Modelling the Hanbury Brown – Twiss Effect
The Mid-Twentieth Century Revolution in Optics

A Talk for HQ3

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I begin by reading from the preface to a textbook that was written in 1967. The two authors were John Klauder and George Sudarshan and they wrote: "Several events of the past decade have fundamentally changed the nature and outlook of the venerable subject of optics. On the experimental side these events include the invention of radically different sources, such as the laser, and the introduction of photon counting correlations and the associated [Hanbury Brown – Twiss] intensity interferometry... On the theoretical side has been the development of a quantum theory of partial coherence and the delineation of similarities as well as differences in the classical and quantum theories."[1]

As you see, the authors mention four events. Let me lay out their sequence in time as a prelude to suggesting a periodization for this bit of history. The Hanbury Brown – Twiss intensity interferometer came first. It was developed for measuring the diameter of radio stars in the years 1949 to 1954.

In the radio star version, electromagnetic radio waves from the stellar source stimulated currents in two separated aerials. Each of the currents fluctuated, and the degree to which their fluctuations were correlated as the separation between the aerials was varied gave the data from which the star's diameter could be computed.

In 1955, Robert Hanbury Brown and Richard Twiss began the project of adapting the intensity interferometer to the measurement of visible stars. Their first step was a proof of principle experiment in the laboratory. It was designed to show that visible light

1 John R. Klauder and E.C.G. Sudarshan, Fundamentals of Quantum Optics, (W.A. Benjamin: 1968), v.

from a common source that struck two separated photoelectric cells would also produce two fluctuating currents and that the fluctuations would also show correlations that varied with the separation of the detectors.[2]

This experiment, which was published in Nature in January 1956, raised a storm of controversy. The first wave of this storm was experimental. In their experiment, light from the source was separated into the two beams that would be parceled out to the two photocells by a half-silvered mirror. To some physicists, a correlation between photons parceled out in this way seemed to contradict the basic principle that a photon is indivisible and should go one way or the other.

Motivated, in part, by this worry, the Canadian physicist Eric Brannen and his associate H.I.S. Ferguson ran an experiment of their own with slightly different equipment.[3] It was published in September 1956 in Nature, and it seemed to challenge Hanbury Brown and Twiss' experimental results. This part of the storm was quickly quieted. By the end of 1956, both Hanbury Brown and others were able to show that the Brannen-Ferguson results did not conflict with their own.

This being settled, scientists began the task of trying to model the Hanbury Brown- Twiss results theoretically.

A variety of models were proposed. Most of the authors made the obligatory genuflection to the proposition that a final theory would need to be thoroughly quantum mechanical. But most of these models treated light as a classical wave.

Now back to the list of Klauder and Sudarshan. The laser was the first item on the list and, of course, it was being proposed in exactly these years. It was not until 1960,

2 "Correlation Between Photons in Two Coherent Beams of Light," Nature 177 (Jan. 7, 1956), 27-29.
3 "The Question of Correlation Between Photons in Coherent Light Rays," Nature 178, (Sept. 1, 1956), 481-482.

however, when the first lasers were actually operated, that they were brought to bear on the issues Hanbury Brown and Twiss had inspired. At that point, it was clear that lasers gave off a very different kind of light from starlight. The question now arose: were the theories that had just been developed to explain intensity interferometry also applicable to laser light?

Now the laser looked as though it would have important civilian and military applications. As a result, it not only raised this and other scientific questions. It also brought with it a flood of new funding. In particular, the United States military was pouring money into it. One result was additional research into its meaning for intensity interferometry.

And this is where we get to the third item on the Klauder-Sudarshan list, the development of a quantum theory of coherence. It was through one such research project that a Harvard professor, Roy J. Glauber, got involved. In 1961, he was asked by a scientist at the American Optical Company to explore the relation of the laser to the Hanbury Brown – Twiss effect.[4] In 1963, after about two years of studying the problem, Glauber put forth a comprehensive theory that deviated from most of the earlier theories by quantizing the electromagnetic field. It offered a new and generalized theory of coherence and it would win him a Nobel prize in physics in 2005.

Now a classical theory of the coherence of light already existed, and had been developed, above all, by the Czech – English physicist Emil Wolf. A good deal of work had gone into it, and a good deal of success had been achieved by it. A jurisdictional fight therefore developed, between the older theory and the new one. It mainly involved

4 Interview with Roy J. Glauber by Joan Lisa Bromberg, April and May, 1987, archived at the Center for History of Physics of the American Institute of Physics.

Glauber and his followers, on the one hand, and Wolf and his group on the other, and it was as bitter as any political battle.

The fourth of the points that Klauder and Sudarshan make follow from this controversy. This was the delineation of the similarities and differences between theories that treated the light field as classical and those that quantized it: That is to say, it was the delineation of those domains where semiclassical theories worked and those for which quantum electrodynamics is indispensable.

This suggests a periodization for what I have called the mid-twentieth century revolution in optics. We can lay down five stages. The first embraces Hanbury Brown and Twiss' proposal to use their instrument for visible stars and includes their proof-of - principle experiment to show that it could be done, and their early struggles with scientists who criticized their experiment, or their ideas or both.

The second stage would be the first group of theories that sought to explain their experimental results. It lasts from about 1956 to 1960. The third is the appearance of functional lasers and the investigations into the implications of laser light for intensity interferometry. I am dating this stage from about 1960 to about 1963.

The fourth stage is Glauber's theory and the controversy with Wolf. And the last, starting about 1965, is the ironing out of the jurisdiction of classical vs. quantum field theories.

But why do I think that historians – and for that matter, also sociologists and philosophers – should pay attention to this mini-revolution? I would argue that each of these five stages provides some interesting topics for those of us who work in the social and philosophical study of sciences. What I want to do in this talk is to lay out some of these topics. I want to talk with you about which might be worth pursuing and find out from you whether some have already been studied.

Stages one and two take place in the 1950s. There are two principal topics I see here. One is in the domain of conceptual developments and the other concerns interactions among physics, electrical engineering and instrumentation.

The conceptual topic has to do with the ideas about light that were used by physicists in the 1950s. This includes ideas about the nature of light, the wave – particle duality for light, the nature of interference, and the associated concept of coherence.
So many people were engaged in the Hanbury Brown – Twiss controversy that we have a good sample of opinions to work with. Besides the models they used, we may try to tease out their attitudes, whether realist or instrumentalist, towards their models.

Let me give you some examples. One of the earliest defenders of Hanbury Brown and Twiss was the Harvard University professor Edward M. Purcell. In an article that was published in the December 29, 1956 issue of Nature, he argued that the correlations the two had found are just what one would expect from the fact that photons obey Bose-Einstein statistics.

He sets the probability that a photoelectron will be ejected from a photocell proportional to P, and then he throws in an agnostic disclaimer: "It makes no difference," he writes, "whether we think of P as the square of an electric field strength or as a photon probability density."[5]

From this, Purcell derives an expression for the fluctuations in the number of photoelectric counts in successive and equal intervals of time and eventually, an

5 "The Question of Correlation Between Photons in Coherent Light Rays," Nature 178 (Dec. 1956), 1449-1450.

expression for the correlation between the fluctuations in that number for two separated photocells.

Purcell also offers – although "warily" - a picture of photons as wave packets. He writes "Think, then, of a stream of wave packets, ... in a random sequence. There is a certain probability that two such trains accidentally overlap. When this occurs they interfere and one may find (to speak rather loosely) four photons, or none, or something in between as a result."

From archival material that Indianara collected and that I am not permitted to read, we know that Purcell disliked this wave packet model. Nevertheless, he uses it in some of his manuscript calculations.

And so do others in the 1950s. An Edinburgh professor of physics named Richard Sillitto uses it in a comment on Purcell's publication. He characterizes the model as "crude". And in the interest of full disclosure, I need to tell you that a few years later he not only repudiates this model but the very usefulness of the idea of a photon.[6]

The physicist A. Theodore Forrester uses the model to explain a much noticed experiment on temporal interference between two Zeeman components of a spectral line of mercury.[7] So the idea of wave-packets were very much part of the palette of conceptions of light that physicists used in for their calculations in the 1950s.

Another example from the late 1950s is the conception of the wave – particle duality that is put forth by Leonard Mandel. Mandel was born in Berlin to a Jewish

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6 "Correlation between Events in Photon Detectors," Nature 179 (June 1, 1957), 1127-1128. "Light waves, radio waves and photons," Bulletin of the Institute of Physics (May 1960), 129-134.

7 Forrester, Richard A. Gudmundsen, and Philip O. Johnson, "Photoelectric Mixing of Incoherent Light," Physical Review 99 No. 6 (September 15, 1955), 1691-1700.

family and brought to England as a child. At the time of the Hanbury Brown – Twiss brouhaha he was teaching at Imperial College, London, in the Department of Physics and Instrument Technology. There we find him in conversation with a more recently arrived refugee physicist, Reinhold Fuerth from Prague. Fuerth had made major contributions to the theory of quantum statistics in the 1930s.

Mandel gives his view of the wave – particle duality in an article which takes Purcell as his point of departure. To arrive at his model, he wields the concept of observability. He writes, "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.[8]

Furthermore, only the intensity of the incoming light is observable and not the amplitude of the electric vector. Indeed, it is not the instantaneous intensity, but only the intensity averaged over a number of cycles.

Let this average intensity be denoted by P. Mandel sets the probability that a photoelectron will be emitted in a unit time proportional to P. He then writes: "The observable P(t) provides the only link between the wave and particle descriptions of the beam.

That is, the wave - particle duality for light becomes a relation between the intensity of a classical wave and the statistical behavior of discrete photoelectrons.

This view of the wave-particle duality is a step in a long story that will reach into the 21st century. In an article published in 2007, Howard Carmichael, of the University of Auckland in New Zealand, dubs it the BKS approach and explains how his group's

8 "Fluctuations of Photon Beams and their Correlations,' Proceedings of the Physical Society 72 (1958)pp. 1037-1048.

experiments have disproved it.[9] I think there may be room for a nice paper stretching from Mandel to Carmichael.

What Mandel does next is to introduce a second probability. The first probability leads to a distribution for the number of photoelectrons emitted in an interval of length T that is classical (Poissonian). But Mandel writes: “This is not, however, a distribution that can be found experimentally. For when P(t) fluctuates at random ... only the ensemble averages ... are observable.”

So he introduces a weighting function for the P and calculates a distribution over an ensemble and this final move gives non-classical statistics.[10] For particular values of the parameters, it leads to Bose -Einstein statistics. And Mandel concludes: “The distribution can be seen to arrive naturally from the association of photons with Gaussian random waves.”

Yet another example of conceptions about light in the late 1950s has to do with interference.

In his autobiography, Hanbury Brown portrays this as a one of the battlegrounds on which the controversy was fought. He writes: “Our work really put the cat among the pigeons. ... to a surprising number of people the idea that the arrival of photons at two separated detectors can ever be correlated was not only heretical but patently absurd, ... I would be waylaid by an indignant physicist in the corridors of the University ... Thrusting a copy of Dirac's <u>Quantum Mechanics</u> under my nose he would point to pages ... where Dirac states that: 'Interference between two different photons can never occur' He would then ask me how I thought photons could arrive in pairs if they didn't interfere with each

9 “Quantum Fluctuations of Light: A Modern Perspective on Wave/Particle Duality,” in <u>Quantum Mechanics at the Crossroads</u>, eds. James Evans and Alan S. Thorndike (Springer” 2007), 182-212.

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other".[11]

Indeed, puzzlement over Dirac's dictum was a key feature of the responses to the Hanbury Brown – Twiss effect. In his 1991 memoir, Hanbury Brown rejects its applicability to his apparatus. But in the theoretical paper the two authors published in 1957, they accept it.[12] They explain it as follows: "When interpreting interference phenomena ... it has been emphasized by Dirac (1947) that one must not talk of interference between two different photons, which never occurs, but rather of the interference of a photon with itself. ... {Now the intensity fluctuations depend upon the beat frequencies between different ... components of the incident radiation] ... one must not interpret a beat frequency as an interference between photons of different energy, but rather as a phenomenon caused by the uncertainty in the energies of the individual photons which may be associated with either of the two Fourier components."[13]

Now Hanbury Brown was a senior scientist at the Jodrell Bank installation of the University of Manchester. And the "indignant physicist" who brandishes Dirac has to have been Leon Rosenfeld, who was a member of the University of Manchester physics department at this time. We know Rosenfeld was closely reading the drafts of the article that Hanbury Brown and Twiss were writing, and suggesting – or even insisting upon changes.

It would be interesting to know why Rosenfeld was so passionate about Dirac's dictum at this point. It would be interesting to know whether it was particularly in Britain that Dirac's statement held sway. For there is a series of papers by Mandel and

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11 The Intensity Interferometer, pp. 120-121.

12 "Interferometry of the intensity fluctuations in light. I. Basic theory: the correlation between photons in coherent beams of radiation," Proc. Royal Society A 242 (1957), 300 – 324.

13 Ibid., p. 308.

his students in the 1960s that also uphold the Dirac dictum even though they deal with interference between the light from two completely independent lasers.

The investigation of interference between beams emitted by independent sources had a considerable history by the 1960s, when Mandel did these experiments. It had been explored for radio, microwave, and optical beams. Mandel was following up on work showing beats, that is, temporal interference, between independent light sources. In ingenious experiments performed with his students, he demonstrated transient spatial interference between the light from two independent lasers. He showed that this effect persisted even when the intensity of the lasers was so low that, on average, only one photon was in the apparatus at a time.[14]

Yet even in these papers, Mandel is still defending the Dirac dictum. He writes: “It might be asked whether the effects discussed here in any way contradict the statement of Dirac. ... The answer is that they clearly do not. Any “localization” of a photon in space – time implied by the photoelectric measurement automatically rules out the possibility of knowing its momentum, and with it the possibility of assigning the photon to one or another beam. ... Just as in conventional interferometry, each photon is to be consideered as being partly in both beams, and “interferes only with itself.”[15]

But was such faithfulness to Dirac peculiar to people trained or working in Britain? Was it true for Edward Purcell or others of the group at Harvard? Was it true here in Berlin, where a group at the Institut fuer spezielle Problem der theoretischen

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14 G. Magyar and L. Mandel, “Interference Fringes Produced by Superposition of Two Independent Maser Light Beams,” Nature 198, (April 20, 1963), 255-256. L. Mandel, “Quantum Theory of Interference Effects Produced by Independent Light Beams,” Physical Review 134, No. 1A (6 April 1964), A10-A15. R. L. Pfleegor and L. Mandel, “Interference of Independent Photon Beams,” Physical Review 159, No. 5 (25 July 1967), 1048-159.

15 “Quantum Theory,” cited above, pp. A 14 – A 15.

Physik at the Deutsche Akademie der Wissenschaften was very much involved in the problem of interference between photons from independent sources?[16]

Helge Kragh tells us that the text where Dirac lays out this dictum was dominant in the 1950s (CHECK). But did it also dominate in the United States and the DDR?

And would a history of experiments and theories about interference from the 1950s through the 1970s be another way to look at the mini-revolution in optics? The interference phenomenon has so many practical applications, ranging from spectroscopy and astronomy to industrial applications. Would a history that tackled this period from that angle allow us to bridge the fields of foundations of physics and applications of physics?

This leads me into the second topic I want to suggest. That is the interaction of physics, electrical engineering, and instrumentation. Obviously, an interaction among the three is very much embodied in the intensity interferometer of Hanbury Brown and Twiss. But another set of instruments worth looking at is photodectors and in particular, the photoelectric cell.

One of the earliest of the Hanbury Brown – Twiss critics was Peter Fellgett at the Cambridge University Observatories. His criticism came precisely out of the work that was being done on photodectors. Sensitive photoelectric cells began to be developed before the second world war. They were developed with great vigor after the war.

By 1950, they have reached a point at which their sensitivity is limited by the fluctuations in the number of photons that strike the photocathode. So scientists like

16 See, for example, H. Paul, W. Brunner and G. Richter, "Interferenz zwischen unabhaengigen Photonen," Ann. Physik 12 (1963), 325-328.

Fellgett have been calculating photon fluctuations. Fellgett's criticism[17] is that the formulas for the fluctuations that they had arrived at differed from the Brown-Twiss and Purcell formulas.

This was a controversy that dragged on for more than two years.[18] It certain weighed on the mind of Hanbury Brown. One wonders whether he and Twiss might have been the more sensitive to this criticism because it came from within the astronomy community. I would ask Indianara to be sensitive to this possibility when she visits the Hanbury Brown archive after the conference.

Now from the point of view of the researches of people like Fellgett, the Hanbury Brown – Twiss experiment is just another case of how fluctuation phenomena are increasing in importance as ever new instruments are developed.

So this is another perspective we could take. As well as embedding the Hanbury Brown – Twiss experiment in a history of interference, or in a history of conceoptions of light, we could tell it as part of a story of instruments.

This has the advantage that we might need to go back to the second world war and the development of instruments that could, for example, use fluctuating electromagnetic fields to jam enemy radar. And we would find that electrical engineers and physicists would be pooling their knowledge of noise in electrical circuits on the one hand, and statistical mechanics, on the other, as they worked together to develop such instruments.

We are used to the importance of World War II radar technology for the invention of the laser and for radio astronomy. But there may be another interesting story here: the

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17 Peter Fellgett, "The Question of Correlation Between Photons in Coherent Beams of Light," Nature 179 (May 11, 1957), 956-957. See also P. B. Fellgett, "Photo-electric Devices in Astronomy," Vistas in Astronomy, ed. Arthur Beer, (Pergamon Press, 1955), 475-490.

18 It was settled in favor of Purcell and Brown – Twiss in "Fluctuations in Photon Streams," by Fellgett, R. Clark Jones and R. Q. Twiss, Nature 184 (Sept. 20, 1959), 967-969.

war-time melding of circuit noise and quantum statistics.

So far, I have limited myself to events that took place before 1960. But the period I called stage 4, the fight between the classical and quantum theories of coherence, might be the juiciest for historians. This is because it mixes conceptual changes with social, philosophical and human factors.

The unusual fierceness of this controversy shows up even in the publications. The physicist Mario Bertolotti characterizes Glauber's comments in the Harvard professor's first paper, as "rather sharp". This is where Glauber writes that a prediction by Mandel and Wolf "is misleading and follows from an inappropriate model of the maser beam."[19]

But one can find still sharper comments. Thus, for example, two partisans of Glauber's approach criticize a paper by a Wolf ally and conclude: "Sudarshan's equivalence theorem is mathematically meaningless and without physical content." [20]

Nevertheless, even while criticizing each other's work, the two camps are each claiming that the other group is building its physics on the back of their own work. Thus Glauber, in a 1987 interview, voices his suspicion that the papers Mandel and Wolf presented at a 1963 conference in Paris was based on his own work. Conversely, in a 1965 article for the Reviews of Modern Physics, Mandel and Wolf lay out the history of Glauber's work in such a way as to imply that some of it was formulated as an analogy with work of their own.[21]

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19 Mario Bertolotti, Masers and Lasers: An Historical Approach (Adam Hilger, Ltd, Bristol: 1983), 217.

20 Dennis Holliday and Martin L. Sage, "Statistical Description of Free Boson Fields," Physical Review 138 (26 April 1965), B485-B487. Quotation on B 487.

21 Interview with Roy J. Glauber by Joan Lisa Bromberg, 21 April 1987 and 12 and 20 May, 1987, on

So there is a tension here between criticizing the other guy and claiming he is appropriating your work. In fact, it would be interesting to know the extent to which they actually were making use of each other's results. It would be a somewhat different question, but also an interesting one, to know the extent to which work in one camp stimulated work in the other. Unraveling this might be a useful exercise in internal history.

But besides this kind of internal history, there is work here also for sociologists. Both sides used some tactics that were essentially social. Terminology is one. It was Glauber, for example, who suggested "quantum optics" as the title for a laser conference that Cecile DeWitt was planning. That is a name that stuck and that brings optics firmly under quantum field theory.

There are statements about the kinds of theories that physicists should favor. In a 1964 paper, Mandel, Sudarshan and Wolf defend the semi-classical approach as "bring[ing] out the essence of the [Hanbury Brown – Twiss] phenomenon much more clearly than most other approaches." And they praise the "simplicity" of this theory and "its wide range of validity."[22]

Glauber, on the other hand, writes: "There is ultimately no substitute for the quantum theory in describing quanta."[23] And he repeatedly stresses the generality of his approach, which covers cases inaccessible to approaches that treat the field classically.[24]

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deposit at the Center for History of Physics at the American Institute of Physics. L. Mandel and E. Wolf, "Coherence Properties of Optical Fields," Rev. of Modern Physics 37, No. 2 (April 1965), 231-287. See the first paragraph of p. 249.

22 L. Mandel, E.C.G. Sudarshan and E. Wolf, "Theory of photoelectric detection of light fluctuations," Proc. Phys. Soc. 84, (1964), 435-444, quotations on 436-436. Reprinted in Seclected Work of Emil Wolf, with Commentary (Singapore: World Scientific: 2001), 215- 224.

23 Roy J. Glauber, "Photon Correlations," Physical Review Letters 10 No. 3 (1 Feb. 1963), 84-86. Quotation on p. 85.

24 To be provided

To look at all the extra-scientific tactics that were employed will require looking at referee reports and ferreting out the papers of the organizers of summer schools and conferences. So there is work to be done here. And it would also be worth seeing the records that have to do with Glauber's 2005 Nobel Prize.

Obviously, there are issues to be found in the interaction of the two subfields of quantum field theory and optics. Did participants come with different sets of mathematical expertise, or experience with different kinds of phenomena or instruments? Did the doubts about the foundations of quantum electrodynamics that were current in the 1960s influence participants?

But I want to close with a project that is part sociology, part physics and part psychology. From what I know, Glauber and Wolf never got over their annoyance at each other. Of course, each must have been sensitive to what this controversy meant for his career and reputation. In addition, however, their personalities seem to have differed profoundly, and to have clashed profoundly. Was Wolf, the courteous Middle European, also the pretentious and pontificating figure that Glauber took him to be? Was Glauber, who did not suffer gladly anything he judged to be confused physics, as unnecessarily aggressive as Wolf thought him?[25] Each of these physicists would make an interesting subject for biography. The project I suggest, then, is a biographical study that would treat their dispute with full candor.

25 Interview with Glauber, cited in note . Interview with Emil Wolf by Joan Lisa Bromberg, 1984, deposited at the Center for History of Physics of the American Institute of Physics.