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Explaining the laser's light: classical versus quantum electrodynamics in the 1960s

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Abstract The laser, first operated in 1960, produced light with coherence properties that demanded explanation. While some attempted a treatment within the framework of classical coherence theory, others insisted that only quantum electrodynamics could give adequate insight and generality. The result was a sharp and rather bitter controversy, conducted over the physics and mathematics that were being deployed, but also over the criteria for doing good science. Three physicists were at the center of this dispute, Emil Wolf, Max Born's collaborator on a canonical text on optics as a branch of classical electromagnetism, Roy J. Glauber, a student of Julian Schwinger and a high-energy particle theorist, and Leonard Mandel, both experimentalist and theorist and versed in the physics of photodetection. The story told here is thus one of three distinct research trajectories and of the explosion that occurred when, pushed into the well-financed field of laser studies, these trajectories collided.

1 Introduction

One feature that marks twentieth-century physics is the complicated and ever-changing relation between classical and quantum physics, from the early uses of classical theories as stepping stones to quantum mechanics,[1] through the application of the latter to

[1] See, among other publications, Darrigol (1992) and Joas and Lehner (2009).

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ever wider subject matters,[2] to the use by late twentieth-century chaos theorists of a hybrid of classical and quantum analyses.[3] An episode in this classical–quantum story occurred in the mid-century and was due, in part, to the invention of the laser and the attempts then made to deal with the peculiar kind of light it generated.

Laser light provided a number of puzzles. Did it also exhibit the recently discovered Hanbury Brown–Twiss effect? Here two beams of light from a conventional light source that were aimed at two different photodetectors showed unexpected correlations. Did it produce waves in the visible part of the spectrum that were as coherent as radio frequency waves? And if it did, how was such coherence to be explained? Were its statistical properties similar to those of conventional light or different?

Three somewhat separated traditions handled light ca. 1960. One was based on classical electromagnetism and applied mainly to optical and astronomical phenomena, where the intensity of the light could be considerable. Another was quantum electrodynamics, which dealt with quanta, but could only deal with the effects of a handful of photons at a time. A third was quantum statistics, which traced its treatment of light back to Albert Einstein and Satyendranath Bose. Practitioners from these, as well as other, research traditions flocked to explain laser light, their numbers swelled by the largess of the US military and its interest in laser weapons and by the eagerness of commercial firms and university scientists to enter a promising new field.[4]

What eventuated was a sharp and rather bitter controversy between the practitioners of classical and quantum electrodynamic approaches: a controversy that can give us some data on the arguments for each of these two styles of physics in the mid-century. But, as well, it can teach us something about knowledge transfer and knowledge growth. For the very existence of the dispute forced the combatants to examine the relation between their theories and techniques and those of the adversary, and led to efforts on both sides that enriched physics.[5]

Three physicists were at the center of these events, and they are the focus of this paper. They were Emil Wolf, fresh from a collaboration with Max Born on a text, the magisterial Principles of Optics, based on classical electromagnetism, Roy J. Glauber, a student of Julian Schwinger and a theorist of high-energy physics, and Leonard Mandel, a scientist equally adept at instrumentation, experiment and theory. The story told here is therefore also one of their research trajectories and of the explosion that occurred when these trajectories, pushed by various factors toward the well-financed and well-promoted field of laser studies, collided there.

The consequences were several. One was the emergence of the large new field of non-classical light. But another was a continued productive use of classical methods. And a third was a searching exploration of the explanatory power of classical vs. quantum calculations. A dramatic result of this last line of research was the proof that the photoelectric effect, long heralded as decisive evidence for the hypothesis of light

[2] Two studies are Hughes (1998) and Joas (2010).

[3] See Wise and Brock (1998) and Bokulich (2008).

[4] The classic articles on the military's interest in quantum electronics are Forman (1987, 1992, 1996). For the laser, see Seidel (1987) and Slayton (2011).

[5] Controversies with these two features are, of course, common in the history of science. See Dascal (1998).

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quanta, could largely be explained classically.[6] In this way, the 1960s controversy over classical vs. quantum theories of laser (and other forms of) light sustains the conclusion authors like Wise and Brock and Bokulich have already put forth: Twentieth-century physics is not simply a tale of the invention of quantum mechanics and its progressive conquest of the fields of classical physics. Rather it tells us of the continued viability of both theories, and their productive interplay.[7]

2 Laser puzzles

Three of the puzzles that scientists pondered, as mentioned above, were the question of whether the Hanbury Brown–Twiss (HBT) effect existed for laser light, the nature of this light's statistics and the explanation of its coherence. The uncertainty that researchers felt is clearly revealed in the published literature.

The work that yielded the HBT effect originated in an attempt to measure the diameter of visible stars. Robert Hanbury Brown and his colleague Richard Quentin Twiss had invented a method for measuring the diameter of radio stars and wanted to show that it could be extended to visible ones. In 1956, they reported a laboratory proof-of-principle experiment. Using a conventional light source—a high-pressure Hg198 arc—they split its beam in two and aimed each of the resulting beams at a separate photomultiplier tube. The position of one tube could be varied. When it was placed in such a way that the beams hitting the two had traveled the same distance, so that the light on the photocathodes was coherent, the fluctuations in the currents from the two detectors were correlated. As the movable tube was positioned so as to reduce the amount of coherence, the correlation decreased.[8] That the current fluctuations from the tubes increased or decreased in tandem suggested that photons were arriving (or failing to arrive) in tandem.

The strategy that Brown and Twiss used to explain these results had two parts. The first was to calculate the correlations for the radio frequency case. Here a radio field strikes two separated aerials and the signals that are excited travel separately through a series of circuit elements until they are introduced into a "correlator" that multiplies them together. The second step was to calculate the correlations for the apparatus of their optical experiment and show that they had the same form as the correlations in the radio frequency case. "Let us consider the case where a linearly polarized plane wave of light is normally incident on two separate photocathodes [that are followed by filters]...we shall show that the average value of the correlation between the a.c. fluctuations in the output currents of these filters is identical with that derived above for the radio case by a classical deterministic theory" (Brown and Twiss p. 311).

It appears that Brown and Twiss were not here juxtaposing classical to quantum electrodynamics. Instead, they were claiming that the phenomenon they had exhibited

[6] See Milonni (1984).

[7] Hasok Chang makes a plea for this sort of situation in Chang (2011).

[8] The history of this experiment and the controversy it aroused are described in Silva and Freire (2013). Some minor mistakes in the description of the Brown–Twiss paper of January 1956 are corrected in *Historical Studies in the Natural Sciences* Volume 45, #3 (2015).

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could be explained by a deterministic wave theory as well as by a stochastic corpuscular one. They referred to the 1909 paper in which Einstein had calculated the fluctuations in the energy contained in a small portion of a cavity filled with thermal radiation. There were two terms to the fluctuations. In the case of the second term, "as Einstein...pointed out, this excess noise is essentially a wave interference effect, but it can be interpreted in the corpuscular picture as the so-called 'bunching' of photons. ... It is of course possible, by means of quantum statistics, to develop the theory ... entirely in terms of the particle picture ... however, we have chosen an alternative approach which emphasizes that the correlation between photons is essentially an interference effect related to the wave picture" (Brown and Twiss 1957, pp. 301, 302).

Now a mercury arc is a source of thermal light, which was thought of as coming from a large number of excited atoms radiating independently. A laser, however, makes use of stimulated emission, in which photons from one atom excite emission in others. Would this nonthermal source also give light with HBT correlations? Opinions varied. In a letter published before the first lasers functioned, John H. Sanders of Oxford thought "[s]uccessful operation of the optical maser would ...provide a light source which would allow an even more convincing demonstration of photon correlation" than that with conventional sources (Sanders 1959). In a paper submitted roughly on the operational laser's first birthday, Leonard Mandel and Emil Wolf wrote: "Until very recently it was difficult to demonstrate [the HBT] effect, because of the very low value of the 'degeneracy parameter' ... of light generated by ordinary sources. ...but the degeneracy parameter of radiation from an optical maser is of quite a different order of magnitude ... thus one may now carry out with relative ease a number of experiments which make use of the Hanbury Brown–Twiss effect."[9]

In contrast, other authors were critical. Thus, two scientists at the Canadian Defence Research Telecommunications Establishment, in a critique of an article on the signal-to-noise ratios calculated for the detection of laser light, pointed out that the author had assumed that calculations good for blackbody light, "i.e., a group of atoms emitting independently in a large number of modes," were also good for laser light (Smith and Williams 1962, p. 337). This kind of assumption could simply not be carried over for this new kind of light. Nor, by extension, could the laser be expected to give the same experimental effects, including the HBT effect.[10]

Doubts about the statistical character of laser light were closely connected with the question of HBT correlations. Because ordinary light was conceived as coming from independent radiators, it was generally held that the quantities that characterized the (intrinsically fluctuating) classical electromagnetic field, for example the electric field strength, obeyed a Gaussian probability distribution. Was that true for lasers? Or, viewing the same question from the point of view of photons, the proof that the photons in a thermal field obey Bose–Einstein statistics depended on the assumption that the atoms that emit them follow a thermal distribution, with more atoms in lower than in

[9] Mandel and Wolf (1961b), quotation on p. 1697. The degeneracy parameter is the average number of photons in the same quantum state or, viewed from the perspective of statistical mechanics, in the same cell of phase space. Still another, similar, early opinion is Givens (1962).

[10] Mandel and Wolf themselves would reverse their opinion, based on Smith and Williams, in an article submitted in February 1963 (Mandel and Wolf 1963, p. 1315).

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upper energy states. Lasers, however, made use of inverted populations, with more of its atoms in the more energetic levels. As University of Maryland physicist Carroll Alley would ask, "Could it be that some statistics intermediate between classical and Bose-Einstein apply to maser light ...?" (Quantum Electronics III, p. 108).

The uncertainties surrounding laser coherence were more complex. The laser is intrinsically of interest to both engineers and physicists. But engineers and physicists had different meanings for the word "coherent." Still worse, there were multiple and discordant meanings for the term within each discipline.

Lamentations about the multiple definitions of coherence and expressions of confusion about its meaning abound. A few examples can illustrate the point. In an article in the American Journal of Physics received in 1955, A. Theodore Forrester began "Certain widespread misconceptions concerning the nature of light, from the classical point of view, seem to have their origin in a rather loose usage of the term coherence" (Forrester 1956, p. 192). Princeton University's Robert H. Dicke, speaking at the first, 1959, quantum electronics conference, held "It is interesting to note that the extension of the concepts of the radio engineer into the optical frequency region where quantum effects are important has yet to be carried out. In this connection, there has been considerable misunderstanding of coherence concepts in the past. ... the photon experiment of Hanbury Brown and Twiss has shown how confusing are coherence concepts when quantum effects are important" (Dicke 1960, p. 573). And Israel R. Senitzky would write in a paper published in 1962, "[An] unsatisfactory situation exists with respect to the concept of coherence ... because of the various different meanings attached to the word 'coherent'" (Senitzky 1962, p. 2864).

Alongside these complaints went attempts on the part of both engineers and physicists to provide coherence with a more satisfactory definition. A letter from Marcel J. E. Golay, received in February 1961 by the Proceedings of the Institute of Radio Engineers, affirmed that "coherence is not a quantitative concept, but a qualitative one; either radiation is coherent, or it is not." To test a source of narrowband radiation for coherence, he suggested heterodyning it with two perfectly monochromatic signals. "The two components of a phasor are obtained." If the phasor traces out a "molehill" centered at the origin, the radiation is incoherent. If it traces out a "'mole-run' centered at the origin but with vanishingly small probability at that origin" it is coherent[11] (Golay 1961).

To this, R. N. Bracewell of Stanford's Radioscience Laboratory, replied "before we can discuss Golay's view that 'either radiation is coherent, or it is not,' we need a definition. The one suggested is not good." To give his own, he went on, "let us limit attention to a signal voltage $v(t)$ derived from the radiation field by an antenna." Bracewell goes on to associate this real quantity with a complex phasor, $F(t)$, and to give a definition of "a (complex) degree of coherence" which, unlike Golay's, was susceptible of gradations. He also dismissed Golay's molehill vs mole-run test with the argument that "both gasoline generators [of electricity] and ideal sine wave generators" can be made to give mole-runs. That distribution, therefore, "does not

[11] Golay was at Perkin-Elmer, an important laser firm.

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qualify as a criterion for coherent signals"[12] (Bracewell 1962). Meanwhile, Senitzky proposed, using the apparatus of the quantum physicist, "[f]or our purposes, it is most convenient to define coherence by means of a consideration related to the uncertainty principle." And he went on to define it in terms of the degree to which the instantaneous value of a dynamical variable, like position or momentum, differed from its average (Senitzky 1962, p. 2865).

All this time, however, a succession of scientists working within the classical theory of electromagnetism, and most prominently the Czech-born British scientist Emil Wolf, had been creating a highly sophisticated definition of coherence. Wolf, who had come to England in 1939 as a boy, had received his Ph.D. in 1948 at Bristol University. His mentor there was E. H. Linfoot, a mathematician and optical scientist, and Wolf had followed Linfoot as his assistant when Linfoot was appointed assistant director at the Cambridge University Observatories. Wolf's earliest work was on the development of the theories of geometrical optics and diffraction for use in the design of practical optical systems.

In January 1951, Wolf left Cambridge to join Max Born in Edinburgh in order to collaborate with him on what would become Principles of Optics, a book that would serve as a canonical text on classical electromagnetism and optics for the rest of the century. Here, while working on the chapters on interference, Wolf began to see the need for more rigorous treatments of coherence.[13]

Born retired in 1953, and in 1954 Wolf moved to the University of Manchester, where, for several years, he was supported by a series of fellowships and contracts. Drawing on recent progress in the mathematics of random processes, he proceeded to publish his results on coherence.[14] Generalizing the work of earlier authors, Wolf defined coherence in terms of the correlations of the fields at two distinct space–time points. Let the two points be P_1 and P_2. Wolf made use of a complex function of space and time, V(P,t), whose real part represented, for example, a Cartesian component of the electric field. He then introduced a "mutual coherence function" that depended upon the time average of the product of V at one space–time point with its complex conjugate at another. From this, a derived quantity, the complex degree of coherence, emerged that took the value 1 for completely coherent light and 0 for light that was completely incoherent. Values between 0 and 1 then identified the far more realistic case of light that was partially coherent.[15] On these foundations, Wolf and his students and collaborators erected an elaborate theoretical structure. It enabled them to deal with partially polarized as well as partially coherent light and to generalize Huygen's principle and other bedrock relations of classical optics. In short, at this point, Wolf had in hand a research program that was giving solid results and that seemed capable of yielding many more.

[12] Bracewell's definition of the degree of coherence is so much like that of Wolf, which I discuss below, that it is worth knowing why he does not cite Wolf.

[13] Wolf discusses his collaboration with Born in detail in Wolf (1983).

[14] The papers Wolf published are listed in the bibliography of Wolf (2001). The subset specifically related to coherence through 1966 are also listed in the bibliographies of Mandel and Wolf (1970).

[15] Wolf gives some of the progress of his thinking in the 1999 lecture "The Development of Optical Coherence Theory" (Wolf 1999).

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At Manchester, Wolf was in the thick of the discussion of the HBT results and their theoretical explanation. Hanbury Brown worked out of Manchester's Jodrell Bank radio astronomy installation. Astrophysicist Franz D. Kahn, then a Lecturer in the university's Department of Astronomy, was publishing an explanation that made use of photons "when quantum physics applies" (Kahn 1958, p. 95). In this environment, Wolf also sought to bring Brown and Twiss' results into connection with the theory he was developing. He computed the correlation of the fluctuations in intensity at two points in an electromagnetic field under the assumption that the field was due to a large number of "randomly emitting atomic radiators." It was, he showed, proportional to the square of his mutual coherence function. What Brown and Twiss had accomplished, from the perspective of his own work, was "a radically new method for the measuring of the correlation between light beams" (Wolf 1957, quotations on pp. 353 and 354).

An essential feature of his theory, and one to which Wolf repeatedly called attention, was that its components were quantities that were observable.[16] One reason among others that made this desirable was that a theory couched in such terms could be applied more successfully to instruments and experimental results. In addition, because his theory took into account the random fluctuations that real light fields inevitably show, and rewrote Maxwell's theory in terms of them, Wolf saw it as generalizing Maxwell's "deterministic" theory into a "statistical" one. "[O]ptical coherence theory ... bears a similar relationship to Maxwell's theory as statistical mechanics bears to Newtonian mechanics" (Wolf 2001, p. 621). These researches were not widely known ca. 1960, not even by optical scientists.[17] But the 1959 publication of the first edition of Principles of Optics, which incorporated Wolf's work, was beginning to disseminate them. Meanwhile, laser scientists were beginning to observe coherence in laser light. Their analyses were based, not on Wolf's work, but on the widely used and simpler criterion that light at two points were coherent if the beams proceeding from them could be made to interfere.[18]

3 Disparate approaches

Two men, Emil Wolf and Leonard Mandel, were particularly well positioned to attack the problems laser light posed. Wolf, as we just saw, had been wrestling with problems of optical coherence for almost a decade. Mandel had played a major role in discussions of the Hanbury Brown–Twiss effect. More unexpected, however, was a foray into laser puzzles by the nuclear and high-energy particle theorist, Roy J. Glauber.

By 1960, when the first lasers were operating, Wolf had a permanent position. It was in the Department of Physics and Astronomy at the University of Rochester in Rochester, New York. Here he found a strong interest in lasers and ample military

[16] See, for example, Wolf (1954).

[17] Even in 1963, Wolf would remark "there seems to be a considerable lack of agreement about the precise meaning of the term 'coherence'. Yet, in the domain of classical optics, a considerable clarification of this term has been obtained ... and a theory has been formulated which provides a satisfactory description of the majority of coherence effects ... from thermal sources" Wolf (1963b), quotation on p. 29.

[18] For example, Nelson and Collins (1961) and Abella and Townes (1961). The following year, 1962, however, would see the appeal to the Wolf theory in such experiments.

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funding to support it. One of his first tasks at Rochester was to chair the program committee for a conference on coherence that the Air Force was promoting (Bromberg 1991, pp. 106–108). Thereafter, to some degree, he turned his attention from partial coherence to the much more complete coherence lasers showed. In one paper, co-authored with Mandel, the two laid their problem out: "the general properties of completely coherent fields have so far not been studied in any detail." They then build up the two notions, of "complete coherence" between the electromagnetic disturbances at two points in an optical field and of "coherent fields," out of Wolf's mutual coherence function and his complex degree of coherence (They also use the concept of the normalized spectral density, which is the Fourier transform of the complex degree of coherence.) (Mandel and Wolf 1961a, quotation on p. 815).

Other papers, though mainly concerned with thermal light, throw a lifeline to lasers. Thus, we have a paper by Wolf alone, submitted in the Spring of 1962, that seeks to determine whether one can deduce a light source's energy spectrum (the dependence of the energy density on the frequency) from the complex degree of coherence even for cases in which the energy density is asymmetric around its average frequency. The paper is an exercise in the mathematics of complex variables, but Wolf points out that it may be useful for obtaining information about the spectrum of the output from an optical maser (Wolf 1962). In another paper, submitted at the end of 1962, Wolf makes use of the laws he had previously discovered for the way in which his mutual coherence functions change as they propagate in space and time. He concludes that "complete spatial coherence may be generated in light which is initially only partially space-coherent or even incoherent," and suggests that "[t]his result throws doubts as to the correctness of the belief that the strong spatial coherence of maser beams has its real origin in stimulated emission." "For our analysis strongly suggests that the propagation of light between the end plates of the two mirrors of the maser cavity, together with the accompanying diffractions at each mirror, [is the true origin]" (Wolf 1963a, quotations on pp. 166 and 168).

The joint papers Wolf and Mandel wrote in these early years incorporated results each was achieving individually, and it is difficult to guess the extent to which each internalized the other's reasonings. But if we try to isolate Wolf's approach, we may conclude, provisionally, that he approached the laser's problems by generalizing and extending the theoretical structures he had already created to deal with thermal light. As he wrote in a paper for an April 1963 conference, "optical coherence theory ... has primarily been developed for the analysis of effects produced with thermal light. ... [its] framework ... can readily be broadened to make the theory applicable to coherence experiments performed with any kind of light, whether from a thermal source, a laser or any other type of source."[19]

The Berlin-born Mandel, like Wolf, came from a family that took refuge in Britain to escape the Nazi holocaust. His Ph.D. research, from Birkbeck College at the University of London, was on cosmic rays, and involved him in work with particle detectors (Scully et al. 2006). From his earliest paper, published while he worked at Imperial Chemical Industries, Mandel was concerned with instruments, but also with the effect

[19] Wolf (1963b), quotation on p. 30. This same generalizing tactic is also notable in Wolf's earlier papers.

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on them of intrinsically stochastic processes, like radioactive decay. Thus, this first article took up this question: Given that the beta rays then used in the measurement of the thickness of samples were emerging in an inherently probabilistic fashion, how did this limit the accuracy of these thickness gauges.[20]

The instruments Mandel considered extended to apparatus like image intensifiers; in these, photons hitting a photocathode liberate photoelectrons that then impinge on a luminescent screen to release still additional photons.[21] Hence, when controversy broke out over the HBT effect and the responses to light of the photodetectors used in demonstrating it, it is not surprising that Mandel—by this time a Lecturer in the Department of Physics, Instrument Technology at Imperial College, London—joined the fray with two theoretical papers (Mandel 1958, 1959).

The papers are best known to physicists for a formula that gives the probability that a detector will register n photoelectrons in an interval of T seconds at time t. Mandel first assumed that the probability that the cathode of a photodetector will release an electron, in the infinitesimal interval between t and $t + \mathrm{d}t$, is proportional to $P(t)$, the classical intensity—averaged over a few cycles—of the wave that strikes it. But because the field varies from moment to moment in a random way, one cannot rest there. Instead, it is necessary to take an average over an ensemble of realizations, in each of which $P(t)$ might differ. Doing so gives a probability distribution that differs from that for classical particles. It enabled Mandel to derive formulas that support Brown and Twiss' analysis.

These papers are equally interesting, however, for the way in which Mandel, like Brown and Twiss, struggles throughout with light's wave-particle duality. To start with, he assigns the waveness of light to the classical field and the particleness to the discreteness of the photoelectrons that are emitted. "Consider a beam of light falling on some photoelectric detector ...Only the photoelectrons and not the photons are, of course, observable and our discussion must therefore be confined to the statistical behavior of the photoelectrons. ... The observable $P(t)$ provides the only link between the wave and the particle descriptions of the beam".[22] But his considerations are not always consistent. Elsewhere he invokes the statistical idea of degeneracy with its basis in a phase space of 6 dimensions, three spatial and three representing the three components of momentum, a space divided into cells of a size dictated by the quantum mechanical uncertainty principle.[23] High degeneracy corresponds to many photons crowded into a single cell. He tells us that when it occurs, the light beam has a wave character, while low degeneracy corresponds to a beam with particle properties.

[20] Mandel (1954). This, of course, is similar to the pre-World War II papers on how Brownian motion limits the accuracy of galvanometers.

[21] For example, Mandel (1955).

[22] Mandel (1958), quotation on p. 1038. Historical treatments of the wave-particle duality generally treat the pre-World War II period and include Folse (1985), Duncan and Janssen (2008) and Camilleri (2006). For aspects of Mandel's encounter with Louis de Broglie's brand of wave-particle dualism, see my manuscript, "Leonard Mandel and Experimental Tests of Quantum Mechanics," available from the Center for History of Physics at the American Institute of Physics.

[23] Because the uncertainty principle requires that the uncertainty in any one of the three spatial dimensions multiplied by the uncertainty in the conjugate component of the momentum must be equal to or larger than h, the cell must be at least of size h^3.

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"The degeneracy is also indicative of whether the wave or the particle properties of the beam predominate" (Mandel 1958, quotation on p. 1041). Again, when examining the result he has gotten for the fluctuations in the numbers of electrons released by a photocathode, with its two terms, he associates the second term with wave effects. "The excess fluctuations given by [the second term of the equation] correspond to the wave interaction noise of Hanbury Brown and Twiss" (Mandel 1958, quotation on p. 1042). "The correlation [between the fluctuations registered by two detectors] is therefore appreciable only when the wave properties, as distinct from the particle properties, of the beam become evident" (Mandel 1958, quotation on p. 1046).[24]

In the years immediately following the first operating lasers, Mandel published both experimental and theoretical papers about them. It was work done while he bounced between his home base in London, where his equipment was, and the University of Rochester, where his friend and collaborator Wolf now worked. One of the earliest theoretical papers, sent in from Rochester, applied the concept of degeneracy to the new light source. "Until very recently all light sources gave rise to nondegenerate beams in the visible ... The purpose of this note is to compare [the degeneracy parameter] for a number of sources and to draw attention to the very large values of [it] which can now be obtained for visible light from the recently developed optical maser."[25]

The high degeneracy of laser light and its consequent wave-like properties was one motivation Mandel gave for an experiment in London with one of his graduate students. Pointing out that it had been shown that light from two independent microwave sources could interfere, the paper postulated, and went on to demonstrate, that light from two independent ruby lasers could also interfere. The authors give a calculation of the effect using a classical analysis. They mention, but do not here engage with, Paul Dirac's quantum mechanics-based dictum that "Interference between two different photons never occurs."[26] Other papers Mandel sent in during 1962 show a continuing attempt to understand the relation between ordinary interference and the HBT effect, between ordinary light and laser light and between the various concepts then abroad of coherence.[27]

In all this work, Mandel's papers reflect the groping for proper notions of laser light that so marked the physics literature in these first post-laser years. What you scarcely find in them, however, are appeals to the conceptual apparatus of quantum electrodynamics (QED). Yet there were physicists who were trying to find solutions within this theoretical framework, and among them was Roy J. Glauber. Unlike Mandel and Wolf, he had not worked on these problems before, but for the phenomena he had studied, he had developed treatments that would prove adaptable to them.

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[24] Mandel's use of statistical mechanics may reflect an influence of Reinhold Fuerth, an authority on classical and quantum statistics, who was at Birkbeck College when Mandel studied there and whom Mandel thanks "for some valuable discussions" (Mandel 1958, p. 1046).

[25] Mandel (1961), quotation on p. 797. This high degeneracy was the reason Mandel and Wolf initially thought that the laser would show the HBT effect.

[26] Magyar and Mandel (1963). The paper may be one of many indications of Mandel's experimental prowess, for the ruby lasers were markedly ill-behaved. Subsequent papers would engage fully with Dirac.

[27] See, for example, Mandel (1962a, b).

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Roy Glauber was born in New York City in 1925. He worked at the Manhattan Project during World War II, and received his Ph.D. in 1949 at Harvard, under Julian Schwinger, with a thesis on the interaction of nucleons with meson fields. The next year he spent on a fellowship at the Institute for Advanced Study at Princeton University. Here, he extended his thesis work to treat two cases. The one that is relevant here is the case in which a classical electric current produces a quantized photon field (Glauber 1951).[28] Glauber wished to get around the perturbation methods then in use, that could only deal with a handful of photons, or other bosons, at a time. "Deficiencies in the mathematical techniques for handling quantized field theories obscure many questions of critical importance, such as the extent to which difficulties of the theory arise from a questionable expansion in powers of a coupling constant, and the importance of higher order corrections" (Glauber 1951, p. 395). Instead of such a perturbation theory expansion, he represented the state of the system by a function with similarities to one Erwin Schroedinger had used decades earlier as a way to portray a wave function that would cohere together, instead of spreading out with time.

After Princeton, and a year spend at the California Institute of Technology filling in for Richard Feynman, Glauber returned to Harvard. Here, one focus of his research was the phenomenon of scattering, especially the scattering of neutrons by nuclei, crystals and gases. In this work, Glauber emphasized the importance of going beyond the simple case of elastic scattering, where no energy is exchanged between the impinging particle and its target. And here too, we find certain parallels to the situations he would treat in lasers. For example, in the paper entitled "Time-Dependent Displacement Correlations and Inelastic Scattering by Crystals," (Glauber 1955), he represents the crystal as a continuous field, $\mathbf{u}(\mathbf{r}, t)$, where $\mathbf{u}$ is the displacement of each point in the crystal from its equilibrium position. The neutron, as it passes through exchanging energy with the field, creates and absorbs the field's quanta (phonons). Correlation functions between the displacements at two separated points were already being used in this type of analysis. Glauber generalized them to include the correlation between the fields at two separated points and two distinct times.

Glauber knew of the HBT effect from Harvard colleagues who were wrestling with it, but he only himself became involved after the first lasers were operated. He was approached then by Saul Bergmann, a theorist from the American Optical Company in Southbridge, Massachusetts. Bergmann had secured a copy of the 1961 Mandel and Wolf paper and wanted to recruit Glauber to help him understand what the HBT effect would be for the light from a laser (Bromberg 1991, pp. 108–110). Mandel and Wolf, as we have seen, used classical electromagnetism in their paper, and Glauber thought it essential to put the analysis in terms of quantum electrodynamics. "There is ultimately no substitute for the quantum theory in describing quanta" (Glauber 1963a, p. 85).

After pondering the problem for about a year, Glauber realized that there might be an analogy between the relation of the lasing medium to laser light and the relation between a classical current and the quantized electromagnetic field it produces that he had studied in his 1951 paper. The lasing medium could be seen as a classical current in

[28] David Kaiser describes life for young theorists at the Institute in these years in Kaiser (2005, pp. 87–93).

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this sense: Macroscopically speaking, it was an electrically polarized medium whose polarization changed with time (Glauber 2006, p. 87). The field the laser produced could then be represented by the same kind of cohering states Glauber had previously used. Glauber now gave them the name of "coherent states." One indication that he was on the right track was that when he wrote down the expansion for an incoherent light beam in terms of coherent states, he got the right statistics: His incoherent beam had a Gaussian distribution.

In a short paper received by Physical Review Letters in December 1962, Glauber laid out these results. He also introduced a quantum mechanical correlation function, C, not between two points in free space but between the photoionization events at two photodetectors separated in space and time. Correlation between the events only occurred when C departed from unity. For coherent states of the field, the correlation function C reduces to unity. Hence, coherent states "lead to no photoionization correlations at all" (Glauber 1963a, quotation on pp. 85–86). It was more likely than not that laser light would turn out to be close in behavior to coherent states. If so, laser light would not show HBT correlations.

The Physical Review Letters paper was a compilation of results without proofs. The next paper Glauber submitted was very different. It was sent to the Physical Review in February 1963 on the eve of his departure for Paris where the Third International Congress on Quantum Electronics was to be held. And it contained not only proofs but tutorials for those of his fellow physicists who did not know QED. "We shall try to construct this paper so that it can be followed with little more than a knowledge of elementary quantum mechanics. Since its subject matter is, in the deepest sense, quantum electrodynamics, we begin with a section that describes the few simple aspects of that subject which are referred to later" (Glauber 1963b, pp. 2529–2530).

The amount of new physics in the paper suggests that Glauber had done a prodigious amount of work in the late fall and early winter. Among the noteworthy results were a much extended definition of correlation and a new concept of coherence. Wolf had written his correlation function in terms of a complex quantity, V. Glauber wrote his in terms of **E**, the electric field vector. But in quantum electrodynamics, the field vectors become operators. And Glauber pointed out that it made sense to write **E** as the sum of two operators, $\mathbf{E}^{+}$ and $\mathbf{E}^{-}$, the first of which could be linked with the annihilation of photons, and the second with their creation. He developed his correlation functions in terms of these operators. There were n of them, where the integer n could take on any value; this corresponded to an experiment with n separated photodetectors.

Wolf had defined coherence of the light between two field points by deriving a complex degree of coherence from his correlation functions and assigning complete coherence to the case in which its absolute value was unity. Glauber defined complete coherence by requiring that all n correlation functions could be factored into independent functions. It was a condition mathematically similar to Wolf's but much extended. If only the first j functions could be so factored, where j was less than or equal to n, the field only had nth order coherence. "In photon coincidence experiments of multiplicity up to and including n, the photon counts registered by the individual counters may then be regarded as statistically independent results. No tendency of photon counts to be statistically correlated will be evident." The fact that the experiments of Hanbury Brown and Twiss did show correlations meant that "light beams from

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ordinary sources ...when made optimally coherent in the first-order sense, still lack second-order coherence" (Glauber 1963b, p. 2535).

Glauber emphasized the much greater variety of light fields revealed by his quantum mechanical analysis than had been considered previously. "The optical definition does not at all distinguish among the many ways in which fields may vary while remaining equally correlated at all pairs of points" (Glauber 1963b, p. 2534). He linked this with the wider domain of experimental data that was just then being enabled by light sources like the laser and by improved photodetectors. These were points Glauber would come back to repeatedly. His work showed that whole classes of radiation fields might exist, and might become observable, that were not dreamt of in classical physics.

4 Conflict and co-option

Wolf and Mandel were also on their way to the Paris meeting in February 1963, Wolf to give an invited paper on recent achievements in his program, Mandel to air new ideas for dealing with laser light. The talks, and those parts of the discussions that were included in the published proceedings, give some insight into a clash that was both intellectual and emotional. Indeed, the personal stakes must have been high. Glauber was arriving after a period of intense work and substantial accomplishment. Wolf, whose decades-long research had been in fields, like optics, often considered marginal, had seen it suddenly moved to center stage with the advent of the laser. The strife became still more intense after Paris, when Indian-born mathematical physicist E.C. George Sudarshan joined the Wolf–Mandel camp and published a so-called equivalence theorem which asserted that every quantum mechanical field could also be given a classical description. It was a fight conducted over the physics and mathematics that were being deployed, but also over the criteria for doing good science, over the validity of the underlying theories, and over versions of history. Yet the combatants were also learning from each other and were even being pushed by the struggle in new directions.

Wolf and Mandel had seen Glauber's "Photon Correlations" paper before the Paris meeting (Wolf 1984). Wolf's invited talk, "Recent Researches on Coherence Properties of Light," (Wolf 1964) did not include it among its 47 references, but an influence of Glauber seems palpable. Thus, instead of starting out with the time averages that Glauber was criticizing, Wolf starts with the kind of ensemble average Glauber favored and then specializes to a time average for stationary fields. Instead of looking at correlations between the fields at two space–time points, he starts with fields at n different points. And he explicitly corrects the suggestion he and Mandel had made, and that Glauber had excoriated in his Physical Review Letters article, that the laser would show enhanced HBT correlations. "[i]t would appear at first sight that it would be a relatively simple matter to demonstrate [the HBT] effect by using laser light. This however, is not so." For the derivation of the effect relies on "the assumption that the probability distribution which characterizes the (real) field U is Gaussian ... [but this] is not at all appropriate for laser light" (Wolf 1964, p. 31). Otherwise, as might be expected, the talk reviewed, or at least mentioned, many of the results of Wolf and his group, and some of Mandel's.

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While Wolf's talk described ongoing results in a program that had already been well formulated, Mandel's was a break with his previous work. The talk showed the same intense interest in wave vs. particle aspects of light that we saw earlier, but this time, in discussing the particle side, Mandel ventured into quantum electrodynamics.[29] He cites a formalism used by two Soviet physicists.[30] In analogy to the relation that exists in quantum mechanics between the electron and the Schroedinger wave function, the Soviet authors posited that photons could be related to a wave function in a similar way. They derived this function, $f(k)$, from Maxwell's equations by suitable substitutions and mathematical manipulations. It was defined in momentum space, but a configuration space function, $f(\boldsymbol{r})$ could, in turn, be deduced from it.

Mandel used the configuration space function. In the simple case in which each of N photons was represented by the same wave function, the square of the function represented the probability of finding a photon at, approximately, a given position at a given time (approximately, because it is not possible to locate a photon at distances smaller than its wave length). A more complicated situation arises when the various photons are in different states. The wave function then involves products of these state functions and any ensemble that is constructed in order to define the statistics of the photons must be built in terms of these more involved functions.

As did Wolf in his invited talk, Mandel introduces higher-order correlations, defining them in terms of the complex functions V(P) that formed the basis for Wolf's classical coherence theory. They are "the classical analogues of a description involving ensembles of multiple photon states" (Mandel 1964a, p. 106). As had Wolf, Mandel used these higher-order correlations to describe the HBT effect.

Glauber's response to Wolf's talk had been "I think you should treat the problem quantum-mechanically." (Quantum Electronics III, p. 34). His own talk, "Quantum Theory of Coherence," did just that (Glauber 1964). It was, in the main, a summary of the two papers he had just submitted. His comments on Mandel's paper, however, were more severe. "I do not ... understand the derivations you have given on the basis of configuration space wave functions for photons, ... since the possibility of having phase information means that the number of photons present is intrinsically indefinite. ... Your derivations take no account of some of the most essential properties of photons. To apply them as you have, is to violate complementarity of the photon number and the phase of the field" (Quantum Electronics III, pp. 108–109).

Wolf, for his part, gave a utilitarian defense. "Of course, one should try to formulate a full quantum-mechanical treatment of coherence, but this may not be very easy to do. For many purposes the classical and semi classical treatments are quite good approximations and in fact have been extremely successful" (Quantum Electronics III, p. 34). "If I understand Dr. Glauber correctly he defined complete coherence in terms of an infinite sequence of correlations. This may be very elegant but perhaps

[29] In examining Mandel's ideas, the Paris paper should be supplemented by two other works Mandel published at about this time. They are Mandel (1963a, b).

[30] Akhiezer and Berestetsky (1953). In his Progress in Optics paper (Mandel 1963b), Mandel also cites two 1930s papers on this formalism by Landau and Peierls and by J. Robert Oppenheimer. These are described by Ole Keller in (2007). For more recent work along this line, see Bialynicki-Birula 1996.

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not so practical. ... I think one must wait to see how useful some of these definitions will [be] for the analysis of experiments" (Quantum Electronics III, p. 119).

From the very start, the dispute called forth searching considerations of the application of classical vs. quantum electromagnetic theory to photoelectric emission and of the proper placement of the quantum–classical boundary. Obviously, these issues, interesting in themselves, were also at the heart of the jurisdictional wrangle. Glauber denied that modern photodetectors could be described by classical theory. "The detectors used [in traditional optical experiments]... measured only intensities which had been averaged over relatively long periods of time. The new light detectors enable us to ask more subtle questions." "The photon counter is an intrinsically quantum mechanical instrument" (Glauber 1965, quotations on pp. 65 and 67). In contrast, the Rochester group held that "[t]he simplicity of the semi-classical theory and its wide range of validity makes in well suited for the analysis of many problems relating to photoelectric detection of light fluctuations. ... a full quantum field theoretical treatment is not at all necessary for the analysis of such problems" (Mandel et al. 1964, p. 436). Glauber placed the quantum–classical boundary at the point at which large numbers of photons were present. "Classical theory, strictly speaking, deals correctly only with ... modes having large occupation numbers" (Glauber 1966a, b, p. 793). Mandel and Wolf countered that "It is a common (but mistaken) belief that semiclassical methods always fail when the light intensity is sufficiently low" (Mandel and Wolf 1966, p. 1033).

These were quarrels about problems in physics, but they were also quarrels about method. The Wolf camp would repeatedly stress the simplicity of the classical methods and the many phenomena they treated. "A theory based on classical and semi-classical concepts cannot ... be expected to be capable of answering all questions relating to coherence." But it has been successful in predicting experiments and leading to an understanding of effects "for which full quantum mechanical treatment is not yet available or is exceedingly complex." What Wolf was advocating was the use of theories whose complication matched the complexity of the problem. It is, of course, a statement about the proper way to do science.[31]

More problematic, and perhaps more interesting, was Rochester's suggestion that classical electrodynamics gives insight into what is actually going on. Hence, we find in a 1964 article by Mandel, "we see that the semiclassical theory may sometimes be just as accurate as the quantized field theory, while providing some valuable intuitive insight into the physics of the problem" (Mandel 1964c, p. B1224). Or in the Mandel–Sudarshan–Wolf paper, "[t]he method employed by Purcell is semi-classical, but it brings out the essence of the phenomenon much more clearly than most other approaches" (Mandel et al. 1964, p. 435).

But what does it mean to get physical insight when the ontology of classical physics has been rejected? When there is a consensus, as we just saw in Wolf's remark, that quantum mechanics is at ground the proper basis for explanation?[32] Indeed, we are

[31] The quotation is from Wolf (1964, p. 14). See also Wolf (1984, p. 49), "You have to match your problem with how it's treated."

[32] Bokulich (2008) discusses precisely this issue.

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not surprised to see the insight argument also appropriated by the Glauber camp, as in an article by one of his students: "It has become important in these new areas of optics [the laser and new techniques for counting photons] to make use of the greater insight and detail afforded by the quantum theory"[33] (Cahill 1965, p. B1566).

Glauber, as we have seen, argued on the basis of the generality of his results. It was not only that there were many more kinds of optical coherence than the classical theory embraced. The experience of radio frequency engineering was also covered in his theory. "There is no need ... to make any material distinction between radio frequency and optical fields" (Glauber 1965, p. 66). Indeed, one of the factors that had led Glauber to his scheme in the first place had been Golay's discussion of coherence and its use of illustrative examples from radio technology.[34]

Glauber was also arguing on ontological grounds. Quantum electrodynamics was the theory that correctly described the behavior of electromagnetic fields, and it was therefore the theory that should be used. "There is ultimately no substitute for the quantum theory in describing quanta." And this too was a statement about the right way to do science. Yet even in the years that he was elaborating his ideas, a fair amount of disaffection with quantum electrodynamics existed. As Mandel would put it some years later: "The critique stems partly from some dissatisfaction with the mathematical framework of Q.E.D., ... and partly from a sense of dissatisfaction with the conceptual framework underlying the theory. [Moreover] some people feel that Q.E.D. is less amenable than most theories to intuitive arguments" (Mandel 1976, p. 29).[35]

Yet there were also commonalities in the two camps' approaches. Both, for example, stressed the importance of building theory in close connection with observation. We have already seen Wolf's insistence on working with theoretical objects that were in principle observable. We have seen Mandel's dictum that any meaningful discussion of photons must refer to the observable photoelectrons. For his part, Glauber laid weight on the circumstance that what photodetectors actually do is destroy impinging photons and that therefore any adequate theory must base itself on the quantum mechanical annihilation operator. "All the familiar methods of detecting light quanta absorb them and thus, in effect, constitute measurements of the annihilation operator" (Glauber 1964, p. 111). It is one of the dramatic turns in this bit of history that a common criterion led to such different constructions.[36]

The battle over coherence took on a new dimension after Wolf returned from Paris. Back in Rochester, he discussed the problem with George Sudarshan and, within weeks, Sudarshan had his equivalence theorem in hand. In a note published in Phys-

[33] See the discussion of insight (i.e., understanding), and some references to the literature, in de Regt (2014).

[34] See also note 8 on p. 2534 of his "Quantum Theory of Optical Coherence" (Glauber 1963b).

[35] But Mandel's conclusion (p. 65) would be "Although semiclassical theories have had considerable success in accounting for many observed effects, ... they fail completely in other cases, and no evidence exists that should cause us to think of giving up Q.E.D. in favor of a semiclassical theory." See also my discussion of Edwin Jaynes' crusade against QED in Bromberg (2006).

[36] We may assume that these criteria (simplicity, convenience, generality and so on) reveal not only the authors' opinions but also their beliefs about what arguments would be persuasive. Hence, they also throw some light on the methodological assumptions abroad in 1960s physics.

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ical Review Letters, and a talk at the April 1963 Symposium on Optical Masers, Sudarshan presented his reasoning (Sudarshan 1963a, b). The gist of the argument was this: Using a set of states that were equivalent to Glauber's coherent states as his basis, and employing the quantum mechanical density matrix, Sudarshan claimed that the expectation values (that is, the averages) of all the quantum mechanical expressions that appeared in Glauber's correlations functions are equivalent to the expectation values derived from a classical treatment. "there is a one-to-one linear *correspondence* between the classical distribution function ... for a classical complex valued signal and the quantum-mechanical density matrix *in the sense that they both yield the same expectation values* for corresponding dynamical variables. Hence, any linear relationships between expectation values (like equations of motion, symmetry properties, etc.) can be equally well handled in terms of either formalism without any loss of generality" (Sudarshan 1963b, p. 47, emphasis in original).

The fight over classical–semiclassical vs. QED methods now expanded to include a debate over the validity of Sudarshan's theorem. It had two inter-related aspects. On the one hand, it was about Sudarshan's mathematics and in particular, about his classical distribution function, which he had named $\phi(z)$. If it were to represent a probability distribution, it should be positive everywhere and well behaved. But the Glauber camp challenged the assumption that it had these properties. One example is a 1964 paper by Dennis Holliday at the Rand Corporation and Martin L. Sage of the University of Oregon, which argued that $\phi(z)$ "cannot be interpreted as a probability-density function ... without a detailed investigation of [its] properties ... for each density matrix. ... one must follow ad hoc rules in using [it] in contrast to well defined and general mathematical rules for using probability densities. Consequently, Sudarshan's equivalence theorem is mathematically meaningless and without physical content" (Holliday and Sage 1965, p. B487).

On the other hand, and while not ignoring the expression's mathematical problems, Glauber turned $\phi(z)$ into an instrument for probing physics—more specifically, for probing the degree of classicality any particular electromagnetic field possesses. Glauber called the function $P(\alpha)$.[37] He derived it from a more general function, labeled R, which held for any field. In the special circumstances that R simplified into a well-behaved P, the field that was being described had a classical analog. Such a field, Glauber identified as having a "P-representation." For the many fields, however, for which $P(\alpha)$ was negative or even undefinable, there was no classical analog. "When the P representation ... is available ... it permits us to find the correlation functions ... by evaluating integrals which are quite similar in form to the classical averages" (Glauber 1966a, p. 793). "Indeed for a broad class of radiation fields which includes ... virtually all of those studied in optics [the P-representation is possible]. ... Its use offers deep insights into the reasons why some of the fundamental laws of optics ...are the same as in classical theory, even when very few quanta are involved" (Glauber 1963c, p. 2776). "Although the P representation possesses many unique virtues, it also possesses one great liability; for many states of the field no such representation of the

[37] The word "function" is used loosely here and does not describe its mathematical status. Glauber addresses the relation between his work and Sudarshan's in the final paragraphs of Glauber (1963c) and represents the two works as simultaneous and independent.

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field can be found in any simple or useful sense." "The variety of pure states available to a quantum-mechanical mode operator is simply much greater than the variety of states available to a classical operator" (Glauber 1966a, p. 793).

It is interesting to see how the two camps described each other's work in these early years. There are instances in which each places the other's efforts as simply one of a number of similar productions found in the literature. There are also times in which the other camp's results are portrayed as edifices erected upon their own efforts or on well-known prior results. Thus, on page 2533 of "Quantum Theory of Coherence," (Glauber 1963b) Glauber writes "classical correlation functions have received a great deal of discussion in recent years, mainly in connection with the theory of noise in radio waves. A detailed application of the classical correlation theory to optics has been made by Wolf." For his part, Wolf, in his talk at the Symposium on Optical Masers, has "the correlation functions which play a central role in [Glauber's] theory may be regarded as the quantum-mechanical transcriptions of the correlation functions of classical theory" (Wolf 1963b, p. 40). And in their 1965 review article, Mandel and Wolf introduce coherent states with a reference to the 1955 text by Leonard Schiff, and a more recent, 1964, text by William Louisell, and then say "The states have recently been studied in detail by Glauber..." (Mandel and Wolf 1965, in note 16 on p. 242).

Such putdowns have the effect of diminishing the other's originality. Their lack of validity derives, of course, from the fact that they ignore the actual route a researcher took: what papers he actually read and what course his thinking actually took. They ignore, in short, the factors to which the historian of ideas pays attention. But they also may bear witness to the degree of specialization in 1960s physics. Mandel, who was extremely catholic in his reading, is an exception. But it is unlikely that Glauber read Wolf's earlier papers or the work of authors, like Frits Zernicke, that Wolf generalized. Nor would I guess that Wolf read the 1950s Glauber. In the main, the citations in their papers form two non-overlapping sets, and it is natural that each would misunderstand the genesis of the other's ideas.

Of course, as well as down-playing each other's contributions, the two sides also stimulated each other. We have already seen some evidences. One was Wolf's increased concern for higher-order correlation functions in light of Glauber's work. Another may well have been Glauber's many discussions of the difference between classical and quantum theories. And Glauber's QED definitions of correlation functions and the criteria for coherence certainly bear a striking resemblance to Wolf's classical theory. Another, particularly noteworthy, example of cross-stimulation may be Mandel's move from a formalism of photon wave functions to one in terms of annihilation operators and coherent states. As early as the fall of 1963, he built his quantum mechanical explanation of the effect he and George Magyar had discovered on the foundation of Glauber's theory (Mandel 1964b).

5 Conclusion

In the end, both sides could claim victories. The facets of lasers' behavior and of the light they produced came to be handled by a multiplicity of models: semiclassical, quantum electrodynamic, engineering. A great deal could be derived from semiclassi-

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cal theory. "As a practical tool [semiclassical radiation theory] is indeed very successful in describing the principal operating features of lasers" (Milonni 2007, p. 113). A great deal, but not everything. For example, generally an ad hoc term had to be added to the semiclassical theory to represent spontaneous emissions by the molecules constituting the laser medium.[38] The quantized theory avoided ad hoc assumptions and gave additional information, like the statistics of the photons comprising the field.

As for our three protagonists, their paths were affected to varying degrees by the work they did during the controversy. Wolf, who had formulated a productive research program in the context of classical electromagnetism well before the laser, continued to develop it. Of principal significance was the research he and his colleagues did on the relation between the coherence of light and its polarization, and on a new way of representing coherence that used, as a variable, frequency instead of time. Applications included the propagation of light through media as diverse as turbulent atmospheres, optical fibers and human tissues (Wolf 2007a, b).

Leonard Mandel, on the other hand, became a leading explorer, as both theorist and experimentalist, of the non-classical light that the controversy introduced. In this role, he examined some of physics' most fundamental questions. An example is a 1991 experiment he did with his graduate students that showed that it was not the making of an observation that determined which of two complementary properties would be manifest, but rather the setting up of an experimental situation that would permit such an observation to be made, whether or not it is, in fact, carried out (Zou et al. 1991).

Glauber, for his part, added to his pre-laser interests research that extended his work on coherent fields, on the P-representation and on the distinction between classical and quantum electromagnetism. He also participated in the application of the machinery of his optical theories to material particles, both bosons and fermions.[39] And in 2005, he received a Nobel Prize in physics for his contributions to the quantum mechanical theory of optical coherence.

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Appendix

Wolf's mutual coherence function may be written as

$$\Gamma(P_1, P_2, \tau) = \langle V(P_1, t+\tau)V^*(P_2, t)\rangle$$

where the angle brackets denote an average over time. His complex degree of coherence is then obtained by normalizing this function (Wolf 1955).

$$\gamma = \frac{\Gamma(P_1, P_2, \tau)}{\sqrt{\Gamma(P_1, P_1, 0) \cdot \Gamma(P_2, P_2, 0)}}$$

38 Mandel and Wolf (1995), Chapter 18, "The single-mode laser." Two other texts that help illustrate the variety of approaches are Sargent et al. (1974) and Siegman (1986).

39 Some examples are Glauber (1966b) and Cahill and Glauber (1969, 1999).

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If the absolute value of γ is unity, it is clear that Γ factors into two independent functions, in conformity with Glauber's prescription for first-order coherence. Wolf's terminology differs. Since $\Gamma(P_1, P_2, \tau)$ connects two different space–time points, he calls it a second-order correlation.

In his 1957 paper on the HBT effect, Wolf calculates the correlation of the fluctuations in intensity at two space–time points. He writes the fluctuation at one of these points as

$$\Delta I = I(t) - \langle I(t)\rangle$$

where

$$I(t) = V^2(t).$$

The correlation of the fluctuations at two points is then

$$\langle \Delta I_1(t) \cdot \Delta I_2(t+\tau)\rangle$$

For any two random functions $X(t)$ and $Y(t)$ that satisfy a two-dimensional Gaussian probability distribution, Wolf proves that

$$\langle \Delta X^2 \cdot \Delta Y^2\rangle = 2\langle XY\rangle^2$$

Setting $X = V_1$ and $Y = V_2$, Wolf is then able to equate the correlation of intensity fluctuations to $2\langle V_1\,(t)\,V_2\,(t+\tau)\rangle^2 = 2\Gamma^2$ (In this publication, Wolf uses the symbol J for Γ).

Glauber writes the electric field operator, $\vec{E}$, as $\int_{-\infty}^{+\infty} \boldsymbol{e}\,(\omega, \boldsymbol{r})\, e^{-i\omega t} \mathrm{d}\omega$.

Then, $\vec{E} = \int_{-\infty}^{0} \boldsymbol{e}\,(\omega, \boldsymbol{r})\, e^{-i\omega t} \mathrm{d}\omega + \int_{0}^{+\infty} \boldsymbol{e}\,(\omega, \boldsymbol{r})\, e^{-i\omega t} \mathrm{d}\omega = \vec{E}^- + \vec{E}^+$.

He cites Dirac's *Principles of Quantum Mechanics* as the authority for interpreting E^+ as a photon annihilation operator. When it acts on a state with n photons, it produces a state with $(n-1)$ photons, while E^- yields an $(n+1)$ state:

"the probability per unit time that a photon be absorbed by an ideal detector at point **r** at time t is proportional to

$$\sum_f \left|\langle f \left| E_\mu^+\,(\boldsymbol{r}, t)\right| i\rangle\right|^2 = \sum_f \langle i \left| E_\mu^-\,(\boldsymbol{r}, t)\right| f\rangle\langle i \left| E_\mu^+\,(\boldsymbol{r}, t)\right| i\rangle$$
$$= \langle i \left| E_\mu^-\,(\boldsymbol{r}, t)\, E_\mu^+\,(\boldsymbol{r}, t)\right| i\rangle."$$

Here $|i>$ is the state of the initial field and $|f>$ is the state of the final field and the sum is over all possible final states (μ is an index that Glauber used to indicate the photon's state of polarization) (Glauber 1963b, quotation on p. 2531).

The probability that n photons are absorbed at n photodetectors then becomes

$$\langle i| E_\mu^-\,(\vec{r}_1, t_1) \ldots E_\mu^-\,(\boldsymbol{r}_0, t_0)\, E_\mu^+\,(\boldsymbol{r}_0, t_0) \ldots E_\mu^+\,(\boldsymbol{r}_1, t_1)\,|i\rangle$$

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For the case where the initial state of the field is imperfectly known, Glauber invokes the quantum mechanical density matrix and writes $tr\left\{\rho E_{\mu}^{-}(\boldsymbol{r},t)\, E_{\mu}^{+}(\boldsymbol{r},t)\right\} = G^{(1)}(\boldsymbol{r}t, \boldsymbol{r}t)$ for the average counting rate of an ideal photodetector. For an n photon coincidence, this becomes $G^{(n)} = tr\{\rho E^{-}(\boldsymbol{r}_1, t_1) \ldots E^{-}(\boldsymbol{r}_n, t_n)\, E^{+}(\boldsymbol{r}_{n+1}, t_{n+1}) \ldots E^{+}(r_{2n}, t_{2n})\}$. Like Wolf, he arrives at a definition of coherence by normalizing these correlation functions. He sets $g^{(n)} = G^{(n)}(x_1 \ldots x_{2n}) / \prod_j \left\{G^{(1)}(x_j, x_j)\right\}^{1/2}$, where x stands for $\mathbf{r}$ and t, and the product is evaluated for j ranging from 1 to $2n$.

"If the field in question possesses nth-order coherence, it must, therefore, have $g^{(j)}(x_1 \ldots x_j, x_j \ldots x_1) = 1$ for $j \leq n$. It follows from the definition of the $g^{(j)}$ that the corresponding values of the correlation functions ... factorize" (Glauber 1963b, 2534–2535).

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