting an important paper prior to publication. Since the sign determines the relative mass order of the (predominantly) ν_e and ν_μ mass eigenstates, most of the (large number of) subsequent theoretical models of neutrino masses would have been incorrect if the mistake had not been corrected.

One of my long-term interests involved the mixing and oscillations between active (i.e., ordinary, or $SU(2)$-doublet) and sterile ($SU(2)$-singlet) neutrinos of the same helicity. In [88] Barger, Leveille, Sandip Pakvasa and I showed that such "second class" oscillations could occur if there are both Majorana and Dirac mass terms, which must be of comparable magnitude for significant mixing. I was always somewhat skeptical of the second class oscillation interpretation of the LSND and related experimental anomalies[11], both because of the nonconfirmation in disappearance experiments, but also for the theoretical difficulty of accounting for two seemingly unrelated types of super-small mass terms needed for the mixing (Majorana and Dirac in most schemes). I emphasized the latter in a number of talks and articles [20, 89], often to the discomfort of some of my colleagues. Nevertheless, I did propose one possible mechanism involving string-motivated higher-dimensional operators [90], and coauthored one phenomenological study [91].

I also wrote an early paper on the cosmological difficulties for active-sterile mixing [92]. The point was that active-sterile oscillations, combined with rescattering of the active component from the thermal plasma, would

[9] I did eventually collaborate on a paper concerning the Solar neutrino and big bang nucleosynthesis implications of the MSW effect [87].

[10] The calculation appeared needlessly complicated because everyone, including me, was using the index of refractions formalism. It is much more straightforward when working directly with the neutrino Lagrangian, e.g., [1].

[11] The status is still unresolved as of this writing.

have efficiently produced the sterile neutrinos prior to big bang nucleosynthesis for most of the parameter range relevant to LSND, in conflict with observational limits on the number of effective neutrino degrees of freedom obtained assuming the standard cosmological model. Unfortunately, the paper was ahead of its time, and the Physical Review could not find a suitable referee for over a year. The long-delayed referee report had minor objections, but by that time other authors had already picked up on the idea and published a second-generation paper, so I did not bother to resubmit. I have always regretted that decision.

Miscellaneous papers on neutrino physics included the possibility of $\nu - \bar{\nu}$ transitions [93] (somewhat related to sterile neutrino mixing); possible large $\nu - \bar{\nu}$ asymmetries [94, 95] (relevant to cosmological limits on mass and mixing and to leptogenesis); a study with David London on the implications of an apparently nonorthogonal[12] neutrino mixing matrix [96] (due to mixing with neutrinos too heavy to be produced by oscillations); and the (extreme) difficulty of observing CP violation in neutrinoless double beta decay [97].

With various collaborators I explored possibilities for neutrino masses in a number of BSM scenarios, including Z' models [61, 70], supersymmetry [98], large extra dimensions [99], and superstring constructions [44, 100, 101, 102]. Various mechanisms were found yielding Majorana, Dirac[13], pseudo-Dirac, or mixed models. For example, small Dirac masses could emerge in extra-dimensional theories, by (exponential) string instanton effects in some string constructions, or from higher-dimensional operators if the leading order Yukawa couplings are forbidden by extra symmetries or string effects[14].

I reviewed aspects of neutrino physics in a number of schools, conferences, and reviews[15], e.g., [5, 11, 20, 89], and was scientific director of TASI-98 on neutrinos [5].

[12] Subsequent studies by others generalized this to nonunitary matrices.

[13] Majorana masses presumably occur at some level since they are not forbidden by an unbroken gauge symmetry. However, they could be much smaller than Dirac mass terms.

[14] I have never dismissed the possibility of Majorana masses, but I was often considered eccentric by some in the neutrino community because of my insistence that Dirac masses should be taken seriously as an alternative.

[15] I recall once reading a short article on neutrino physics by the (then) Director General of CERN. Midway through the paper were two surprises. The first was that one paragraph was copied word for word from one of my articles (without citation). The other was that he went on to draw exactly the opposite conclusion from my own one (which I believe was correct)! After my query an aide replied apologetically that the lack or citation was inadvertent.

Supersymmetry

My work in supersymmetry generally came about as specific BSM applications of other topics[16]. These included gauge coupling unification [74, 75, 76]; extensions of the MSSM [68], such as a solution to the μ problem [65, 66], extended neutralino [60] or Higgs [59] sectors, and alternative spectra [64]; baryogenesis [71]; Z' mediation of supersymmetry breaking [62, 63]; and neutrino masses [61], including a novel mechanism for small Dirac masses via suppressed nonholomorphic terms [98]. Other projects involved possible charge- and color-violating minima [103], and a possible T-odd observable in heavy squark decays [104].

Superstring phenomenology

I was never a formal string theorist. Rather, I collaborated with Mirjam Cvetič, Jim Halverson, and others on the implications of classes of string constructions, trying to gain insight into which types of BSM physics occur frequently in the string landscape, and which are unlikely[17]. In particular, I was interested in possible "stringy remnants" that might occur beyond the standard model or MSSM. These I define as new low-energy particles, interactions, symmetries, mechanisms, or constraints that are frequently left over from underlying string constructions and which are "just there", i.e., they do not necessarily solve any specific low-energy problem. For reviews of such remnants, see [105, 106].

Conversely, I was never interested in the more fashionable area of trying to find specific constructions that would reproduce the MSSM or predict the values of the Yukawa couplings, supersymmetry breaking parameters, etc. That was in part because the vast majority of the constructions that had been explored contained additional remnants. Furthermore, I thought it unlikely that anyone would ever find the "right" point in the landscape, and that attempts would probably not yield much insight since the results are so dependent on the details of the compactifications of the extra dimensions.

[16] Also of interest was a letter organized by Pierre Ramond, Mary K. Gaillard, and I to the managements of Fermilab and the Tevatron experiments, which I believe had considerable influence on their searches for supersymmetry. This is described in [18].

[17] For example, most string constructions do not yield any gauge group representations larger than singlets, bifundamentals, symmetric or antisymmetric products, and adjoints (for exceptions, see, e.g., [105]). The distinction between "string likely" and "string unlikely" is similar in spirit to later work by others on the "swampland conjectures".

With various collaborators I explored phenomenological implications of several types of quasi-realistic string constructions appearing in the literature. These included free fermionic constructions, with emphasis on constraints allowing low-scale supersymmetry breaking [107, 108, 109]; intersecting brane constructions [110, 111, 112], including a review article [113] with Ralph Blumenhagen, Cvetič, and Gary Shiu; and the four-dimensional $Z(3)$ orientifold [114]. Other studies focussed on the implications of "stringy" consistency conditions apparently not required by field theory [39, 44].

Commonly occurring stringy remnants include quasi-chiral exotics (i.e., fermions that are chiral under the standard model but not under additional symmetries), such as quark or lepton isosinglet or isodoublet pairs (e.g. [36, 39]); unusual quantum numbers [39], such as lepton doublets with charges $(\pm 1, \pm 2)$, fractionally-charged color singlets [38], leptoquarks, or diquarks; family nonuniversality [39, 57]; hidden or quasi-hidden sectors; extended gauge structures [39, 65, 69], such as additional anomalous or nonanomalous $U(1)'$s; a dynamical effective μ parameter in the MSSM [65, 66, 115]; extended scalar/scalarino sectors, especially with $SU(2)$ singlets, additional doublets, and possibly hyperchargeless triplets [39, 44]; nonstandard neutrino mass mechanisms [20, 44, 100, 101, 102]; nonstandard gauge unification [78]; and nonperturbative [39] or geometric Yukawa interactions [111].

I was also a coauthor of two phenomenological studies [116, 117] of reported experimental evidence for BSM physics. Though both effects turned out to be erroneous, the papers did indicate how typical stringy remnants could be relevant to the low-energy world.

Cosmology

My occasional excursions into cosmology always involved implications or constraints on other research interests, many of which have been mentioned previously. These included magnetic monopoles in grand unifications [73], possible charge and color breaking minima in supersymmetry [103], and the possibility of finding ourselves in a metastable vacuum [118]. There were also a number of studies related to neutrino physics, such as relic neutrinos [86], neutrino asymmetries [94, 95], sterile neutrinos [92], and big bang nucleosynthesis (BBN) [70, 92, 119]. Other works included implications of various possible BSM physics. These included extra scalar singlets or $U(1)'$s, affecting BBN [70], cold dark matter [42, 43], and electroweak baryogenesis [43, 71]; fractionally charged color singlets [38][18]; and time variation of coupling “constants” [77].

Additional information

Additional information, including links to my CV, research statement, slides of lectures and a graduate course at Princeton, videos of a lecture series at the Perimeter Institute, supplementary materials for my graduate text [1], and the AIP oral history transcript [18] can be found at web.sas.upenn.edu/pgl/.

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