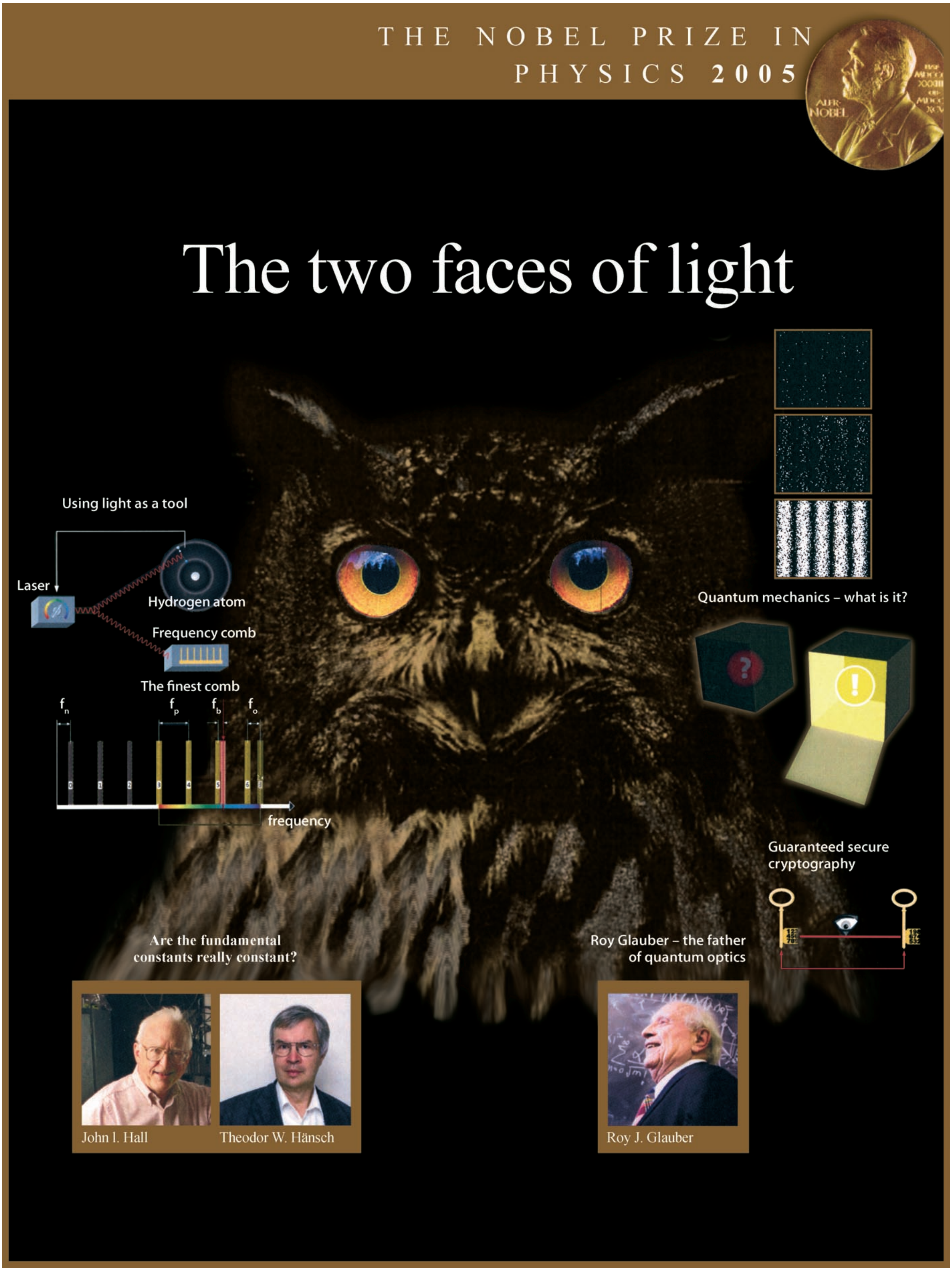

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One Hundred Years of Light Quanta (Nobel Lecture)**

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It can't have escaped you, after so many recent reminders, that this year marks the one hundredth birthday of the light quantum. I thought I would tell you a few things about its century long biography. Of course we have had light quanta on earth for eons, in fact ever since the good Lord said "let there be quantum electrodynamics"—which is a modern translation, of course, from the biblical Aramaic. So in this talk I'll try to tell you what quantum optics is about, but there will hardly be enough time to tell you of the many new directions in which it has led us. Several of those are directions that we would scarcely have anticipated as all of this work started.

My own involvement in this subject began somewhere around the middle of the last century, but I would like to describe some of the background of the scene I entered at that point as a student. Let's begin, for a moment, even before the quantum theory was set in motion by Planck. It is important to recall some of the remarkable things that were found in the 19th century, thanks principally to the work of Thomas Young and Augustin Fresnel. They established within the first 20 years of the 19th century that light is a wave phenomenon, and that these waves, of whatever sort they might be, interpenetrate one another like waves on the surface of a pond. The wave displacements, in other words, add up algebraically. That's called superposition, and it was found thus that if you have two waves that remain lastingly in step with one another, they can add up constructively, and thereby reinforce one another in some places, or they can even oscillate oppositely to one another, and thereby cancel one another out locally. That would be what we call destructive interference.

Interference phenomena were very well understood by about 1820. On the other hand it wasn't at all understood what made up the underlying waves until the fundamental laws of electricity and magnetism were gathered together and augmented in a remarkable way by James Clerk Maxwell, who developed thereby the electrodynamics we know today. Maxwell's theory showed that light waves consist of oscillating electric and magnetic fields. The theory has been so perfect in describing the dynamics of electricity and magnetism over laboratory scale distances, that it has remained precisely intact. It has needed no fundamental additions in the years since the 1860s, apart from those concerning the quantum theory. It serves still, in fact, as the basis for the discussion and analysis of virtually all the optical instrumentation we have ever developed. That overwhelming and continuing success may eventually have led to a certain complacency. It seemed to imply that the field of optics, by the middle of the 20th century, scarcely needed to take any notice of the granular nature of light. Studying the behavior of light quanta was then left to the atomic and elementary particle physicists—whose interests were largely directed toward other phenomena.

The story of the quantum theory, of course, really begins with Max Planck. Planck in 1900 was confronted with many measurements of the spectral distribution of the thermal radiation that is given off by a hot object. It was known that under ideally defined conditions, that is, for complete (or black) absorbers and correspondingly perfect emitters this is a unique radiation distribution. The intensities of its color distribution, under such ideal conditions, should depend only on temperature and not at all on the character of the materials that are doing the radiating. That defines the so-called black-body distribution. Planck, following others, tried finding a formula that expresses the shape of that black-body color spectrum. Something of its shape was known at low frequencies, and there was a good guess present for its shape at high frequencies.

The remarkable thing that Planck did first was simply to devise an empirical formula that interpolates between those two extremes. It was a relatively simple formula and it involved one constant which he had to adjust in order to fit the data at a single temperature. Then having done that, he found his formula worked at other temperatures. He presented the formula to the Germany Physical Society[1] on October 19, 1900 and it turned out to be successful in describing still newer data. Within a few weeks the formula seemed to be established as a uniquely correct expression for the spectral distribution of thermal radiation.

The next question obviously was: did this formula have a logical derivation of any sort? There Planck, who was a sophisticated theorist, ran into a bit of trouble. First of all he understood from his thermodynamic background that he could base his discussion on nearly any model of matter, however oversimplified it might be, as long as it absorbed and emitted light efficiently. So he based his model on the mechanical system he understood best, a collection of one-dimensional harmonic oscillators, each of them oscillating rather like a weight at the

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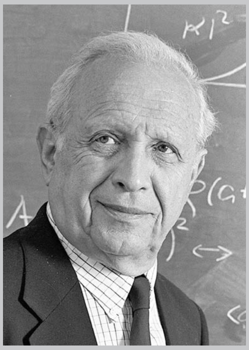


What is it that makes a dedicated scientist out of a kid with an everyday background? Is it the ungovernable forces that seem to shape all our lives, or is it the development of our own curiosity and tastes that tips the balance of randomness? I've always been puzzled by those questions and can't claim to have found serious answers. Perhaps these recollections will reveal one, even if it escapes me as I write.

To be a traveling salesman in the 1920s gave one possession of a company-owned car, acquaintance with a potentially vast area of the country, and a slightly better income than one would earn within the tight confines of New York City. My father, having enjoyed some experience with that life before getting married in 1924, couldn't wait to get back on the road, a possibility he had to postpone for about three years until his wife and new-born son, at last aged two, were ready to travel. The itinerant life was a restless one and quite disconnected. After long hours spent driving through endless farmlands we would stay overnight at the houses of farmers who had hung the sign "Tourists-Vacancy" near the road—never two successive nights in the same house. That was long before the days of roadside motels. Even hotels were scarce in some of the small towns we visited. Rural electrification was not yet a reality, and I became quite accustomed to the smells of illumination by kerosene and acetylene lamps, as well as all the odors of barnyards and outdoor plumbing.

The periods in which my father was visiting customers, in whatever small town we were passing through posed a problem for my mother. Trained as an elementary school teacher, but pregnant before she had begun teaching, she was determined to make these passages as instructive for me as she could. The most interesting place in each town, as well as I could make out, was the fire department. We received guided tours of their living quarters and fire engines all over the Midwest. Where fire departments were lacking, visits to assorted courtrooms, police departments and even local lockups would do for my introduction to civics.

In one Cleveland hotel room when I was four, we actually had a radio. It occupied a wooden cabinet about the size of a steamer trunk. I remember insisting there must be a man inside it. He had given his name as Maurice Chevalier. Discovering that the cabinet top was hinged, I opened it and can feel still my bafflement at discovering within it only a few glowing radio tubes.

The 1929 market crash had an immediate impact for me. The company my father was working for failed and the car we had been using was repossessed. The result was my first ride on a train, an exciting experience that there was no occasion to repeat, once my father had another job and a most imposing new car, a Marmon, a kind of Cadillac of its day, and one of many brands destined for early extinction.

The arrival of a baby sister in 1931 and my need to begin school meant that we had somehow to settle down. My folks decided we would return to New York, but the only way to do it, under the circumstances, was for our family to move into a crowded apartment in upper Manhattan, with my father's mother and aunt. It was quite a shock, moving there from the wide-open expanses of the middle west, to sit in the crowded classrooms of an ancient school building. I had very little experience playing with other kids in the small towns we had visited, and had no idea how to deal with the crowds of kids who managed somehow to play on the concrete sidewalks and in the adjoining gutters.

Life on the road. Standing on the bumber of my father's car, Ohio, 1930.

My mother was talented at crafts of various sorts. She sewed and embroidered well and, though untrained, she sketched and painted quite skillfully. She encouraged me to draw as soon as I could hold pencils or crayons steadily. That was the beginning of my career as a creative artist, specializing in speeding trains, airplanes and the occasional dirigible. It was a necessary release from the need to get fresh air by playing on the sidewalks.

After one school year in Manhattan, we found an apartment in Sunnyside, an attractive area of Queens, and moved there in 1932. It was a kind of deliverance. The neighborhood was spacious and not yet fully built up. It consisted largely of modest single family houses that all had at least a small area devoted to yards or gardens. All the blocks of individual houses and even our apartment building had central areas of lawn with space for children to play—not withstanding all the "Please keep off the grass" signs. There were even vacant lots with tiny hills and semi-permanent puddles that lent excitement to the four daily treks to and from school.

Sunnyside's residents consisted mostly of young families, quite a few contending with the unemployment so widespread in those depression years. The building for Public School 150 however was clean, well lit, and quite new. Its teachers were mostly young and optimistic, a vigorous contrast to the atmosphere that prevailed more generally in the country. The school's annual Christmas play, written by the fifth and sixth graders in 1932 was entitled, "Santa's Depression". It depicted Santa Claus as being broke, and unable to afford to make his usual rounds, until everyone pitched in to help him out. In the same period there were demonstrations against foreclosures on home mortgages going on in the neighborhood streets. Those depression years cast a long shadow over the lives of children no less than their parents.

My earlier years had left me with no experience in sports of any kind so it wasn't easy discovering how to engage in outdoor exercise. Unlike Manhattan, however, Sunnyside had many residential streets with little traffic. The best solution to my exercise problem was roller skating—with steelwheeled skates that clamped to one's everyday shoes. Those steel wheels were quite noisy rolling or scraping on concrete, notwithstanding their good ball bearings, and I wore down to those very bearings many sets of skate wheels, cruising the neighborhood streets.

Electricity mystified me throughout childhood and I vividly remember once at age seven trying to see what it was all about. Plugging lamp cords into wall sockets must lead to the flow of something through those wires, but whatever it was, one never got to see it before it was swallowed up by the lamp. One morning I awoke early, determined to catch sight of it. I screwed the wires of a short length of lamp cord into a male plug and inserted it into a wall socket, leaving free the frayed wires at the other end. There was a bright blue flash at that end, accompanied by a muffled bang. That was followed by silence, till my parents awoke and began wondering why none of the light switches seemed to be working. The fuse was easily replaced, but I never overcame my surprise that what passed so silently through slender wires could behave so aggressively.

My most interesting projects were the ones I could pursue indoors like building models of contemporary airplanes of all shapes and of ships and locomotives. I had no cash allowance to spend on such projects and so was wholly dependent on gifts of construction kits from uncles and aunts. When those were scarce, as they sometimes were, I ventured into other areas, attempting to use crate wood to construct the projects suggested in various instruction books written for young boys. The most interesting of these projects usually failed, and I began to conclude the authors could never themselves have really built the exciting things they were describing. Their version of a guitar, for example, fashioned from a cigar box and some cheese box wood never had the rigidity to permit stretching a guitar string tightly enough. My guitar looked a bit like one but couldn't sound a single note.

There were other failures, many of them, but each brought new experience in the use of hand tools. An uncle, to encourage this construction bent, presented me with a three-year subscription to Popular Mechanics magazine. That magazine, besides celebrating all of the mechanical wonders of the age, included brief plans for all sorts of home projects: door chimes, folding tables, towel racks, bookends and knife sharpeners. The subscription did a great deal to keep my interest in mechanical things alive, but I can't say I ever succeeded in building any of those worthy projects. And I doubt that anyone who didn't have a machine shop at his disposal ever did either.

All the sawing, drilling and sanding I was doing at home left little time for drawing and painting, but there was ample opportunity to pursue those interests in school. Tempera paints were available there, a certain amount of free time, and a good deal of encouragement from the teachers, who felt a need to keep the backs of their classrooms decorated with mural paintings executed by the kids. They were painted over large areas of brown wrapping paper that covered the rear blackboards. I enjoyed designing those huge works and loved the freedom painting them gave me from sitting at my classroom desk.

The school produced a magazine every term and when my design for the cover of the Christmas 1935 issue was accepted, I felt like the Michelangelo of the fifth grade. In fact I did have some involvement with sculpture as well. Small carvings in soap, greatly encouraged by the Procter and Gamble Corporation, were a medium of the day, and I made many of them, mostly of musicians playing instruments. But a more conventional medium was Plasticine clay, which remains permanently soft. Those sculptures tended not to last long, but we managed, with a teacher, to take a few to a real sculptor's studio, and I was fascinated there to learn to make plaster molds and permanent castings.

If I was fully determined in the fifth and sixth grades to become an artist of one sort or another, it was not without a certain note of caution. My uncle, Sam Adler, was a gifted artist who had not yet succeeded in selling any of his work, nor did those years seem to promise that he ever would. His advice to me was that becoming an artist was an excellent idea, provided my motivation was so strong as to leave no alternative. I began then to feel that my artwork was not spontaneous enough, that if I were a true artist I shouldn't have to think so hard before even starting drawings; they should just pour out more instinctively. My involvement with art receded to a hobby.

The years in which the depression lingered must have been difficult ones for the owners of apartment buildings. Faced with many vacancies, they offered rent-free months and other incentives to new tenants, so there was always a certain degree of restlessness among the city's apartment dwellers.

In 1936, when I was ten, my parents decided that the higher ground of the Bronx—and the top floor of a six story apartment house—would be a better place to live than the flat sea-level expanse of Long Island. A precipitous increase of the local population density went with that move, and it became once again impossible for me or my sister to spend much time outdoors, in the streets.

My first salvation was reading. I visited the local public library regularly and began reading the great adventure stories of Jules Verne, Alexander Dumas and Walter Scott. The junior high school I went to seemed mired in a curriculum too timid to do anything serious, and altogether flat-footed at what it did undertake. Mathematics, I remember, consisted of memorizing the decimal equivalents of the familiar "business fractions" and doing compound interest calculations out longhand. I was so put off by those lessons I occasionally got failing grades. Our premature introduction to French required our memorizing a list of proverbs which didn't literally translate into their English counterparts.

That junior high school experience was typified by what was called "music appreciation" in the auditorium assemblies. The principal, a

My cover for the PS 150, Queens, NY School Magazine, December 1935.

Mr. Snyder, had himself written words to accompany several dozen themes of the great works of music. Singing his rhyming words he evidently felt, set to the themes of the great composers, should imprint those masterpieces on our young memories. Indeed they did, but it was at the expense of burdening those themes permanently with his infernal doggerel.

But that school did offer my first exposure to science and it was exciting. We were shown how to coil wire around nails and make them into electromagnets with the current from dry cells. Those 6 volt dry cells, widely used to power doorbells, cost 25 cents in the local 5 and 10. From that time on I was never without them.

My ambition to be an artist was further dampened by an art appreciation course largely devoted to biographies of the less scandalous painters, and punctuated by black and white lantern slides of their masterpieces. That course also had a creative element devised to avoid, at all costs, creating any sort of untidiness in the classroom. I was encouraged to draw with pastel crayons, again on a large sheet of wrapping paper hung at the back of the classroom, while the other kids who felt less inclined toward art, were set to copying mounted cartoon panels on drawing paper. Neither the pastels for me, which were intended evidently to make the room look like an art class, nor the cartoon exercises for the other kids, seemed to have any instructive value.

My lingering interests in art presently became centered on puppetry and marionettes. The instructions I saw for making them in some magazine articles and a handbook seemed to offer an interesting combination of sculpture and construction. After fashioning several puppet heads of papier maché and painting them, I set about constructing their marionette bodies and string controls. When it came the turn of our class to present a play in the school auditorium I volunteered to produce a small troupe of marionettes and a stage appropriate for the class presentation. Our decision to stage the fairy tale "Rumplestiltskin" turned out to set a more imposing task than I had imagined. Fortunately my mother came to my aid, offering not only to costume the marionettes but to help in constructing several. The task kept both of us busy for a solid month. The eventual presentation by the class, speaking for and operating the marionettes, must have been some sort of success since we had to repeat it several times. But I was ultimately embarrassed by the fact that so much of the work had been visibly my mother's, and resolved that any further projects of mine would be wholly independent.

When did my interest in science become more serious? It really wasn't too serious, I'd have to admit, until still another change of location. My parents, realizing in 1937 that the move to the Bronx had not been a success, decided to move to an apartment at the north end of Manhattan. We lived in a more spacious neighborhood there and across a peaceful street from Inwood Park—the only uncultivated area left in Manhattan. The school I attended there for the ninth grade, which was nominally the first year of high school, was materially less boring than the prior year's. Algebra was an altogether new beginning and even a redemption for mathematics. It was finally freed of all that dismal arithmetic. That was a joy more than sufficient to overcome the uselessness of so many of the procedures that the curriculum did include. Who, even in those days, could imagine seriously needing to carry out long division of lengthy polynomials, or see any need to teach that procedure to children? No one with experience beyond teacher training could have been responsible for that curriculum.

But general science was another of the subjects we studied, and the energy and enthusiasm of its young teachers more than made up for its attenuated subject matter. I had read an elementary book on astronomy by that time and had been taken by my Aunt Sarah on an exciting visit to the Hayden Planetarium. I found that I could easily visualize the diurnal motions of the stars, the monthly motion of the moon, and somewhat more sketchily the motions of the readily visible planets, Jupiter, Saturn and Venus. The images associated with astronomy quickly captured my imagination, and I began to read about it everything I could find.

The encyclopedia had some simplistic diagrams of how a telescope works, and they seemed to assure me that I could build one from some ordinary magnifying glasses I had accumulated. I did that and was amazed by the rainbow colored edges I saw on the image of the moon and presently dismayed by its overall fuzzy quality. It took a bit of reading to discover what the trouble was—chromatic aberration, endemic to all such primitive refractors. The cure—the only one accessible to me—would be to build a reflecting telescope. But that would be a long-term project, fortunately one that had already been pioneered by quite a few adult amateur astronomers. There was a book, in two volumes, in fact, that drew together the experiences of several amateurs and gave a good deal of guidance, if not detailed instruction, for grinding, polishing and figuring the mirror, and for constructing the remaining optical elements and the mounting. Going through the entire procedure required nine months of work. Coarse and fine grinding of the mirror took only a couple of weeks, but polishing and figuring it to its final shape consumed months. Building a stable yet flexible mounting for the telescope, one that would permit me to follow objects in the sky, compensating for the earth's rotation was another matter entirely. I had only a few hand tools appropriate to woodworking, and still no access to machine tools of any sort. Constructing the wooden cell to house the mirror involved strenuous use of a coping saw and a wood file for several days on end. The steel polar axis for the equatorial mounting was originally the steering shaft of a Ford car. The proprietor of the junkyard I found it in was happy to give it to me free. But I had somehow to put a 41° bend into that shaft, to equal the latitude of New York. I drew an outline of the bend I needed on a sheet of asbestos and took it off together with the three foot shaft to a garage that I knew refashioned truck housings. The owner was tickled by the project, heated the shaft in his forge till it glowed brightly and pounded it into the precise shape I needed. It must have taken him a good three-quarters of an hour altogether, and I felt I owed him payment for his time. He thought the matter over, and I recall his smile as he said that would come to 25 cents. In fact I got a good deal of aid over those months from people who were pleased to help an ambitious kid with virtually no money to spend on his projects. My accumulated savings of $10 were no more than half spent during those nine months.

Stability of the telescope mounting demanded that it be fairly massive but not too heavy to be carried by hand. The only way I could use it, after all, was to carry it upstairs to the roof of the apartment building. A weight of 40 or 50 pounds seemed appropriate for the base of the mounting, and I would have to make it of cast concrete. A schoolmate kindly brought me a sack of cement and a bag of sand contributed by his father, a local contractor. I fashioned a mold of the appropriate shape from recycled box wood and filled it with the concrete mixture called for by the instructions on the cement sack. The only place available to me for the casting operation was the wooden floor of my bedroom, between my bed and work desk. I had taken some precautions against the leakage of a little water by covering the floor first with waxed paper and a layer of newspaper. My understanding of the setting of concrete was that some miracle of chemistry would incorporate all of the water into the finally hardened product, with none left to leak out onto the floor. That is how plaster of Paris had hardened. But the result was a memorable lesson. I couldn't say what fraction of the hardening was eventually due to drainage, but it must have been appreciable. I had to spend an entire day mopping up pools of water around the hardening mass. I suppose when concrete sidewalks harden their leakage just seeps into the ground below. In my case it would have been the apartment downstairs.

Observing with the telescope wasn't too easy either. In winter the apartment house roof was cold and often windy. Because of the city lights the sky was rarely dark enough to permit seeing the fainter objects, usually diffuse nebulae or distant galaxies. Still there were the thrilling topography of the moon, frequent views of the major planets and countless planetary nebulae, double stars, and clusters of all sorts. Lacking the means to find the fainter objects mechanically, I had to go about tracking them down by locating their positions on star maps relative to the brighter stars or objects easier to find. By putting in at least a little time on most clear nights I managed over the next year or two to find most of the hundred or so extended objects catalogued by the Italian astronomer Messier. I even managed to fashion a film holder and cardboard shutter for the telescope so that I took through it a sequence of moon pictures during the lunar eclipse of November 8, 1938.

The possibility of performing optical tests as exquisitely sensitive as the Foucault test of the telescope mirror's figure with even the most primitive sorts of equipment convinced me that optics was full of miracles. Some other miracles I had seen involved the mysteries of light polarization. The Polaroid Corporation was sponsoring an exhibit I had visited at the Museum of Science and Industry at Rockefeller Center that showed, among other things, the remarkable colors that appeared in transparent materials like cellophane when seen between crossed sheets of Polaroid film. How could I procure any of the magical Polaroid film? That seemed hopeless for a 12-year old, but I had heard of the possibility of light polarization by reflection. The best reflectors for the purpose would be smooth and black—to avoid the complications of transmission. My father, who at that time in 1937 was selling jewelry displays made of just such black glass, found me several rectangular pieces of the right size. I was able then to mount all the optical elements, including a 25 watt light bulb within a cigar box and use the device to reveal the same sorts of polarization phenomena I had seen at the museum. Seeing the unseen in that way turned out to be as much of a thrill as any I had with the telescope.

In the late 30s an organization with the imposing name The American Institute of the City of New York began organizing activities for young people interested in science. They held science congresses during the Christmas vacations and science fairs during the spring school break, both at the Museum of Natural History. The science congresses were patterned after professional scientific meetings, and split just as incoherently into many sessions, according to fields and specialties. Each session had several ten minute talks presented by the kids as contributed papers. One of those presentations in 1937 was my own description of the plans for the forthcoming 200-inch telescope at Mt. Palomar. It was a visionary image that kept my spirits up while I was having troubles of my own building my 6-inch diameter telescope. The sponsoring institute saw to it that our talks were attended by at least a sprinkling of mature scientists whom they could somehow persuade to volunteer. I was flattered that my own talk was attended, if only briefly, by Dr. Clyde Fisher, the curator of the Hayden Planetarium. One of his assistant lecturers, Dorothy Bennett, stayed for the whole ten minutes and dropped a suggestion to me that added immensely to my experience over the next four years.

Dorothy Bennett was something of a wonder. Seeking a career in New York, she had arrived there as a fresh graduate of the University of Minnesota just in time for the economic debacle of 1929. With boundless energy and no prior acquaintance with astronomy she found a position working on the plans for the city's new planetarium. One of her many inspirations was to begin in 1930 a citywide astronomy club for kids of high school age. It met on Saturday evenings biweekly, in an imposing auditorium on the top floor of the Roosevelt Memorial building, adjacent to the Planetarium. There the kids, who came in by subway from the far reaches of the city, heard invited lectures by real astronomers. It was that club that Miss Bennett suggested I try attending. I was indeed excited by it and caught up in it from the first meeting I went to. It then formed a large part of my life till I went off to college.

Watched over by Dorothy in a kind of godmotherly role, the Junior Astronomy Club actually had a permanent office in a former watchman's apartment in the basement of the Roosevelt Memorial. There it held committee meetings, originated large mailings to the membership and ground out its monthly mimeographed publication, the Junior Astronomy News. I rushed to take part in all of those programs, ceaselessly amazed that the club could manage all of its activities on dues that only came to 25 cents per year. The secret of that miracle was that Dorothy had assigned to the club the royalties of a book she had inspired, *The Handbook of the Heavens*, and the proceeds from the sales of a rotating star map, a planisphere she designed. Enough copies of those publications had been sold to keep the club afloat for over ten years. Dorothy left the planetarium for a position in the publishing industry in 1939, entrusting supervision of our club to a group of its older alumni, who carried on the tradition for quite a few years more.

I often wondered what happened to Dorothy in the years after that. She didn't just vanish into the publishing world, I found. Within a couple of years she had become the originator and editor of the Little Golden Books of Simon and Schuster. Those small paperbacks, devoted at first to assorted topics in natural science or history, became one of the wonders of the publishing industry. They were colorfully illustrated and were sold in vast numbers at newsstands and stores everywhere. Countless kids must have owed their knowledge of fossils, seashells, or trees to those books and to Dorothy. When eventually the publishers decided to extend their franchise into more commercial and less educational material, Dorothy left them and took up a succession of new careers in archaeology and ethnography. Her adventures extended to many other novel areas of public education.

In September 1938 a new high school was opened by the city, with the declared intention of providing a more extensive background in science. That school, the Bronx High School of Science was to have an entrance examination and a freshly chosen staff of young teachers. It was established however in an old building still used as an annex for a traditional local school, and three years had to pass before its growing student body had displaced the more disaffected population originally present. It was interesting being a pioneer in this way, but not without problems. Although the two populations didn't overlap in classes they did –and experienced friction—everywhere else.

My choice of this high school required long trolley car rides between upper Manhattan and the Bronx, but it proved fortunate in several respects. The kids were better informed about most things than average high school kids, and were often interesting to talk to. Not many of them entertained ambitions of becoming scientists however. They were there, mainly, it seemed, in search of somewhat higher educational standards. The lawyers, doctors and businessmen who emerged from my cohort, in fact, greatly outnumbered the handful of eventual scientists. Although all high schools offered some elective courses, it would have been difficult in most of them, to take both a science and a math course in each year. If we were able to do that, it was at the expense of studying Latin or taking a second modern language course. I was more than pleased at the time by those omissions, but have come to regret them since.

Whatever may have been the weaknesses of the school's physical plant or its curriculum, the faculty members seemed to make up for them. They were mostly young, energetic and unjaded. We seemed to have the depression years to thank for that. Most of the teachers had graduated from the tuition-free city colleges during the early 30s and, seeing no future in continuing their studies, had taken refuge in positions with the school system. The subjects they taught, like European history and economics, seemed to have real substance, for a change, and mathematics stood out among them. It was the real thing, not just an introduction one would have to repeat and improve upon in college. When algebra became more serious in the second year of high school it became more interesting. My teacher in intermediate algebra, Samuel Altwerger, appreciating my involvement with astronomy and my growing enthusiasm for mathematics, suggested that it might be a good idea for me to learn calculus. He assured me I could learn it just by reading a textbook. He gave me one small book for that purpose and borrowed a larger one for me from the library. I found, to my surprise, that he was right. I had no trouble with these and absorbed an understanding of elementary calculus quickly. In fact that was well before I really needed calculus, but the experience marked a kind of turning point for me. I had never felt inclined toward mathematics before, but what I had learned by the time I reached college permitted me to skip several elementary courses there.

However much I came to like mathematics, my passion was still building optical instruments. I had been reading about the pivotal role played by spectroscopy in developing an understanding of atoms, and I resolved to build a spectroscope myself. Most of its parts would have to be made of metal, and that meant even more numbing use of hand files, this time not on wood, but brass. It wasn't difficult putting the spectroscope together. Neither its structure nor its optics presented other problems. But there was one central element missing. I had neither a prism nor a diffraction grating to use as the dispersive device that generates the spectrum. Fortunately the principal of the new high school, Dr. Morris Meister, had been given a replica diffraction grating as a graduation present, and he was happy to loan it to me. That spectroscope, entered in the 1939 science fair, won two prizes. I had very little chance to use it after that, since the American Institute exhibited it over many months in a display case at the New York World's Fair of 1939 and its repetition in 1940.

The Junior Astronomy Club also had an involvement in those World's Fairs. Part of the extensive Westinghouse exhibit was devoted to the scientific hobbies of kids of high school age. I was happy to organize demonstrations of the grinding of telescope

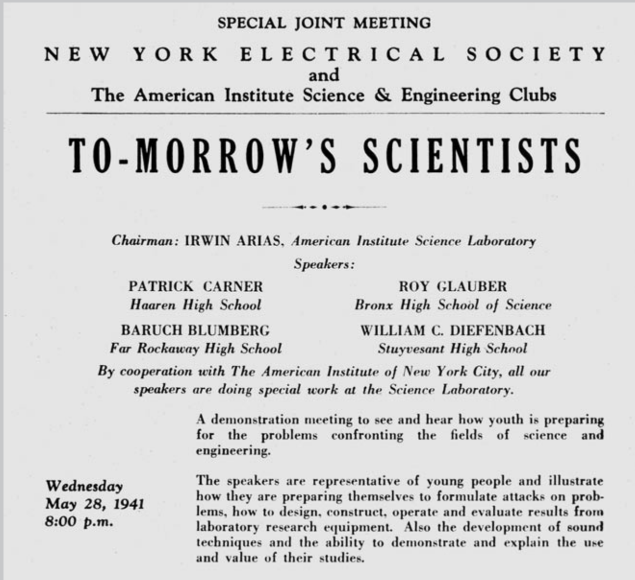
Taken at December 1940 Science Congress talk. At left is a 6 inch richest-field telescope f4, much more portable than my original f8, in the center is the diffraction grating spectroscope, at right a photomicrographic camera, with odd bits of paraphernalia in the foreground.

mirrors for the exhibit and enlist a succession of our club members, each to spend a week or two on public display at the task. When my own turns came I became good friends with the young chemist who worked next to me, notwithstanding the shower of ashes his synthetic volcano blew over my optical surfaces. Young Frank Pierson never did become a chemist. He became a well-known screen writer and for several years president of the Academy of Motion Picture Arts and Sciences.

In the Science Congress of 1939, I gave a talk that presented some of the photographs I had managed to take through my telescope, my spectroscope, and through a borrowed microscope. It won one of the prizes, a visit to the Westinghouse Corporation in Pittsburgh, Pennsylvania, where I had a chance to visit their "atom smasher", a vertically mounted Van de Graaf generator, and to talk briefly with a couple of real scientists, including a well-known theorist, E. U. Condon. Then, as a climax to the trip, I was ushered into the office of the president of the corporation. He promptly drew from his top desk drawer a tattered old pocket notebook. It was his official record, he explained, of the hours he had worked for the company at the turn of the century for a wage of only a few cents an hour.

The junior year of high school meant starting to think about going to college. The teacher assigned as my guidance counselor, thinking perhaps of the experience of his colleagues, assured me that there were too few positions available anywhere for astronomers or physicists, and that I would be best off going to an engineering school. He felt I should apply to a range of them, but he saw Rensselaer Polytech as the ideal compromise. The father of my best friend, a Harvard graduate himself, gave me rather different advice. Disappointed at the unlikelihood of his own son's admission to Harvard, he guessed that I might make it. More to the point, he suggested that scholarship support could be available. Neither my parents nor I would otherwise have been so presumptuous as to imagine that large leap in social status. I did fill out the lengthy Harvard applications, however, and took the several required examinations. The application for the scholarship awarded by the New York Harvard Club involved a searching interview conducted in a large, oak-paneled room by a dean and half a dozen club member contributors. I was eventually admitted to a number of colleges, including Rensselaer, but without scholarship aid. Harvard, on the other hand, granted me a Harvard Club scholarship, while making it clear that there were many more exams to take before I would be declared admitted.

Beginning at Harvard in the fall of 1941 meant suddenly being treated like a member of the gentry. We had waiter service at our dining tables and daily printed menus listing alternative dishes. Of course, some fraction of the waiters were fellow classmates, working for board. Our society was stratified in many other ways as well. The rents for the dormitory rooms were graded according to their location, with the result that the scholarship students were clustered in the less desirable areas. They never got to meet the occupants of the higher priced real estate. I scarcely minded any of that. I had come from a different world than those normal Harvard students. College was for them primarily a social experience, overlaid by a burden of course work. For me, on the other hand, having skipped a couple of grades along the way, and some two years younger than most of my classmates, it was the other way around. I enjoyed a few social contacts, but worked hard at my



SPECIAL JOINT MEETING

NEW YORK ELECTRICAL SOCIETY

and

The American Institute Science & Engineering Clubs

TO-MORROW'S SCIENTISTS

Chairman: IRWIN ARIAS, *American Institute Science Laboratory*

Speakers:

PATRICK CARNER
Haaren High School

ROY GLAUBER
Bronx High School of Science

BARUCH BLUMBERG
Far Rockaway High School

WILLIAM C. DIEFENBACH
Stuyvesant High School

By cooperation with The American Institute of New York City, all our speakers are doing special work at the Science Laboratory.

A demonstration meeting to see and hear how youth is preparing for the problems confronting the fields of science and engineering.

Wednesday
May 28, 1941
8:00 p.m.

The speakers are representative of young people and illustrate how they are preparing themselves to formulate attacks on problems, how to design, construct, operate and evaluate results from laboratory research equipment. Also the development of sound techniques and the ability to demonstrate and explain the use and value of their studies.

May 1941 announcement of talks at the Electrical Engineering Auditorium by four high school students. My talk was about photographs I had taken with the instruments I had shown at the 1940 Science Congress. The talk by Baruch Blumberg dealt with a model refrigerator he had constructed. He didn't continue with physics, however, and switched to medicine, winning the Nobel Prize in 1976 for the discovery of the hepatitis B virus. The chairman, Irwin Arias, also turned out to be a distinghuished hepatologist.

studies, finding them demanding at times, but on the whole well planned and satisfying.

That freshman year was punctuated on December 7 by the Japanese attack on Pearl Harbor and by the entry the next day of the U.S. into the war in Europe as well as the Pacific. The next few months saw significant changes in our lives as students. The rather searching physics course I was taking had been planned as the first half of a two year cycle. Because faculty members were departing for war work the remaining year-long course would not be offered as planned. It would instead be packed into the second semester of the first-year course. That proved to be quite a tall order but a fast way of learning.

The entire school then began operating during the summer and accelerating its course programs with the thought of providing as much education as possible before the young men left for the armed forces. In the meantime Harvard's dining halls lost their graciousness and were transformed into the cafeteria-style mess halls; they have been ever since. The draft age, then 21, was presently lowered to 18 and the university began losing students in large numbers. With its faculty depleted the Physics Department announced that its graduate courses were shortly to be given for the last time "for the duration." That announcement made it a good idea to jump directly into the graduate courses, skipping the intermediate ones which had looked neither demanding nor very interesting anyway. It was with the war thus cracking the whip that I managed to assimilate most of the courses of a graduate school education by the time I turned 18 in September 1943. At that point I felt ready for war work myself and filled out a questionnaire sent out by an agency called the National Roster of Scientific Personnel. Its purpose was to ascertain scientific training and try to place people accordingly.

The armed forces by that time had become vastly larger than the country's immediate needs. The Army developed what it called a Specialized Training Program, in effect for storage of its legions in the universities for a year or so, until they would be needed in the invasion of Europe. The program exposed a large population of draftees to college courses for the first time and was a productive experiment in education. I was given a position teaching elementary physics in the program and had my hands full doing that along with taking a full program of courses of my own.

Then one day in October 1943, a stranger in a dark suit appeared in the Physics Department office evidently asking for me. He introduced himself as a Mr. Trytten from Washington, D.C. and asked to speak privately to me. We withdrew to a faculty meeting room in which the blinds were never raised. Closing the door, he asked if I would be interested in joining a new project that was engaged in interesting work. That it was "out west" was the most he would tell me about either the location of the place or what it was doing. It sounded fascinating nonetheless, and I found the security questionnaires he put before me easy to fill out. Having so little prior history helped. His seeking me out seemed to relate to my having filled out the National Roster blanks. It was then a matter of several weeks before my security clearance had been completed and I was instructed to send whatever belongings I needed to the now famous Post Office Box 1663, Santa Fe, New Mexico. In my case it was a trunk sent not by mail but by Railway Express. There were many occasions, then and later to imagine what a capacious P.O. box that one must be.

I could find many tiny hints at what was going on out there, all of them questionable and several, as it later became clear, completely wrong. The most solid hint was in fact a negative one. For about two years after the discovery of uranium fission in 1939 there had been occasional notes in the New York Times speculating on the possibility of starting a chain reaction. They had stopped appearing, it was hard to say just when, but at least two years earlier.

So I had no idea whether it had become a dead issue, or my offer of a position implied some real progress toward a chain reaction.

The train ride from New York to Lamy, New Mexico, the stop for Santa Fe, consumed two and a half days. A driver from Los Alamos had come to the station principally to meet a short man in a black overcoat, but took me along, stopping first at an unassuming project office in Santa Fe, where I learned that the man in the overcoat was John Von Neumann, a legendary mathematician.

The ride from Santa Fe up to "the Hill" was an experience I shall never forget. First there was the breathtaking scenery of the canyons of the Pajarito Plateau. Then there was my fellow passenger, John Von Neumann, who engaged in a lively conversation for most of the trip with the driver, whom I learned only later was a mathematician who had worked with "Johnny" earlier. With a thought perhaps of maintaining security, they discussed some calculations underway using the most outlandish mathematical terminology, and describing mathematical errors in physical terms that I knew represented physical impossibilities. The ride was an incredible mixture of visual thrills and intellectual enigmas.

I was astonished, shortly after arrival at the project, to be told that the chain reaction had long since been achieved in Chicago and the present intention was to construct a reaction fast enough to be a bomb. It was disturbing news and I recoiled from it at first, but the challenges and uncertainties involved helped reconcile me to it. More importantly, I felt, as everyone else on the project did, that whatever these uncertainties might be, the Germans, possessing the same understanding we had, were likely to be working on the bomb as well. And if they reached that goal before we did they would not be sentimental about using it to stave off eventual defeat. That fear applied only to the known expertise of the Germans. The conflict with Japan didn't appear to motivate anyone's involvement in the project.

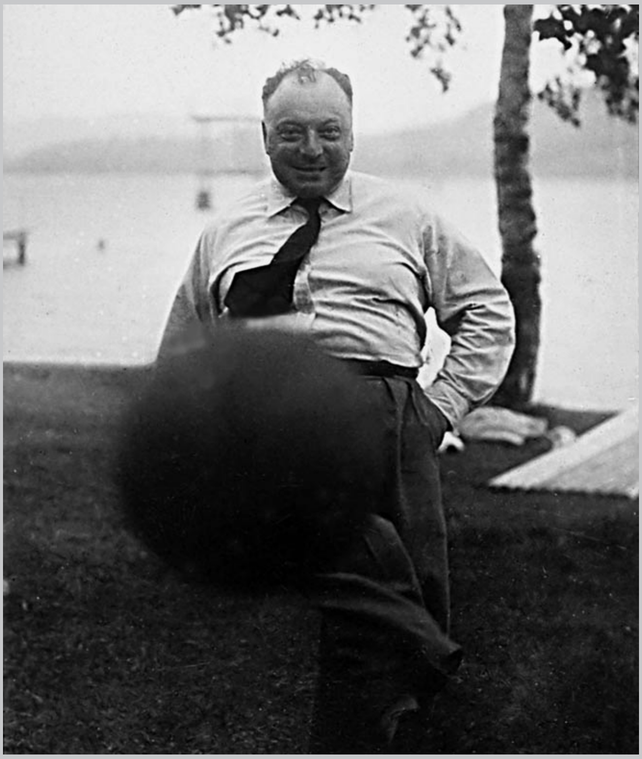
The project was only a few months old when I joined it but most of its eventual leaders were already there. Not many were yet well-known. They were remarkably youthful. Oppenheimer in his late thirties was one of the oldest. He had a universal understanding of the work and an eloquence in describing it that kept us spellbound. Hans Bethe, the leader of the theoretical division, had a penetrating understanding that seemed capable of formulating absolutely anything quantitatively and evaluating it effortlessly, an aura he maintained even many years later. Feynman was there as leader of a small theoretical group. He was often cantankerously teasing the security people. His lectures were always offbeat performances demonstrating novel approaches to problems in ways devoted as much to entertainment as to the technical message. There were others, too many to mention, and among them as an occasional visitor, Niels Bohr, whom we called Nicholas Baker for obvious reasons, together with his son Aage.

Overwhelmed by these giants, my own position in the Theory Division at age 18 was a modest one. There were many problems in neutron diffusion such as finding the critical mass that required more careful formulations than had been carried out in the earliest projections. I worked on those for the better part of the two years I spent at Los Alamos and wrote three lengthy secret papers on those subjects.

There were many delays before the Trinity Test of the bomb in July 1945 and with them the uncertainty of how well it would work grew steadily. Unable to secure a position among the experimenters at Alomogordo, I had to be content with watching for the flash from the top of Sandia Peak near Albuquerque. I saw the flash indeed and some of the glow that followed from a distance of over a hundred miles. The test was followed by some tense days, leading up to August 6, when the use of the bomb at Hiroshima was announced. One thing the portentous announcement meant was a certain release from secrecy. We could now resume contact with the outside world. We could say, if only in general terms what we had been working on. But there were no celebrations of any sort until the war was over a few days later.

Resuming the life of an undergraduate at Harvard early in 1946 proved surprisingly difficult, even though I needed only a few credits to graduate. Having had a team of assistants to do calculations for me at Los Alamos didn't make it any easier, I found, to do my own homework back at school, particularly when I felt I had moved beyond all that. Fortunately that time was brief, and then I became a graduate student. But I had already taken most of the graduate courses on offer, and so was largely left on my own, being allowed to register, in effect, for independent reading and research. The principal reason for my remaining at Harvard was the addition of Julian Schwinger to the faculty. I had met him during a brief appearance he made at Los Alamos, late in 1945, and was immediately so impressed with his knowledge and his incredibly informative lecturing style that I felt he was unique among teachers and would be the ideal thesis advisor as well. I became friendly with Julian over the next three years and was never less than amazed by his ability to construct elegant mathematical structures that would permit him to see further than any of his contemporaries. There were times in those postwar years when it seemed he was responsible for most of the progress in theoretical physics, and very likely would be for years to come. His lectures were brilliantly delivered and notes on them were highly prized and reproduced wherever they could be found. Many students crowded in to work with him, however, and he limited the time he spent with them, so they didn't always produce great theses. Though nominally registered to work with Julian, I actually worked by myself and produced in 1949 a quantum field theoretical thesis that was useful to my later development, but scarcely much better than the others of the day.

Robert Oppenheimer, who seemed to know more of me than I had imagined, invited me to spend my first postdoctoral year in Princeton at the Institute for Advanced Study. The group of 20 or so postdocs who gathered there included quite a few eventual leaders of the postwar generation of theorists. None had a stable position anywhere else and so the atmosphere was quite competitive.



Life with Wolfgang Pauli; a Spring 1950 outing. Prepared to photograph Pauli kicking the ball into the lake, as he had done earlier, I stood to one side, carefully aiming the camera at him. Pauli indeed kicked the ball, and I managed to snap the shutter just before the camera hit me squarely in the face.

In the first term of 1950, Wolfgang Pauli was scheduled to visit the Institute. Following the advice of friends who had worked in Zürich, I arranged with Pauli to return with him to Zürich and work with him until the fall of that year when I would return to Princeton and the Institute. Having a few months to live in Zürich and to travel over Europe was the principal experience of that encounter. Pauli at age 50 had relaxed into the role of a critic and was no longer inspiring much research. He did retain a mordant sense of humor, however, and was forever doing his best to tease me. Teasing others as well, if not insulting them outright, he was always interesting to be around.

After another year I spent at the Institute, Oppy found me a teaching position. It was only a temporary one, replacing Feynman at Caltech. Feynman was to spend the year in Brazil, where by his own account, he worked hard on the bongo drums, and Caltech needed someone to teach quantum mechanics. The chemistry department out there, under Linus Pauling, seemed to be an exceptionally active one. My research for the year was devoted to resolving a puzzle they had encountered in studying electron diffraction by molecules. Solving the problem didn't interest me in molecules very much, but it did involve me deeply in scattering problems in which the incident particles were of wavelength much smaller than the ranges of interactions. Those problems, I understood, would become steadily more important in nuclear physics as accelerator energies were increased. I continued studying those prob-

At the Les Houches Summer School, July 1954. Don Hughes spoke about neutron physics. Fermi about pion scattering, and I lectured on particle collision theory.

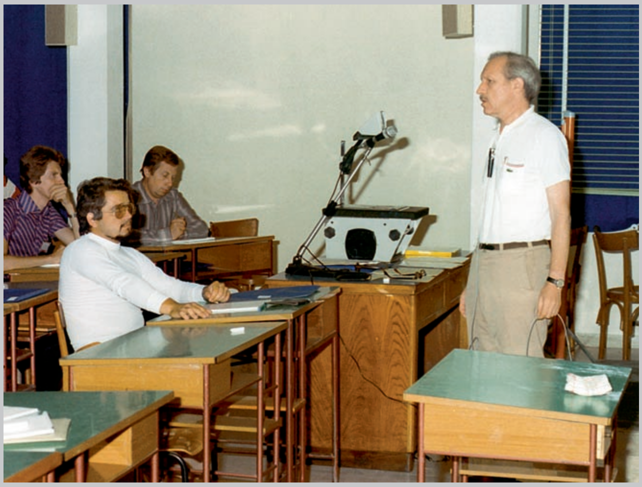
lems then when I was invited back to Harvard in the fall of 1952 and for some years after that. The result was a species of nuclear diffraction theory analogous in some ways to optical diffraction theory, but generalized to include inelastic collisions between incident particles and complex nuclear systems. The theory is even used these days to treat the high-energy collisions of pairs of heavy nuclei.

Once I was back at Harvard I began to climb the academic ladder of professorial positions and was able to direct the thesis work of a number of gifted students. Theoretical physicists weren't nearly as specialized in those days as they are now. All of theory was considered one's province and so those theses ranged over half a dozen fields, as did my own work.

The late 50s proved to be an exciting time for many reasons. A radically new light source, the laser, was being developed and there were questions in the air regarding the quantum structure of its output. That was particularly so in view of the surprising discovery of quantum correlations in ordinary light by Hanbury Brown and Twiss. A second source of excitement, all my own, was that I had met the young woman I was to marry, Cynthia Rich, and had been going out with her since 1957. We married in July 1960, bought a contemporary house a year later, and settled into quite a happy life together. That was the period in which I began to work on quantum optics with a surmise that the Hanbury Brown–Twiss correlation would be found absent from a stable laser beam, and then followed it with a sequence of more general papers on photon statistics and the meaning of coherence.

Our first child, a son Jeffrey, arrived in 1963. I remember feeling his arrival was a kind of redemption, a species of renewal for which I was more than grateful at age 38. I was doing a good deal of traveling in those days, particularly during vacations, and it always amazed me how transportable the baby was. We had no trouble taking him on short domestic trips anywhere, but thinking back on my own experience perhaps, waited till he was nearly 4 before taking him on a longer trip to Geneva for a sabbatical at CERN. My work in this period gravitated back to high-energy collision theory, since experiments had begun to reveal many of the results my diffractive multiple scattering theory had predicted.

Our second child, a daughter Valerie, didn't arrive until 1970, and by that time our placid and comfortable academic life had been roiled up in many ways. Years of demonstrations against the Vietnam War, the anguish of the black liberation movement, and finally the bitter recriminations of militant feminism had left the world of our university seriously fragmented. My wife, joining with the militants, decided that the days of traditional marriage were over, and that her own should be one of the first to go. The law, she found, would permit her to end it, of her own choice, while retaining custody of the children. Devastated by her decision, I simply couldn't believe she would hew to it, and it took some time to try to reach a settlement. By that time, indeed, she no longer sought active custody of the children, and having taken care of them earlier, I proceeded thereafter to raise them as a single father. It was a time-consuming occupation, but an immensely rewarding one, and I managed fortunately to remain involved and reasonably productive in my work. I'm sure there is some number of papers I never got to write as a result, but raising those children and seeing them succeed was not an experience I would trade for the missing papers or any sort of recognition. Both Jeff and Val have families of their own now and are busy raising my grandchildren. I envy them that privilege, and wish I had the opportunity to be raising them all myself.



Spreading the gospel at a July 1977 summer school organized by F. T. Arecchi at Villa le Pianore, Versilia, Italy. The message considerably outlasted the moustache.

end of a spring. They had to be electrically charged. He knew from Maxwell exactly how these charged oscillators interact with the electromagnetic field. They both radiate and absorb in a way he could calculate. So then he ought to be able to find the equilibrium between these oscillators and the radiation field, which acted as a kind of thermal reservoir—and which he never made any claim to discuss in detail.

He found that he could not secure a derivation for his magic formula for the radiation distribution unless he made an assumption which, from a philosophical standpoint, he found all but unacceptable. The assumption was that the harmonic oscillators he was discussing had to possess energies that were distributed, not as the continuous variables one expected, but confined instead to discrete and regularly spaced values. The oscillators of frequency ν would have to be restricted to energy values that were integer multiples, that is, n-fold multiples (with $n=0, 1, 2, 3\ldots$) of something he called the quantum of energy, $h\nu$.

That number h was, in effect, the single number that he had to introduce in order to fit his magic formula to the observed data at a single temperature. So he was saying, in effect, that these hypothetical harmonic oscillators representing a simplified image of matter could have only a sequence amounting to a "ladder" of energy states. That assumption permits us to see immediately why the thermal radiation distribution must fall off rapidly with rising frequency. The energy steps between the oscillator states grow larger, according to his assumption, as you raise the frequency, but thermal excitation energies, on the other hand, are quite restricted in magnitude at any fixed temperature. High-frequency oscillators at thermal equilibrium would never even reach the first step of excitation. Hence there tends to be very little high-frequency radiation present at thermal equilibrium. Planck presented this revolutionary suggestion[2] to the Physical Society on December 14, 1900, although he could scarcely believe it himself.

The next great innovation came in 1905 from the young Albert Einstein, employed still at the Bern Patent Office. Einstein first observed that Planck's formula for the entropy of the radiation distribution, when he examined its high-frequency contributions, looked like the entropy of a perfect gas of free particles of energy $h\nu$. That was a suggestion that light itself might be discrete in nature, but hardly a conclusive one.

To reach a stronger conclusion he turned to an examination of the photoelectric effect, which had first been observed in 1887 by Heinrich Hertz. Shining monochromatic light on metal surfaces drives electrons out of the metals, but only if the frequency of the light exceeds a certain threshold value characteristic of each metal. It would have been most reasonable to expect that as you shine more intense light on those metals the electrons would come out faster, that is with higher velocities in response to the stronger oscillating electric fields—but they don't. They come out always with the same velocities, provided that the incident light is of a frequency higher than the threshold frequency. If it were below that frequency there would be no photoelectrons at all.

The only response that the metals make to increasing the intensity of light lies in producing more photoelectrons. Einstein had a naively simple explanation for that.[3] The light itself, he assumed, consists of localized energy packets and each possesses one quantum of energy. When light strikes the metal, each packet is absorbed by a single electron. That electron then flies off with a unique energy, an energy which is just the packet energy $h\nu$ minus whatever energy the electron needs to expend in order to escape the metal.

It took until about 1914–1916 to secure an adequate verification of Einstein's law for the energies of the photoelectrons. When Millikan succeeded in doing that, it seemed clear that Einstein was right, and that light does indeed consist of quantized energy packets. It was thus Einstein who fathered the light quantum, in one of the several seminal papers he wrote in the year 1905.

To follow the history a bit further, Einstein began to realize in 1909 that his energy packets would have a momentum which, according to Maxwell, should be their energy divided by the velocity of light. These presumably localized packets would have to be emitted in single directions if they were to remain localized, or to constitute "Nadelstrahlung" (needle radiation), very different in behavior from the broadly continuous angular distribution of radiation that would spread from harmonic oscillators according to the Maxwell theory. A random distribution of these needle radiations would look appropriately continuous, but what was disturbing about that was the randomness with which these needle radiations could appear. That was evidently the first of the random variables in the quantum theory that began disturbing Einstein and kept nettling him for the rest of his life.

In 1916 Einstein found another and very much more congenial way of deriving Planck's distribution by discussing the rate at which atoms radiate. Very little was known about atoms at that stage save that they must be capable of absorbing and giving off radiation. An atom lodged in a radiation field would surely have its constituent charges shaken by the field oscillations, and that shaking could lead either to the absorption of radiation or to the emission of still more radiation. Those were the processes of absorption or emission induced by the prior presence of radiation. But Einstein found that thermal equilibrium between matter and radiation could only be reached if, in addition to these induced processes, there exists also a spontaneous process, one in which an excited atom emits radiation even in the absence of any prior radiation field. It would be analogous to radioactive decays discovered by Rutherford. The rates at which these processes take place were governed by Einstein's famous B and A coefficients respectively. The existence of spontaneous radiation turned out to be an important guide to the construction of quantum electrodynamics.

Some doubts about the quantized nature of light inevitably persisted, but many of them were dispelled by Compton's discovery in 1922 that X-ray quanta are scattered by electrons according to the same rules as govern the collisions of billiard balls. They both obey the conservation rules for energy and momentum in much the same way. It became clear that the particle picture of light quanta, whatever were the dilemmas that accompanied it, was here to stay.

The next dramatic developments of the quantum theory, of course, took place between the years 1924 and 1926. They had the effect of ascribing to material particles such as electrons much of the same wavelike behavior as had long since been understood to characterize light. In those developments de Broglie, Heisenberg, Schrödinger and others accomplished literal miracles in explaining the structure of atoms. But however much this invention of modern quantum mechanics succeeded in laying the groundwork for a more general theory of the structure of matter, it seemed at first to have little new bearing on the understanding of electromagnetic phenomena. The spontaneous emission of light persisted as an outstanding puzzle.

Thus there remained a period of a couple of years more in which we described radiation processes in terms that have usually been called "semiclassical." Now the term "classical" is an interesting one—because, as you know, every field of study has its classics. Many of the classics that we are familiar with go back two or three thousand years in history. Some are less old, but all share an antique if not an ancient character. In physics we are a great deal more precise, as well as contemporary. Anything that we understood or could have understood prior to the date of Planck's paper, December 14, 1900, is to us "classical." Those understandings are our classics. It is the introduction of Planck's constant that marks the transition from the classical era to our modern one.

The true "semiclassical era," on the other hand, lasted only about two years. It ended formally with the discovery[4] by Paul Dirac that one must treat the vacuum, that is to say empty space, as a dynamical system. The energy distributed through space in an electromagnetic field had been shown by Maxwell to be a quadratic expression in the electric and magnetic field strengths. Those quadratic expressions are formally identical in their structure to the mathematical expressions for the energies of mechanical harmonic oscillators. Dirac observed that even though there may not seem to be any organized fields present in the vacuum, those mathematically defined oscillators that described the field energy would make contributions that could not be overlooked. The quantum mechanical nature of the oscillators would add an important but hitherto neglected correction to the argument of Planck.

Planck had said the energies of harmonic oscillators are restricted to values n times the quantum energy, $h\nu$, and the fully developed quantum mechanics had shown in fact that those energies are not $nh\nu$ but $(n+{}^1\!/_2)h\nu$. All of the intervals between energy levels remained unchanged, but the quantum mechanical uncertainty principle required that additional ${}^1\!/_2\,h\nu$ to be present. We can never have a harmonic oscillator completely empty of energy because that would require its position coordinate and its momentum simultaneously to have the precise values zero.

So, according to Dirac, the electromagnetic field is made up of field amplitudes that can oscillate harmonically. But these amplitudes, because of the ever-present half quantum of energy ${}^1\!/_2\,h\nu$, can never be permanently at rest. They must always have their fundamental excitations, the so-called "zero-point fluctuations" going on. The vacuum then is an active dynamical system. It is not empty. It is forever buzzing with weak electromagnetic fields. They are part of the ground state of emptiness. We can withdraw no energy at all from those fluctuating electromagnetic fields. We have to regard them nonetheless as real and present even though we are denied any way of perceiving them directly.

An immediate consequence of this picture was the unification of the notions of spontaneous and induced emission. Spontaneous emission is emission induced by those zero-point oscillations of the electromagnetic field. Furthermore it furnishes, in a sense, an indirect way of perceiving the zero-point fluctuations by amplifying them. Quantum amplifiers tend to generate background noise that consists of radiation induced by those vacuum fluctuations.

It is worth pointing out a small shift in terminology that took place in the late 1920s. Once material particles were found to exhibit some of the wavelike behavior of light quanta, it seemed appropriate to acknowledge that the light quanta themselves might be elementary particles, and to call them "photons" as suggested by G. N. Lewis in 1926. They seemed every bit as discrete as material particles, even if their existence was more transitory, and they were at times freely created or annihilated.

The countless optical experiments that had been performed by the middle of the 20th century were in one or another way based on detecting only the intensity of light. It may even have seemed there wasn't anything else worth measuring. Furthermore those measurements were generally made with steady light beams traversing passive media. It proved quite easy therefore to describe those measurements in simple and essentially classical terms. A characteristic first mathematical step was to split the expression for the oscillating electric field E into two complex conjugate terms given in Equations (1) and (2):

$$E = E^{(+)} + E^{(-)}, \tag{1}$$

$$E^{(-)} = (E^{(+)})^*, \tag{2}$$

with the understanding that $E^{(+)}$ contains only positive frequency terms, that is, those varying as $e^{-i\omega t}$ for all $\omega > 0$, and $E^{(-)}$ contains only negative frequency terms $e^{i\omega t}$. This is a separation familiar to electrical engineers and motivated entirely by the mathematical convenience of dealing with exponential functions. It has no physical motivation in the context of classical theory, since the two complex fields $E^{(\pm)}$ are physically equivalent. They furnish identical descriptions of classical theory.

Each of the fields $E^{(\pm)}(\boldsymbol{r}t)$ depends in general on both the space coordinate $\boldsymbol{r}$ and time t. The instantaneous field intensity at $\boldsymbol{r},t$ would then be [Eq. (3)]:

$$|E^{(+)}(\boldsymbol{r},t)|^2 = E^{(-)}(\boldsymbol{r},t)E^{(+)}(\boldsymbol{r},t). \tag{3}$$

In practice it was always an average intensity that was measured, usually a time average.

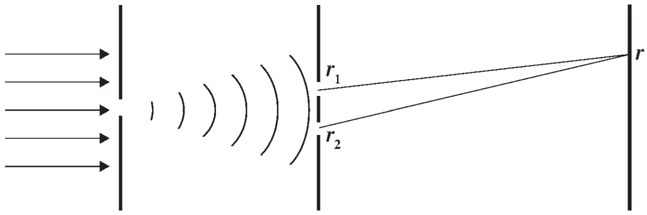


Figure 1. Young's experiment. Light passing through a pinhole in the first screen falls on two closely spaced pinholes in a second screen. The superposition of the waves radiated by those pinholes at $\boldsymbol{r}_1$ and $\boldsymbol{r}_2$ leads to interference fringes seen at points $\boldsymbol{r}$ on the third screen.

The truly ingenious element of many optical experiments, going all the way back to Young's double-pinhole experiment, was the means their design afforded to superpose the fields arriving at different space-time points before the intensity observations were made. Thus in Young's experiment, shown in Figure 1, light penetrating a single pinhole in the first screen passes through two pinholes in the second screen and then is detected as it falls on a third screen. The field $E^{(+)}(\boldsymbol{r}t)$ at any point on the latter screen is the superposition of two waves radiated from the two prior pinholes with a slight difference in their arrival times at the third screen, due to the slightly different distances they have to travel.

If we wanted to discuss the resulting light intensities in detail we would find it most convenient to do that in terms of a field correlation function which we shall define as Equation (4):

$$G^{(1)}(\boldsymbol{r}_1t_1,\boldsymbol{r}_2t_2) = \langle E^{(-)}(\boldsymbol{r}_1t_1)E^{(+)}(\boldsymbol{r}_2t_2)\rangle. \tag{4}$$

This is a complex-valued function that depends, in general, on both space-time points $\boldsymbol{r}_1t_1$ and $\boldsymbol{r}_2t_2$. The angular brackets $\langle\ldots\rangle$ indicate that an average value is somehow taken, as we have noted. The average intensity of the field at the single point $\boldsymbol{r}t$ is then just $G^{(1)}(\boldsymbol{r}t,\boldsymbol{r}t)$.

If the field $E^{(+)}(\boldsymbol{r}t)$ at any point on the third screen is given by the sum of two fields, that is, proportional to $E^{(+)}(\boldsymbol{r}_1t_1)+E^{(+)}(\boldsymbol{r}_2t_2)$, then it is easy to see that the average intensity at $\boldsymbol{r}t$ on the screen is given by a sum of the four correlation functions,

$$G^{(1)}(\boldsymbol{r}_1t_1\boldsymbol{r}_1t_1)+G^{(1)}(\boldsymbol{r}_2t_2\boldsymbol{r}_2t_2)+G^{(1)}(\boldsymbol{r}_1t_1\boldsymbol{r}_2t_2)+G^{(1)}(\boldsymbol{r}_2t_2\boldsymbol{r}_1t_1). \tag{5}$$

The first two of these terms are the separate contributions of the two pinholes in the second screen, that is, the intensities they would contribute individually if each alone were present. Those smooth intensity distributions are supplemented however by the latter two terms of the sum, which represent the characteristic interference effect of the superposed waves. They are the terms that lead to the intensity fringes observed by Young.

Intensity fringes of that sort assume the greatest possible contrast or visibility when the cross-correlation terms like $G^{(1)}(\boldsymbol{r}_1t_1\boldsymbol{r}_2t_2)$ are as large in magnitude as possible. But there is a simple limitation imposed on the magnitude of such correlations by a familiar inequality. There is a formal sense in which cross-correlation functions like $G^{(1)}(\boldsymbol{r}_1t_1\boldsymbol{r}_2t_2)$ are analogous to the scalar products of two vectors and are thus subject to a Schwarz inequality. The squared absolute value of that correlation function can then never exceed the product of the two intensities. If we let x abbreviate a coordinate pair $\boldsymbol{r},t$, we must have the inequality (6),

$$|G^{(1)}(x_1x_2)|^2 \leq G^{(1)}(x_1x_1)G^{(1)}(x_2x_2). \tag{6}$$

The upper bound to the cross-correlation is attained if we have Equation (7):

$$|G^{(1)}(x_1x_2)|^2 = G^{(1)}(x_1x_1)G^{(1)}(x_2x_2), \tag{7}$$

and with it we achieve maximum fringe contrast. We shall then speak of the fields at x_1 and x_2 as being optically coherent with one another. That is the definition of relative coherence that optics has traditionally used.[5]

There is another way of stating the condition for optical coherence that is also quite useful, particularly when we are discussing coherence at pairs of points extending over some specified region in space-time. Let us assume that it is possible to find a positive frequency field $\mathscr{E}(\boldsymbol{r}t)$ satisfying the appropriate Maxwell equations and such that the correlation function (4) factorizes into the form of Equation (8):

$$G^{(1)}(\boldsymbol{r}_1t_1,\boldsymbol{r}_2t_2) = \mathscr{E}^*(\boldsymbol{r}_1t_1)\mathscr{E}(\boldsymbol{r}_2t_2). \tag{8}$$

While the necessity of this factorization property requires a bit of proof,[6] it is at least clear that it does bring about the optical coherence that we have defined by means of the upper bound in the inequality (6) since in that case we have Equation (9):

$$|G^{(1)}(\boldsymbol{r}_1t_1\boldsymbol{r}_2t_2)|^2 = |\mathscr{E}(\boldsymbol{r}_1t_1)|^2|\mathscr{E}(\boldsymbol{r}_2t_2)|^2. \tag{9}$$

In the quantum theory, physical variables such as $E^{(\pm)}(\boldsymbol{r}t)$ are associated, not with simple complex numbers, but with operators on the Hilbert-space vectors $|\,\rangle$ that represent the state of the system, which in the present case is the electromagnetic field. Multiplication of the operators $E^{(+)}(\boldsymbol{r}_1t_1)$ and $E^{(-)}(\boldsymbol{r}_2t_2)$ is not in general commutative, and the two operators can be demonstrated to act in altogether different ways on the vectors $|\,\rangle$ that represent the state of the field. The operator $E^{(+)}$, in particular, can be shown to be an annihilation operator. It lowers by one the number of quanta present in the field. Applied to an n-photon state, $|n\rangle$, it reduces it to an $n-1$ photon state, $|n-1\rangle$. Further applications of $E^{(+)}(\boldsymbol{r}t)$ keep reducing the number of quanta present still further, but the sequence must end with the $n=0$ or vacuum state, $|vac\rangle$, in which there are no quanta left. In that state we must finally have Equation (10):

$$E^{(+)}(\boldsymbol{r}t)\ |vac\rangle = 0. \tag{10}$$

The operator adjoint to $E^{(+)}$, which is $E^{(-)}$, must have the property of raising an n-photon state to an $n+1$ photon state, so we may be sure, for example, that the state $E^{(-)}(\boldsymbol{r}t)\ |vac\rangle$ is a one-photon state. Since the vacuum state can not be reached by raising the number of photons, we must also require the relation [Eq. (11)]:

$$\langle vac|E^{(-)}(\boldsymbol{r}t) = 0, \tag{11}$$

which is adjoint to Equation (10).

The results of quantum measurements often depend on the way in which the measurements are carried out. The most useful and informative ways of discussing such experiments are usually those based on the physics of the measurement process itself. To discuss measurements of the intensity of light then we should understand the functioning of the device that detects or counts photons.

Such devices generally work by absorbing quanta and registering each such absorption process, for example, by the detection of an emitted photoelectron. We need not go into any of the details of the photoabsorption process to see the general nature of the expression for the photon counting probability. All we need to assume is that our idealized detector at the point $\boldsymbol{r}$ has a negligibly small size and has a photoabsorption probability that is independent of frequency so that it can be regarded as probing the field at a well-defined time t. Then if the field makes a transition from an initial state $|i\rangle$ to a final state $|f\rangle$ in which there is one photon fewer, the probability amplitude for that particular transition is given by the scalar product—or matrix element

$$\langle f|E^{(+)}(\boldsymbol{r}t)\ |i\rangle. \tag{12}$$

To find the total transition probability we must find the squared modulus of this amplitude and sum it over the complete set of possible final states $|f\rangle$ for the field. The expression for the completeness of the set of states $|f\rangle$ is given in Equation (13 a):

$$\sum_f |f\rangle\langle f| = 1, \tag{13a}$$

so that we then have a total transition probability proportional to [Eq. (13 b)]:

$$\begin{aligned} |\langle f|\sum_f |f\rangle E^{(+)}(\boldsymbol{r}t)\ |i\rangle|^2 &= \sum_f \langle i|E^{(-)}(\boldsymbol{r}t)\ |f\rangle\langle f|E^{(+)}(\boldsymbol{r}t)\ |i\rangle \\ &= \langle i|E^{(-)}(\boldsymbol{r}t)E^{(+)}(\boldsymbol{r}t)\ |i\rangle. \end{aligned} \tag{13b}$$

It is worth repeating here that in the quantum theory the fields $E^{(\pm)}$ are non-commuting operators rather than simple numbers. Thus one could not reverse their ordering in the expression (13) while preserving its meaning. In the classical theory we discussed earlier $E^{(+)}$ and $E^{(-)}$ are simple numbers that convey equivalent information. There is no physical distinction between photoabsorption and emission since there are no classical photons. The fact that the quantum energy $h\nu$ vanishes for $h\rightarrow 0$ removes any distinction between positive and negative values of the frequency variable.

The initial state of the field in our photon counting experiment depends, of course, on the output of whatever light source we use, and very few sources produce pure quantum states of any sort. We must thus regard the state $|i\rangle$ as depending in general on some set of random and uncontrollable parameters characteristic of the source. The counting statistics we actually measure then may vary from one repetition of the experiment to another. The figure we would quote must be regarded as the average taken over these repetitions. The neatest way of specifying the random properties of the state $|i\rangle$ is to define what von Neumann called the density operator [Eq. (14)]:

$$\rho = \{|i\rangle\langle i|\}_{\rm av}, \tag{14}$$

which is the statistical average of the outer product of the vector $|i\rangle$ with itself. That expression permits us to write the average of the counting probability as Equation (15):

$$\{\langle i|E^{(-)}(\boldsymbol{r}t)E^{(+)}(\boldsymbol{r}t)\ |i\rangle\}_{\rm av} = {\rm Trace}\{\rho E^{(-)}(\boldsymbol{r}t)E^{(+)}\boldsymbol{r}t)\}. \tag{15}$$

Interference experiments like those of Young and Michelson, as we have noted earlier, often proceed by measuring the intensities of linear combinations of the fields characteristic of two different space-time points. To find the counting probability in a field like $E^{(+)}(\boldsymbol{r}_1t_1)+E^{(+)}(\boldsymbol{r}_2t_2)$, for example, we will need to know expressions like that of Equation (15) but with two different space-time arguments $\boldsymbol{r}_1t_1$ and $\boldsymbol{r}_2t_2$. It is convenient then to define the quantum theoretical form of the correlation function (4) as [Eq. (16)]:

$$G^{(1)}(\boldsymbol{r}_1t_1\boldsymbol{r}_2t_2) = {\rm Trace}\{\rho E^{(-)}(\boldsymbol{r}_1t_1)E^{(+)}(\boldsymbol{r}_2t_2)\}. \tag{16}$$

This function has the same scalar product structure as the classical function (4) and can be shown likewise to obey the inequality (6). Once again we can take the upper bound of the modulus of this cross-correlation function or equivalently the factorization condition (8) to define optical coherence.

It is worth noting at this point that optical experiments aimed at achieving a high degree of coherence have almost always accomplished it by using the most nearly monochromatic light attainable. The reason for that is made clear by the factorization condition (8). These experiments were always based on steady or statistically stationary light sources. What we mean by a steady state is that the function $G^{(1)}$ with two different time arguments, t_1 and t_2, can in fact only depend on their difference t_1-t_2. Optical coherence then requires [Eq. (17)]:

$$G^{(1)}(t_1-t_2) = \mathcal{E}^*(t_1)\mathcal{E}(t_2). \tag{17}$$

The only possible solution of such a functional equation for the function $\mathcal{E}(t)$ is one that oscillates with a single positive frequency. The requirement of monochromaticity thus follows from the limitation to steady sources. The factorization condition (8), on the other hand, defines optical coherence more generally for non-steady sources as well.

Although the energies of visible light quanta are quite small on the everyday scale, techniques for detecting them individually have existed for many decades. The simplest methods are based on the photoelectric effect and the use of electron photomultipliers to produce well-defined current pulses. The possibility of detecting individual quanta raises interesting questions concerning their statistical distributions, distributions that should in principle be quite accessible to measurement. We might imagine, for example, putting a quantum counter in a given light beam and asking for the distribution of time intervals between successive counts. Statistical problems of that sort were never, to my knowledge, addressed until the importance of quantum correlations began to become clear in the 1950s. Until that time virtually all optical experiments measured only average intensities or quantum counting rates, and the correlation function $G^{(1)}$ was all we needed to describe them. It was in that decade, however, that several new sorts of experiments requiring a more general approach were begun. That period seemed to mark the beginning of quantum optics as a relatively new or rejuvenated field.

In the experiment I found most interesting, R. Hanbury Brown and R. Q. Twiss developed a new form of interferometry.[7] They were interested at first in measuring the angular sizes of radiowave sources in the sky and found they could accomplish that by using two antennas, as shown in Figure 2, with a detector attached to each of them to remove the high-frequency oscillations of the field. The noisy low-frequency signals that were left were then sent to a central device that multiplied them together and recorded their time-averaged values.

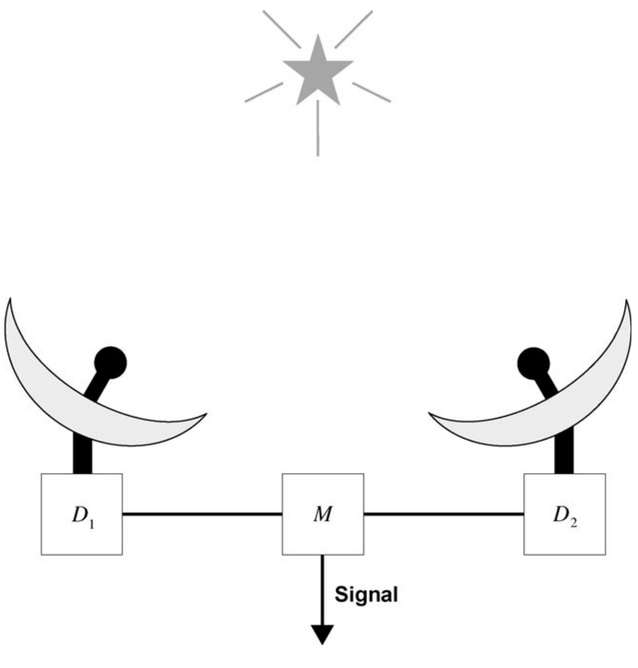


Figure 2. The intensity interferometry scheme of Hanbury Brown and Twiss. Radiofrequency waves are received and detected at two antennas. The filtered low-frequency signals that result are sent to a device that furnishes an output proportional to their product.

Each of the two detectors then was producing an output proportional to the square of the incident field, and the central device was recording a quantity that was quartic in the field strengths.

It is easy to show, by using classical expressions for the field strengths, that the quartic expression contains a measurable interference term, and by exploiting it Hanbury Brown and Twiss did measure the angular sizes of many radiosources. They then asked themselves whether they couldn't perform the same sort of "intensity interferometry" with visible light, and thereby measure the angular diameters of visible stars. Although it seemed altogether logical that they could do that, the interference effect would have to involve the detection of pairs of photons and they were evidently inhibited in imagining the required interference effect by a statement Dirac makes in the first chapter of his famous textbook on quantum mechanics.[8] In it he is discussing why one sees intensity fringes in the Michelson interferometer, and says in ringingly clear terms "Each photon then interferes only with itself. Interference between two different photons never occurs."

It is worth recalling at this point that interference simply means that the probability amplitudes for alternative and indistinguishable histories must be added together algebraically. It is not the photons that interfere physically, it is their probability amplitudes that interfere—and probability amplitudes can be defined equally well for arbitrary numbers of photons.

Evidently Hanbury Brown and Twiss remained uncertain on this point and undertook an experiment[9] to determine whether pairs of photons can indeed interfere. Their experimental arrangement is shown in Figure 3. The light source is an extremely monochromatic discharge tube. The light from that source is collimated and sent to a half-silvered mirror which sends the separated beams to two separate photodetectors. The more or less random output signals of those two detectors are multiplied together, as they were in the radiofrequency experiments, and then averaged. The resulting averages showed a slight tendency for both of the photodetectors to register photons simultaneously (Figure 4). The effect could be removed by displacing one of the counters and thus introducing an effective time delay between them. The coincidence effect

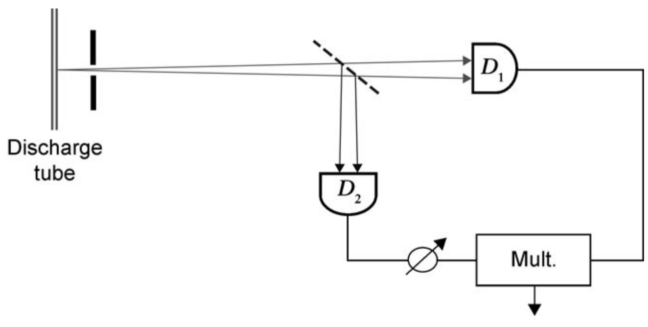


Figure 3. The Hanbury Brown–Twiss photon correlation experiment. Light from an extremely monochromatic discharge tube falls on a half-silvered mirror which sends the split beam to two separate photodetectors. The random photocurrents from the two detectors are multiplied together and then averaged. The variable time delay indicated is actually achieved by varying the distance of the detector D_2 from the mirror.

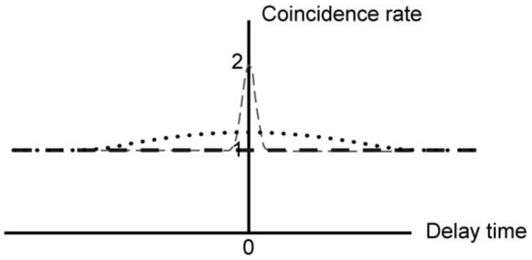


Figure 4. The photon coincidence rate measured rises slightly above the constant background of accidental coincidences for sufficiently small time delays. The observed rise was actually weakened in magnitude and extended over longer time delays by the relatively slow response of the photodetectors. With ideal detectors it would take the more sharply peaked form shown.

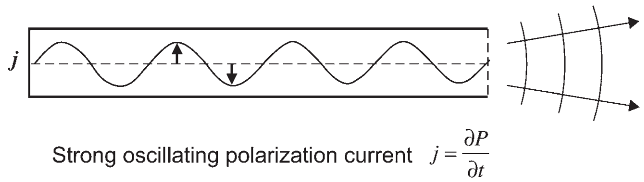


Figure 5. Schematic picture of a gas laser. The standing light wave in the discharge tube generates an intense transverse polarization current in the gas. Its oscillation sustains the standing wave and generates the radiated beam.

thus observed was greatly weakened by the poor time resolution of the detectors, but it raised considerable surprise nonetheless. The observation of temporal correlations between photons in a steady beam was something altogether new. The experiment has been repeated several times, with better resolution, and the correlation effect has emerged in each case more clearly.[10]

The correlation effect was enough of a surprise to call for a clear explanation. The closest it came to that was a clever argument[11] by Purcell who used the semiclassical form of the radiation theory in conjunction with a formula for the relaxation time of radiofrequency noise developed in wartime radar studies. It seemed to indicate that the photon correlation time would be increased by just using a more monochromatic light source.

The late 1950s were, of course, the time in which the laser was being developed, but it was not until 1960 that the helium–neon laser[12] was on the scene with its extremely monochromatic and stable beams. The question then arose: what are the correlations of the photons in a laser beam? Would they extend, as one might guess, over much longer time intervals as the beam became more monochromatic? I puzzled over the question for some time, I must admit, since it seemed to me, even without any detailed theory of the laser mechanism, that there would not be any such extended correlation.

The oscillating electric current that radiates light in a laser tube is not a current of free charges. It is a polarization current of bound charges oscillating in a direction perpendicular to the axis of the tube (Figure 5). If it is sufficiently strong it can be regarded as a predetermined classical current, one that suffers negligible recoil when individual photons are emitted. Such currents, I knew,[13] emitted Poisson distributions of photons, which indicated clearly that the photons were statistically independent of one another. It seemed then that a laser beam would show no Hanbury Brown–Twiss photon correlations at all.

How then would one describe the delayed-coincidence counting measurement of Hanbury Brown and Twiss? If the two photon counters are sensitive at the space-time points $\boldsymbol{r}_1 t_1$ and $\boldsymbol{r}_2 t_2$ we will need to make use of the annihilation operators $E^{(+)}(\boldsymbol{r}_1 t_1)$ and $E^{(+)}(\boldsymbol{r}_2 t_2)$ (which do, in fact commute). The amplitude for the field to go from the state $|i\rangle$ to a state $|f\rangle$ with two quanta fewer is [Eq. (18)]:

$$\langle f|E^{(+)}(\boldsymbol{r}_2 t_2)E^{(+)}(\boldsymbol{r}_1 t_1)\ |i\rangle. \tag{18}$$

When this expression is squared, summed over final states $|f\rangle$ and averaged over the initial states $|i\rangle$ we have a new kind of correlation function that we can write as Equation (19):

$$\begin{aligned} &G^{(2)}(\boldsymbol{r}_1 t_1 \boldsymbol{r}_2 t_2 \boldsymbol{r}_2 t_2 \boldsymbol{r}_1 t_1) = \\ &\mathrm{Trace}\{\rho E^{(-)}(\boldsymbol{r}_1 t_1)E^{(-)}(\boldsymbol{r}_2 t_2)E^{(+)}(\boldsymbol{r}_2 t_2)E^{(+)}(\boldsymbol{r}_1 t_1)\}. \end{aligned} \tag{19}$$

This is a special case of a somewhat more general second order correlation function that we can write (with the abbreviation $x_j = \boldsymbol{r}_j t_j$) as [Eq. (20)]:

$$G^{(2)}(x_1 x_2 x_3 x_4) = \mathrm{Trace}\{\rho E^{(-)}(x_1)E^{(-)}(x_2)E^{(+)}(x_3)E^{(+)}(x_4)\}. \tag{20}$$

Now Hanbury Brown and Twiss had seen to it that the beams falling on their two detectors were as coherent as possible in the usual optical sense. The function $G^{(1)}$ should thus have satisfied the factorization condition (8), but that statement doesn't at all imply any corresponding factorization property of the functions $G^{(2)}$ given by Equations (19) or (20).

We are free to define a kind of second-order coherence by requiring a parallel factorization of $G^{(2)}$ [Eq. (21)],

$$G^{(2)}(x_1 x_2 x_3 x_4) = \mathrm{E}^*(x_1)\mathrm{E}^*(x_2)\mathrm{E}(x_3)\mathrm{E}(x_4), \tag{21}$$

and the definition can be a useful one even though the Hanbury Brown–Twiss correlation assures us that no such factorization is present in their experiment. If it were present the coincidence rate according to Equation (21) would be proportional to [Eq. (22)]:

$$G^{(2)}(x_1 x_2 x_2 x_1) = G^{(1)}(x_1 x_1)\ G^{(1)}(x_2 x_2), \tag{22}$$

that is, to the product of the two average intensities measured separately—and that is what was not found. Ordinary light beams, that is, light from ordinary sources, even extremely monochromatic ones as in the Hanbury Brown–Twiss experiment, do not have any such second order coherence.

We can go on defining still higher-order forms of coherence by defining nth-order correlation functions $G^{(n)}$ that depend on $2n$ space-time coordinates. The usefulness of such functions may not be clear since carrying out the n-fold delayed coincidence counting experiments that measure them would be quite difficult in practice. It is nonetheless useful to discuss such functions since they do turn out to play an essential role in most calculations of the statistical distributions of photons. If we turn on a photon counter for any given length of time, for example, the number of photons it records will be a random integer. Repeating the experiment many times will lead us to a distribution function for that number. To predict those distributions[14] we need, in general, to know the correlation functions $G^{(n)}$ of arbitrary orders.

Once we are defining higher-order forms of coherence, it is worth asking whether we can find fields that lead to factorization of the complete set of correlation functions $G^{(n)}$. If so, we could speak of those as possessing full coherence. Now, are there any such states of the field? In fact there are lots of them, and some can describe precisely the fields generated by predetermined classical current distributions. These fields have the remarkable property that annihilating a single quantum in them by means of the operator $E^{(+)}$ leaves the field essentially unchanged. It just multiplies the state vector by an ordinary number. That is a relation we can write as Equation (23):

$$E^{(+)}(\boldsymbol{r}t)\ |\rangle = \mathscr{E}(\boldsymbol{r}t)\ |\rangle, \tag{23}$$

where $\mathscr{E}(\boldsymbol{r}t)$ is a positive frequency function of the space-time point $\boldsymbol{r}t$. It is immediately clear that such states must have indefinite numbers of quanta present. Only in that way can they remain unchanged when one quantum is removed. This remarkable relation does in fact hold for all of the quantum states radiated by a classical current distribution, and in that case the function $\mathscr{E}(\boldsymbol{r}t)$ happens to be the classical solution for the electric field.

Any state vector that obeys the relation (23) will also obey the adjoint relation [Eq. (24)]:

$$\langle\,|E^{(-)}(\boldsymbol{r}t) = \mathscr{E}^*(\boldsymbol{r}t)\ \langle\,|. \tag{24}$$

Hence the nth-order correlation function will indeed factorize into the form of Equation (25):

$$G^{(n)}(x_1\ldots.x_{2n}) = \mathscr{E}^*(x_1)\ldots.\mathscr{E}^*(x_n)\mathscr{E}(x_{n+1})\ldots.\mathscr{E}(x_{2n}) \tag{25}$$

that we require for nth order coherence. Such states represent fully coherent fields, and delayed coincidence counting measurements carried out in them will reveal no photon correlations at all. To explain, for example, the Hanbury Brown–Twiss correlations we must use not pure coherent states but mixtures of them, for which the factorization conditions like Equation (25) no longer hold. To see how these mixtures arise, it helps to discuss the modes of oscillation of the field individually.

The electromagnetic field in free space has a continuum of possible frequencies of oscillation, and a continuum of available modes of spatial oscillation at any given frequency. It is often simpler, instead of discussing all these modes at once, to isolate a single mode and discuss the behavior of that one alone. The field as a whole is then a sum of the contributions of the individual modes. In fact when experiments are carried out within reflecting enclosures the field modes form a discrete set, and their contributions are often physically separable.

The oscillations of a single mode of the field, as we have noted earlier, are essentially the same as those of a harmonic oscillator. The nth excitation state of the oscillator represents the presence of exactly n light quanta in that mode. The operator that decreases the quantum number of the oscillator is usually written as a, and the adjoint operator—which raises the quantum number by one unit as $a^\dagger$. These operators then obey the relation [Eq. (26)]:

$$aa^\dagger - a^\dagger a = 1, \tag{26}$$

which shows that their multiplication is not commutative. We can take the field operator $E^{(+)}(\boldsymbol{r}t)$ for the mode we are studying to be proportional to the operator a. Then any state vector for the mode that obeys the relation (23) will have the property [Eq. (27)]:

$$a|\,\rangle = \alpha|\,\rangle \tag{27}$$

where α is some complex number. It is not difficult to solve for the state vectors that satisfy the relation (27) for any given value of α. They can be expressed as a sum taken over all possible quantum-number states $|n\rangle$, $n=0, 1, 2\ldots$ that takes the form of Equation (28):

$$|\alpha\rangle = e^{-\frac{1}{2}|\alpha|^2}\sum_{n=0}^{\infty}\frac{\alpha^n}{\sqrt{n!}}|n\rangle, \tag{28}$$

in which we have chosen to label the state with the arbitrary complex parameter α. The states $|\alpha\rangle$ are fully coherent states of the field mode.

The squared moduli of the coefficients of the state $|n\rangle$ in Equation (28) tell us the probability for the presence of n quanta in the mode, and those numbers do indeed form a Poisson distribution, one with the mean value of n equal to $|\alpha|^2$. The coherent states form a complete set of states in the sense that any state of the mode can be expressed as a suitable sum taken over them. As we have defined them they are equivalent to certain oscillator states defined by Schrödinger[15] in his earliest discussions of wave functions. Known thus from the very beginning of wave mechanics, they seemed not to have found any important role in the earlier development of the theory.

Coherent excitations of fields have a particularly simple way of combining. Let us suppose that one excitation mechanism brings a field mode from its empty state $|0\rangle$ to the coherent state $|\alpha_1\rangle$. A second mechanism could bring it, for example, from the state $|0\rangle$ to the state $|\alpha_2\rangle$. If the two mechanisms act simultaneously the resulting state can be written as $e^{i\phi}|\alpha_1+\alpha_2\rangle$ where $e^{i\phi}$ is a phase factor that depends on α_1 and

α_2, but we don't need to know it since it cancels out of the expression for the density operator [Eq. (29)]:

$$\rho = |\alpha_1 + \alpha_2\rangle\langle\alpha_1 + \alpha_2|. \tag{29}$$

This relation embodies the superposition principle for field excitations and tells us all about the resulting quantum statistics. It is easily generalized to treat the superposition of many excitations. If, say, j coherent excitations were present, we should have a density operator [Eq. (30)]:

$$\rho = |\alpha_1 + \ldots + \alpha_j\rangle\langle\alpha_1 + \ldots\alpha_j|. \tag{30}$$

Let us suppose now that the individual excitation amplitudes α_j are in one or another sense random complex numbers. Then the sum $\alpha_1 + \ldots + \alpha_j$ will describe a suitably defined random walk in the complex plane. In the limit $j \to \infty$ the probability distribution for the sum $\alpha = \alpha_1 + \ldots + \alpha_j$ will be given by a Gaussian distribution which we can write as Equation (31):

$$P(\alpha) = \frac{1}{\pi\langle n\rangle} e^{-\frac{|\alpha|^2}{\langle n\rangle}}, \tag{31}$$

in which the mean value of $|\alpha|^2$ which has been written as $\langle n\rangle$, is the mean number of quanta in the mode.

The density operator that describes this sort of random excitation is a probabilistic mixture of coherent states [Eq. (32)],

$$\rho = \frac{1}{\pi\langle n\rangle}\int e^{-\frac{|\alpha|^2}{\langle n\rangle}}|\alpha\rangle\langle\alpha|d^2\alpha, \tag{32}$$

where $d^2\alpha$ is an element of area in the complex plane. When we express ρ in terms of m-quantum states by using the expansion (28) we find [Eq. (33)]:

$$\rho = \frac{1}{1+\langle n\rangle}\sum_{m=0}^{\infty}\left(\frac{\langle n\rangle}{1+\langle n\rangle}\right)^m |m\rangle\langle m|. \tag{33}$$

This kind of random excitation mechanism is thus always associated with a geometrical or fixed-ratio distribution of quantum numbers (Figure 6). In the best known example of the latter, the Planck distribution, we have $\langle n\rangle = \left(e^{\frac{h\nu}{kT}} - 1\right)^{-1}$, and the density operator (33) then contains the familiar thermal weights $e^{-\frac{mh\nu}{kT}}$.

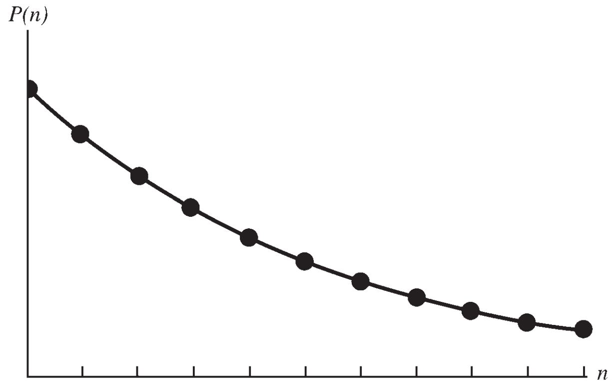


Figure 6. Geometrical or fixed-ratio sequence of probabilities for the presence of n quanta in a mode that is excited chaotically.

There is something remarkably universal about the geometrical sequence of n-quantum probabilities. The image of chaotic excitation we have derived it from, on the other hand, excitation in effect by a random collection of lasers, may well seem rather specialized. It may be useful therefore to have a more general way of characterizing the same distribution. If a quantum state is specified by the density operator ρ, we may associate with it an entropy S given by Equation (34):

$$S = -\mathrm{Trace}(\rho\log\rho), \tag{34}$$

which is a measure, roughly speaking, of the disorder characteristic of the state. The most disordered, or chaotic, state is reached by maximizing S, but in finding the maximum we must observe two constraints. The first is [Eq. (35)]:

$$\mathrm{Trace}\,\rho = 1, \tag{35}$$

which says simply that all probabilities add up to one. The second is [Eq. (36)]:

$$\mathrm{Trace}\,(\rho a^\dagger a) = \langle n\rangle, \tag{36}$$

which fixes the average occupation number of the mode.

When S is maximized, subject to these two constraints, we find indeed that the density operator ρ takes the form given by Equation (33). The geometrical distribution is thus uniquely representative of chaotic excitation. Most ordinary light sources consist of vast numbers of atoms radiating as nearly independently of one another as the field equations will permit. It should be no surprise then that these are largely maximum entropy or chaotic sources. When many modes are excited, the light they radiate is, in effect, colored noise and indistinguishable from appropriately filtered black body radiation.

For chaotic sources, the density operator (32) permits us to evaluate all of the higher-order correlation functions $G^{(n)}(x_1 \ldots x_{2n})$. In fact they can all be reduced[14] to sums of products of first order correlation functions $G^{(1)}(x_i x_j)$. In particular, for example, the Hanbury Brown–Twiss coincidence rate corresponding to the two space time points x_1 and x_2 can be written as Equation (37):

$$G^{(2)}(x_1x_2x_2x_1) = G^{(1)}(x_1x_1)G^{(1)}(x_2x_2) + G^{(1)}(x_1x_2)G^{(1)}(x_2x_1). \tag{37}$$

The first of the two terms on the right side of this equation is simply the product of the two counting rates that would be measured at x_1 and x_2 independently. The second term is the additional delayed coincidence rate detected first by Hanbury Brown and Twiss, and it is indeed contributed by a two-photon interference effect. If we let $x_1 = x_2$, which corresponds to zero time delay in their experiment, we see that [Eq. (38)]:

$$G^{(2)}(x_1x_1x_1x_1) = 2\{G^{(1)}(x_1x_1)\}^2, \tag{38}$$

or the coincidence rate for vanishing time delay should be double the background or accidental rate.

The Gaussian representation of the density operator in terms of coherent states is an example of a broader class of so-called "diagonal representations" that are quite convenient to use—when they are available. If the density operator for a single mode, for example, can be written in the form of Equation (39):

$$\rho = \int \mathrm{P}(\alpha)\ |\alpha\rangle\langle\alpha| d^2\alpha \tag{39}$$

then the expectation values of operator products like $a^{+n}a^m$ can be evaluated as simple integrals over the function P such as [Eq. (40)]:

$$\{a^{\dagger n}a^m\}_{\mathrm{av}} = \int \mathrm{P}(\alpha)\alpha^{*n}\alpha^m d^2\alpha. \tag{40}$$

The function $\mathrm{P}(\alpha)$ then takes on some of the role of a probability density, but that can be a bit misleading since the condition that the probabilities derived from ρ all be positive or zero does not require $\mathrm{P}(\alpha)$ to be positive definite. It can and sometimes does take on negative values over limited areas of the α-plane in certain physical examples, and it may also be singular. It is a member, as we shall see, of a broader class of quasiprobability densities. The representation (39), the P-representation, unfortunately is not always available.[16,17] It can not be defined, for example, for the familiar "squeezed" states of the field in which one or the other of the complementary uncertainties is smaller than that of the coherent states.

The difference between a monochromatic laser beam and a chaotic beam is most easily expressed in terms of the function $\mathrm{P}(\alpha)$. For a stationary laser beam the function P depends only on the magnitude of α and vanishes unless $|\alpha|$ assumes some fixed value. A graph of that function P is shown in Figure 7, where it can be compared with the Gaussian function for the same mean occupation number $\langle n\rangle$ given by Equation (31).

How do we measure the statistical properties of photon distributions? A relatively simple way is to place a photon counter in a light beam behind either a mechanical or an electrical shutter. If we open the shutter for a given length of time t, the counter will register some random number n of photons. By repeating that measurement sufficiently many times we can establish a statistical distribution for those random integers n. The analysis necessary to derive this distribution mathematically can be a bit complicated since it requires, in general, a knowledge of all the higher-order correlation functions. Experimental measurements of the distribution, conversely, can tell us about those correlation functions.

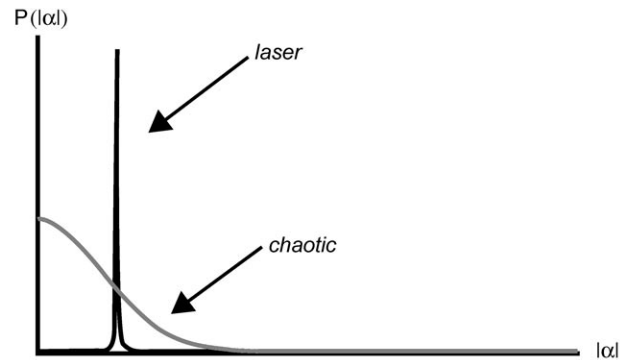


Figure 7. The quasiprobability function $\mathrm{P}(|\alpha|)$ for a chaotic excitation is Gaussian in form, while for a stable laser beam it takes on non-zero values only near a fixed value of $|\alpha|$.

For the two cases in which we already know all the correlation functions, it is particularly easy to find the photocount distributions. If the average rate at which photons are recorded is w, then the mean number recorded in time t is [Eq. (41)]:

$$\langle n\rangle = wt. \tag{41}$$

In a coherent beam the result for the probability of n photocounts is just the Poisson distribution [Eq. (42)]:

$$p_{\mathrm{n}}(t) = \frac{(wt)^n}{n!}e^{-wt}. \tag{42}$$

In a chaotic beam, on the other hand, the probability of counting n quanta is given by the rather more spread-out distribution [Eq. (43)]:

$$p_n(t) = \frac{1}{1+wt}\left(\frac{wt}{1+wt}\right)^n. \tag{43}$$

These results, which are fairly obvious from the occupation number probabilities implicit in Equations (28) and (33) are illustrated in Figure 8.

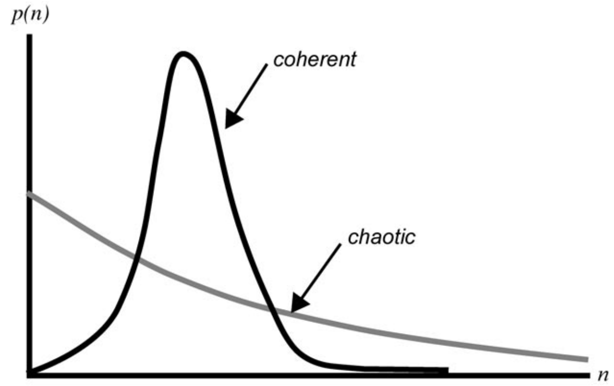


Figure 8. The two $\mathrm{P}(|\alpha|)$ distributions of Figure 7 lead to different photon occupation number distributions $p(n)$: for chaotic excitation a geometric distribution, for coherent excitation a Poisson distribution.

Here is a closely related question that can also be investigated experimentally without much difficulty. If we open the shutter in front of the counter at an arbitrary moment, some random interval of time will pass before the first photon is counted. What is the distribution of those random times? In a steady coherent beam, in fact, it is just an exponential distribution [Eq. (44)]:

$$\mathscr{W}_{coh} = we^{-wt}, \quad (44)$$

while in a chaotic beam it assumes the less obvious form [Eq. (45)]:

$$\mathscr{W}_{ch}(t) = \frac{w}{(1+wt)^2}. \quad (45)$$

There is an alternative way of finding a distribution of time intervals. Instead of simply opening a shutter at an arbitrary moment, we can begin the measurement with the registration of a given photocount at time zero and then ask what is the distribution, of time intervals until the next photocount. This distribution, which we may write as $\mathscr{W}(0|t)$, takes the same form for a coherent beam as it does for the measurement described earlier, which starts at arbitrary moments [Eq. (46)],

$$\mathscr{W}_{coh}(0|t) = we^{-wt} = \mathscr{W}_{coh}(t). \quad (46)$$

This identity is simply a restatement of the statistically independent or uncorrelated quality of all photons in a coherent beam.

For a chaotic beam, on the other hand, the distribution $\mathscr{W}_{ch}(0|t)$ takes a form quite different from $\mathscr{W}_{ch}(t)$. It is [Eq. (47)]:

$$\mathscr{W}_{ch}(0|t) = \frac{2w}{(1+wt)^3}, \quad (47)$$

an expression which exceeds $\mathscr{W}_{ch}(t)$ for times for which $wt<1$, and is in fact twice as large as $\mathscr{W}_{ch}(t)$ for $t=0$ (Figure 9). The

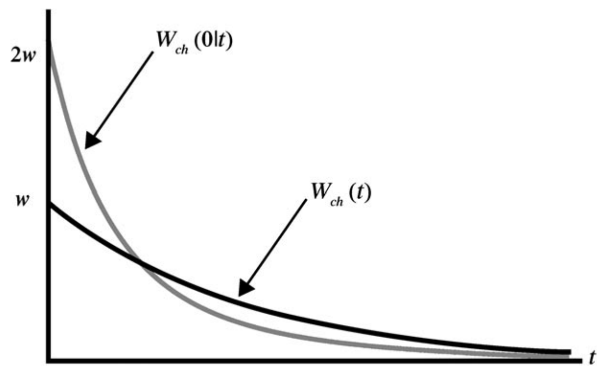


Figure 9. Time interval distributions for counting experiments in a chaotically excited mode: $\mathscr{W}_{ch}(t)$ is the distribution of intervals from an arbitrary moment until the first photocount. $\mathscr{W}_{ch}(0|t)$ is the distribution of intervals between two successive photocounts.

reason for that lies in the Gaussian distribution of amplitudes implicit in Equations (31) and (32). The very fact that we have counted a photon at $t=0$ makes it more probable that the field amplitude α has fluctuated to a large value at that moment, and hence the probability for counting a second photon remains larger than average for some time later. The functions $\mathscr{W}_{ch}(t)$ and $\mathscr{W}_{ch}(0|t)$ are compared in Figure 8.

All of the experiments we have discussed thus far are based on the procedure of photon counting, whether with individual counters or with several of them arranged to be sensitive in delayed coincidence. The functions they measure, the correlation functions $G^{(n)}$, are all expectation values of products of field operators written in a particular order. If one reads from right to left, the annihilation operator always precedes the creation operators in our correlation functions, as they do, for example, in Equation (19) for $G^{(2)}$. It is that so-called "normal ordering" that gives the coherent states, and the quasiprobability density $P(\alpha)$ the special roles they occupy in discussing this class of experiments.

But there are other kinds of expectation values that one sometimes needs in order to discuss other classes of experiments. These could, for example, involve symmetrically ordered sums of operator products, or even anti-normally ordered products which are opposite to the normally ordered ones. The commutation relations for the multiplication of field operators will ultimately relate all these expectation values to one another, but it is often possible to find much simpler ways of evaluating them. There exists a quasiprobability density that plays much the same role for symmetrized products as the function P does for the normally ordered ones. It is, in fact, the function Wigner[18] devised in 1932 as a kind of quantum mechanical replacement for the classical phase space density. For anti-normally ordered operator products, the role of the quasiprobability density is taken over by the expectation value which for a single mode is $\frac{1}{\pi}\langle\alpha|\rho|\alpha\rangle$. The three quasiprobability densities associated with the three operator orderings and whatever experiments they describe are all members of a larger family that can be shown to have many properties in common.[17]

The developments I have described to you were all relatively early ones in the development of the field we now call quantum optics. The further developments that have come in rapid succession in recent years are too numerous to recount here. Let me just mention a few. A great variety of careful measurements of photon counting distributions and correlations of the type we have discussed have been carried out[19] and furnish clear agreement with the theory. They have furthermore shown in detail how the properties of laser beams change as they rise in power from below threshold to above it.

The fully quantum mechanical theory of the laser was difficult to develop[20] since the laser is an intrinsically nonlinear device, but only through such a theory can its quantum noise properties be understood. The theories of a considerable assortment of other kinds of oscillators and amplifiers have now been worked out.

Nonlinear optics has furnished us with new classes of quantum phenomena such as parametric down conversion in which a single photon is split into a pair of highly correlated or entangled photons. Entanglement has been a rich source of the quantum phenomena that are perhaps most interesting—and baffling—in everyday terms.

It is worth emphasizing that the mathematical tools we have developed for dealing with light quanta can be applied equally well to the much broader class of particles obeying Bose–Einstein statistics. These include atoms of ^{4}He, ^{23}Na, ^{87}Rb, and all of the others which have recently been Bose-condensed by optical means. When proper account is taken of the atomic interactions and the non-vanishing atomic masses, the coherent state formalism is found to furnish useful descriptions of the behavior of these bosonic gases.

The formalism seems likewise to apply to subatomic particles, to bosons that are only short-lived. The pions that emerge by hundreds or even thousands from the high-energy collisions of heavy ions are also bosons. The pions of similar charge have a clearly noticeable tendency to be emitted with closely correlated momenta, an effect which is evidently analogous to the Hanbury Brown–Twiss correlation of photons, and invites the same sort of analysis.[21]

Particles obeying Fermi–Dirac statistics, of course, behave quite differently from photons or pions. No more than a single one of them ever occupies any given quantum state. This kind of reckoning associated with fermion fields is radically different therefore from the sort we have associated with bosons, like photons. It has proved possible, nonetheless, to develop an algebraic scheme[22] for calculating expectation values of products of fermion fields that is remarkably parallel to the one we have described for photon fields. There is a one-to-one correspondence between the mathematical operations and expressions for boson fields on the one hand and fermion fields on the other. That correspondence has promise of proving useful in describing the dynamics of degenerate fermion gases.

I'd like, as a final note, to share with you an experience I had in 1951, while I was a postdoc at the Institute for Advanced Study in Princeton. Possessed by the habit of working late at night—in fact on photon statistics[13] at the time—I didn't often appear at my Institute desk early in the day. Occasionally I walked out to the Institute around noon, and that was closer to the end of the work day for Professor Einstein. Our paths thus crossed quite a few times, and on one of those occasions I had ventured to bring my camera. He seemed more than willing to let me take his picture as if acknowledging his role as a local landmark, and he stood for me just as rigidly still. Here, in Figure 10, is the hitherto unpublished result. I shall always treasure that image, and harbor the enduring wish I had been able to ask him just a few questions about that remarkable year, 1905.

Figure 10. Professor Einstein, encountered in the spring of 1951 in Princeton, NJ.

Keywords: electrodynamics · Nobel lecture · optical coherence · photons · quantum optics

[1] M. Planck, *Ann. Phys.* **1900**, *1*, 69.
[2] M. Planck, *Ann. Phys.* **1900**, *1*, 719.
[3] A. Einstein, *Ann. Phys.* **1905**, *17*, 132.
[4] P. A. M. Dirac, *Proc. R. Soc. London Ser. A* **1927**, *114*, 243; P. A. M. Dirac, *Proc. R. Soc. Ser. A* **1927**, *114*, 710.
[5] M. Born, E. Wolf, *Principles of Optics*, Pergamon, London, **1959**, chap. X.
[6] U. M. Titulaer, R. J. Glauber, *Phys. Rev. [Sect.] B* **1965**, *140*, 676; U. M. Titulaer, R. J. Glauber, *Phys. Rev.* **1966**, *145*, 1041.
[7] R. Hanbury Brown, R. Q. Twiss, *Philos. Mag.* **1954**, *45*, 663.
[8] P. A. M. Dirac, *The Principles of Quantum Mechanics, 4th ed.*, Oxford University Press, **1958**, p. 9.
[9] R. Hanbury Brown, R. Q. Twiss, *Nature* **1956**, *177*, 27; R. Hanbury Brown, R. Q. Twiss, *Proc. R. Soc. London Ser. A* **1957**, *242*, 300; R. Hanbury Brown, R. Q. Twiss, *Proc. R. Soc. London Ser. A* **1957**, *243*, 291.
[10] G. A. Rebka, R. V. Pound, *Nature* **1957**, *180*, 1035; D. B. Scarl, *Phys. Rev.* **1968**, *175*, 1661.
[11] E. M. Purcell, *Nature* **1956**, *178*, 1449.
[12] A. Javan, W. R. Bennett, D. R. Herriot, *Phys. Rev. Lett.* **1961**, *6*, 106.
[13] R. J. Glauber, *Phys. Rev.* **1951**, *84*, 395.
[14] R. J. Glauber in *Quantum Optics and Electronics (Les Houches 1964)* (Eds.: C. de Witt, A. Blandin, C. Cohen-Tannoudji), Gordon and Breach, New York, **1965**, p. 63.
[15] E. Schrödinger, *Naturwissenschaften* **1926**, *14*, 664.
[16] E. C. G. Sudarshan, *Phys. Rev. Lett.* **1963**, *10*, 277.
[17] K. E. Cahill, R. J. Glauber, *Phys. Rev.* **1969**, *177*, 1857; K. E. Cahill, R. J. Glauber, *Phys. Rev.* **1969**, *177*, 1882.
[18] E. Wigner, *Phys. Rev.* **1932**, *40*, 749.
[19] See for example, F. T. Arecchi in *Quantum Optics, Course XLII, Enrico Fermi School, Varenna, 1969* (Ed.: R. J. Glauber), Academic Press, New York, **1969**; E. Jakeman, E. R. Pike, *J. Phys. A* **1969**, *2*, 411.
[20] M. O. Scully, W. E. Lamb, Jr., *Phys. Rev.* **1967**, *159*, 208; M. Lax in *Brandeis University Summer Institute Lectures (1966), Vol. II* (Eds.: M. Cretien et al.), Gordon and Breach, New York, **1968**; "Laser Theory": H. Haken in *Encyclopedia of Physics XXV/2c* (Ed.: S. Flügge), Springer, Heidelberg, **1970**.
[21] R. J. Glauber, *Quantum Optics and Heavy Ion Physics*, http://arxiv.org/nucl-th/0604021, **2006**.
[22] K. E. Cahill, R. J. Glauber, *Phys. Rev. A* **1999**, *59*, 1538.

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