Journal of the European Optical Society - Rapid Publications 5, 10044s (2010) www.jeos.org

Early days of coherence theory and the first Rochester conference on coherence

E. Wolf
ewlupus@pas.rochester.edu

Institute of Optics, University of Rochester, Wilmot Building 275 Hutchison Road, Rochester, NY 14627-0186, USA

The terms coherence and correlations seem to have entered the optics vocabulary about the beginning of the twentieth century, many years after Maxwell discovered that light was an electromagnetic phenomenon. Prior to that time there were only a few investigations, which have a bearing on this subject. The first one was made by a distinguished French optical scientist E. Verdet who around 1865 asked a question which is equivalent to the following: if sunlight illuminates directly two pinholes in an opaque screen, how close must the pinholes be, so that the light which emerges from them can form interference fringes on superposition? He estimated the distance to be about 1/50 millimeter. In modern language this small distance is the diameter of the area of coherence formed by sunlight on the surface of the earth. [DOI: 10.2971/jeos.2010.10044s]

Keywords: coherence theory

The terms coherence and correlations seem to have entered the optics vocabulary about the beginning of the twentieth century, many years after Maxwell discovered that light was an electromagnetic phenomenon. Prior to that time there were only a few investigations, which have a bearing on this subject. The first one was made by a distinguished French optical scientist E. Verdet who around 1865 asked a question which is equivalent to the following: if sunlight illuminates directly two pinholes in an opaque screen, how close must the pinholes be, so that the light which emerges from them can form interference fringes on superposition? He estimated the distance to be about 1/50 millimeter. In modern language this small distance is the diameter of the area of coherence formed by sunlight on the surface of the earth.

Little, if anything, concerning coherence was done after that for more than forty years, until 1907, when Max von Laue published two papers concerning the entropy of partially coherent ray bundles. In the first of these papers von Laue introduced a quantitative measure of correlation between two light beams. This was probably the first definition of a degree of coherence of light, but von Laues investigations did not attract much attention and they have been largely forgotten. In fact, until about the middle of the 1940s hardly anything more was written on this subject. Nor did two papers by Erwin Schrödinger receive much attention. One of Schrödingers papers, published in 1920, dealt with coherence and interference. The other, published in 1926, was a precursor of important work done several decades later by Roy Glauber on the subject of coherent states. Earlier, around 1890, Michelson introduced two interferometric techniques, one for measuring energy distribution in spectral lines, the other for measuring stellar diameters. It was not until very much later that it was realized that the first of these methods implicitly uses the concept and the properties of temporal coherence, the other those of spatial coherence.

A turning point in the development of coherence theory was the publication of a paper by Fritz Zernike in 1939 in which he introduced a precise measure of spatial coherence in light fluctuations at two points in an optical field, the so-called mutual intensity and also its normalized version, the degree of coherence. He showed that these quantities could be determined from simple experiments, namely, from measurements of the sharpness of interference fringes formed in a Youngs interference pattern. He established a number of interesting properties of the mutual intensity and of the degree of coherence and he also formulated a basic law of optical coherence theory, known today as the van Cittert-Zernike theorem (P. H. van Cittert derived it, in 1934, under somewhat more restricted conditions). It explains, in quantitative terms, how a completely incoherent source may give rise to partially coherent light, and in some cases even very highly coherent light in some region of space on free propagation. Thus until

FIG. 1 Emil Wolf at first Coherence Conference in 1960.

Received September 17, 2010; published September 23, 2010 ISSN 1990-2573

about 1940 there were only a few publications dealing with or closely related to coherence; but it is of interest to note that among the authors of these publications were some very distinguished scientists, including four Nobel Prize winners, namely A. A. Michelson, M. von Laue, E. Schrödinger and F. Zernike.

Following the publication of Zernikes paper, his theory was applied to a number of problems of instrumental optics, notably by H. H. Hopkins and his students in England. Many of the results obtained by this group showed that coherence concepts are very relevant for the design of optical systems and for the understanding of their performance. Around that time, in the early 1950s, I was working at Edinburgh University, collaborating with Max Born in the writing of a book, Principles of Optics. When I came to writing the chapter on interference of light and I examined the various better-known books on optics, I was very disappointed with them; all of them discussed interference only in connection with monochromatic waves. Not a single textbook that I examined took into account fluctuations in realistic sources and in realistic light fields. As I pondered upon how to treat interference in the book we were writing, I gradually realized that a satisfactory treatment of interference requires a generalization of Zernikes mutual intensity and of his degree of coherence. The quantities Zernike introduced were functions which characterized correlations between light vibrations at two points in an optical field, at the same instants of time. I soon found that to formulate a broader and more rigorous theory one must generalize Zernikes concept of the mutual intensity. Namely, one needs to take as a measure of coherence the correlation of the field fluctuations not only at two points in space, but also at two instances of time. The correlation function which I introduced for this purpose in 1955 is known as the mutual coherence function, and this function satisfies rigorously two wave equations in free space. It turned out that all the results relating to propagation of the mutual intensity and of the degree of coherence derived previously, in particular the van Cittert-Zernike theorem, are approximate solutions of these two wave equations. When I discovered these two equations (which are now almost obvious but they were not so in the 1950s), I phoned Born from my Edinburgh home and told him that I found some rather exciting new results which I would like to discuss with him. Born suggested we meet for lunch. I came to his office at lunch time, and as he was putting on his coat he asked me what the excitement was all about. I said, "Professor Born I have discovered that not only the optical field propagates as a wave, but so do its coherence properties." Born looked at me rather sadly, put his arm on my shoulder as if to comfort me, and said, "Wolf, you have always been such a sensible fellow but now you have gone completely crazy." There is another amusing story relating to my interaction with Born about coherence. In 1956 Born was already in retirement and I was on a visiting appointment at New York University, still working on our book. One day I received a letter from Born in which he asked me why the manuscript was not yet finished. I wrote back saying that the manuscript is almost completed, except for a chapter on partial coherence on which I was still working. Born replied at once saying, "Wolf, who apart from you is interested in coherence? Leave the chapter out and send the manuscript to the printers."

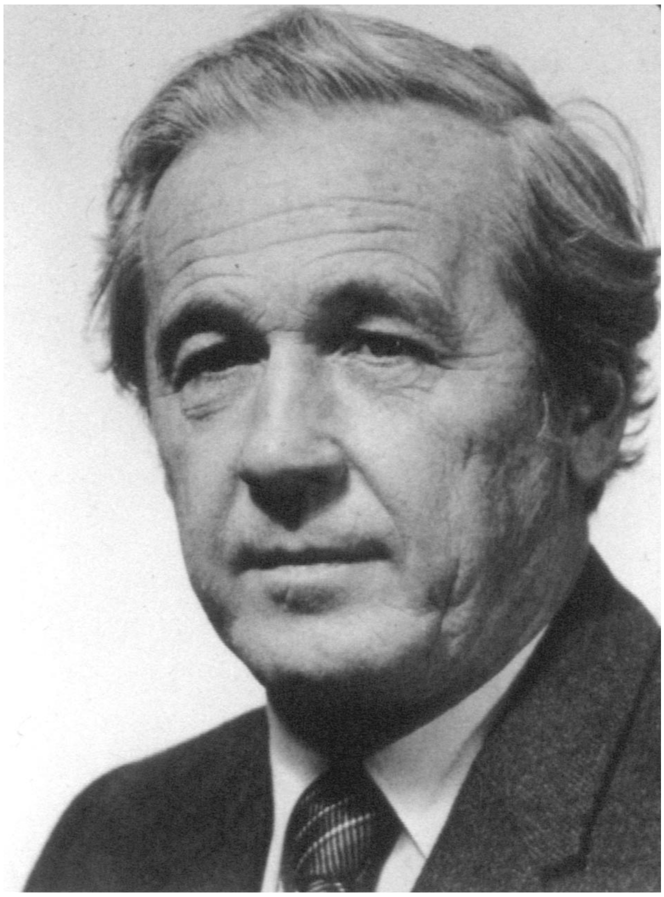

FIG. 2 Robert Hopkins and Emil Wolf organized the first Coherence Conference in 1960.

I finished the chapter anyway and our book was published in 1959, only a few months before the invention of the laser, and many of the reviews of our book which were then appearing stressed that Principles of Optics contained an account of coherence theory, which had become of crucial importance to the understanding of some features of laser light. Born was then as happy as I was that the chapter was included. It was not a foresight on my part to include the chapter on coherence at what in retrospect was undoubtedly the right time; rather it was the consequence of my desire to treat an important phenomenon, namely interference of light, on a more realistic basis than was done previously. Incidentally, soon after our book was published I received a very nice letter from Born in which he praised our collaboration and said, "Wolf, I cannot recall a single occasion when we disagreed about anything."

I should add that I hope that you will not regard the two stories which I just told you about my interaction with Born as indicating any disrespect for him. Like most scientists who were fortunate to have had the opportunity to interact with Born, I had the greatest respect for him, not only as a scientist but also as a kind and warm human being. The stories I told you just illustrate that his first reaction to new ideas was frequently rather critical, but if he was wrong he usually quickly realized it and would then apologize. Born retired in 1953 from the Tait Chair of Natural Philosophy which he held at the University of Edinburgh for seventeen years, and a year later I left Edinburgh to take up a post-doctoral appointment at Manchester University in England. Some time after I arrived in Manch-

FIG. 3 R. Hanbury Brown leads blackboard discussion at the first Coherence.

ester, I received a phone call from Professor Henry Lipson, a distinguished crystallographer who was then the head of the physics department at the Manchester College of Technology. He said, "We are doing some optical experiments here, using Fourier optics, to get a better interpretation of X-ray diffraction data, utilizing the analogue between diffraction of X-rays and diffraction of light. We seem to be getting some spurious effects which we do not understand. We have a Ph.D. student whose name is Brian Thompson. He thinks that this problem has something to do with partial coherence. We do not know anything about partial coherence but Brian tells me that you might be able to help us." I went to look at the experiments Professor Lipson spoke about. After Brian and I talked for a while I felt that he may well be right and we decided to do some joint work to clarify this. Brian was to do some controlled experiments and I was to do the theory. After a few weeks we met and compared our results which were essentially concerned with Youngs interference experiments with light of different degrees of coherence. When we compared our results, there were some serious discrepancies between them. I thought, of course, that Brians experiments were not right and he thought that my theory was wrong. We could not get to the bottom of it. A few weeks later Brian phoned me and said, "I checked the masks with the pinholes which have been used here for many years and I found that they were wrongly calibrated. When I corrected my results taking this into account, yours and my results agree completely." That was the beginning of a long friendship! Our results, which we soon published, were the first experimental verifications of some of the predictions of Zernikes theory of partial coherence. Incidentally, fairly recently, about forty years after our paper was published, the experiments were repeated by a group of Italian scientists using more modern techniques. They obtained essentially the same results as we did, with higher accuracy.

Whilst in Manchester an important development took place that had a profound influence on the future evolution of the field of optical coherence, namely the discovery by Hanbury Brown and Twiss around 1956 of the possibility of determining the degree of coherence of thermal light by means of photoelectric experiments; and they suggested that by the use of this technique one could measure stellar diameters in a novel way. The experiments were carried out at the Jodrell Bank Radio Station, close to and part of the University of Manchester. Having been at Manchester at that time I was fortunate of being able to learn first hand about these experiments just as they were being performed and I witnessed a very great deal of controversy that surrounded them.

The Hanbury Brown-Twiss experiments were the first ones to draw attention to higher order correlations between light fluctuations at two space-time points. Such correlations are not encountered in ordinary interference experiments. There were many misunderstandings and controversies surrounding the experiments. It was claimed, for exampleby some quite distinguished physiciststhat the existence of this effect would contradict the basic principles of quantum mechanics. Faulty experimental evidence was provided for this claim. When these "negative experiments" were carefully analyzed, it turned out that those who performed them greatly misjudged the value of signal-to-noise ratio. It was later found that in some of these experiments the signal-to-noise ratio was exceedingly small because of the very low photon degeneracy of the only kind of light, which was then available, namely thermal light. It was later estimated that in one of the experiments, 1011 years (which is somewhat longer than the age of the Earth) would have been needed to observe the Hanbury Brown-Twiss effect! The controversy was resolved by a beautiful short paper published in Nature in 1956 by Edward Purcell whose credentials were not questioned because of his fame, especially in the field of nuclear magnetic resonance for which he was awarded the Physics Nobel Prize. Purcells considerations were the starting point of the analysis presented in two papers by Leonard

Mandel in 1958 and 1959; in one he derived what today is known in the theory of photo-count statistics as “Mandels formula.” It is the basic formula relating to the photoelectric detection of light fluctuations.

Around the time when Hanbury Brown and Twiss performed their experiments, another group of scientists, Forrester, Gmundsen and Johnson, performed another important experiment which involved photoelectric detection of light. They succeeded in generating beats from the superposition of mutually incoherent light beamsan effect they called photoelectric mixing. These experiments attracted much attention when it was realized that they threw some light on the controversy surrounding the Hanbury Brown effect. Let me now return to my stay in Manchester during the time when these developments were taking place. My appointment at the University of Manchester was ending in 1958 and I was looking for a more permanent academic employment. It was not easy in those days to find such an appointment and I worried a great deal about it. During the Easter vacation in 1958 I was away from Manchester, correcting proofs of Principles of Optics, and I asked a secretary in the physics department to forward to me the proofs which were being sent to me to Manchester in batches by the printers. However, a batch of them did not reach me. When I returned to Manchester, I asked the secretary whether she forwarded to me everything which arrived for me and she said, “Of course!” I was not convinced, and the next day when she went to lunch I had a good look around her office. In one of the cupboards I found not only the missing batch of proofs but also a letter from the University of Rochester addressed to me. I opened it and found that it was from Professor Robert Hopkins, then the director of The Institute of Optics, asking me whether I would be interested to join the faculty. He mentioned that he would be in England in about two weeks time and if I was interested we could meet and talk about it. Those were the days when faxes and email were not available, but I managed to get a message to him just in time and we met in Manchester in early July 1958.

Hopkins told me that he was approached by William Rodney of the Air Force Office of Scientific Research, who told him that the U.S. Air Force was very much interested in the possibility of generating optical radiation whose coherence properties would be similar to those attainable with microwaves. He encouraged Hopkins to organize a conference on that subject. Hopkins was interested. He knew of my work on coherence and when we met in Manchester, he said that if I joined the faculty of the Institute, that he would like me to take charge of organizing a conference on coherence, which the Air Force Office of Scientific Research had already offered to fund. I accepted his invitation and I came to Rochester a few months later. I have often thought since then how much easier the life of directors and of department chairmen must have been in those days. For Hopkins was able to offer me an academic appointment at The Institute of Optics without needing the consent of the tenured faculty of the Institute or the approval of the dean and the provost. Maybe I was just lucky! I also often wondered what my future would have been had I not found his letter in the cupboard of the secretary at Manchester University.

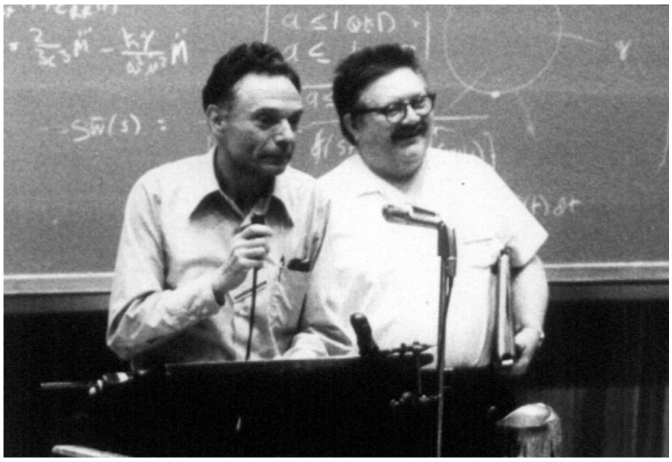

FIG. 4 Senitzky and Jaynes argue at CQOII.

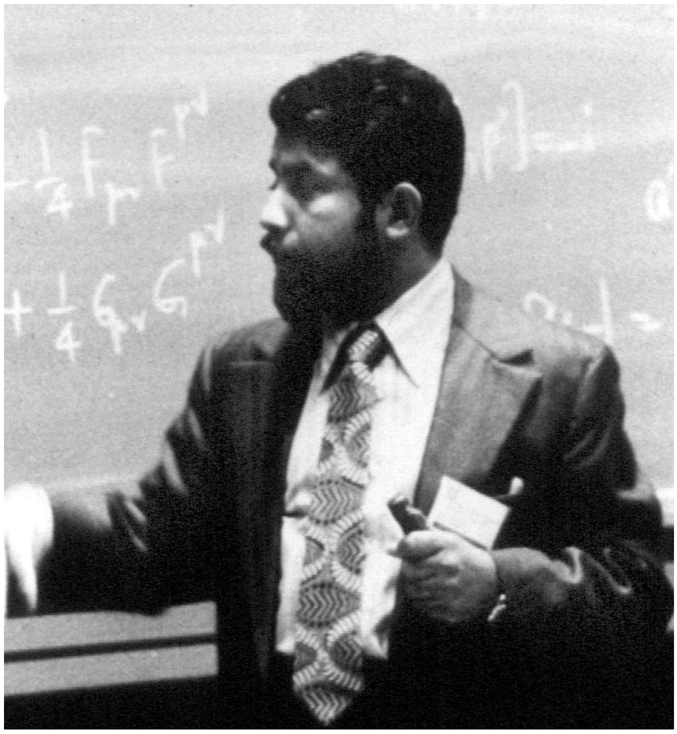

FIG. 5 George Sudarshan, and early collaborator with Wolf and Mandel on coherence theory.

I arrived in Rochester in early July 1959, and Hopkins and I immediately started putting together an organizing committee. By todays standards it was a small conferencethere were twenty-six papers and participants from seven countries. However, it was attended by about two hundred scientists from many countries. The program was divided into six sessions: basic experiments, properties of partially coherent fields, coherent scattering, stimulated emission, interferometric techniques in optics and in radio astronomy, and coherence problems of instrumental optics. Although it was a small conference, the participants included practically all the pioneers.

Among the highlights: C. H. Townes was co-author of a paper presented by one of his collaborators about coherence and stimulated emission devices, A. T. Forrester discussed mixing

FIG. 6 Melvin Lax at CQOII.

FIG. 7 Emil Wolf in 2004.

of incoherent light, and I. R. Sentzky talked about quantum-mechanical treatment of coupled molecular systems. There was also a paper by S. Pancharatnam, whose work was the forerunner of the concept of the Berry phase. R. H. Dicke spoke about coherence and the concept of transition spin, and H. Gamo presented a matrix formulation of theory of partial coherence. Eli Snitzer gave a paper titled "Coherence Properties of Visible Light Propagation in Dielectric Wave Guides." His presentation included a mosaic of photographs, which were precursors to the sort of figures which most of you have probably seen many times since thennamely those of laser modes.

The conference turned out to be very timely indeed. It took place June 2729, 1960. Less than two weeks later, on July 7, 1960, the New York Times reported that Theodore Maiman succeeded in obtaining inversion of population and laser emission. Maimans brief note on the subject was published the following month, on August 6, 1960, in Nature. In retrospect there seems to be no doubt that the conference achieved what Rodney and the Air Force Office of Scientific Research was hoping for when they offered to provide funding for it. It certainly stimulated interest in the field of optical coherence and led to many new developments. An excellent and a fuller account of this aspect of the conference is given in a book, The Laser in America, 19501970, by Joan Bromberg, a historian of science.

Participants of the first conference included many famous scientists, and in almost every conference in this series some physics Nobel Prize winners participated. In addition to Edward Purcell who participated in the first meeting and Charles Townes who coauthored a paper presented there, later meetings were attended by Nobel Prize winners Willis Lamb, Nicholas Blombergen, Arthur Schalow, and Claude Cohen-Tannoudji. Encouraged by the success of the 1960 Rochester Conference, more such conferences followed. By the time the second conference took place in 1966, Leonard Mandel had become my colleague after coming to Rochester in 1964 from England. He took a very active role in the organization of the second and the successive conferences. By then the field of quantum optics began to emerge, and many significant contributions to it were reported at these meetings. The name of the conferences was then appropriately changed to Conference on Coherence and Quantum Optics. The number of participants and of the contributed papers rapidly increased from each conference to the next.

The second conference included papers which closely reflect the birth and the development of quantum optics, in which Mandel played a leading role. There was a paper by Roy Glauber, who presented an outline of quantum optics. Willis Lamb and Marlan Scully presented the first detailed quantum theoretical treatment of the laserthen called the optical maser. Mandel and Davidson described experiments or measurements of triple photon correlations, which could be used to study sixth-order correlations in electromagnetic fields. Later conferences covered topics such as the generation of squeezed states, manipulation of atomic velocities using lasers, optical cavity QED, laser dynamics and many other subjects which are now central in quantum optics. Just like the first one, all the subsequent conferences in this series were supported by the Air Force Office of Scientific Research, largely due to the initiative of Dr. Schlossberg.

I would like to pay a tribute to someone who has contributed so much to quantum optics in general and to these conferences in particular, namely Leonard Mandel. I take great pride in having been responsible for him coming to Rochester, for having had the privilege of collaborating with him on many papers and on a book, and for having been close friends with him for about forty-five years. I mentioned earlier that after Principles of Optics was published, Born wrote to me and said that he did not recall a single occasion when we disagreed on anything. Well, as I indicated, this was not entirely true. But it is true to say that during my much longer association with Len Mandel which extended over several decades, there was not a single occasion, as far as I can remember, when we had any disagreement, neither about science, nor about much trickier subjects such as departmental and national politics.Those of you who knew Len must be aware that apart from being a great scientist, he was a very kind, gentle and compassionate person. I salute the memory of a very dear friend whom I greatly miss.

ACKNOWLEDGEMENTS

The author and the editors would like to express their appreciation to Professor Carlos Stroud, editor of the volume "Jewel in the Crown", (Meliora Press, University of Rochester, 2004) for permission to reproduce this article.