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INDIANARA LIMA SILVA

# UMA HISTÓRIA DO CONCEITO DE FÓTON NA SEGUNDA METADE DO SÉCULO XX: PARA ALÉM DE HISTÓRIAS DO MODELO BOLA DE BILHAR

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Tese apresentada ao Programa de Pós-Graduação em Ensino, Filosofia e História das Ciências, da Universidade Federal da Bahia e da Universidade Estadual de Feira de Santana, como requisito parcial para a obtenção do grau de Doutora em Ensino, Filosofia e História das Ciências, sob a orientação do Prof. Dr. Olival Freire Jr.

Com estágio de doutoramento no Program in Science, Tecnology, and Society do Massachusetts Institute of Technology sob a supervisão do Prof. Dr. David Kaiser, através do apoio financeiro concedido pela Fulbright e CAPES.

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COMISSÃO EXAMINADORA

Prof. Dr. Olival Freire Jr. (Orientador)
Universidade Federal da Bahia (UFBA)

Prof. Dr. David Kaiser
Massachusetts Institute of Technology (MIT)

Prof. Dr. Luiz Davidovich
Universidade Federal do Rio de Janeiro (UFRJ)

Prof. Dr. Thomás Haddad
Universidade de São Paulo (USP)

Prof. Dr. Aurino Ribeiro Filho
Universidade Federal da Bahia (UFBA)

Prof. Dr. André Luis Mattedi Dias

Universidade Federal da Bahia (UFBA)

Salvador-Ba

2013

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# RESUMO

A literatura de história da ciência tem sugerido que o estabelecimento do conceito de fóton na física aconteceu logo após a década de 1930. Todavia, argumentamos que desenvolvimentos teóricos e experimentais culminaram, após a década de 1950, em uma reformulação do conceito de fóton. O nosso estudo histórico considerou as contribuições dos cientistas britânicos Robert Hanbury Brown e Richard Quentin Twiss, do físico norte-americano Prêmio Nobel Roy J. Glauber, e dos experimentos que evidenciaram a natureza quântica da luz, nas discussões sobre o fóton e o seu conceito. Ademais, refletimos sobre a prática científica dos nossos personagens a partir dos estudos filosóficos de Peter Galison, e dos sociológicos de Pierre Bourdieu.

**Palavras-chaves: Conceito de Fóton, Natureza da Luz, Dualidade Onda-partícula, Óptica Quântica, História da Física, Prática Científica, Ensino de Física**

# ABSTRACT

The literature on the history of science has suggested that the establishment of the concept of the photon in physics occurred soon after the 1930s. However, we argue that after the 1950s theoretical and experimental developments contributed to revisit the concept of the photon. Our historical analysis focuses on the contributions of the British scientists Robert Hanbury Brown and Richard Quentin Twiss, of the Nobel Prize winner American physicist Roy J. Glauber, and of experiments that showed the quantum nature of light, in the discussions regarding to the photon and its concept. Moreover, we drew some reflections about the scientific practice of our protagonists through Peter Galison's philosophical studies and Pierre Bourdieu's sociological analysis.

**Keywords: The Concept of the Photon, Nature of Light, Wave-particle Duality, Quantum Optics, History of Physics, Scientific Practice, Physics Teaching**

## SUMÁRIO

## O século do fóton:

## delineando o objeto da tese, aspectos historiográficos e fontes

Desde a segunda metade do século XX até os dias atuais, os fótons têm se destacado na pesquisa de ponta em física e em outras áreas. Além do estabelecimento do campo da óptica quântica em meados das décadas de 1960-1970, hoje em intenso desenvolvimento, uma nova área de pesquisa emergiu durante a transição dos séculos, a fotônica. Mas, o que levaria à criação de uma outra área de pesquisa? Em que tal campo difere da óptica quântica? Como se deu a reorganização institucional ou a criação de novos institutos? Em quais áreas a fôtonica está sendo aplicada? Estas questões merecem a atenção de historiadores da ciência. Apesar de não ser o foco da nossa análise, o estudo histórico da criação da nova área da fotônica, destacaremos alguns elementos que nos ajudarão a compreender o quão importante o fóton se tornou na pesquisa de ponta nos últimos anos. Embora o termo "fotônica" já tivesse sido utilizado em títulos de artigos publicados a partir da década de 1930[1], conforme a Web of Science, o estabelecimento e a demarcação daquela nova área aconteceu com a criação de sociedades, instituições, revistas científicas e conferências dedicadas exclusivamente à fotônica. No final da década de 1980, a *IEEE Lasers and Electro-Optics Society* (LEOS) fundada em 1965, cujo nome foi modificado em 2009 para *IEEE Photonics Society*, criou a primeira revista científica chamada

[1] Os primeiros artigos a utilizarem o termo "fotônica" nos seus respectivos títulos foram os seguintes: D. V. Posejpal, Journal de Physique et le Radium **3**, 390 (1932); J. Clay, Physica **2**, 811 (1935); J. Clay e C. Levert, Physica **12**, 321 (1946); L. Tenaglia, Nuovo Cimento **17**, 423 (1960); S. Iwao, Helvetica Physica Acta **38**, 251 (1965). É importante destacar que tais artigos não tiveram uma repercussão significativa na comunidade científica, segundo dados obtidos da Web of Science Database.

*Photonics Technology Letters*, em vigor até o momento.[2] A justificativa dada pela sociedade a favor da mudança de seu nome reflete parte do interesse daquele novo campo, "[o] novo nome reflete as aplicações em expansão associadas com o campo de interesse da sociedade e melhor representa a sua visão, missão e escopo. A fotônica tem sido utilizada amplamente para descrever o campo mais amplo de pesquisa e aplicações relacionadas com a geração, controle e detecção da luz, incluindo comunicações em fibra óptica, lasers, CDs, DVDs, scanners em supermercados e câmeras digitais"[3]. Na própria página da referida sociedade, lê-se *IEEE Photonics Society: transforming science into technology*. Isto é, o ponto forte desta nova área, que está relacionada com a óptica quântica, informação quântica, eletrônica quântica, optomecânica, eletro-óptica, óptica-eletrônica, é a sua aplicação tecnológica e industrial.

Em 2003 e 2005, criou-se respectivamente a *European Photonics Industry Consortium* e a *Photonics21*, ambas são financiadas por segmentos industriais e outros setores interessados no desenvolvimento da pesquisa em fotônica na Europa. Em sua apresentação, a *Photonics21* ressalta que "a sua missão é a coordenação de atividades de pesquisa e de desenvolvimento na Europa... de modo que os parceiros contribuam para a educação, a pesquisa básica, a pesquisa e o desenvolvimento aplicada à fabricação, e todas as aplicações relevantes". E continua, "a entrada no 'século do fóton' requer uma iniciativa europeia comum que permita à indústria e à pesquisa defender as suas iniciativas pendentes para explorar as aplicações futuras quase

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[2] IEEE é um acrônimo para The Institute of Electrical and Electronics Engineers; Outras revistas científicas, ver Photonics Spectra, Optics & Photonics Focus, Nature Photonics, Photonics news, e Photonics Online.

[3] IEEE Lasers & Electro-Optics Society History. Disponível em http://www.ieeeghn.org/wiki/index.php/IEEE_Lasers_%26_Electro-Optics_Society_History. Acesso em 21 de Janeiro de 2013.

ilimitadas da luz e para colher os benefícios esperados em termos de criação de empregos e de riqueza... Sem uma forte liderança europeia nas tecnologias fotônicas, estas indústrias ficarão vulneráveis à forte concorrência dos EUA e da Ásia".[4] Além de incentivar o entrelaçamento mais profícuo entre a pesquisa básica e a aplicada, a *Photonics21* também almeja fortalecer as indústrias na corrida tecnológica e competição por mercados entre Europa, EUA e Ásia.

Na seção *Research Highlights* da newsletter da *IEEE Photonic Society* foi publicado em 2011 o artigo *An Overview of EU-funded Photonics Research* por Thomas Skordas e Gabriella Leo, em que se discute a pesquisa em fotônica no cenário europeu. Segundo os autores, a indústria fotônica europeia é a lider de mercado em áreas importantes da fotônica, tais como, comunicações, biofotônica, iluminação, fotovotaicos, tecnologias de laser industrial, e em proteção e segurança. O campo da fotônica entre 2007 e 2012 já recebeu cerca de €300 milhões de investimentos do Sexto Programa do Quadro de Pesquisa da UE (FP6). Ainda segundo o artigo, há uma nova estratégia da UE para 2020 que é a de "desenvolver uma economia baseada no conhecimento e inovação (crescimento inteligente); promover uma economia mais eficiente, mais verde e competitiva (crescimento sustentável); e fomentar uma economia de emprego elevado (crescimento inclusivo)". As tecnologias fotônicas podem desempenhar um papel importante na estratégia da UE para 2020 na tríade ciência-tecnologia-sociedade. Por exemplo, tornando possível comunicações na era terabit e, assim, "aumentando dramaticamente a capacidade de dados e a velocidade de transmissão de dados, enquanto reduzem a pegada de carbono das redes e o custo geral por bit. Podem superar as limitações da eletrônica em

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[4] Disponível em http://www.photonics21.org/AboutPhotonics21/Presentation.php . Acesso em 21 de Janeiro de 2013.

computadores através da computação completamente óptica. Podem revolucionar a saúde e fornecer várias formas de detectar, tratar, e prevenir câncer e outras doenças graves. Também podem desempenhar um papel central na abordagem de outros grandes desafios, tais como a eficiência energética e o movimento para a economia de baixo carbono"[5]. Onde há promessas tão atrativas, há financiamento e pesquisa de ponta. Deste modo, é compreensível o aumento significativo no número de instituições voltadas à pesquisa em fotônica em todo o mundo, além do crescimento no número de conferências sobre o tema[6].

O mais recente ilustre resultado que está relacionado com a pesquisa naquela área foi a nomeação do Prêmio Nobel de Física de 2012. Os físicos Serge Haroche e David J. Wineland foram laureados com o Nobel pela pesquisa em métodos experimentais capazes de medir e manipular sistemas quânticos individuais[7].

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[5] Research Highlights An Overview of EU-funded Photonics Research. Disponível em http://photonicssociety.org/newsletters/jun11/RH_Overview.html . Acesso em 21 de Janeiro de 2013.

[6] Entre os diversos centros e instituições que realizam pesquisa em fotônica, destacamos, por exemplo, no Brasil, Laboratório de Ótica Quântica da Universidade Federal do Rio de Janeiro; em Berlim, Max-Born-Institut für Nichtlineare Optik und Kurzzeitspektroskopie (MBI); em Boulder, National Institute of Standards and Technology (NIST) – Quantum Electronics and Photonics Division; em New York, University of Rochester Institute of Optics; em Orlando, CREOL | The College of Optics & Photonics at the University of Central Florida; na França, Institut des Nanotechnologies de Lyon (INL), no México, Centro de Investigaciones en Óptica; na Espanha, Institut de Ciències Fotòniques (ICFO); na Polonia, Wojskowa Akademia Techniczna Instytut Optoelektroniki; em Viena, Institut für Quantenoptik und Quanteninformation; em Paris, Laboratoire Kastler Brossel da Ecole Normale supérieure.

[7] The Nobel Prize in Physics 2012. Disponível em http://www.nobelprize.org/nobel_prizes/physics/laureates/2012/. Acesso em 21 de Janeiro de 2013.

O caso da fotônica faz-nos lembrar o da "era da eletrônica", assim como ficou conhecida no século XX. Os dois campos têm em comum o resgate de um termo, já conhecido, para a demarcação de uma nova área de pesquisa. Como é sabido a revolução na eletricidade aconteceu com o desenvolvimento de dois dispositivos – a válvula eletrônica e o transistor – os quais permitiram grandes avanços tecnológicos como o advento do radar, o desenvolvimento das telecomunicações, dos computadores analógicos e digitais, da televisão, do processo de controle industrial, entre outros. Inicialmente, o termo "electronics" era concebido como uma área da física que estudava as propriedades dos materiais e o comportamento dos elétrons. Hoje, a área que abrange tal campo de pesquisa é denominada "physical electronics". E, a eletrônica tornou-se o campo das aplicações tecnológicas Este novo direcionamento da pesquisa em eletrônica teve início com a criação de um periódico, *Electronics*, em 1929 pela McGraw-Hill. Segundo Charles Susskind, foi a primeira vez que o termo foi utilizado na descrição da "eletrônica" como uma área tecnológica ou industrial, ainda que o termo já tivesse sido usado anteriormente[8].

Devido às inúmeras aplicações tecnológicas, este será, de fato, o século do fóton? As promessas são muitas, mas, a resposta a tal questionamento será dada pela nova geração de historiadores da ciência, esperemos. Não obstante, podemos aprender bastante sobre o fóton e o seu conceito olhando para o século passado, o que pode contribuir para o entendimento tanto histórico quanto conceitual de um novo campo de pesquisa que tem atraído a atenção de vários pesquisadores ao redor do mundo e de diversas agências de financiamento.

A história do conceito de fóton no século XX é o tema central desta tese, cujo principal objetivo é relatar o modo pelo qual tal conceito foi sendo construído e reestruturado ao longo das

[8] C. Susskind, The origin of the term 'electronics', disponível em http://ieeexplore.ieee.org/stamp/stamp.jsp?arnumber=05219631. Acesso em 26 de Janeiro de 2013.

décadas de 1950-60 e as de 1970-80. A tese a ser defendida é a de que, ao contrário do que é sabido e disseminado na literatura de história da ciência, o conceito de fóton não se estabeleceu na física após a década de 1930. Pelo contrário, desenvolvimentos teóricos e experimentais da segunda metade do século XX desafiaram o conceito comumentemente concebido pelos físicos – fóton como uma partícula pequena, indivisível e localizada – lançando, assim, uma nova luz sobre o conceito de fóton. O tema em análise é muito bem-vindo e instrutivo para ajudar-nos a compreender os caminhos ulteriores que conduziram tanto para a evolução de um novo e, promissor, campo de pesquisa quanto para os avanços teóricos e experimentais da óptica quântica. A tese é constituída de dois estudos de caso tendo como personagens os cientistas britânicos Robert Hanbury Brown (1916-2002) e Richard Quentin Twiss (1920-2005), e o físico teórico norte-americano Nobel Prize Roy J. Glauber (1925- ), embora outros personagens também aparecerão durante a nossa narrativa[9]. Esta abordagem baseada em estudos de caso é a atual tendência na historiografia da ciência, através da qual é possível destacar a forma pela qual "as descobertas científicas foram produtos de situações locais particulares e práticas comuns com

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[9] O tema da tese e, portanto, os estudos de caso, foi escolhido durante uma disciplina ministrada pelo Prof. Dr. Olival Freire Jr. sobre os fundamentos da teoria quântica. Nesta disciplina, discutimos detalhadamente o livro didático The Quantum Challenge publicado pelos físicos George Greenstein e Arthur G. Zajonc, os quais dedicaram o segundo capítulo aos Photons. Ficamos surpresos com as novas informações contidas nele sobre as controvérsias em tornos dos fótons após a década de 1930, cuja primeira seção era bastante provocativa Do Photons Exist? . Então, decidimos averiguar se aquela narrativa tinha algum respaldo histórico, e eis que surgiu o presente projeto de pesquisa. É importante destacar que alguns temas de pesquisa podem surgir em situações atípicas, através de um capítulo de livro, como foi o caso. Todavia, não há garantias de que o tema será, de fato, frutífero o bastante para produzir um artigo, ou, mesmo, uma tese. Ou seja, é necessário muita reflexão e discussão com outros pesquisadores da área, além da pesquisa sobre o nível de plausibilidade do que foi dito ou escrito.

toda as suas contigências históricas e sociais"[10]. Isto é, cada episódio histórico é fruto de um local e tempo determinado, e sofre influências de um contexto específico (científico, social, institucional, político, entre outros). E o seu estudo é realizado de forma mais profunda e circunscrita a períodos de tempo mais curtos do que aquele apresentado nas grandes narrativas. Apesar das vantagens dos estudos de caso, há um movimento na historiografia da ciência em resgatar a *big-picture* da história da ciência ou visão de generalistas[11]. Na nossa análise, entretanto, optamos pelo estudo de caso já que os nossos episódios históricos envolvem um grande número de personagens, cuja formação, ambiente institucional e contexto são bastante distintos um do outro. A tese também está organizada em forma de artigos independentes, cada estudo de caso constituirá um artigo-capítulo. Por esta razão, peço-lhes antecipadamente desculpas por eventuais repetições que se farão necessárias no decorrer da tese.

Construimos uma "história do conhecimento", na qual as "*technicalities of scientific practice"*[12] são essenciais para entendermos a atividade científica dos nossos personagens e os seus múltiplos contextos e dimensões, a partir da análise dos registros de laboratórios, manuscritos, artigos científicos e livros publicados, autobiografias e memórias, e correspondências dos nossos personagens. A nossa narrativa foi, portanto, construída através do diálogo entre as fontes originais e arquivos pessoais e a literatura secundária escrita por historiadores da ciência. Também utilizamos o recurso da historiografia cientométrica que

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[10] R. E. Kohler e K. M. Olesko. Introduction: Clio meets Science. In: _________. (Eds.). Clio meets Science: The Challenges of History, Osiris **27**, 3 (2012).

[11] Ibid.

[12] O. Darrigol, For a History of Knowledge. In: K. Gavroglu, Kostas e J. Renn (Eds.). Positioning the History of Science, 2007, p. 33-34.

possibilitou uma análise quantitativa da dinâmica de citações de artigos científicos a cada ano e o seu impacto nas diversas áreas do conhecimento[13]. Outra fonte historiográfica que utilizamos foi a história oral, amplamente utilizada na escrita da história contemporânea, que é utilizada como uma "fonte complementar" que contribuirá para o entendimento de fatos, da ciência moderna, que ainda estão "obscuros". É importante destacar que a história oral não deve ser utilizada como uma única fonte já que a memória do personagem está sujeita a lapsos, e o mesmo pode escolher que tipo de imagem ou narrativa lhe é mais coveniente. Logo, é essencial contrapor os dados provenientes da história oral com outras fontes (artigos, arquivos pessoais, autobiografias, entre outras)[14].

Por fim, a nossa narrativa pode ser caracterizada como uma análise histórica em tempo e espaço definidos, na qual respeitamos os aspectos técnicos da atividade científica, à época, e a sua relação com os seus múltiplos contextos, evitando, deste modo, o anacronismo[15]. Nesta tese, utilizamos os arquivos pessoais de Robert Hanbury Brown depositados na Royal Society em Londres, os de Edward M. Purcell localizados na Harvard University, bem como documentos do Arquivos Léon Rosenfeld, no Niels Bohr Archive, em Copenhague. A pesquisa nos dois

[13] F. A. A. Freitas e O. Freire Jr., RBHC **2**, 129 (2003); H. Kragh, Introdução a Historiografia das Ciências. Trad. Ana Simões e Henrique Leitão. Portugal: Porto Editora, 2003.

[14] V. Alberti. Manual de História Oral (5ª ed.). Rio de Janeiro: Editora FGV, 2005; L. Hoddeson. The conflict of memories and documents: dilemmas and pragmatics of oral history. In: R. E. Doel e T. Sorderqvist. The Historiography of Contemporary Science, Technology, and Medicine: written recent science. New York: Taylor and Francis Group, 2006.

[15] O. Dumoulin. Anacronismo. In: A. Burguière (Org.). Dicionário das Ciências Históricas. Trad. Henrique de Araújo Mesquista. Rio de Janeiro: Imago Ed., 1993, 47-48.

primeiros arquivos foi realizada por mim mesma e a documentação do terceiro nos foi enviada pela gentileza daquele arquivo. Enquanto o segundo capítulo foi enriquecido com a utilização de tais arquivos pessoais, o capítulo seguinte ainda carece dos arquivos de Roy J. Glauber, Emil Wolf e Leonard Mandel. O que contribuiu significativamente, contudo, para a escrita do capítulo dedicado aos estudos de Glauber e aos desenvolvimentos das décadas de 1970 e 1980 foi as entrevistas realizadas com Roy J. Glauber no Lyman Laboratory em 2012, e outra com o físico francês Alain Aspect no Institut d'Optique em 2011. Também utilizamos as entrevistas depositadas no American Institute of Physics dos físicos Emil Wolf e Leonard Mandel.

Esta análise histórica torna-se relevante por duas razões. Por um lado, estudos sobre o fóton e o seu conceito não estão bem documentados na literatura da história da ciência. Como veremos, a literatura secundária abrange apenas os primeiros desenvolvimentos sobre o conceito de fóton, até meados da década de 1930. A contribuição original da tese é evidenciar que as discussões e controvérsias sobre o fóton, que pareciam ter sido resolvidas após aquele período, ressurgiram com força na década de 1950, e tiveram desdobramentos nas décadas posteriores. Isto é, a história do conceito de fóton está longe de resumir-se às contribuições de Albert Einstein, Robert Millikan, Arthur H. Compton, e Niels Bohr. Ao comentar um dos artigos que compõe esta tese, o historiador Helge Kragh deixou extremamente claro a contribuição de nosso primeiro estudo de caso, "[t]he paper deals with a topic that cannot be found elsewhere in the history of science literature, namely, the debate concerning the concept of the photon initiated by a 1956 paper by Hanbury Brown and Twiss. It is very well documented and partly based on archival sources, and it gives a clear and comprehensive account of the subject under investigation"[16]. Quanto ao segundo capítulo, a história do desenvolvimento teórico dos estados

[16] Opinião externada em um parecer emitido pelo mesmo por meio da Historical Studies in the Natural Sciences.

coerentes por Roy Glauber, a criação da óptica quântica, e as suas influências no entendimento do conceito de fóton, são uma lacuna na história da ciência. Apesar de ter um projeto de pesquisa relacionado à história da óptica quântica nos EUA entre 1950 e 1970, a historiadora Joan Bromberg ainda não publicou sobre o trabalho de Glauber. Até onde temos conhecimento, não há nenhuma análise histórica publicada sobre aquele tema.

Além da sua contribuição para a literatura da história da ciência, a história do conceito de fóton no século XX também pode contribuir para o ensino de física. As questões teóricas sobre o fóton fazem parte, como sabemos, do currículo de alunos de graduação, e até mesmo, de nível médio. Geralmente, os estudantes terminam um curso de física geral para engenheiros e físicos com a imagem de um fóton como "uma partícula indivisível", uma entidade clássica. Mesmo na década de 1950, físicos ainda possuíam aquele tipo de representação em mente, o que dificultou o entendimento de questões experimentais e teóricas. Diante das dificuldades inerentes à própria complexidade do conceito de fóton, que está longe de ser resumido àquela definição, surgem os seguintes questionamentos: Que conceito de fóton deveríamos ensinar, então? O que representa um fóton para a óptica quântica? Um conceito de fóton mais sofisticado e elaborado o ajudaria a compreender conceitos chaves do novo século subjacentes à era da informação quântica? Neste sentido, acreditamos que esta tese poderá contribuir para avançarmos na discussão e no entendimento destas e outras questões. O primeiro passo seria, portanto, compreender os aspectos históricos subjacentes ao conceito de fóton após a década de 1950, e, em seguida, refletir sobre a sua implicação didática. Deste modo, os alunos poderiam ter a oportunidade de compreender e refletir (sobre) alguns aspectros intrínsecos ao processo de construção do saber científico, desenvolver raciocínio lógico e crítico, perceber as relações entre os diferentes contextos e personagens, além de permitir a aprendizagem de conceitos científicos[17].

A tese está organizada como segue. A introdução compreende, além destas considerações, um panorama da evolução do conceito de fóton ao longo do século XX e na transição para o século XXI. Este panorama será publicado, na forma de um capítulo, no livro "Ciência na Transição dos Séculos: Conceitos, Práticas e Historicidade" organizado por Olival freire Jr., Charbel El-Hani e Ileana Greca, e editado pela Universidade Federal da Bahia através de financiamento da Fapesb. O segundo capítulo é dedicado ao experimento realizado pelos britânicos Robert Hanbury Brown e Richard Quentin Twiss em 1956, e a controvérsia em torno dele. Devido a urgência no envio do material para a defesa, tal capítulo ainda está no formato da Historical Studies in the Natural Sciences onde o artigo foi recentemente publicado. Contudo, trabalharemos para que na versão final o segundo capítulo esteja com uma formatação análoga aos demais capítulos. No terceiro capítulo, apresentamos a teoria quântica da coerência desenvolvida em 1963 pelo fisico teórico Roy J. Glauber. Em particular, estamos interessados em compreender o conceito de fóton que emergiu da óptica quântica. Ainda neste capítulo, analisamos brevemente os experimentos realizados na década de 1970 e na de 1980 que evidenciaram a natureza quântica da luz. Por fim, concluimos com algumas reflexões sobre a prática científica dos nossos personagens que permeou os capítulos precedentes.

[17] M. Mathews. Science teaching: the role of history and philosophy of science. New York: Routledge, 1994.

## O Conceito de Fóton na Transição dos Séculos:
## do modelo "bola de bilhar" para ...

### Introdução

Os conceitos de fóton e fotônica são tão ubíquos na ciência e na tecnologia contemporâneas que é pensável que o conceito de fóton está bem estabelecido na Física desde que foi primeiramente sugerido, no início do século XX. De fato, pode-se pensar que, após os resultados experimentais que corroboraram o efeito fotoelétrico e o efeito Compton, não se tinha mais o que discutir sobre o fóton e o seu conceito. A resposta à indagação: o que é um fóton?, então, poderia ser respondida quase que automaticamente: uma entidade pequena e indivisível, cuja energia e *momentum* são conservados no processo de interação entre a radiação e a matéria e cuja representação é comumente conhecida como bola de bilhar. Hoje, em pleno século XXI, a resposta àquela questão não é tão automática e nem mesmo trivial. Ao ser entrevistado em meados de junho de 2012, o prêmio Nobel de física Roy J. Glauber (1925-) recebeu a seguinte pergunta: que conceito de fóton emergiu da óptica quântica, especialmente, dos estados coerentes? Após alguns momentos de pausa, silêncio, os quais deram origem àquelas reticências que fazem parte do título deste capítulo. Glauber (2012, p. 13), então, respondeu:

> O que é um fóton? É uma partícula pontual? Não. É um pacote de onda? Bem, talvez […]. Então, o que é? Para mim, é principalmente hoje apenas uma

> excitação do estado quântico.[1] Não posso facilmente construir imagens deles [fótons], mas, sei como fazer matemática.

O fóton tornou-se um conceito tão complexo e delicado durante a transição dos séculos, que muitos físicos dedicaram-se à matemática em detrimento das questões conceituais subjacentes àquele conceito. Nessa análise, tais questões são revisitadas de modo a se discutir as dificuldades associadas à compreensão de um conceito que há mais de cem anos foi introduzido na comunidade científica, mas, mesmo assim, nunca deixou de ser um conceito controverso.

Neste capítulo, discutiremos de forma condensada o modo pelo qual o conceito de fóton foi sendo construído, questionado, e reformulado durante a passagem dos séculos. A seção I dedica-se aos primeiros desenvolvimentos da Teoria Quântica, em particular àqueles relacionados com os *quanta* de luz entre os anos de 1905 e 1930. Na seção II, abordaremos uma parte pouco conhecida da história da Física que se refere às discussões sobre o conceito de fóton após a década de 1950 e os experimentos que desempenharam um papel fundamental no entendimento da natureza da luz. Na última seção, examinaremos as discussões contemporâneas (ainda em curso) sobre o fóton em pleno século XXI. Por fim, algumas considerações finais serão apresentadas. Neste estudo, utilizamos como fontes a literatura primária publicada pelos nossos personagens e a literatura secundária que aborda o tema em questão. Além disto, fizemos uso do recurso da cientiometria no intuito de verificarmos a dinâmica de citações do vocábulo *photonic*

[1] Em Teoria Quântica, o conhecimento do estado quântico de um sistema é necessário para se fazer predições acerca do comportamento futuro do sistema. Nesse caso, é possível determinar as probabilidades para todas as observáveis de um sistema físico por meio de um estado quântico. Para mais detalhes, consultar Ballentine, 2009.

durante a transição do século XX para o XXI.[2] A nossa narrativa constitui-se de uma análise história acerca do conceito de fóton, a partir da qual tentamos compreender, à época, os problemas enfrentados e as soluções sugeridas pelos físicos, evitando, portanto, o anacronismo.

## O tradicional conceito de fóton entre 1905 e 1930

Como é sabido entre físicos, historiadores e filósofos da ciência, o conceito de fóton – termo cunhado em 1926 pelo físico-químico norte-americano Gilbert N. Lewis (1875-1946) – foi introduzido há mais de cem anos.[3] A história sobre o conceito de fóton geralmente apresentada na literatura inicia-se no ano miraculoso de Einstein, 1905, até meados da década de 1930. Vejamos, então, a visão comumente difundida pela história da ciência sobre o desenvolvimento do conceito de fóton no século XX baseada em alguns estudos. (BRUSH, 2007; DARRIGOL, 2009; FICK; KANT, 2009; JAMMER, 1966; KRAGH, 1999, 2009; KUHN, 1987; MEHRA; RECHENBERG, 1982; PATY, 2009; SANCHÉZ-RON, 2001; STUEWER, 1975; TAKETANI; NAGASAKI, 2001; WHEATON, 1983)

A hipótese de que a radiação seria apenas emitida ou absorvida pela matéria através de quantidades discretas de energia foi introduzida em 1905 por Albert Einstein (1879-1955) após a conejctura de que a radiação "[...] consistia em um número finito de quanta de energia, localizados em pontos do espaço que se movem sem se dividir, e que poderiam somente ser

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[2] Sobre a cientiometria e a história da ciência, ver Freitas; Freire Júnior, 2003.

[3] Embora o termo *photon* tenha sido cunhado em um contexto diferente daquele da criação da Teoria Quântica – Lewis estava à procura de uma teoria para a valência química dos elementos ─ a palavra *photon* passou a ser empregada pelos pais fundadores da Teoria Quântica no final da década de 1920. (LAMB, 1995)

produzidos e absorvidos como unidades completas". (EINSTEIN, 1905, p. 202) Com essa hipótese, Einstein obteve êxito na explicação de fenômenos que a teoria clássica não era capaz de fazê-lo, tais como o efeito fotoelétrico, a fotoluminescência, e a regra de Stokes.[4] Apesar disso, a hipótese do *quantum* de luz proposta por Einstein receberia muitas críticas de físicos, a saber, Max Planck (1858-1947), Max von Laue (1879-1960), Wilhelm Wien (1864-1928), Arnold J. W. Sommerfeld (1868-1951), e Niels Bohr (1885-1962). No ano de 1921, Einstein receberia, contudo, o mais renomado reconhecimento da sua formulação teórica para a explicação do efeito fotoelétrico, o Nobel de Física. E embora o físico experimental norte-americano Robert A. Millikan (1868-1953) tivesse se tornado um crítico da hipótese dos quanta, ele receberia o Nobel de Física de 1923 justamente pelo seu resultado empírico a favor das predições de Einstein para o efeito fotoelétrico. Em 1927, após anos em busca de uma explicação clássica para o efeito que viria a ser chamado de efeito Compton, o físico norte-americano Arthur H. Compton (1892-1962) explicou o espalhamento dos raios-X pela matéria através da hipótese dos quanta, observando uma satisfatória concordância entre a sua abordagem teórica e os resultados experimentais. Compton também concluiu que houve uma conservação da energia e do *momentum* durante cada interação singular entre a radiacão e a matéria. Ou seja, cada colisão singular entre o fóton e o elétron (tipo bola de bilhar) obedeceria às leis de conservação.[5]

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[4] O efeito fotoelétrico explica o porquê da emissão de elétrons de um material, comumentemente metálico, à medida que radiação eletromagnética incide sobre ele. A fotoluminescência está associada à emissão de luz de qualquer material após a absorção de radiação. Já a regra de Stokes refere-se ao fato de que a frequência da radiação emitida por um material deve ser menor ou igual à frequência da radiação absorvida, obedecendo, assim, o princípio da conservação de energia.

[5] Para uma leitura sobre o período anterior à descoberta do efeito Compton, consultar Silva, I; Freire Júnior; Silva, A, 2011.

Tais resultados empíricos obtidos por Millikan e Compton pareciam confirmar a hipótese de Einstein de que a radiação era uma grandeza quantizada, e não uma forma contínua de energia. Todavia, os resultados obtidos por Compton não convenceram a Bohr que, juntamente com Hendrik Kramers (1894-1952) e John C. Slater (1900-1976), publicou em 1924 uma abordagem estatística do fenômeno em que a energia e o *momentum* apenas seriam conservados estatisticamente. Na teoria de Bohr-Kramers-Slater (BKS), a radiação foi considerada uma onda eletromagnética clássica – hipótese contrária aos quanta de Einstein – e a quantização seria apenas introduzida nas transições entre os níveis de energia dos átomos. Eis que surgiu uma disputa entre a teoria BKS e os resultados de Compton. Tal disputa foi resolvida em 1925, pelos físicos alemães Walther Bothe (1891-1957) e Hans Geiger (1882-1945), os quais confirmaram experimentalmente a validade da lei de conservação da energia para os processos atômicos, o que estava de acordo com os dados de observação encontrados por Compton. No mesmo período, o próprio Compton e o seu estudante A. W. Simon observaram novamente o mesmo efeito.

A hipótese dos quanta sobreviveu, até mesmo, às abordagens semiclássicas. No mesmo ano em que Compton foi laureado com o Nobel de Física pelo efeito Compton, os físicos Erwin Schrödinger (1887-1961), Guido Beck (1903-1988), e Gregor Wentzel (1898-1978) publicaram separadamente explicações semiclássicas para o efeito Compton (Schrödinger) e o efeito fotoelétrico (Beck e Wentzel). Analogamente à abordagem BKS, a radiação era considerada uma onda eletromagnética clássica e apenas a matéria era quantizada. Desse modo, nenhum conceito de fóton seria introduzido *antes* do processo de interação entre a radiação e a matéria. (BECK, 1927; SCHRÖDINGER, 1927; WENTZEL, 1927) Tais abordagens publicadas no final da década de 1920, até onde temos conhecimento, ainda não foram exaustivamente exploradas pela literatura de história da ciência.[6]

Apesar de ter sobrevivido às críticas e àquelas novas abordagens, a hipótese dos quanta de luz ainda não era capaz de explicar o grande dilema da dualidade onda-partícula para a luz: Como explicar os fenômenos ópticos, tais como interferência e difração, a partir da hipótese dos quanta de luz? A resposta àquela questão veio em 1927, tendo Bohr como o seu proponente. Naquele período, "[...] Bohr abandonou a sua oposição à LQH [light quantum hypothesis] , e inventou um conceito, a 'complementaridade', para explicar como (ou melhor, afirmar que) pares de conceitos aparentemente incompatíveis, tais como onda e partícula, podem ser ambos válidos ao mesmo tempo". (BRUSH, 2007, p. 225-226) Durante uma palestra em uma conferência em Como, na Itália, Bohr discutiu a noção de espaço-tempo (a medida da posição de um elétron localizado em um ponto definido no espaço em um determinado instante de tempo) e introduziu o conceito de complementaridade através do qual é sugerido que os conceitos clássicos são complementares, mas mutuamente exclusivos.[7] Isso significa que as variáveis dinâmicas *momentum* e posição, por exemplo, são complementares (os dois conceitos são essenciais na descrição clássica completa de

[6] Embora os artigos de Brush (2007), Scott (1967), Stuewer (1975) citem os artigos publicados por Schrödinger sobre o efeito Compton, os autores não fazem uma análise mais profunda sobre os mesmos. Mesmo na obra de referência sobre a biografia de Schrödinger, o historiador Moore não mencionou a sua resistência em aceitar a explicação quântica do efeito Compton. (MOORE, 1989) Quanto ao historiador Jammer (1966, p. 171), ele mencionou que Schrödinger "[...] atribuiu à física uma realidade exclusivamente ondulatória depois do advento da mecânica ondulatória". No entanto, ele não ressaltou sua explicação semiclássica para o efeito Compton como uma ilustração da sua resistência à realidade da natureza corpuscular da radiação.

[7] Para mais detalhes, ver Camilieri (2007), Held (1994) e Stapp (2009).

um sistema físico). Contudo, eles também são mutuamente exclusivos (uma vez que a posição de uma partícula é determinada, perde-se informação sobre o *momentum* da mesma).[8]

Em torno de 1935, como argumentado pelo historiador Stephen Brush (2007), a hipótese do *quantum* de luz já tinha sido estabelecida na comunidade de físicos devido ao efeito Compton, ao efeito fotoelétrico, e a outros fenômenos relacionados com os raios X. E, também acrescentaríamos em termos teóricos o papel da complementaridade na tentativa de resolver a dualidade onda-partícula.

A história do estabelecimento do conceito de fóton na comunidade científica está bem documentada pela literatura secundária entre os anos de 1905 a 1930. A visão difundida pela história da ciência, como discutida anteriormente, sugere-nos que as discussões sobre o fóton e o seu conceito já haviam arrefecidas no final da década de 1930.

A própria etimologia do vocábulo *photon* (*photo* – luz e *on* – unidade) já expressa por si só a forma pela qual o conceito de fóton foi sendo empregado e interpretado pela comunidade de físicos após a década de 1930: *uma unidade de luz*. [9] Tal definição também pode ser encontrada nos mais renomados livros didáticos de Teoria Quântica, utilizados na formação de físicos

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[8] Bohr também propôs o princípio da correspondência justificando, assim, a utilização de expressões clássicas na Teoria Quântica e a sua interpretação a partir de conceitos clássicos. Como destacado por Brigitte Falkenburg (2009, p. 126) "[...]ele justificou a sua visão de complementaridade da mecânica quântica em termos da correspondência entre fenômenos quânticos mutuamente exclusivos, por um lado, e os conceitos clássicos de onda e partícula, por outro lado".

[9] Photon. In: Oxford Online Etymology Dictionary. Disponível em: http://www.etymonline.com/index.php?allowed_in_frame=0&search=photon&searchmode=none. Acesso em : 14 sep. 2012.

durante o século XX, a saber, *Atomic Physics* escrito por Max Born, e *The Principles of Quantum Mechanics* por Paul Dirac. Em seu livro, Born (1970, p. 82) define o *photon* como segue: "De acordo com a hipótese dos quanta de luz (fótons) [...] a luz consiste de quanta (corpúsculos) de energia hv, os quais viajam através do espaco como um conjunto de balas com a velocidade da luz". Já Dirac (1958, p. 2) menciona que

> [...] os fenômenos tais como emissão fotoelétrica e espalhamento de elétrons livres [...] mostram que a luz é composta por partículas pequenas. Estas partículas, que são chamadas de fótons, têm energia e momentum definidos... e apresentam ser tão reais quanto a existência de elétrons, ou qualquer outra partícula em física. Uma fracão de fóton nunca é observada.

Tais citações ilustram como o tradicional conceito de fóton da velha Teoria Quântica foi interpretado como sendo uma entidade pequena e indivisível de energia entre as décadas de 1930 e 1950, mesmo após o desenvolvimento da estatística de Bose-Einstein.

Nesse contexto, o que é pouco conhecido na história da Física Moderna é o fato de que o tradicional conceito de fóton, desenvolvido pela velha Teoria Quântica e disseminado por reconhecidos físicos como Born e Dirac, tornar-se-ia (mais uma vez) alvo de discussões entre físicos no final da década de 1950, alastrando-se até o século XXI. O tradicional conceito de fóton construído ao longo dos anos de 1905, 1916, e 1927 seria colocado novamente em dúvida em 1956; e, reestruturado com o nascimento da óptica quântica em 1963. Tal conceito que emergiu da óptica quântica foi corroborado na década de 1970 com a observação do fenômeno de *antibunching* da luz. As discussões sobre o conceito de fóton não ficaram, portanto, adormecidas na virada do século XX para o XXI; ao contrário, vários acontecimentos impulsionaram novas interpretações e reflexões sobre esse conceito.

**O conceito de fóton nas décadas de 1970 e 1980**

Antes de apresentarmos as discussões sobre o conceito de fóton nos anos 1970 e 1980, iniciamos esta seção com o final da década de 1950. Os cientistas britânicos Robert Hanbury Brown (1916-2002) e Richard Quentin Twiss (1920-2005) detectaram, em 1956, uma correlação entre fótons que parecia ser contrária às predições da Teoria Quântica, especialmente, ao tradicional conceito de fóton. À epoca, Hanbury Brown e Twiss estavam interessados em construir um interferômetro, muito mais preciso do que o de Michelson, para determinar o diâmetro angular das estrelas que emitiam ondas de rádio. Em 1952, eles já haviam construído um novo interferômetro, e obtiveram satisfatoriamente os diâmetros das estrelas Cygnus e Cassiopeia. (SILVA; FREIRE JÚNIOR, 2013)

Durante uma de suas medições, Hanbury Brown e Twiss perceberam que, apesar das fortes flutuações na intensidade dos sinais devido aos efeitos da ionosfera, o interferômetro estava funcionamento devidamente Devido à eficiência desse instrumento, eles decidiram verificar se seria possível utilizá-lo para o mesmo fim, mas, agora, trabalhando com estrelas que emitiam na faixa do visível. Como Hanbury Brown e Twiss não tinham certeza de que a mesma abordagem teórica poderia ser aplicada à faixa do visível, o primeiro passo foi, então, realizar um teste de laboratório antes de adaptar o interferômetro de rádio para a faixa do visível. Para tal, Hanbury Brown e Twiss utilizaram como estrela artificial uma lâmpada de mercúrio de baixa intensidade. Ao fazer isso, eles deixaram o campo da radioastronomia inserindo-se no da óptica.

O que Hanbury Brown e Twiss não imaginariam era que aquele teste de laboratório causaria uma acalorada controvérsia na comunidade de físicos. (SILVA; FREIRE JÚNIOR, 2013)

No teste de laboratório, a fonte de luz artificial proveniente do arco de mercúrio incidia em um espelho semitransparente e a radiação era, então, dividida em duas componentes que seriam detectadas independentemente por dois fotomultiplicadores. Após um determinado período de tempo, Hanbury Brown e Twiss observaram que o tempo de chegada dos fótons estava correlacionado, ou seja, fótons estavam sendo detectados simultaneamente no interferômetro. Porém, segundo o conceito de fóton da velha Teoria Quântica, nenhuma correlação sistemática entre fótons deveria ser detectada quando a fonte utilizada era de baixa intensidade. Em outras palavras, considerando o fóton como uma partícula pequena e indivisível, e que o experimento estava lidando com fótons um a um, o resultado de Hanbury Brown-Twiss (HBT) parecia ser um absurdo. Caso o fizesse, seria necessário supor que fótons eram partículas divisíveis de modo que dois fótons pudessem chegar ao mesmo tempo em detectores diferentes separados à mesma distância; ou, como descrito ironicamente por Hanbury Brown, supor que um fóton estaria esperando o outro no espaço até que eles pudessem ser detectados simultaneamente. Para compreender o porquê dos resultados experimentais obtidos por Hanbury Brown e Twiss foi necessário revisitar o tradicional conceito de fóton da velha Teoria Quântica. (SILVA; FREIRE JÚNIOR, 2013)

Além de suscitar discussões sobre o conceito de fóton no final da década de 1950, o experimento HBT também teve um papel importante (juntamente com o desenvolvimento do *laser*) na criação da óptica quântica. (GLAUBER, 2005) Em 1963, Glauber publicou de forma bastante sofisticada uma Teoria Quântica da coerência – contribuindo para a criação da disciplina Óptica Quântica – na qual o campo eletromagnético passou a ser representado por estados coerentes. Hoje, a contribuição teórica de Glauber é conhecida como "estados coerentes" ou "estados de Glauber", a qual lhe rendeu o Prêmio Nobel de Física de 2005. Considerando o conceito de estados coerentes, autoestados do operador aniquilação de fótons, Glauber destacou que a correlação entre fótons observada por Hanbury Brown e Twiss era devido ou a misturas incoerentes ou a superposições dos estados coerentes. (BERTOLOTTI, 1974, p. 217)

A abordagem teórica proposta por Glauber para o campo eletromagnético, na óptica, também foi alvo de controvésia e disputa com o grupo de pesquisa de Rochester liderado pelos físicos Leonard Mandel (1927-2001) e Emil Wolf (1922 -). Em seu formalismo teórico, Glauber quantizou tanto o campo eletromagnético quanto à matéria – uma abordagem completamente quântica –, o que era antagônico à ideia de que o tratamento clássico ou semiclássico poderia ser suficiente para uma teoria da coerência. Ou seja, a disputa de Mandel e Wolf com Glauber estava relacionada à necessidade, ou não, da quantização do campo eletromagnético no campo da óptica. (BERTOLOTTI, 1974, p. 227-228)

Esses dois acontecimentos – a correlação entre fótons observada por HBT e, principalmente, os estados coerentes de Glauber – motivaram os debates teóricos acerca do conceito de fóton nas décadas de 1970 e 1980. Até mesmo antes da década de 1970, o físico Richard Sillitto (1923-2005) já havia discutido a evolução do tradicional conceito de fóton, e a dificuldade que ele impunha no entendimento do experimento HBT. (SILLITTO, 1960)

Em 1972, os físicos norte-americanos Marlan O. Scully (1939-) e Murray Sargent III (1941-) abriram o artigo *The concept of the photon* mencionando que "[...] a imagem de *fuzzy-ball* de um fóton geralmente conduz a dificuldades desnecessárias". Scully e Sargent III (1972, p. 38) externaram a principal questão, à época, levantada: até que ponto a quantização do campo eletromagnético era, de fato, necessária e útil? Os autores argumentaram que a teoria semiclássica, em que o campo eletromagnético era tratado classicamente de acordo com as equações de Maxwell e a matéria quantizada era capaz de explicar, relativamente bem, boa aproximação entre fenômenos tais como o efeito fotoelétrico, emissão estimulada, e fluorescência ressonante.[10]

A aceitação da quantização da radiação antes do processo de detecção dependia, de certo modo, do quão feliz a teoria semiclássica era na explicação daqueles resultados experimentais. Até meados da década de 1970, o efeito fotoelétrico, efeito Compton, e até mesmo o experimento HBT, por exemplo, não precisavam necessariamente de uma abordagem completamente quântica para a sua explicação. Desse modo, o conceito de fóton não desempenhava um papel decisivo na compreensão de tais fenômenos, até mesmo após o desenvolvimento da Teoria Quântica da radiação por Glauber.

O efeito *antibunching*, contudo, mudaria o curso dessa história. Esse fenômeno é consensualmente apontado pela comunidade de físicos como sendo uma evidência experimental a favor da necessidade de quantização do campo eletromagnético já que, até esse momento, a explicação de efeitos ópticos não requeriria necessariamente o advento da natureza quântica da luz, o conceito de fóton. Tal efeito foi observado em 1977 pelos físicos H. Jeff Kimble, Mario Dagenais e Mandel, utilizando um esquema experimental semelhante àquele do experimento HBT. No entanto, Kimble e colegas utilizaram como fonte uma luz fluorescente proveniente de átomos de sódio excitados por um feixe de *laser*, enquanto HBT usaram uma fonte térmica, caótica. Nesse experimento, Kimble, Dagenais e Mandel observaram uma anticorrelação entre os

[10] A emissão estimulada refere-se ao processo pelo qual a matéria, ao interagir com uma onda eletromagnética, pode perder energia, e, como consequência, produzir um novo fóton. No caso da fluorescência ressonante, é a fluorescência de um átomo ou molécula em que a radiação emitida é de mesma frequência daquela absorvida.

fótons detectados, diferentemente do experimento HBT em que uma correlação foi observada. O resultado HBT pode ser interpretado como *bunching* de fótons – a tendência de fótons chegarem em pares no espelho semitransparente devido à estatística de Bose-Einstein –, já o experimento de Kimble, Dagenais e Mandel mostrou o efeito contrário,o *antibunching*. Esse foi o primeiro efeito em que uma explicação semiclássica já não era capaz de torná-lo inteligível, mas, sim, a quantização do campo eletromagnético; ou seja, uma teoria quântica para a luz tornava-se necessária. (BASEIA, 1995; KIMBLE; DAGENAIS; MANDEL, 1977; KNIGHT, 1977; WALLAS, 1979)

Os debates sobre o conceito de fóton entre as décadas de 1950 e 1970 também influenciaram as discussões posteriores. De um lado, o efeito HBT parecia ter estremecido as bases do tradicional conceito de fóton uma vez que ele era irreconciliável com o modelo bolha de bilhar. O efeito *antibunching*, por outro lado, parecia trazer à tona a natureza quântica da radiação. Mas, seria uma natureza corpuscular *a la* Einstein? Reflexões sobre tais questões e outras sugiram, por exemplo, na seção Letters to the Editor do *American Journal of Physics* entre os anos de 1981 e 1984, a partir das quais físicos mostraram-se bastante interessados em compreender, sugerir, e criticar as representações ou definições para o fóton que surgiam naquela época. (ARMSTRONG, 1983; BERGER, 1981; FREEMAN, 1984; SINGH, 1984)

Entre uma discussão teórica e outra, em 1986, outro resultado experimental também contribuiria significativamente para evidenciar a necessidade de uma Teoria Quântica para a radiação. Esse experimento foi realizado, em condições quase ideais, pela equipe francesa liderada pelo físico Alain Aspect (1947-) no qual foram utilizados, pela primeira vez, estados de fótons singulares incidindo em um divisor de feixe. O resultado encontrado foi uma forte anticorrelação entre fótons nos dois lados do divisor de feixe. Tal resultado experimental confirmava as predições da Teoria Quântica em relação aos estados de fótons singulares, e, consequentemente, discordava de qualquer modelo clássico de luz. (GRANGIER; ROGER; ASPECT, 1986)

**O ensino da Teoria Quântica: o que é mesmo um fóton?**

Além daqueles debates teóricos e resultados experimentais, questões didáticas subjacentes ao ensino do conceito de fóton também fizeram parte da agenda dos físicos no final da década de 1980. O que parecia ser um conceito relativamente simples tornou-se complexo, e eis que surge a questão: Que conceito (ou modelo) de fóton deveria ser ensinado nos cursos de Física Quântica? À época, físicos já haviam reconhecido que o conceito de fóton havia se tornado uma das mais importantes questões didáticas da Física Moderna. Em seu artigo *Photon in introductory quantum physics*, por exemplo, o físico J. Strnad (1986, p. 650) sugeriu que, em um nível introdutório, o conceito de fóton poderia ser trabalhado a partir das discussões sobre o efeito fotoelétrico (fótons como quanta de energia) e sobre o efeito Compton (fótons como energia e *momentum* dos quanta),não mencionando nada sobre a posição de um fóton, evitando, assim, veementemente as analogias entre fótons e elétrons. Após discutir outros modelos de representar um fóton, Strnad (1986, p. 652) destacou que "[...] havia um *mainstream* de interpretações relacionadas com os fótons, assim como *sidestreams*", e concluiu que seria de extrema importância distingui-las uma das outras no ensino introdutório de física quântica.

Ao sistematizar os *mainstreams* e *sidestreams* referentes ao conceito de fóton, Kidd, Ardini e Anton (1989, p. 30) destacaram que, de fato, "[h]istoricamente, o termo fóton representa, pelo menos, quatro modelos distintos e carrega diferentes conotações para estudantes e para físicos praticantes”. Os autores discutiram cada modelo para o fóton de acordo com a seguinte categorização: fóton I (modelo de partícula), o qual foi introduzido em 1905 por Einstein e é aquele comumentemente discutido nos livros didáticos; fóton II (modelo de singularidade), segundo o qual, o fóton é descrito matematicamente como uma singularidade no campo eletromagnético; fóton III (modelo pacote de onda), a partir do qual fotons são representados simplesmente em termos de trens de onda clássica; fóton IV (modelo da eletrodinâmica quântica), em que o fóton é descrito matematicamente como uma excitação de um estado quântico. Refletindo sobre o ensino do conceito de fóton, Kidd, Ardini e Anton concluiram que, a menos que o fóton corpuscular seja discutido historicamente, ele deveria ser evitado nos textos elementares. A sugestão de Kidd, Ardini e Anton (1989) é a de discutir os modelos semiclássicos mais abragentes como uma primeira aproximação à versão moderna da eletrodinâmica quântica.

Outros físicos também veem no tradicional conceito de fóton uma dificuldade mais do que uma boa estratégia de ensino. O físico norte-americano Willis E. Lamb (1913-2008), por exemplo, criticou aquele modelo ao destacar que

> [e]stá na hora de abandonar o uso da palavra “fóton” e de um conceito ruim que brevemente terá um século de idade. A radiação não consiste de partículas, e o limite clássico da Quantum Theory of Radiation, isto é, o não-quântico, é descrito pelas equações de Maxwell para os campos eletromagnéticos, os quais não encolvem partículas. Tratar a radiação em termos de partículas é como utilizar frases comuns, tais como *You know* ou *I mean*... Para um amigo do Charlie Brown, ele serviria como uma espécie de cobertor de segurança. (LAMB, 1995, p. 84)

Diferentemente do proposto por Lamb, todavia, o ensino do conceito de fóton ainda baseia-se no modelo de partícula – pontual e localizável. Sabe-se que tal modelo, no entanto, está muito aquém do moderno conceito de fóton. Neste cenário, duas possibilidades podem ser levadas em consideração com o intuito de inserir as discussões sobre o fóton e o seu conceito no nível universitário. Primeira, se o ensino acerca do conceito de fóton basear-se na construção de imagens, então, é essencial que a complementaridade esteja subjacente a tal discussão, utilizando, assim, os conceitos clássicos de onda e partícula. Segunda, se a finalidade é discutir o contemporâneo conceito de fóton, logo, torna-se imprescindível a utilização de uma abordagem instrumentalista em que imagens não desempenham nenhum papel fundamental nos estudos sobre o fóton, mas, sim, uma abordagem teórica e matemática muito mais sofisticada e abstrata do que a que aparecerá na possibilidade anterior.[11]

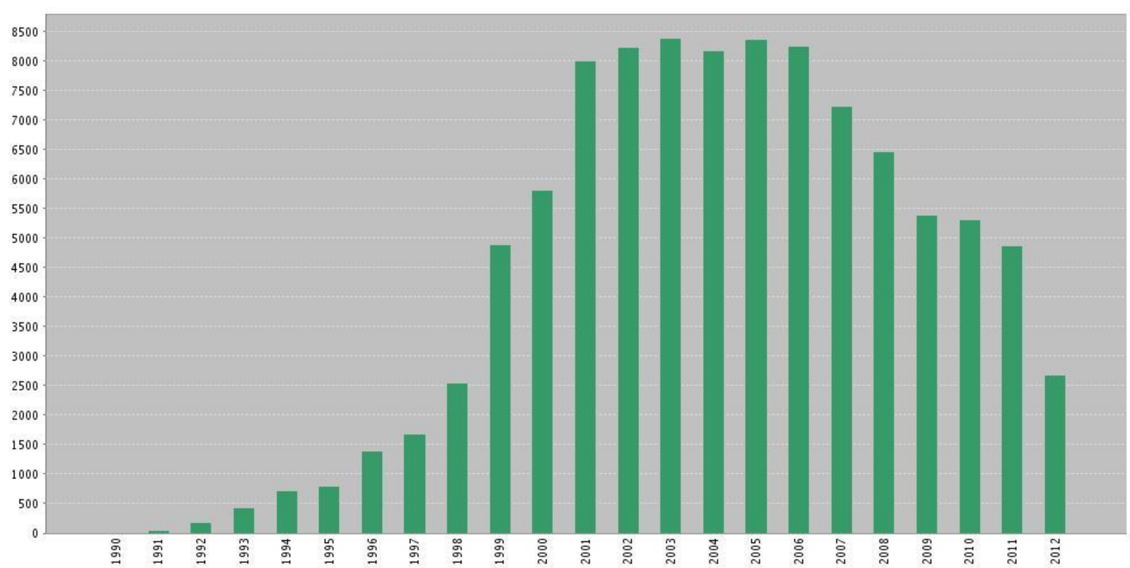
**O fóton revisitado no século XXI**

As discussões sobre o conceito de fóton também chegaram ao século XXI. Nos últimos anos, aplicações e técnicas associadas ao fóton têm se tornado uma promessa no campo da Informação. Como bem destacado pelo renomado físico quântico austríaco Anton Zeilinger (1945-) e colegas (2005, p. 230), a "[...] pesquisa em propriedades quânticas da luz (óptica quântica) desencadeou a evolução de todo um campo de processamento de informação quântica, o qual atualmente promete novas tecnologias, tais como criptografia quântica e até mesmo computadores quânticos". Diante de tais promessas, uma nova era – a da fotônica – floresceu neste século, e, junto a ela, também houve um crescimento significativo no número de publicações e de conferências dedicadas ao fóton e o seu conceito, além da criação de revistas científicas especializadas no tema. O efeito da "era da fotônica" na comunidade científica pode

[11] Para uma discussão sobre o ensino da Teoria Quântica, consultar Greca e Freire Júnior (2013).

ser evidenciado pelo gráfico a seguir obtido através das bases de dados da Web of Science, o qual descreve a evolução na dinâmica de citações em que a palavra *photonic* é citada em títulos de de obras publicadas entre 1990 e 2012.[12] Como é observado, na virada do século XX para o XXI, houve um aumento substancial no número de publicações em revistas, atas de conferências e em livros didáticos, no campo da fotônica. Mesmo com o declínio de publicações após 2006, o número de artigos ainda se mantém em patamar muito elevado acima de 1990.

Gráfico 1 – A pesquisa em fotônica na transição do século XX para o XXI




Fonte: Web of Science, acesso em 14/09/2012.

[12] As bases de dados de citações selecionadas foram as seguintes: Science Citation Index Expanded (SCI-EXPANDED) - 1899-present; Social Sciences Citation Index (SSCI) - 1956-present; Conference Proceedings Citation Index - Science (CPCI-S) - 1990-present; Conference Proceedings Citation Index - Social Science & Humanities (CPCI-SSH) - 1990-present; Book Citation Index– Science (BKCI-S) - 2005-present; Book Citation Index – Social Sciences & Humanities (BKCI-SSH) - 2005-present.

The International Society for Optics and Photonics (SPIE), por exemplo, tem realizado conferências desde 2003 cujo tema principal é The Nature of Light: What is a Photon?. A renomada revista *Nature* também criou uma nova revista dedicada exclusivamente aos fótons: Nature Photonics.[13] Além disso, físicos têm dedicado capítulos de livros-textos, ou até mesmo todo um livro, à discussão das questões relacionadas aos fótons. No livro *The Quantum Challenge* publicado pelos físicos George Greeinstein e Arthur Zajonc, eles dedicaram o segundo capítulo aos Photons. Os editores Chandra Rpychoudhuri, A. F. Kracklauer e Kathy Creath sistematizaram uma coletânea de artigos, já publicados em uma revista de número especial da Optical Society of America (OSA), nos anais da Society of Photo-Optical Instrumentation Engineers e na revista cientifíca Science, e publicaram o livro homônimo daquelas conferências realizadas desde 2003, *The Nature of Light: What is a Photon?* para discutir a natureza da luz. Tal livro é uma boa introdução para aqueles interessados em compreender os atuais desenvolvimentos e debates teóricos referentes ao fóton. Mais recentemente, o próprio Zeilinger publicou o livro *Dance of Photons* dedicado à discussão sobre os fótons desde Einstein à teleportação quântica. (GREENSTEIN; ZAJONC, 2006; RPYCHOUDHURI; KRACKLAUER; CREATH, 2008; ZEILINGER, 2010)

Na primeira seção, Critical Reviews of Mainstream Photon Model do livro *The Nature of Light*, o conceito de fóton foi revisitado. O físico norte-americano Arthur Zajonc (2008, p. 9), o mesmo que foi mencionado anteriormente, enfatiza que “[a] meu ver, Einstein estava certo em alertar-nos sobre a luz [...][o] nosso entendimento tem aumentado significativamente nos [últimos] 100 anos desde Planck, mas, suspeito que a luz continuará nos confundindo, enquanto

[13]Mais informações, ver: SPIE, disponível em http://spie.org/; NaturePhotonics, disponível em http://www.nature.com/nphoton/index.html.

que simultaneamente atrairnos-a para inquirir incessantemente em sua natureza". Já o físico Rodney Loudon (2008, p. 21), autor do clássico livro *The Quantum Theory of Light*, destaca que "[a] questão levantada [o que é um fóton?] tem uma variedade de respostas, as quais convergem completamente para uma imagem coerente deste objeto um tanto elusivo". O físico David Finkelstein (2008, p. 23) menciona, contudo, que aquela não deveria ser a principal questão a ser respondida em uma perspectiva experimental, mas, sim, "o que os fótons fazem?", e, assim,"[...] poderíamos definir o que os fótons são, se ainda o desejássemos, pelo que eles fazem". Isto é, o argumento de Finkelstein é o de que seria mais fundamental descrever o processo no qual o fóton faz parte do que o próprio objeto em si, já que "[...] provavelmente nunca seremos capazes de visualizar um fóton". (FINKELSTEIN, 2008, p. 34)

Compartilhando a ideia de que a compreensão do conceito de fóton torna-se mais intelígivel a partir da análise do fóton no processo, Muthukrishnan, Scully e Zubairy (2008, p. 38), ressaltam a importância de "[p]assarmos a elucidar o conceito de fóton através de experimentos específicos (real ou de pensamento) que demonstrem a necessidade do e lançam luz sobre o significado do 'photon'". Os autores responderam à questão, o que é o fóton, a partir das próprias palavras de Glauber (ano, p. xx): "[o] fóton é o que um fotodetector detecta". Em relação à questão da localização do fóton, "onde ele está?", eles enfatizaram que o "[...] fóton está *onde* o fotodetector o detecta". (MUTHUKRISHNAN; SCULLY; ZUBAIRY, 2008, p. 38-39) Na verdade, segundo eles, a questão que deveria estar por trás daquelas duas era se "[...] poderíamos considerar o fóton como uma 'partícula' verdadeira que é localizada no espaço". O problema é que, de acordo com o princípio de Heisenberg, não é possível determinar precisamente a posição e o momentum de uma partícula simultaneamente. Ou seja, como poderíamos localizar um fóton, "partícula", sem violar o princípio de incerteza? Muthukrishnan, Scully e Zubairy discutiram, então, a ideia de representar os fótons, no domínio espacial, por uma função de onda.[14] O que de acordo com os autores facilitaria o entendimento de fenômenos, como interferência quântica e emaranhamento, a partir da noção de funções de onda de um-fóton e de dois-fótons, possibilitando, assim, analogias com a óptica ondulatória clássica. (MUTHUKRISHNAN; SCULLY; ZUBAIRY, 2008)

O centenário aniversário do fóton foi celebrado exaltando o quão complexo é o seu conceito até mesmo neste século:

> Desde 1905, o fóton já percorreu um longo caminho, ponderando que foi considerado inicialmente para ser apenas um "artifício matemático" ou um conceito sem qualquer significado mais profundo [...] Mas o que exatamente queremos dizer com um "fóton" hoje e qual evidência experimental temos para sustentar o conceito de fóton? (ZEILINGER et al., 2005, 230)

Na tentativa de responder a tais questões, Zeilinger, Weihs, Jennewein e Aspelmeyer (2005) discutem experimentos modernos que têm confirmado a natureza quântica da luz, a saber, o experimento realizado em 1974 pelo físico norte-americano John Clauser (1942-) no qual ele utilizou uma fonte que emitia fótons em pares; o experimento de *antibunching* de fótons; o experimento de interferência de um fóton singular, já mencionado anteriormente, realizado pelo grupo de pesquisa liderado por Aspect; o experimento de interferência de dois fótons, cujo resultado é conhecido como efeito Hong-Ou-Mandel, que foi executado em 1987. Tais experimentos desempenharam um papel importante a favor da necessidade de quantização do campo eletromagnético, ou de uma abordagem quântica para a luz.. Todavia, Zeilinger e colegas

[14] O problema reside no fato de que não há um operador posição para os fótons.

(2005, p. 233) também destacam a principal dificuldade na compreensão do fenômeno de interferência quântica relacionado àqueles experimentos: “O principal problema conceitual é que tendemos a materializar – considerar bastante realisticamente – conceitos como onda e partícula”. Nenhum problema haveria, por exemplo, se o estado quântico fosse representado simplesmente por uma onda. No entanto, é preciso ter cuidado para *não* mencionar que há uma onda se propagando através de um aparato de dupla fenda, ou do interferômetro de Mach-Zehnder. (ZEILINGER et al., 2005, p. 233)

Como é enfatizado por Zeilinger e colaboradores (2005, p. 233), “[...] o estado quântico é simplesmente uma ferramenta para calcular probabilidades”. Mas, *não* as probabilidades de se encontrar um fóton em algum lugar, mas, sim, “[...] as probabilidades de um detector de fótons disparar se inserido em um determinado lugar”. Os autores também destacam a que o conceito de fóton deveria estar associado:

> Pode-se estar tentado, como estava Einstein, em considerar o fóton como sendo localizado em algum lugar conosco apenas conhecendo aquele lugar. Mas, sempre que falarmos sobre uma partícula, ou mais especificamente,um fóton, devemos apenas associá-la ao ‘click’ a que o detector refere-se. (ZEILINGER et al., 2005, p. 233)

O conceito de fóton associado à informação quântica é baseado na quantização do campo eletromagnético, e, assim,“[...] o conceito de fóton como uma partícula individual é menos importante” naquele contexto. Zeilinger e colaboradores (2005, p. 236) destacam a importância daqueles modernos experimentos com fótons no surgimento de um novo campo de investigação, e finalizam a celebração do conceito de fóton do seguinte modo:

> Embora tais experimentos atualmente arruinaram o ponto de vista de Einstein [sobre EPR], eles deram origem a novos campos de processamento de informação quântica. Mas, os problemas conceituais não estão completamente resolvidos. Isto é significado pelo amplo espectro de diferentes interpretações da física quântica as quais competem umas com as outras. Em nossa opinião, um traço comum de muitas interpretações é que entidades são consideradas ser 'real' além da necessidade.

## Considerações Finais

O conceito de fóton, que parecia ser algo resolvido desde a década de 1930, passou por várias novas interpretações durante a transição do século XX para o XXI. E, neste século, ainda não encontramos um consenso na resposta à indagação, afinal, qual é mesmo o conceito contemporâneo de fóton? Alguns físicos preferem defini-lo, segundo a eletrodinâmica quântica, como uma unidade de excitação relacionada com um modo quantizado do campo eletromagnético; outros preferem representá-lo através de funções de onda; e a maioria deles preferem fazer cálculos ao invés de debruçar-se em questões conceituais sobre a natureza da luz. Uma das grandes dificuldades em discutir e compreender o conceito de fóton está inerente à imagem que temos dele. Se nos prendermos à ideia de partícula sugerida por Einstein em 1905, não seremos capazes de entender conceitos-chaves do nosso século referentes à nova era da informação quântica, e até mesmo os experimentos realizados no século passado. O que há de consenso sobre o conceito de fóton da óptica quântica é o fato de que ele não deve ser representado simplesmente por uma entidade pequena, indivisível e localizável ─o famoso modelo bola de bilhar.

Gostaríamos de substituir as reticências do título deste capítulo pelas sábias palavras de Einstein (1951, p. 183) ditas em 1951: "Todos estes cinquenta anos de reflexão não me trouxeram próximo à resposta à questão, ‘O que são os quanta?’ Hoje em dia, qualquer Tom, Dick e Harry pensa que sabe, mas ele está enganado”.[15]

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[15] Na língua inglesa, a expressão *every Tom, Dick and Harry* é geralmente utilizada para remeter-se a qualquer pessoa. O que equivaleria a “fulano, sicrano e beltrano” na língua portuguesa.

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# The Concept of the Photon in Question:

# The Controversy Surrounding the HBT Effect circa 1956—1958[1]

## Introduction

As is well known, after the early development of quantum theory, radiation was defined as a collection of indivisible particles—photons—whose energy and momentum were conserved during its interaction with matter.[2] However, an astonishing and unexpected experimental result, published thirty years after the creation of the quantum theory, called the canonical concept of the photon into question. This experiment was carried out in 1956 by the British scientists Robert Hanbury Brown (1916–2002) and Richard Quentin Twiss (1920–2005). Hanbury Brown and Twiss were neither part of the community involved in discussions about the foundations of quantum theory nor researchers investigating the fundamental concepts of physics. Rather, they were involved in applying physical concepts to astronomy and consequently introducing new methods in that field. Their work ''put the cat among the

[1] Paper published in the *Historical Studies in the Natural Sciences*, Vol. 43, Number 4, pps. 453–491. ISSN 1939-1811, electronic ISSN 1939-182X. 

[2] For instance, see: Olivier Darrigol, ''A Simplified Genesis of Quantum Mechanics,'' Studies in History and Philosophy of Modern Physics 40, no. 2 (2009): 151–66; Stephen Brush, ''How Ideas Became Knowledge: The Light-Quantum Hypothesis 1905–1935,'' HSPS 37, no. 2 (2007): 205–46; Helge Kragh, Quantum Generations: A History of Physics in the Twentieth Century (Princeton, NJ: Princeton University Press, 1999); Max Jammer, The Conceptual Development of Quantum Mechanics (New York: McGraw-Hill, 1966); Bruce R. Wheaton, The Tiger and the Shark: Empirical Roots of Wave-Particle Dualism (Cambridge: Cambridge University Press, 1983); Roger H. Stuewer, The Compton Effect: Turning Point in Physics (New York: Science History Publications, 1975); Thomas S. Kuhn, Black-Body Theory and the Quantum Discontinuity, 1894–1912 (Chicago: University of Chicago Press, 1978).

pigeons.''[3] Indeed, HBT's results stirred up a heated controversy in the community of physicists.[4] Hanbury Brown and Twiss were awarded the Albert Michelson Medal in 1982 for their work of 1956.

In the Hanbury Brown–Twiss (HBT) experiment, a low-intensity beam of light was split into two components by a half-silvered mirror, and then the components were detected separately through two photomultipliers. Hanbury Brown and Twiss claimed that two photons had been detected at the same time. Because of the intensity of the source used, it had been expected that only single photons were arriving at the mirror in a certain time interval. Thus, a question arose: how could they find a correlation between photons if, as proposed by Einstein and spread widely by quantum theory textbooks, these are indivisible particles? From the perspective of the conventional concept of the photon, the HBT experimental result seemed to go against the foundations of the quantum theory because there would be no way to detect two photons at the same time at two different detectors, assuming a photon as a small localized indivisible particle. The debate over the HBT results was

[3] Robert Hanbury Brown, Boffin: A Personal Story of the Early Days of Radar: Radioastronomy and Quantum Optics (New York: Taylor & Francis Group, 1991), 117–34, on 120.

[4] Other scholars have also highlighted the controversial debate about the Hanbury Brown and Twiss experiment: David Owen Edge and Michael Joseph Mulkay, Astronomy Transformed: The Emergence of Radioastronomy in Britain (New York: Wiley-Interscience, 1976), on 146; B. Lovell and R. M. May, ''Robert Hanbury Brown (1916–2002),'' Nature 416 (2002): 34; J. Davis and B. Lovell, ''Robert Hanbury Brown 1916–2002,'' Historical Records of Australian Science 14 (2003): 459–83, on 469; B. Tango, ''Richard Quentin Twiss 1920–2005,'' Astronomy & Geophysics 47 (2006): 4.38; Mario Bertolotti, Masers and Lasers—An Historical Approach (Bristol: Adam Hilger, 1983), on 203; Joan Bromberg, ''Modelling the Hanbury Brown Twiss Effect—The Mid- Twentieth Century Revolution in Optics'' (paper presented at the HQ–3 Conference on the History of Quantum Physics, Berlin, June 28–July 2, 2010).

intense, so intense, indeed, that, as remarked later by Hanbury Brown, some physicists even went as far as to claim that he and Twiss misunderstood the quantum theory.[5]

In hindsight, physicists can easily see what was at stake. Nowadays, physics makes a clear distinction between attenuated light and light described by number states (Fock states), that is, states with a well-defined number of photons. For the former, the probability of finding $n$ photons is given by a Poisson distribution, and there is indeed a slim chance of getting two photons at a time. As for the latter, in the case of a single-photon state where such a distribution is not valid, the probability of finding $n$ photons (with $n \neq 1$) is null. The HBT sources were of the former kind. However, physicists came to understand this after applying quantum field methods to optics and developing quantum optics. Surely, it would be an anachronism to use a current explanation for what happened at that time.[6] Our argument is that, in fact, the HBT experimental results questioned the standard concept of the photon proposed by the old quantum theory, arousing a heated experimental and theoretical controversy.

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[5] Robert Hanbury Brown, ‘‘Paraboloids, Galaxies and Stars: Memories of Jodrell Bank,’’ in The Early Years of Radioastronomy: Reflections Fifty Years after Jansky’s Discovery, ed. W. T. Sullivan III (Cambridge: Cambridge University Press, 1984), 213–35, on 230. While it is more precise to refer to the social phenomenon under study as a scientific controversy, from time to time we describe it as a debate or dispute, each of which has a looser meaning. On scientific controversies, see H. Tristram Engelhardt, Jr. and Arthur L. Caplan, eds., Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology (Cambridge: Cambridge University Press, 1987), and the special issue on ‘‘Controversies’’ in Science in Context 11, no. 2 (1998).

[6] R. G. W. Brown and E. R. Pike, ‘‘A History of Optical and Optoelectronic Physics in the Twentieth Century,’’ in Twentieth Century Physics, vol. III, ed. Laurie Brown, Abraham Pais, and Sir Brian Pippard (New York: American Institute of Physics and Bristol/Philadelphia: Institute of Physics Publishing, 1995), on 1421–22, 1439–41, and 1459–60.

Although the quantum statistics of radiation had been developed by Satyendra Nath Bose (1894–1974) and by Albert Einstein (1879–1955), resulting in ‘‘the abandonment of the classical concept of individually identifiable particles,’’ in the late 1950s physicists still interpreted photons as distinguishable entities— indivisible objects—as we will see during the HBT debate.[7] At that time, three concepts of the photon emerged: photons as indivisible entities, photons as bosons constituting the electromagnetic field, and photons as wave packets. Even though our focus is on how the HBT experiment encouraged physicists to revisit the concept of the photon, it is important to mention that the HBT results also played a fundamental role in the renaissance of a classical discipline— optics—which had not been developing so fully for a long time.[8] After the HBT experiment and the development of the laser, physicists turned their attention to optics again, contributing experimentally and theoretically to its renaissance.

In addition to contributing to the development of quantum optics, astronomy, and optics at the time, the HBT effect is currently part of vanguard contemporary physics in different fields such as high-energy physics, nuclear physics, atomic physics, and condensed matter physics. As the Web of Science indicates, for instance, Hanbury Brown and Twiss's 1956 paper has 509 citations, of which 259 are in articles published in the twenty-first century. Even in the early 1960s, physicists envisioned applications of the HBT effect into military devices. In fact, Marvin Goldberger, Kenneth Watson, and Hal Lewis, working for

[7] Olivier Darrigol, ‘‘Statistics and Combinatorics in Early Quantum Theory, II: Early Symptoma of Indistinguishability and Holism,’’ HSPS 21, no. 2 (1991): 237–98, on 239.

[8] Kragh, Quantum Generations (ref. 1), 390.

the American defense with the Jason project, used this effect with radar in order to measure the size of incoming warheads.[9]

In this paper, we examine the 1956 Hanbury Brown and Twiss episode, their experimental results and theoretical model, the repercussions in the physics community, and the controversy surrounding their work between 1956 and 1958. In particular, we investigate the conceptual debate, especially about the nature of the photon, which the HBT experiment stirred up. In the first section, we present the background to Hanbury Brown and Twiss and their experiment and results; the next section focuses on the controversy surrounding this experiment circa 1956–58; the third section is dedicated to the responses given by our protagonists to criticism; and finally, we briefly consider the influence of the HBT experiment on the early development of quantum optics.

**The HBT Experiment**

The HBT experiment appeared in the context of astronomical interferometry, a field which was born in the nineteenth century following the acceptance of the wave nature of light. Interferometers exploit the nature of waves to superimpose waves and thus obtain information about them. In this case, the formation of interference fringes occurs when superimposed waves have phases whose differences are constant. Indeed, interferometry became a widely used technique in science and engineering and led the 1907 Nobel Prize to be awarded to Albert A. Michelson (1852–1931) precisely ‘‘for his optical precision

[9] This research was carried out on 20 Aug 2012. See also Ann Finkbeiner, The Jasons – The Secret History of Science’s Postwar Elite (New York: Penguin, 2006), 51.

instruments and the spectroscopic and metrological investigations carried out with their aid.''[10]

In addition to these applications, and following an early suggestion of Armand H. Fizeau (1819–96), Michelson launched the use of interferometers for measuring the angular diameters of stars. By 1890, Michelson had already developed the theory for this use. He showed that by masking the objective of a telescope except for two slits separated by a distance $b$, the fringes would disappear when $\alpha = \lambda/b$, where $\alpha$ is the angular diameter and $\lambda$ the wavelength of the incoming light. As this magnitude is also the resolution of a telescope with aperture b, and there are limitations to building telescopes with larger apertures, Michelson thought of using two mirrors of a refractometer to capture light and then send the two beams through a system of mirrors to be recorded. The two signals are then multiplied, giving a product with regular maxima and minima that are the equivalent to the fringes in the traditional Michelson interferometer. The fringe visibility depends on the separation of the mirrors and the angular diameter of the source, and it may be obtained through the Fourier transform of the brightness distribution of the source. The distance between mirrors could be greater than the size of the telescope objectives, thus amplifying the resolution. With such a device Michelson hoped to circumvent the effects of atmospheric turbulence that had so far limited the resolution of optical telescopes. The Michelson stellar interferometer, however, spent thirty years in hibernation. In 1920, after a time delay which has intrigued historians, the apparatus of the Mount Wilson Observatory was eventually used by Michelson and F. Pease to measure the diameter of the red giant star Betelgeuse.[11]

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[10] Nobelprize.org, ''The Nobel Prize in Physics 1907,'' at http://www.nobelprize.org/nobel_prizes/physics/laureates/1907/ (accessed 11 Dec 2012).

[11] M. H. Cohen et al., ''Radio Interferometry at One-Thousandth Second of Arc,'' Science 162, no. 3849 (1968): 88–94; Albert A. Michelson, ''On the Application of Interference Methods to Astronomical

It was in the early 1930s that Karl Jansky (1905–50), working as an engineer at Bell Laboratories, recorded the first reception of radio waves coming from outer space. After the war, radio astronomy rapidly became a subfield of astronomy, with scientists drawing most of their skills and machinery from wartime experience with radar. It comes as no surprise, then, as its chroniclers have put it, that, according to Fujinobu Takahashi, ‘‘the initial advances in radio astronomy were achieved by the astronomers of the victors in World War II.’’ Michelson’s stellar interferometer was then converted to measure angular diameters of radio cosmic sources, but this new application of interferometry came at a cost, as we will see.[12]

Both Hanbury Brown and Twiss were born in India, which was then part of the British Empire. In 1935, Hanbury Brown received his B.Sc. in electrical engineering from the University of London and spent eleven years working on the secret development of radar for Britain’s Air Ministry. During World War II, he also conducted research on radar at the U.S. Naval Research Laboratory in Washington, DC. During 1947–49, Hanbury Brown became a consulting engineer in the field of radar for companies in France, the United States, and the United Kingdom, having as a senior partner Robert Watson-Watt (1892– 1973), a Scottish physicist who had played an important role in the wartime development of radar in the U.K. In 1949, Hanbury Brown began research for a doctoral degree at the Jodrell Bank radioastronomy research center at the University of Manchester.[13]

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Measurements,’’ Philosophical Magazine 30, no. 182 (1890): 1–21; David H. DeVorkin, ‘‘Michelson and the Problem of Stellar Diameters,’’ Journal for the History of Astronomy 6 (1975): 1–18.

[12] Edge and Mulkay, Astronomy Transformed (ref. 3); Fujinobu Takahashi et al., Very Long Baseline Interferometer (Tokyo: Ohmsha, 2000), on 1.

[13] J. Davis and B. Lovell, ‘‘Robert Hanbury Brown 1916–2002,’’ Historical Records of Aus- tralian Science 14 (2003): 459–83, on 462–63 and 464; B. Lovell and R. M. May, ‘‘Robert Hanbury Brown (1916–2002),’’ Nature 416 (2002): 34. For details about the radar in World War II, see H. E. Guerlac, Radar in World War II (New

Twiss, who was slightly younger, completed the Mathematical Tripos at Cambridge in 1941 and received his doctoral degree from MIT in 1949, working on the theory of magnetrons. During WWII, Twiss worked on radar in the Admiralty, the Naval Service of the British Armed Forces. Upon returning to the United Kingdom after getting his degree from MIT, Twiss carried out research on electromagnetic radiation and in 1955 was part of the research group at the Division of Radiophysics in Sydney, Australia.[14]

It was at Jodrell Bank that Hanbury Brown started to do research on radio astronomy. In the early 1950s, the institute had changed its research focus, moving from meteor studies to research on radio stars, which contributed significantly to the development of radio astronomy.[15] At that time, one of the problems faced by radio astronomers was to determine the ontology of the sky. This could be done by measuring the angular diameter of objects, and, depending on the value found, they could be characterized as nebulae, galaxies, or stars.[16]

The Michelson interferometer, indeed, a Michelson stellar interferometer adapted for radio sources, was used to measure the angular diameter of the objects at the end of the 1940s. In this apparatus (Fig. 1) the two vertical aerials A and B detected the radio signals which transited the plane normal to a horizontal baseline L (a conventional cable). The

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York: American Institute of Physics, 1987). For studies on the Jodrell Bank, consult B. Lovell, The Story of Jodrell Bank (Oxford: Oxford University Press, 1968); B. Lovell, Out of the Zenith: Jodrell Bank, 1957–70 (Oxford: Oxford University Press, 1973); B. Lovell, The Jodrell Bank Telescopes (Oxford: Oxford University Press, 1985); J. Agar, Science & Spectacle: The Work of Jodrell Bank in Post-War British Culture (Amsterdam: Harwood Academic, 1998).

[14] Tango, ‘‘Richard Quentin Twiss’’ (ref. 3), 4.38.

[15] Edge and Mulkay, Astronomy Transformed (ref. 3), 19–20.

[16] R. Hanbury Brown, interview by Ragbir Bhathal, 1995, RHB, Box 2, Section A.31, on 11–12.

outputs were detected by a receiver connected to the center of the cable, and then registered by a power recorder; the oscillations observed during the transit of the source were similar to the interference fringes in a Michelson stellar interferometer.[17] In the case of a large angular size, it would be necessary to have a small separation between the aerials A and B to measure the angular diameter. For small ones, however, the separation of the aerials would have to be extremely large.

There were two major problems with this kind of interferometer. The horizontal baseline between the two aerials could be extended to fifty kilo- meters without causing any perturbation in the system. Nevertheless, due to the instability of the phase in the transmission process through the cables, the measurements of the angular diameter might be especially inaccurate when using long baselines.[18] This limitation on the size of the separation restricted the use of the Michelson interferometer for only some radio sources. Moreover, using a very long cable to connect the aerials made the interferometer ‘‘both cumbersome and expensive.’’ The other difficulty with the Michelson interferometer was associated with its excessive sensitivity to ionospheric effects that might interfere with the measurements.[19]

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[17] Robert Hanbury Brown and Richard Q. Twiss, ‘‘A New Type of Interferometer for Use in Radioastronomy,’’ Philosophical Magazine 45 (1954): 663–82, on 664.

[18] Robert Hanbury Brown, R. C. Jennison, and M. K. Das Gupta, ‘‘Apparent Angular Sizes of Discrete Radio Sources,’’ Nature 170 (1952): 1061–63, on 1061.

[19] Hanbury Brown and Twiss, ‘‘New Type of Interferometer’’ (ref. 16), 663–64, 667, 678. The ionosphere, composed of free electrons, is able to cause the reflection and absorption of radio wave changing, thus, the radiation intensity arriving at the earth’s surface.

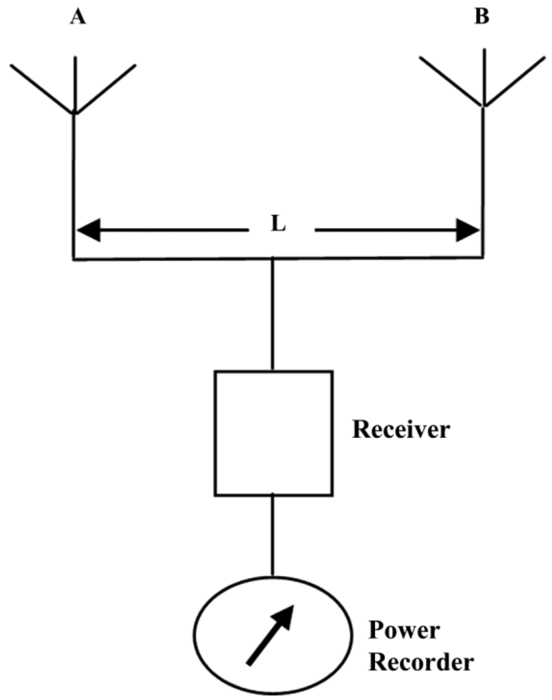


Fig. 1: Simplified diagram of Michelson's interferometer used in radio astronomy. Source: Hanbury Brown and Twiss, ''New Type of Interferometer'' (ref. 16), 664.

The construction of a new type of interferometer able to work with a long baseline required significant engineering skills and new techniques. Hanbury Brown demonstrated these in constructing the ''intensity interferometer,'' the use of which would make it possible to compare the intensities at two different points of an electromagnetic field, instead of comparing the amplitude and phase of the oscillations in the Michelson interferometer. As he later recounted,

> [L]ate one night in 1949 I was wondering whether, if I were to take ''snap- shots'' of the noise received from a radio source on oscilloscopes at the outputs of two spaced receivers, I could compare these snapshots. The answer to that question led me

> directly to the idea of an interferometer in which the intensities of two noise-like signals are compared instead of their amplitude and phase.[20]

The idea of an intensity interferometer was thus born. However, Hanbury Brown needed a sophisticated theoretical model for it. He therefore asked his friend Vivian Bowden to find someone who could help with the mathematics to model the intensity interferometer. According to Hanbury Brown, ‘‘[u]nfortunately I didn’t know enough mathematics to work out the answer ... Vivian found me someone called Richard Twiss who, like me, was born in India of an Army family, but, unlike me, was a talented mathematician.’’[21] The ensuring collaboration between Hanbury Brown and Twiss is a not unfamiliar example of interaction between an experimentalist and theoretician working together in order to construct a new instrument. This collaboration, however, was con- ducted at a distance: Hanbury Brown was part of the Jodrell Bank group at the University of Manchester, and Twiss worked for the Services Electronics Research Laboratory at Baldock located thirty-eight miles from London. Even though they had some face-to-face meetings, their collaborative work was conducted by postal correspondence.

During his first visit to the University of Manchester, Hanbury Brown ex- plained to Twiss his idea for a new type of interferometer and asked him to verify mathematically how sensitive the new instrument might be. Afterwards, work ing on the mathematics, Twiss concluded that (as later remembered by Hanbury Brown), ‘‘[t]his idea of yours is no good, it doesn’t work!’’ However, when Hanbury Brown and Twiss were reviewing Twiss’s calculations, they found that there was a small mistake in one of the integrals, and after correcting it, there was no doubt that the interferometer would work properly. Nonetheless, it

[20] Hanbury Brown, ‘‘Paraboloids, Galaxies and Stars’’ (ref. 4), 226–27.

[21] Hanbury Brown, Boffin (ref. 2), 105.

would not be sufficiently sensitive to obtain the angular diameter of most radio sources, only the two strongest ones, Cygnus and Cassiopeia. While Twiss was working on the theory for the new interferometer in detail, Hanbury Brown, with the assistance of his research students Roger C. Jennison and Mrinal K. Das Gupta, constructed the new intensity interferometer.[22]

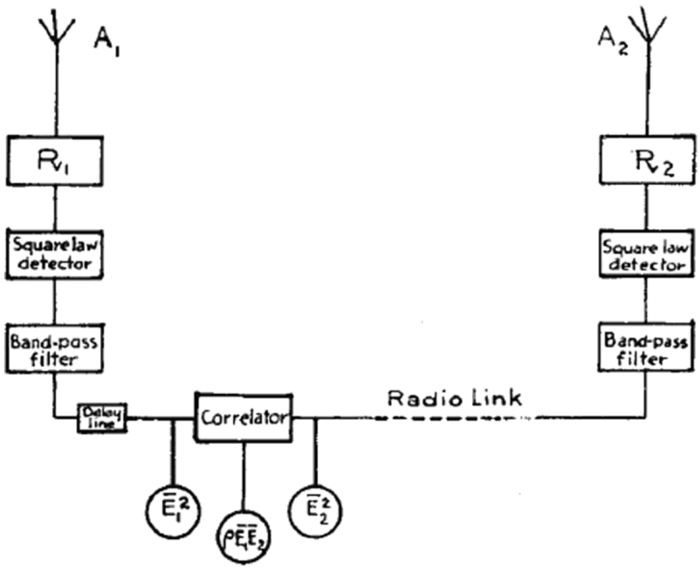


Fig. 2: Outline of the new type of interferometer developed by Hanbury Brown at Jodrell Bank. Source: Hanbury Brown, Jennison and Das Gupta, ''Apparent Angular Sizes'' (ref. 17), 1061.

By 1950 the intensity interferometer had been built. In this instrument (Fig. 2) the signals were detected by two aerials $A_1$ and $A_2$, which were connected through two independent receivers $R_1$ and $R_2$; the outputs were separately rectified in each square-law detector (output voltage proportional to the square of the electric field) and then passed through the two low- frequency filters; the two low-frequency outputs were multiplied together in a correlator, thus obtaining their ''cross-correlation,'' which would be the measurement of the similarity of two different signals, or waveforms. Hanbury Brown and Twiss derived the

[22] Ibid., 105–06.

following expression for the cross-correlation coefficient, which is analogous to that for the visibility of the fringes in the Michelson stellar interferometer:

$$\rho = \frac{sin^2\left(\frac{\pi\alpha b}{\lambda}\right)}{\left(\frac{\pi\alpha b}{\lambda^2}\right)} \tag{1}$$

where *a* is the angular width of an equivalent rectangular source of constant surface intensity, *b* is the length of the baseline, and *λ* is the wavelength.[23] Equation (1) means that if a suitable baseline is chosen, it is possible to obtain a value for the angular diameter of the source.

The correlator, comparing the two signals using the technique of Fourier analysis, was able to isolate specific components of a compound waveform. In order to use short baselines, a radio link could be inserted between the two aerials; for extremely long baselines the signals could be recorded on tapes and correlated later. This was an advantage compared to the Michelson interferometer, whose inaccuracy was exacerbated when using very long baselines. In their first theoretical model for the new interferometer, Hanbury Brown, Jennison, and Das Gupta calculated the cross-correlation as a function of the apparent angular diameter of the radio sources, the effective length of the baseline, and the wavelength.[24]

Even though the new interferometer successfully measured the angular diameter of Cassiopeia and Cygnus when compared to other measurements available, Hanbury Brown recounted later that he was deeply disappointed at the final result. Because it was not necessary to use a long baseline—only a few kilometers—to measure the angular diameter of these sources, ‘‘there was no need to have developed the intensity interferometer; we could have done the same job with a conventional interferometer in half time and with half effort.

[23] Hanbury Brown, Jennison, and Das Gupta, ‘‘Apparent Angular Sizes’’ (ref. 17), 1062.

[24] Ibid., 1061; Hanbury Brown, ‘‘Paraboloids, Galaxies and Stars’’ (ref. 4), 227.

We had built a steam-roller to crack a nut.''[25] The effort involved in the construction of an intensity interferometer thus seemed to have been unnecessary. Nonetheless, one interesting finding brought Hanbury Brown and Twiss to the realization that such an interferometer could be used to measure the angular diameter not of radio-emitting stars, but of bright, visible stars.

Observing the intensity interferometer in use during one of Twiss's visits to Jodrell Bank, Twiss and Hanbury Brown realized that it was working success- fully, even though ‘‘on that particular day the signal was scintillating violently as it passed through the ionosphere and [they] noticed that, although the strength of the signals in the two antennae were fluctuating wildly, their correlation was unchanged.''[26] Then, by analogy, they conjectured that the intensity interferometer seemed to be able to work accurately in a turbulent medium, that is, with the fluctuation density of the atmosphere. This had been a limitation of the Michelson stellar interferometer, when used for optical sources, in addition to the issue of long baselines. Because of that advantage, Hanbury Brown and Twiss decided to construct an interferometer for optical astronomy using the same principles as the intensity interferometer in radio- astronomy, allowing measurements of the angular diameter of bright stars to be made.

By 1954, Hanbury Brown and Twiss shifted their interest from radioastronomy to the domain of optics. During the second half of the 1950s, as argued by the historians of science David Edge and Michael Mulkay, ‘‘radio astronomers began to extend the application of their

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[25] Hanbury Brown, ‘‘Paraboloids, Galaxies and Stars'' (ref. 4), 228.

[26] Ibid., 228; Hanbury Brown, Boffin (ref. 2), 118; Hanbury Brown, interviewed by Ragbir Bhathal (ref. 15), 117.

techniques to areas that previously had been exclusively optical.''[27] In the winter of 1956, Hanbury Brown and Twiss published an article entitled ''The Correlation between Photons in Two Coherent Beams of Light,'' coherence implying that there is a constant phase relationship between two values of the electromagnetic field at separated points or separated times. In this paper, they reported a laboratory test to verify if the same techniques and principles used in the HBT intensity interferometer for radio astronomy could be applied to optical astronomy. Such a test was carried out with an artificial source of light, a high-pressure mercury arc. In doing so, Hanbury Brown and Twiss left in fact the field of radio astronomy and turned to optics.

In the HBT intensity interferometer, working with radio waves, a correlation between intensity fluctuations at two different points could be obtained. Nevertheless, acknowledging ''this fundamental effect has never been directly observed with light, and indeed its very existence has been questioned,'' Hanbury Brown and Twiss decided to perform a laboratory test before building their stellar interferometer to investigate whether or not there would be a correlation between beams of light.[28]

In the HBT optical system (Fig. 3), the light source was focused by a lens and sent through a system of filters. The beam of light was divided by a semitransparent mirror to focus on the cathodes of the photomultipliers $C_1$ and $C_2$. The fluctuations in the output were amplified and multiplied together in a correlator. The correlation in the fluctuations was thus obtained through an integrating motor. The photomultiplier $C_1$ could move vertically and consequently the measurements could be obtained in two different ways: first, when the

[27] Edge and Mulkay, Astronomy Transformed (ref. 3), 277.

[28] Robert Hanbury Brown and Richard Quentin Twiss, ''Correlation between Photons in Two Coherent Beams of Light,'' Nature 177, no. 4497 (1956a): 27–29, on 27; Hanbury Brown, ''Paraboloids, Galaxies and Stars'' (ref. 4), 229.

optical paths from the mirror to the cathodes $C_1$ and $C_2$ were at the same length, that is, superimposed when viewed from the source; and second, when those paths were not the same length but rather separated by a distance *d*.[29]

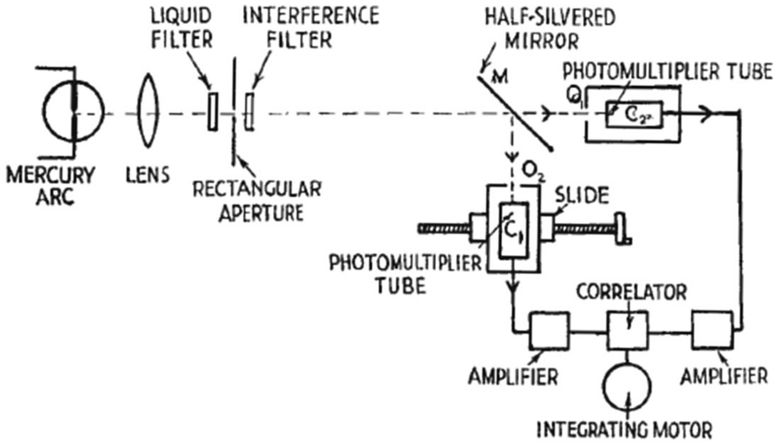


Fig. 2. Simplified diagram of the apparatus

Fig. 3: The optical system set-up by Hanbury Brown and Twiss. Source: Hanbury Brown and Twiss, ‘‘Correlation between Photons’’ (ref. 27), 28.

The theoretical model for the HBT optical interferometer was calculated using a semiclassical approach. Hanbury Brown and Twiss considered radiation from the mercury source as a classical wave, but they also used the quantization of the radiation in the photoelectric emission for photodetection. Assuming the probability of the emission of a photoelectron to be proportional to the square of the amplitude of the incident light, Hanbury Brown and Twiss calculated the correlations between the fluctuations in the current from the cathodes by using classical electromagnetic wave theory. In their theoretical studies they obtained first the correlation S(0) when the two cathodes were superimposed,[30]

[29] Hanbury Brown and Twiss, ‘‘Correlation between Photons’’ (ref. 27), 27–28.

[30] Ibid., 28.

$$S(0) = A.T.b_\nu.f\left(\frac{\alpha_1\theta_1\pi}{\lambda_0}\right).f\left(\frac{\alpha_2\theta_2\pi}{\lambda_0}\right)\int \alpha^2(\nu).n_0^2(\nu).d\nu, \qquad (2)$$

then determined the associated root-mean-square fluctuations N,

$$N = A.T.\frac{2m}{m-1}.b_\nu(b_\nu T)^{-\frac{1}{2}}\int \alpha(\nu).n_0(\nu).d\nu \qquad (3)$$

In these equations, *A* is a constant of proportionality, *T* is the time of observation, $\alpha(v)$ is the quantum efficiency of the photocathodes at a frequency $v$, $n_0(v)$ is the number of quanta incident, *bv* is the bandwidth of the amplifiers, *m/(m–1)* is the excess noise, $a_1$ and $a_2$ are the horizontal and vertical dimensions of the photocathode apertures, $\theta_1$ and $\theta_2$ are the angular dimensions of the source as viewed from the photocathodes, and $\lambda_0$ is the mean wavelength of the light. Hanbury Brown and Twiss mentioned that the factor

$$f\left(\frac{\alpha\theta\pi}{\lambda_0}\right)$$

could be found through the dimensionless parameter given by

$$\eta = \alpha\theta/\lambda_0$$

When T << 1, *S(d)* is proportional to the square of the Fourier transform of the intensity distribution of the source, as in the case of an experiment with visual stars. On the other hand, when T >> 1, the correlation function does not depend on the actual width of the source.[31]

The HBT experiment was carried out first with the two photomultipliers superimposed *(d = 0)*, and then with the photomultipliers separated *(d = 1.8 cm)*. It took six hours to run the experiment for each situation; the counting was done at five-minute intervals.

[31] Ibid.

Hanbury Brown and Twiss measured for each run the factor *Se(0)/Ne*, where the experimental values of *S* and *N* come from the equations (2) and (3), that is:

$$\frac{m-1}{m}\int \alpha^2(\nu)n_0^2(\nu)d\nu \bigg/ \int \alpha(\nu)n_0(\nu)d\nu, \qquad (4)$$

which was obtained experimentally from the spectrum of the incident light, the direct current, and the gain and output noise of the photomultipliers. The HBT expression (2) thus provided the correlation between the numbers of photoelectrons detected at a time interval of observation. During the measurement, the photomultipliers $C_1$ and $C_2$ detected the outputs separately, and then the correlation between the intensity fluctuations was obtained through the correlator.[32]

Table 1. COMPARISON BETWEEN THE THEORETICAL AND EXPERIMENTAL VALUES OF THE CORRELATION

| | Cathodes superimposed ($d = 0$) | | Cathodes separated ($d = 2a = 1\cdot8$ cm.) | |
|---|---|---|---|---|
| | Experimental ratio of correlation to r.m.s. deviation $S_e(0)/N_e$ | Theoretical ratio of correlation to r.m.s. deviation $S(0)/N$ | Experimental ratio of correlation to r.m.s. deviation $S_e(d)/N_e$ | Theoretical ratio of correlation to r.m.s. deviation $S(d)/N$ |
| 1 | $+7\cdot4$ | $+8\cdot4$ | $-0\cdot4$ | $\sim 0$ |
| 2 | $+6\cdot6$ | $+8\cdot0$ | $+0\cdot5$ | $\sim 0$ |
| 3 | $+7\cdot6$ | $+8\cdot4$ | $+1\cdot7$ | $\sim 0$ |
| 4 | $+4\cdot2$ | $+5\cdot2$ | $-0\cdot3$ | $\sim 0$ |

Source: Hanbury Brown and Twiss, ‘‘Correlation between Photons’’ (ref. 27), 29.

[32] Ibid.

As shown in Table 1, when the cathodes were superimposed, Hanbury Brown and Twiss observed a high correlation between the arrival times of photons in two coherent light beams in close agreement with the calculated theoretical value. However, the observed correlation decreased when the separation between the two photomultipliers was altered. When the photomultipliers $C_1$ and $C_2$ were separated by a distance d, the time of arrival photons at the photomultiplier $C_2$ was registered first, earlier than at the photomultiplier $C_1$ because the distance between the mirror and the photomultiplier $C_1$ was greater. As a result, no correlation between photons was observed.[33]

Comparing their theoretical values with the experimental results, Hanbury Brown and Twiss concluded that ‘‘the experiment shows beyond question that the photons in two coherent beams of light are correlated, and this correlation is preserved in the process of photoelectric emission.’’ Noting a small difference between the theoretical and experimental results, they mentioned that it was probably due to defects in the optical system. Thus, the HBT experimental test seemed to confirm the possibility of constructing an optical interferometer using the same principles as the intensity interferometer for radio astronomy.[34]

Hanbury Brown and Twiss had therefore observed photons arriving at the same time at the two different photomultipliers. Yet their conclusion provoked a heated debate in the physics community, some of whose members claimed in articles and correspondence that the HBT experimental results were ‘‘nonsense.’’ As Hanbury Brown and Twiss had used a low-intensity source, it would be expected that only individual photons were arriving at the half-silvered mirror in a given time interval, and hence no correlation between photons would be observed. In the following section, we present the controversial issues related to the HBT optical interferometer circa 1956–58.

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[33] Ibid., 29.

[34] Ibid.

## The Controversy

The first objection to the HBT experimental results arose in 1956 through an experiment conducted at the University of Western Ontario by Eric Brannen, of the department of physics, and his graduate student Harry I. S. Ferguson. Their motivation was to verify whether or not there would be a correlation between photons as found by the Hanbury Brown–Twiss optical interferometer. Commenting on the HBT results in the columns of Nature, Brannen and Ferguson openly claimed that ‘‘if such a correlation did exist, it would call for a major revision of some fundamental concepts in quantum mechanics,’’ thus justifying the Brannen-Ferguson (BF) experiment. The BF experiment was virtually identical to the HBT experiment, aside from the detection process. Even before publishing their article, Brannen and Ferguson shared their results with Hanbury Brown and Twiss by suggesting that the HBT results could have been due to fluctuations in light intensity from the source and thus ‘‘it is possible that your radio telescope is reacting to a large number of photons, rather than to the behavior of individual photons.’’ Hanbury Brown reacted to it, ‘‘I would welcome the publication of your experimental results, but I would reluctantly advise that you should not draw the conclusion that they disprove the correlation observed by us … I, personally, would welcome seeing a paper published which says that my own work is wrong ... I am a great lover of scientific controversy, because I find I learn a lot from it.’’[35]

The BF apparatus used an electronic detection system to detect possible coincidences between two individual photons detected separately by the two photomultipliers over a period of time. Unlike in the HBT experiment, a linear photomultiplier was used and hence the

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[35] Eric Brannen and Harry I. S. Ferguson, ‘‘Question of Correlation between Photons in Coherent Light Rays,’’ Nature 178 (1956): 481–82, on 482. E. Brannen and H. I. S. Ferguson to R. Hanbury Brown and R. Q. Twiss, 5 May 1956, and R. Hanbury Brown to E. Brannen, 5 Jun 1956, both in the Jodrell Bank Archives, University of Manchester, UK.

intensity fluctuations in each detector were recorded, and a correlator combined the current outputs. Thus, the principal difference between the two detection systems was the fact that while Brannen and Ferguson detected individual photons at a time interval, Hanbury Brown and Twiss compared the intensity fluctuations of the outputs at the two detectors through a correlator. In the Brannen-Ferguson experiment (Fig. 4), as in the HBT setup, a light produced by high-pressure mercury passed through a filter and lens system, and then through a pinhole so that only monochromatic light reached the mirror; the beam was then split by a half-silvered mirror, and each separate beam of light was sent to the two photomultipliers; the coincidences were counted electronically.[36]

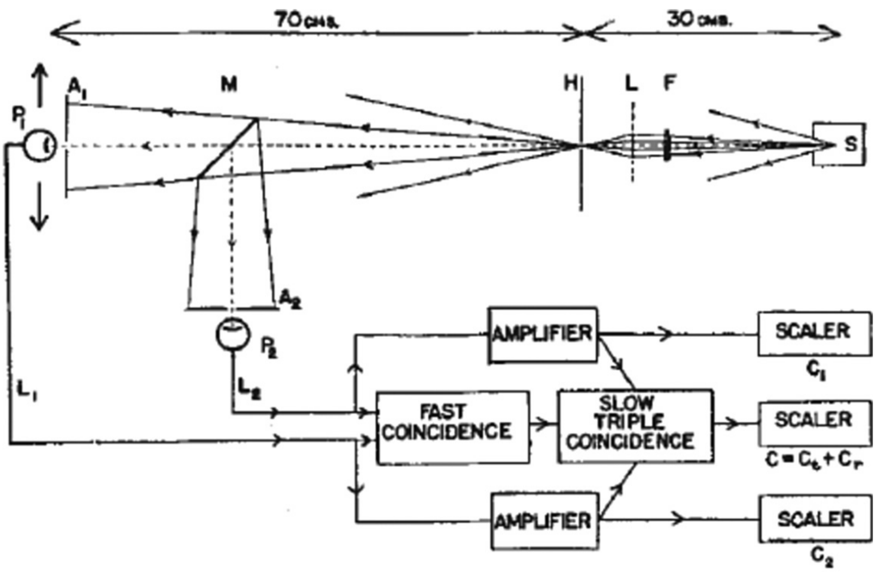


**Fig. 1. Simplified diagram of apparatus**

Fig. 4: The Brannen-Ferguson experimental diagram. Source: Brannen and Ferguson, ‘‘Question of Correlation’’ (ref. 34), 481.

After performing their experiment, Brannen and Ferguson did not find any significant correlation (less than 0.01 percent) between photons in coherent light rays. Such an

[36] Ibid., 481.

experimental result, according to them, agreed significantly with an experiment carried out even before the HBT experiment by the Hungarian physicist Lajos Ja´nossy (1912–78) and his research group from the Central Research Institute of Physics in Budapest. Brannen and Ferguson highlighted that Jánossy also agreed that if the existence of the HBT correlation between photons were confirmed, the foundations of the quantum theory should be revisited.[37]

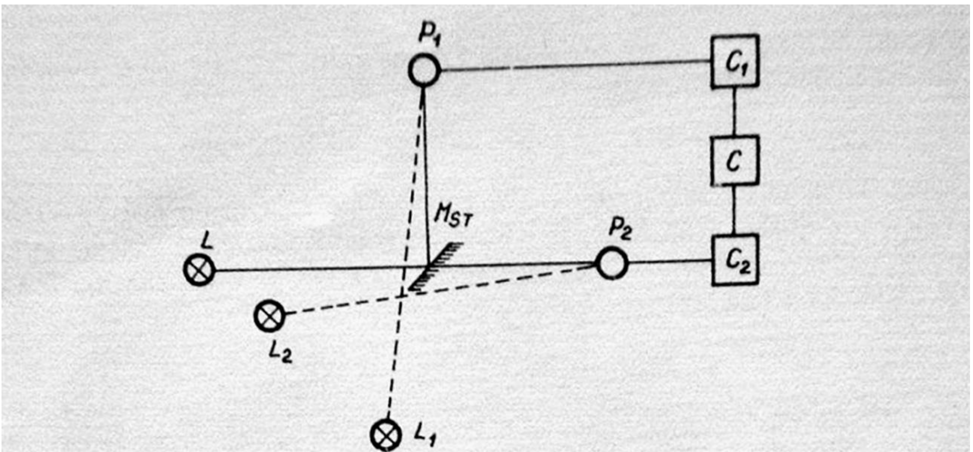


Fig. 1 L light sources, $M_{ST}$ semi-transparent mirror, $P_1$, $P_2$ photo-multipliers. $C_1$, $C_2$ counting arrangement for the individual photons, C counting arrangement for coincidences

Fig. 5: The set-up of the AJV experiment published in 1955. Source: RHB, Box 18, Section E.64.

Jánossy, for his part, had always been interested in the foundations of the quantum theory, in addition to his interests in cosmic radiation, the theory of relativity, and the philosophy of physics.[38] By the early 1950s, Jánossy had become one of the critics of the Copenhagen

[37] Ibid., 481–82.

[38] L. Pál, ‘‘L. Jánossy 1912–1978,’’ Acta Physica Academiae Scientiarum Hungaricae 43, no. 1 (1978): I–IV.

interpretation of quantum theory.[39] This justified the motivation behind A. Ádám, Jánossy, and L. Varga (AJV)'s experiment ‘‘to investigate the validity of th[e] prediction of [the] quantum theory.’’[40] In their article, Ádám and colleagues proposed an experiment (Fig. 5) in which a low-intensity source *L* was split into two components by a half-silvered mirror and each component was sent to photomultipliers $P_1$ and $P_2$. The counters $C_1$ and $C_2$ were used to detect individual photons, and *C* recorded the coincidences. According to the authors and their interpretation of the prediction of the conventional quantum theory, if one assumes that photons are indivisible particles, they should be either in one component of a beam or in the other one after being split by the mirror. As a result, no systematic coincidences between photons detected separately by the two photo- multipliers would be observed.[41]

As a detection process, the AJV experiment used a coincidence counter to detect individual photons, as later used in the BF experiment, which was different from the HBT measurement process. In order to determine whether the coincidences detected by the photomultipliers were systematic—as HBT would later claim—or purely accidental—as traditional quantum theory seemed to predict—Ádám and his co-workers performed a version of their experiment with two independent sources, $L_1$ and $L_2$. As defined by them, the number of systematic coincidences would be obtained through the number of possible coincidences in the case of the source *L* (coherent light) minus the number of accidental coincidences from the independent sources $L_1$ and $L_2$ (incoherent light). Ádám and colleagues expected ‘‘merely’’ chance coincidences between the independent sources, and also systematic

[39] See L. Jánossy, ‘‘The Physical Aspects of the Wave-Particle Problem,’’ Acta Physica Academiae Scientiarum Hungaricae 1, no. 4 (1952): 423–67.

[40] A. Ádám, L. Jánossy and P. Varga, ‘‘Observations on Coherent Light Beams by Means of Photomultipliers,’’ translated into English from Acta Physica Academiae Scientiarum Hungaricae 4, no. 4 (1955): 301–15, in RHB, Box 18, Section E.64, on 57.

[41] Ibid.

coincidences between the two components of the source *L* after splitting by the mirror.[42] After performing the experiment, they observed that the number of systematic co- incidences between photons was approximately 0.6 percent—an experimentally insignificant figure—and concluded that ‘‘in agreement with the predictions of [the] quantum theory, the photons of two coherent light beams are independent of each other, or at least the biggest part of such photons are independent of each other.’’[43]

The AJV experiment seemed to confirm the principles of quantum theory as interpreted by Ádám and co-workers. However, a result that seemed to contradict quantum theory would appear a year later with the HBT experimental results. In their 1956 paper, Hanbury Brown and Twiss did not mention the AJV results. However, because they highlighted that a correlation between photons had never been observed, it seems likely that Hanbury Brown and Twiss could have had some knowledge of the AJV experiment. Thus, the HBT experimental results were significantly different from those of the BF and AJV experiments, and likewise in violation of the prediction of quantum theory, at least as discussed in the 1955 article by Ádám et al.

If the HBT experimental results were correct, it would be necessary to sup- pose that, for instance, photons—contrary to what Einstein had proposed in 1905—could be divisible particles, making it possible to detect them at the same time at two different photomultipliers; or, as Hanbury Brown later put it himself, ‘‘one would have to imagine photons hanging out waiting for each other in space!’’[44] Hanbury Brown continued, ‘‘[t]he basic trouble was that one can think about light in two different ways, as a wave or as particles. Richard and I had treated light as a wave which on arriving at the phototube causes the emission of a

[42] Ibid., 58.

[43] Ibid., 63.

[44] Hanbury Brown, Boffin (ref. 2), 121.

photoelectron ... . However if you insist on thinking of light as a stream of independent particles like ping pong balls, which is what most physicists—especially particle physicists—prefer to do, then it is impossible to see how the arrival times of these particles can be correlated.'' This is why the HBT results were viewed by many physicists as ''not only heretical ... but patently absurd.''[45] The standard concept of the photon thus seemed to be irreconcilable with the HBT correlation.

The criticisms provoked by the HBT results, as well highlighted by Han- bury Brown, revealed the way in which some physicists understood the concept of the photon at that time. Because of the low intensity of the source used in the HBT experiment, some physicists expected that only individual photons were arriving at a half-silvered mirror in a certain time interval. Hence, assuming the mainstream concept of the photon, as a billiard-ball model, each photon should be either reflected or transmitted by a mirror, and should only be recorded in one of the photomultipliers at a time. As a result, the chances of a correlation between photons should be theoretically zero—or extremely small, allowing for some defects in an actual experiment. Some physicists would not have believed that individual photons from a single beam could be detected simultaneously by two different detectors, as they had been in the HBT experiment. Of course, such difficulties disappeared completely if the incident light were considered to be a classical electromagnetic wave. In this case, there would be no doubt that two different points of an electromagnetic field might be correlated and detected simultaneously, even after having been split through a half-silvered mirror, since the incident light was coherent.

The first physicist to defend the HBT results was the American 1952 Nobel laureate Edward M. Purcell (1912–97) of Harvard University. At the time the HBT experiment was being carried out, Purcell had shown an interest in radio astronomy, working with his

[45] Ibid., 120–21.

graduate student Harold I. Ewen on the construction of a horn antenna to investigate the interstellar medium.[46] In an effort to settle the dispute between the HBT and BF experimental results, Purcell wrote to the editors of Nature, attaching an article that would be his contribution to the discussions on the correlation between photons. In the correspondence, Purcell made it absolutely clear that he was serving as a ‘‘volunteer for the defense of [Hanbury] Brown and Twiss.’’ Even though Purcell had thought that ‘‘if the issue is as simple as I believe it to be, it would be a pity to leave it unresolved for long,’’ the controversy over the HBT experiment was just beginning. In fact, it would take approximately two years to resolve it.[47]

Purcell was the first to suggest that ‘‘the Brown-Twiss effect, far from requiring a revision of quantum mechanics, is an instructive illustration of its elementary principles,’’ even though some physicists had criticized the HBT results based on it. In his interpretation of the HBT results, Purcell examined the problem through the statistical fluctuations of a system of bosons. Assuming that the probability of ejection of a photoelectron at a time $T$ as a function of the square of an electric field ($P$) and an experimental constant ($\alpha$) was $\alpha\bar{P}T$, Purcell determined the number of counts of the two photomultipliers at the same time interval separately, and then recombined the outputs from the two photomultipliers, finding a correlation in the number of photoelectrons detected given by

$$\overline{\Delta n_1 \Delta n_2} = \frac{1}{2}\overline{n_1^2}\tau_0 / T \qquad (5)$$

[46] John S. Ridgen, ‘‘Edward Mills Purcell, August 30, 1912–March 7, 1997,’’ Physics in Perspective 13, no. 1 (2011): 91–103, on 96; Robert V. Pound, ‘‘Edward Mills Purcell 1912–1997,’’

Biographical Memoirs 78 (2000): 3–24.

[47] E. M. Purcell to the Editors of Nature, 9 Nov 1956, EMP, Box 3, Folder Calculations and Correspondence on Brown-Twiss Experiment and Brannen and Ferguson Correspondence, 1956–1957.

in which $\tau_0$ is a correlation time determined by the light spectrum, and $T$ is a fixed time interval.[48]

Although the HBT equation and the Purcell equation were calculated using different theoretical approaches, Purcell's derivation represented, according to him, ''the positive cross-correlation effect of [Hanbury] Brown and Twiss.'' The term ''positive'' seems to be related to the fact that Hanbury Brown and Twiss had observed a correlation between photons when they should not have, according to the traditional picture of the photon. The value of the cross- correlation could be different depending upon the nature of the particle used in an HBT-type experiment. Using a beam of electrons, for instance, arriving at a nonpolarized mirror, the cross-correlation would be $(\overline{\Delta n_1 \Delta n_2} < 0)$; or using a beam of classical particles, it would be $(\overline{\Delta n_1 \Delta n_2} = 0)$. A null cross- correlation would only be found by sending classical particles to be split by a half-silvered mirror. These results should be expected, as stated by Purcell, since there might be a difference between the behavior of fermions, bosons, and classical particles.[49]

Unlike Brannen and Ferguson, Purcell suggested that ''[t]he Brown-Twiss effect is thus, from a particle point of view, a characteristic quantum effect,'' being simply a consequence of a system of bosons. Such a quantum effect was a result of the ''clumping'' of the photons.[50] It seems that Purcell used the term ''clumping'' for the ''bunching of photons,'' the probability of two photons reaching a certain point at the same time. In the HBT experiment, the light was produced by many different atoms, characteristic of a mercury

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[48] E. M. Purcell, ''Question of Correlation between Photons in Coherent Light Rays,'' Nature 178 (1956): 1449–50.

[49] Ibid., 1450.

[50] Ibid.

source. Hence, when the first atom had already emitted a photon, the second atom began to be almost instantaneously excited and then emitted another photon in a short period of time. Thus, the use of the standard concept of the photon would no longer be controversial in the HBT experiment, since some photons might arrive in pairs at the half-silvered mirror. That was why the HBT results had showed a correlation between pairs of photon counts.

However, ‘‘[i]f one insists on representing a photon by wave-packets,’’ as stated by Purcell, the HBT results could be explained as the probability of two trains, a stream of wave packets in a random sequence, accidentally overlapping.[51] Therefore, Purcell suggested two ways to interpret the HBT results, depending on which picture of light was embraced: the wave aspect of light (in which the phenomenon could be explained through an overlapping wavepackets approach); or, the corpuscular aspect of light (according to which the phenomenon would be a signature of photon bunching).

Regarding the opposite experimental results, Purcell pointed out that the BF experiment did not detect, as in the HBT experiment, a correlation between photons because the observing time required to verify it depended on the resolving time of the apparatus and the stability of the source. That is, the HBT experiment was much more sensitive and accurate than the BF experiment.[52]

A copy of Purcell’s correspondence and article was also sent to the protagonists of the debate. Aware of Purcell’s work, Hanbury Brown wrote to him saying that he had strongly recommended the publication of his article to the editors of Nature. Recognizing the importance of having a Nobel laureate on his side in the controversy, Hanbury Brown also remarked that ‘‘although we can defend ourselves, it is nice to have an ally! [M]any

[51] Ibid., 1449.

[52] Ibid.

physicists,'' as Hanbury Brown observed, ''like to think of photons as independent little chaps who are loath to link hands as all proper bosons should.''[53]52 Even after the development of Bose-Einstein statistics, from which the so-called bosons were born, some physicists still interpreted—in the case of the HBT experiment—a photon as a classical particle, a distinguishable entity described by Boltzmann statistics. Assuming the mainstream concept of the photon, the HBT results—an observation of a correlation between photons—did not make sense at all. Unlike some physicists, Purcell not only defended the HBT results, but also explained them using the properties of bosons. Hanbury Brown had agreed with Purcell that the HBT results were consistent with the elementary quantum theory, although he and Twiss had not used it in their theoretical approach published in 1956.

In correspondence with Purcell, Twiss highlighted that even though he and Hanbury Brown wanted to use the quantum theory in the first draft, they had ''laid a great stress on the interpretation in terms of the corpuscular picture of light,'' using concepts such as the uncertainty principle and photon bunching. Nevertheless, the difficulty disappeared when the Belgian physicist Léon Rosenfeld (1904–74), who was one of the great defenders of the Copenhagen interpretation of the quantum theory[54] and who also worked at the University of Manchester, suggested a ''sort of language at any price'' to Hanbury Brown and Twiss. It was a ''language'' based on the semiclassical approach: the HBT experiment illustrated the

[53] R. H. Brown to E. M. Purcell, 15 Nov 1956, EMP (ref. 46).

[54] On Rosenfeld, see Anja Jacobsen, Léon Rosenfeld: Physics, Philosophy, and Politics in the Twentieth Century (London: World Scientific Publishing, 2012); A. Jacobsen, ''The Complementarity between the Collective and the Individual Rosenfeld and Cold War History of Science,'' Minerva: A Review of Science, Learning and Policy 46 (2008): 195–214; A. Jacobsen, ''Léon Rosenfeld's Marxist Defense of Complementarity,'' HSPS 37 (2007): 3–34.

wave aspect of light, and the quantum theory would only be used to interpret the detection process.[55]

‘‘In the end, after a deal of squawking, (which is not of much one at 12,000 miles range anyway),’’ as recounted by Twiss, who was then part of the Division of Radiophysics in Sydney,

> I was convinced that this was the better course since though it is certainly quite legitimate to me to use the photon concept throughout, as long as one knows exactly what [one was] mainly doing, it is only too likely to mislead the chaps who are always liable to forget that photons behave very different from classical particles ... . However, nobody has any trouble believing that intensity fluctuations due to interference between waves emitted [from] different parts of the source can be correlated at different points in the field of the observer.[56]55

The choice of an interpretation based on classical theory, instead of using a fully quantum theory, seems to have been pragmatic. On the one hand, even if Hanbury Brown and Twiss wanted to use quantum theory, there was doubt as to which concept of the photon should be taken into account. Thus, it was better to avoid an unclear interpretation of the phenomenon. On the other hand, a theoretical approach based on classical theory seemed to be much more understandable and acceptable because the phenomenon could be explained clearly through an interference effect. Nonetheless, the HBT interpretation came in for criticism.

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[55] R. Q. Twiss to E. M. Purcell, n.d., EMP (ref. 46). Twiss, however, kept his inclinations for the bunching terminology, as he wrote to Rosenfeld, ‘‘I have also always referred to the bunching of photons as ‘so called bunching,’ though had I been in England I would have liked to argue that this particular piece of the corpuscular terminology is not unhelpful.’’ R. Twiss to L. Rosenfeld, 19 Oct 1956, Rosenfeld Papers, Niels Bohr Archive, Copenhagen.

[56] Ibid.

Commenting on Purcell's interpretation of the HBT results, Brannen and Ferguson criticized Hanbury Brown and Twiss by claiming that ''they expected [a] correlation even at low light intensities to the limit of only one photon being in the system at a time (to speak loosely),''[57] which would contradict the foundations of quantum theory. As described previously, as long as only individual photons were reaching a half-silvered mirror, it became extremely difficult to understand how Hanbury Brown and Twiss would have detected a correlation between two photons at two different detectors. Brannen and Ferguson claimed that they would carry out more experiments using a constant low-intensity source so that ''only one photon will be in the system at a time, in order to keep away any effects due to photon bunching.'' According to them, the positive correlation observed in the HBT experiment could be due to fluctuations in the source which could not be perfectly constant.[58]

Even though Purcell had mentioned the photon overlap model in response to the Brannen and Ferguson question about the HBT interpretation, he suggested that

> talking about interference of photons is the easiest way to go astray in such matters. To try to represent a photon by a wave-packet is asking for trouble. On the other hand the classical calculation, a la Brown and Twiss, of the fluctuations in P is a perfectly sound and rigorous procedure. The electromagnetic field is a classical field after all, which is why the Brown-Twiss effect only appears odd if one looks at it from a particle point of view; its oddness being simply the peculiarity of bosons.[59]

Clearly, Purcell did not like the idea of representing a photon as a wavepacket; however, the HBT theoretical model—based on the wave theory—seemed to be accurate and consistent

[57] E. Brannen and H. I. S. Ferguson to Edward M. Purcell, 29 Nov 1956, EMP (ref. 46).

[58] Ibid.

[59] Ibid. (emphasis in original).

with the experimental results. Because the electro- magnetic field was a classical field, as mentioned by Purcell, one would expect to interpret the results through a semiclassical approach. Nonetheless, a difficulty arose from assuming the corpuscular picture of light. Such a difficulty might be solved by considering that the behavior of an ensemble of bosons differed from the behavior of classical particles. Of course, there would be no correlation between classical particles in a HBT-type experiment, but there would be systematic correlations between bosons because of the nature of those particles.

Hanbury Brown and Twiss, on the one hand, and Purcell, on the other hand, had different feelings about their involvement in the controversy. Han- bury Brown and Twiss, according to Hanbury Brown's later recollections, were almost excommunicated from the community of physicists.[60] Purcell wrote to Brannen cheerfully that ''[w]e have had a lot of fun around here arguing about these questions, and I must say I have learned some physics in the course of it, which makes me grateful for the stimulation provided by the intrepid experimenters, yourselves included, who have gone back to really fundamental experiments.''[61]

In a draft article sent to Purcell that would eventually be published by Brannen, W. H. Wehlau, and Ferguson, the authors reiterated that the HBT correlation was not expected by quantum theory because ''a single photon cannot be split!'' According to Brannen and co-workers, Purcell had interpreted the HBT results as ''an effect due to the interference between pairs of photons overlapping accidentally in time at the half silvered mirror.'' However, such an explanation could not be provided, according to them, because a low-intensity source had been used in the HBT experiment. Had Hanbury Brown and Twiss dealt

[60] Hanbury Brown, Boffin (ref. 2), 121.

[61] E. M. Purcell to E. Brannen, 17 Dec 1956, EMP (ref. 46).

with high-intensity light, photon overlap would have been plausible. ‘‘[S]ince one always has doubts about ‘reasonable assumptions’ when the elusive photon is concerned,’’ as highlighted by Brannen and colleagues, a rigorous theoretical approach was desired. They therefore ended the draft with the following question: ‘‘If a photon is detected at one place how does this affect its capabilities of producing interference with another ‘photon’ at other places [?].’’[62] It seemed to be far from straightforward to understand how the HBT results could be explained as interference between wavepackets overlapping when the experiment might deal with single photons. This was a paradox. The experiment conducted with low-intensity light—and therefore in the individual-photon regime—seemed to be explained successfully by a semiclassical approach, assuming the wave aspect of light. The HBT theoretical approach provided an explanation through a semiclassical model in a domain in which quantum theory should preside.

Answering Brannen's critics, Purcell claimed that ‘‘[p]ersonally I am not particularly fond of the explanation in terms of overlapping photons; it is both awkward to refine quantitatively and it is tricky unless one is very careful.’’ Rather, he was merely giving an interpretation based on it in response to Brannen's question.[63] In fact, in his 1956 article he did not interpret the HBT results by means of the photon-overlap model, examining the problem statistically in terms of the number of photons arriving in a certain time interval. However, Purcell mentioned photon overlap as an alternative interpretation for the HBT results as long as one interpreted photons as wavepackets.

Unlike Purcell, the physicist Richard M. Sillitto, from the University of Edinburgh, did explain the HBT results through the overlapping wavepackets model, assuming that each

[62] E. Brannen to E. M. Purcell, 27 Feb 1957, EMP (ref. 46).

[63] E. M. Purcell to E. Brannen, 1 Mar 1957, ibid.

separate wavepacket emitted by different atoms would be detected by a photocathode. Sillitto determined the mean square fluctuations in the number of photoelectrons counted in a time interval. His result was similar to Purcell's. Because of an interference in the probability amplitude (caused by wavepackets superimposing coherently), Hanbury Brown and Twiss observed what Sillitto called ''abnormal'' fluctuations. That is, when overlapping wavepackets are superimposed coherently, there would be a higher probability of the emission of a pair of electrons within the overlap time. Regarding concerns about the concept of the photon, Sillitto claimed that his explanation did not imply that interference between photons, viewed as particles at that time, would create four or no photons. That is, if a photon could interfere with another one, in the end, there would have been either two more photons, or zero photons, which disagreed completely with the laws of conservation. Because ''[t]he photon is not a particle,'' according to him, ''it does not survive a counting process unchanged, and it is detectable only through its interaction with matter.'' Sillitto continued, ''[w]hat does emerge from the argument above—and what can be understood in terms of this crude model and suffices to explain the experimental results—is that the interference between photons produces a distortion of the distribution time of the events by which photons are detected.''[64] Once more the concept of the photon appeared as a kind of obstacle to interpreting the HBT results. Sillitto's approach, instead of assuming photons as classical particles, represented photons as wavepackets. As a result, ''interference between photons'' did not mean ''interference between particles,'' but instead interference between the amplitudes of probability of overlapping wavepackets.

Another physicist who also participated in the discussions on the HBT results was Peter Fellgett from the Observatory at the University of Cambridge. Fellgett criticized the HBT ''semiclassical assumption,'' according to which the probability of the emission of a

[64] R. M. Sillitto, ''Correlation between Events in Photon Detectors,'' Nature 179 (1957): 1127–28, on 1127.

photoelectron was proportional to the magnitude of the electric vector, claiming that such an assumption could be made successfully for radio astronomy, but not in optics. The HBT theoretical approach, as highlighted by Fellgett, ‘‘apparently conflicts with arguments of a thermodynamic nature.’’ In order to show the limitation of the HBT semiclassical approach, Fellgett noted that a similar assumption would successfully describe the behavior of electrons, but not that of an assembly of bosons. He also argued that the electric field was not observable in optics, and that photons were not distinguishable particles (and therefore it was impossible to identify them between the source and the detector). Fellgett concluded: ‘‘semi-classical ideas, in fact, do not include the totality of our knowledge about the properties of radiation.’’[65]

Attempting to show the weakness of the HBT theory, Fellgett compared the HBT formula to another one, derived separately and previously by him and by the American physicist R. Clark Jones (from the foundations of thermodynamics) for the fluctuations in the number of photons absorbed by a body of emissivity E in an enclosure. As a result, Fellgett concluded that the Fellgett and Clark Jones equations would rely upon the emissivity of the body, whereas the HBT one was a function of the quantum efficiency of the photomultiplier. This result thus seemed to show that the HBT theoretical approach was not in agreement with thermodynamics.[66]

Moreover, Fellgett desired ‘‘a refined experimental method’’ to observe a true correlation between photons since the HBT ‘‘experiment ... belongs to the class in which an effect dependent on the ‘wave’ properties of light is observed in circumstances where the

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[65] P. Fellgett, ‘‘Question of Correlation between Photons in Coherent Beams of Light,’’ Nature 179 (1957): 956–57, on 956.

[66] Ibid., 957.

‘particle’ properties predominate.’’ The wave aspect of light would predominate, as mentioned by Fellgett, when there were many photons per unit volume. However, the particle aspect would come to the fore when a few photons occupied the same unit volume.[67]

Writing to Purcell after learning about Fellgett’s article, Twiss suggested that he had begun to write an answer back, and ‘‘[t]o prepare this I had to plough through a vast number of papers on the fluctuations in radiation fields and came away with the firm conviction that the theory is in a pretty unhealthy mess. .. . I feel that much of the trouble is caused by trying to use thermodynamics in the wrong way.’’[68]

Owing to the theoretical controversy between the Fellgett and Clark Jones equations and HBT, Clark Jones, who worked for the Polaroid Corporation in the U.S., decided to circulate a report among physicists. Entitled ‘‘On the Disagreement between Hanbury-Brown and Twiss, and Fellgett and Jones,’’ it discussed the principal disagreements between those derivations. Differing from Fellgett’s point of view, Clark Jones wrote, ‘‘I believe that Hanbury-Brown and Twiss is correct in stating that our results are not applicable to a phototube, and their results are the correct one.’’ By the time Fellgett and Clark Jones had separately calculated the fluctuations in a body of emissivity based on principles of thermodynamics, their formulas agreed with each other. The fundamental difference between these derivations, as stated by Clark Jones, was that the HBT equation was calculated for a radiation source of finite temperature and photomultipliers at zero absolute temperature, while he and Fellgett assumed that the detector was in thermal equilibrium with the system.

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[67] Ibid., 956.

[68] R. Q. Twiss to E. M. Purcell, 30 May 1957, EMP (ref. 46).

Taking into account these different experimental conditions, Clark Jones concluded that ‘‘both [equations are] correct in their respective field of application.’’[69]

The German-American physicist Leonard Mandel (1927–2001) also contributed significantly to the discussions on the HBT results, demonstrating that his theoretical approach to the problem agreed with the HBT and Purcell derivations, but disagreed with those of Fellgett and Clark Jones. However, Mandel’s most significant achievement was to show that the number of photons arriving at a certain time interval obeyed the pure Bose-Einstein distribution when the coherence time (a period of time over which the light beams were still coherent) was much smaller than the bandwidth of light. Representing photons as Gaussian random waves, Mandel determined the correlation between fluctuations in two beams as a function of the degeneracy of the beams defined as the number of photons occupying the same Bose cell. His analysis was similar to the statistical approach of Purcell. Mandel also highlighted that ‘‘the degeneracy is also indicative of whether the wave or the particle properties of the beam predominate.’’[70] That is, if there were two or more photons occupying the same cell in phase space, the wave properties would predominate. Nonetheless, if there were only one photon in a single cell, the particle properties would be observed.

‘‘Since the correlation depends essentially on two or more photons sharing cells in phase space,’’ as stated by Mandel, ‘‘it depends on the degeneracy... [that] varies with the intensity of the beams.’’ It seems that Mandel’s equation provided the connection between the ‘‘bunching’’ of photons and the HBT correlations. Thus, if two or more photons shared

[69] R. Clark Jones, ‘‘On the Disagreement between Hanbury-Brown and Twiss, and Fellgett and Jones,’’ in RHB, Box 18, Section E.61.

[70] L. Mandel, ‘‘Fluctuations of Photon Beams and Their Correlations,’’ Proceedings of the Physical Society of London 72 (1958): 1037–48, on 1041.

the same Bose cell, there would be a higher probability of detecting two photons at the same time at two different detectors. However, there would be no correlation as long as only a single photon was occupying a cell. As a result, the HBT correlation between photons did make sense. Mandel concluded that ‘‘[t]he correlation is therefore appreciable only when the wave properties, as distinct from the particle properties, of the beam become evident. This confirms the view of Hanbury Brown and Twiss ... that the effect should be regarded basically as a wave effect and shows it will be more difficult to detect in an experiment with light than with radio waves.’’ The HBT results could be considered as a ‘‘wave effect’’ because the degree of coherence, which describes how correlated waves are, provided information about the phase of the beams through the correlation measurements.[71]

Other physicists who also participated in the HBT theoretical debate were the Czech-American physicist Emil Wolf (born 1922) and Lajos Jánossy. Wolf demonstrated theoretically that there was a correlation, whose value was proportional to the square of the coherence function, between two arbitrary points in a stationary optical field.[72] In another article, Wolf also determined, from a classical wave theory point of view, that it was possible to measure the degree of polarization of a light beam by using the HBT results.[73] Jánossy, whose work with Ádám and Varga we have already mentioned, discussed the problem from a classical viewpoint as well.[74] While Wolf had claimed that his theoretical results agreed with the HBT results, Jánossy mentioned that it would be necessary to perform more experiments to observe the ‘‘effect’’ since the Brannen-Ferguson experiment had not detected any

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[71] Ibid., 1046.

[72] E. Wolf, ‘‘Intensity Fluctuations in Stationary Optical Fields,’’ Philosophical Magazine 2 (1956): 351–54.

[73] E. Wolf, ‘‘Correlation between Photons in Partially Polarized Light Beams,’’ Proceedings of the Physical Society of London 76 (1960): 424–26.

[74] L. Jánossy, ‘‘On the Classical Fluctuations of a Beam of Light,’’ Nuovo Cimento 6 (1957): 111–24.

significant correlation. Another physicist involved in the controversy was F. D. Kahn from the University of Manchester. Kahn's arguments will be discussed in the next section, as Hanbury Brown and Twiss made use of his arguments to defend themselves.

Because of the widespread criticism of the HBT experimental results, Hanbury Brown and Twiss decided to publish no more notes in the columns of Nature to argue in their favor, but a collection of four articles. The next section is dedicated to their more sophisticated experimental and theoretical arguments.[75]

**The End of the Controversy**

Responding to the criticism based on quantum theory, Hanbury Brown and Twiss revisited the most basic, yet controversial, concept from the Copenhagen interpretation of quantum theory: complementarity. Complementarity, first introduced in 1927 by the Danish physicist Niels Bohr (1885–1962) during the International Congress of Physics in Como, is the idea (in brief) that some concepts and pictures coming from classical physics, such as the wave and particle picture of light, are mutually exclusive concepts. For instance, if the wave aspect of light is observed in a specific experiment, the corpuscular one must be absent, even though those two concepts might be required for a complete description of a phenomenon. HBT's choice of complementarity to demarcate the frontiers between the wave and corpuscular

[75] These articles were published before the collection: R. Hanbury Brown and R. Twiss, ''Correlation between Photons in 2 Coherent Beams of Light,'' Nature 177 (1956b): 1046–48; R. Hanbury Brown and R. Twiss, ''Question of Correlation between Photons in Coherent Beams of Light,'' Nature 178 (1956c): 1447–48; R. Twiss, A. G. Little, and R. Hanbury Brown, ''Correlation between Photons, in Coherent Beams of Light, Detected by a Coincidence Counting Tech- nique,'' Nature 180 (1957): 324–26; R. Twiss and R. Hanbury Brown, ''Question of Correlation between Photons in a Coherent Light Rays,'' Nature 179 (1957): 1128–29.

aspects of light was drawn from their interaction with Rosenfeld, one of the most eloquent of Bohr's disciples.

Hanbury Brown and Twiss summarized Bohr's complementarity by writing that ''a particular experiment can exemplify the wave or the particle aspect of light but not both.'' As the HBT experiment exhibited the wave aspect of light (if the light detected at each one detector was arranged to interfere, an interference pattern would be observed), the particle aspect would not come to the fore in the same experimental arrangement. Consequently, the concept of the pho- ton was introduced only in the detection process, and not throughout the entire experiment. Accordingly, the wave or corpuscular aspect of light could be observed, depending on the experimental apparatus. If one used a linear multiplier to register the correlations between intensity fluctuations, the wave picture of light would be present. If, instead of using a linear multiplier, one used a coincidence counter able to detect individual events, the corpuscular picture would come to the fore.[76]

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[76] R. Hanbury Brown and R. Twiss, ''Interferometry of the Intensity Fluctuations in Light:

I. Basic Theory—The Correlation Between Photons in Coherent Beams of Radiation,'' Proceedings of the Royal Society of London Series A–Mathematical and Physical Sciences 242 (1957): 300–24, on 300–01; R. Hanbury Brown and R. Twiss, ''Interferometry of the Intensity Fluctuations in Light: II. An Experimental Test of the Theory for Partially Coherent Light,'' Proceedings of the Royal Society of London Series A–Mathematical and Physical Sciences 243 (1958): 291–319, on 291–92. Applications of the HBT interferometer to astronomy are in R. Hanbury Brown and R. Twiss, ''Interferometry of the Intensity Fluctuations in Light: III. Applications to Astronomy,'' Proceedings of the Royal Society of London Series A–Mathematical and Physical Sciences 248 (1958): 199–221; R. Hanbury Brown and R. Twiss, ''Interferometry of the Intensity Fluctuations in Light: IV. A Test of an Intensity Interferometer on Sirius A,'' Proceedings of the Royal Society of London Series A–Mathematical and Physical Sciences 248 (1958): 222–37.

Although Hanbury Brown and Twiss had chosen ‘‘an alternative approach’’ based on classical wave theory to interpret their experimental results, the phenomenon could also be understood through the corpuscular aspect of light, considering the bunching of photons interpretation, or modeling it from a statistical quantum analysis. It was F. Kahn who explained the HBT results through another concept of the Copenhagen doctrine—the uncertainty principle—which was proposed in 1927 by the German physicist Werner Heisenberg (1901–76).[77] In Heisenberg’s principle, a precise measurement of the position of a particle, for instance, causes indeterminacy in its momentum, and vice versa. Using the uncertainty principle to explain the HBT results, Kahn asserted that ‘‘[i]n a quantum picture it is the remaining uncertainty of the number of photons in each wave-train which makes the experiment work. The uncertainty is large in the radio experiment and small in the light experiment, with converse effects on the uncertainty in phase.’’ That is, the uncertainty relation between the number of photons arriving at a receiver and the phase of the wave-train would be $\Delta n \Delta \phi \approx 1$.[78] In the HBT experiment, if the information about the phase were accurately obtained, there would be indeterminacy in the number of photons in the system.

After determining the rate of detection of photons for the HBT instrument, Kahn concluded that his theoretical model based on principles of quantum statistics agreed with the HBT semiclassical results and disagreed with the Fellgett results. Moreover, assuming that the electric fields at the two receivers were completely correlated, Kahn demonstrated that the probability of the coincidence of arrival of a pair of photons would depend on the visibility of

[77] Hanbury Brown and Twiss, ‘‘Fluctuations in Light: I. Basic Theory’’ (ref. 75), 302.

[78] F. D. Kahn, ‘‘On Photon Coincidences and Hanbury Brown’s Interferometer,’’ Optica Acta: International Journal of Optics 5, nos. 3–4 (1958): 93–100, on 94.

the interference fringes as long as the light from those receivers were arranged to interfere in the HBT experiment.[79]

In an attempt to show the potential of the classical wave theory to explain the HBT experiment, Hanbury Brown and Twiss discussed a Gedankenexperiment described in Max Born's 1945 textbook Atomic Physics.[80] In a diffraction grating experiment, one assumes that low-intensity light reaches a grating with two parallel slits, and that the light, which passes through the slits, will be observed on a screen. Although the source light is extremely weak, so that only individual photons reach the grating, ''the experiment still illustrates the wave aspect of light, since the particle aspect can only really be brought out by observations in which the position of a single quantum is measured at two successive instants of time.'' Yet if such a position is measured in the experiment, the fringes of interference will disappear, and the corpuscular aspect of light would come to the fore. In other words, the appearance of the interference pattern would be a consequence of Heisenberg's uncertainty principle. Likewise, Hanbury Brown and Twiss argued that if one substituted the concepts of momentum and position of a photon from a diffraction experiment to those of energy and time, there would arise an ''interference pattern in time, the beat phenomenon of the correlation experiment,'' because of the uncertainty in the energy of the photons. In the HBT experiment, the interference pattern could also be destroyed, as described by Hanbury Brown and Twiss, by using a highly monochromatic source of light or by inserting a prism in the apparatus. As a result, it would be possible to measure the energy of incident photons accurately.[81]

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[79] Ibid., 95.

[80] M. Born, Atomic Physics (London/Glasgow: Blackie & Son Limited, 1945).

[81] However, as we know from quantum optics developments after the HBT controversy, the disappearance of correlation would instead require sources producing one-photon states.

Hanbury Brown and Twiss ended by claiming that ‘‘since the radiation field can be treated classically in the case of the diffraction grating, it is only to be expected that it can be treated classically in analyzing the correlation experiment.’’ However, if one insisted upon considering the particle aspect of light, the beat frequency (interference) in the HBT experiment would be ‘‘caused by the uncertainty in the energies of the individual photons which may be associated with either of the two Fourier components of the radiation field.’’[82] They continued by arguing that ‘‘[w]hen interpreting interference phenomena according to the corpuscular theory of radiation, 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. This point was originally made for the case of spatial interference, as in an interferometer, but the arguments on which it is based are equally valid for temporal interference as in the phenomenon of a beat frequency.’’[83]

In a more sophisticated theoretical model for the HBT experiment, Han- bury Brown and Twiss again used ‘‘a purely classical theory’’ to determine the mean square fluctuations in the emission current of a single phototube, and obtained an equation comprising two terms called the shot noise and the wave interaction noise. On the one hand, the shot noise term was interpreted as a consequence of the discrete nature of the electrons in the detection process, and hence it did not rely upon the quantization of the radiation field at all. The wave interaction noise, on the other hand, might be interpreted through the classical theory as due to the beats (interference effect) of the different Fourier components of the radiation field. However, as long as the corpuscular nature of light predominated, the wave interaction noise

[82] Hanbury Brown and Twiss, ‘‘Fluctuations in Light: I. Basic Theory’’ (ref. 75), 308.

[83] Ibid.

could be interpreted as the excess photon noise caused by the bunching of photons (as a consequence of Bose-Einstein statistics).[84]83

Hanbury Brown and Twiss also improved their apparatus and carried out more experiments, again observing the HBT effect successfully.[85] Previously, they had compared ‘‘the theoretical performance of three equipments,’’ considering the experimental parameters of each experiment, and found the time necessary to observe ‘‘a significant correlation’’ to be a thousand years for the Brannen and Ferguson experiment and $10^{11}$ years for the experiment of Ádám and co-workers, while the HBT experiment came off impressively at ten minutes.[86] Hanbury Brown and Twiss also applied their interferometer to optical astronomy, successfully obtaining the angular diameter of the bright star Sirius when compared to available measurements.[87] Such a successful application in astronomy helped consolidate recognition of the HBT effect in the field of optics.

Other evidence corroborating the HBT results was obtained by R. V. Pound and his student G. A. Rebka at the Lyman Laboratory of Physics at Harvard University in 1957. These researchers reported the validity of the HBT experimental results by observing the same correlation between photons.[88] Knowing previously through Purcell that Rebka and

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[84] Ibid., 321.

[85] Hanbury Brown and R. Twiss, ‘‘Fluctuations in Light: II. Experimental Test’’ (ref. 75), 291 and 308.

[86] Hanbury Brown and Twiss, ‘‘Question of Correlation between Photons in Coherent Beams of Light’’ (ref. 74), 1448.

[87] Hanbury Brown and Twiss, ‘‘Correlation between Photons in 2 Coherent Beams of Light’’ (ref. 74); Hanbury Brown and Twiss, ‘‘Question of Correlation between Photons in Coherent Beams of Light’’ (ref. 74).

[88] G. A. Rebka and R. V. Pound, ‘‘Time-Correlated Photons,’’ Nature 180 (1957): 1035–36; Rebka and Pound would perform an important experiment to test Einstein’s general theory of relativity in 1959. See Kragh, Quantum Generations (ref. 1), 362–63.

Pound would carry out a ‘‘local version of the experiment,’’ Twiss expressed how a positive result could aid in the funding of the construction of HBT interferometers: ‘‘I hope that Rebka’s experiment is soon successful and I should be very interested to hear how he is getting on. We pressed ahead with ours to buttress our claim for money to build our large mirrors and any independent check that all our results were not obtained by fudging would doubtless carry weight with the cash disbursers.’’[89]

In 1958, Brannen and colleagues actually observed a positive correlation be- tween photons, thus confirming the HBT results as well and contradicting their earlier experimental results.[90] In private correspondence, Brannen recognized the HBT results as a physical phenomenon, noting that ‘‘[i]t seems to be well established now that our initial criticisms were unfounded. We [Brannen and Ferguson] thought (rashly it seems) that you were considering the ‘splitting of individual photons’, if you will pardon the phraseology, and our initial experiments were designed to test such a conjecture, which everyone would agree was contrary to quantum mechanics .. . . [A]s your calculations showed, our first experiments could not show such an effect whereas yours could.’’[91] The experimental controversy between the Brannen and Ferguson experiment and the HBT experiment thus ended in 1959. At the same time, the theoretical dispute between Fellgett and Hanbury Brown and Twiss also came to an end. Circulating a new report ‘‘The Resolution of the Controversy among Hanbury-Brown and Twiss, and Fellgett and Jones,’’ Clark Jones confirmed the validity of

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[89] E.M. Purcell to R. Q. Twiss, 10 May 1957, EMP (ref. 46).

[90] E. Brannen, H. I. S. Ferguson, and W. Wehlau, ‘‘Photon Correlation in Coherent Light Beams,’’ Canadian Journal Physics 36 (1958): 871–74.

[91] E. Brannen to R. Hanbury Brown, 22 May 1959, RHB, Box 18, Section E.61.

the HBT fluctuations theory.[92] Moreover, Fellgett, Clark Jones, and Twiss also published an article, ‘‘Fluctuations in Photon Streams,’’ in which they solved the theoretical controversy by displaying that ‘‘a number of objections to the thermodynamics approach originally put forward by Hanbury Brown and Twiss are invalid,’’ and that both calculations were correct since they had been derived from different experimental conditions.[93]

Reflecting on the controversy in 1991, Hanbury Brown noted that ‘‘we had quite a hard job persuading people that to talk about the behavior of a beam of light as though it is a stream of independent photons which preserve their individual identities from emission to absorption is a gross misuse of the concept of a photon and gives the wrong answer.’’ Rather, ‘‘[w]e had to persuade our opponents, many of whom were surprisingly irate, that there is no satisfactory mental picture of light which gives the right answer to this particular problem and that the only way of getting the right answer was to do mathematics.’’ Although Hanbury Brown and Twiss had had to face the severe criticism and some funding issues to support the construction of the stellar interferometer, ‘‘[a]ll this controversy taught many physicists something new about the nature of light.’’[94] We can note that after solving the debate surrounding the HBT laboratory experiment, Hanbury Brown and Twiss finally constructed their Narrabri Stellar Interferometer located 370 miles north of Sydney in Australia.

## Some concluding remarks

[92] R. Clark Jones, ‘‘The Resolution of the Controversy among Hanbury-Brown and Twiss, and Fellgett and Jones,’’ RBH, Box 18, Section E.61.

[93] P. Fellgett, R. Clark Jones, and R. Twiss, ‘‘Fluctuations in Photon Streams,’’ Nature 184 (1959): 967–69, on 968.

[94] Hanbury Brown, Boffin (ref. 2), 121–23.

The controversy surrounding the HBT results contributed to a debate on the concept of the photon in the late 1950s, mobilizing twelve theoretical and experimental physicists around the world from different institutions and back- grounds. It was a controversy that taught many physicists about the boundary between theoretical and experimental physics, and in particular, about the nature of light. The HBT controversy arose in the community of physics as a result of the way some physicists understood the concept of the photon from old quantum theory. As highlighted by Richard Sillitto, ‘‘[i]t is one of the interesting features of [the HBT] result that it cannot be understood in terms of the crude—too crude!—model of a beam of light as a stream of discrete, indivisible, corpuscular photon.’’[95] Thus, the HBT results seemed to contradict the predictions of old quantum theory, which assumed a corpuscular picture of the photon. The HBT debate revealed how some physicists still viewed photons as classical particles, even after the development of the Bose- Einstein statistics. As discussed previously, the HBT experiment was explained through two different pictures: the wave aspect of light (according to which the radiation is a classical electromagnetic wave and the matter is quantized); or, the corpuscular aspect of light (in which the HBT results are explained through ‘‘photon bunching’’ as a consequence of the behavior of the bosons).

Nowadays, the HBT correlation is known as the HBT effect (as it was called initially by Purcell in 1956): any correlation between intensity fluctuations in two photomultipliers when using a thermal, or chaotic, source. With the benefit of hindsight, it is easy to understand why the HBT experiment was able to detect such a correlation: HBT’s work did not deal with single photons because of the characteristic of the source used in their experiment. An anti- correlation between photons (or ‘‘photon anti-bunching’’) would be detected in the 1970s after the development of the laser and quantum optics. The HBT effect

[95] R. M. Sillitto, ‘‘Light Waves, Radio Waves, and Photons,’’ The Institute of Physics— Bulletin 11, no. 5 (1960): 129–34, on 131.

became such a landmark in physics that physicists and textbooks came to describe it as heralding the birth of quantum optics. Even when Hanbury Brown and Twiss were awarded the Albert Michelson Medal in 1982, their contribution to physics was summarized with the words: ‘‘it is safe to say that much of the early history of quantum optics has its roots in the Hanbury Brown–Twiss effect and that this phenomenon can rightly be viewed as a cornerstone of modern optical science.’’[96]

In fact, as asserted by the American physicist Roy Glauber, who was awarded the Nobel Prize in 2005 for the development of the coherent states of the electromagnetic field, the HBT effect indirectly inspired his own work. In his autobiography, Glauber remarked:

> [T]he late 50’s proved to be an exciting time for many reasons. A radically new light source, the laser, was being developed and there were questions in the air regarding the quantum structure of its output. That was particularly so in view of the surprising discovery of quantum correlations in ordinary light by Hanbury Brown and Twiss. ... That was the period in which I began to work on quantum optics with a surmise that the Hanbury Brown- Twiss correlation would be found absent from a stable laser beam, and then followed it with a sequence of more general papers on photon statistics and the meaning of coherence.[97]

[96] Hanbury Brown, Boffin (ref. 2), 120.

[97] R. Glauber, ‘‘Autobiography,’’ Nobelprize.org, at http://www.nobelprize.org/nobel_prizes/physics/laureates/2005/glauber-autobio.html (accessed 8 Aug 2012). Glauber’s quantum explanation of the HBT results will be examined in a following paper, together with the concept of the photon that emerged with quantum optics.

In his 1963 article, Glauber explained the HBT results by the quantization of the electromagnetic field in optics.[98] A question arises: Which concept of the photon emerged from quantum optics? Was such a discussion on the agenda of physicists at the time? If not, why not? Were physicists pragmatists in the sense of using only mathematical arguments, instead of expending philosophical energy in an effort to understand or interpret what the photon was? These questions merit further investigation.

The HBT episode illustrates how long it took for the concept of the photon to coalesce to what we take for granted today—unsettled in 1905, 1916, and 1927; unsettled even in the 1950s, fifty years after its introduction. The HBT experiment showed that physicists could not interpret photons by means of the ‘‘too crude corpuscular model’’ when a thermal source was used. Using laser and certain quantum states of light as sources, however, physicists actually observed in the 1970s and 1980s an anti-correlation between photons. Photons, now

[98] While it is beyond the scope of this paper to analyze Glauber’s work, his use of quantum field methods to deal with light coherence should be noted (Brown and Pike, ‘‘Optical and Optoelectronic Physics’’ [ref. 5], on 1439–41). According to Bromberg, Glauber ‘‘had worked in quantum field theory and nuclear physics.’’ Even though at the time he ‘‘had not himself being doing research on optical coherence, ... he had been in touch with it because some of the theoretical and experimental work was being done in the Harvard physics department.’’ And yet, he asked himself, ‘‘how can Hanbury Brown and Twiss’s results, and the completely coherent character of laser light, be embodied in a fully quantum-mechanical theory?’’ In Joan Bromberg, The Laser in America: 1950–1970 (Cambridge, MA: MIT Press, 1991), on 109. In contradistinction, the physicists involved in the debate on the HBT experiment did not mobilize such resources. It deserves to be investigated how much of this choice was related to their training and how much was due to the fact that they explained HBT results without looking for a general treatment of light from which to derive the HBT results.

mathematically represented by number states, came back to physics. The presentation and discussion of such experiments, however, is a matter for future studies.[99]

The success of the semiclassical approach also contributed to physicists' not considering the photon concept, or the quantization of radiation, before the detection process. The HBT argument, for instance, was sufficient to explain the experimental results. Moreover, looking back to the early development of quantum theory, semiclassical models were used separately by Erwin Schrödinger to examine the Compton effect and by Guido Beck and by Gregory Wentzel to discuss the photoelectric effect.[100] Even in the late 1960s, the physicists Willis Lamb and Marlan Scully revisited the semiclassical approach and analyzed the ''Photoelectron Effect without Photons.''[101] Nevertheless, it seemed that it was only with the development of a full quantum theory of light and later experiments that a

[99] John F. Clauser, ''Experimental Distinction Between the Quantum and Classical Field Theoretical Predictions for the Photoelectric Effect,'' Physical Review 9 (1974): 853–60; H. J. Kimble, M. Dagenais, and L. Mandel, ''Photon Antibunching in Resonance Fluorescence,'' Physical Review Letters 39 (1977): 691–95; C. K. Hong and Leonard Mandel, ''Experimental Realization of a Localized One-photon State,'' Physical Review Letters 56 (1986): 58–60; Philippe Grangier, G. Roger, and Alain Aspect, ''Experimental Evidence for a Photon Anticorrelation Effect on a Beam Splitter: A New Light on Single-Photon Interference,'' Europhysics Letters 1, no. 4 (1986): 173–79.

[100] See E. Schrödinger, ''U¨ ber den Comptoneffect,'' Annalen der Physik 82, no. 2 (1927): 257–64; G. Beck, ''Zur Theorie des Photoeffekts,'' Zeitschrift fu¨r Physik 41, no. 10 (1927): 443–52; G. Wentzel, ''Zur Theorie des photoelektrischen Effekts,'' Zeitschrift fu¨r Physik 40, no. 8 (1927): 574–89. Those approaches were revisited in the 1980s by J. Strnad, ''The Compton Effect: Schrödinger's Treatment,'' European Journal of Physics 7 (1986): 217–21; and J. N. Dodd, ''Compton Effect: A Classical Treatment,'' European Journal of Physics 4 (1983): 205–11.

[101] W. E. Lamb and M. O. Scully, ''The Photoelectric Effect without Photons,'' Polarisation Matière et Rayonnement, Jubilee Volume in Honor of Alfred Kastler (Paris: Presses Universitaires de France, 1969), 363-69.

sophisticated concept of the photon came to the fore, and hence semiclassical models no longer provided a completely satisfactory description of light.

Although the concept of the photon seemed to have been fixed after the early development of quantum theory, the HBT debate helped physicists to revisit and reinterpret, or understand, the concept of the photon. The HBT experiment also illustrates how the performance of experiments has contributed to interpreting the foundations of quantum theory.

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# A New Light on the Concept of the Photon:

# Beyond the 'Fuzzy-ball' Picture

## Introduction

The concept of the photon would never be the same after the experimental and theoretical developments of the second half of the twentieth century. The first experiment to challenge the standard concept of the photon was carried out in the 1950s by the astronomer and physicist Robert Hanbury Brown (1916-2002) and the mathematician Richard Quentin Twiss (1920-2005) while they were designing an intensity interferometer to determine the angular diameter of visible stars. In the Hanbury Brown-Twiss (HBT) experimental set up, a low intensity light source was split by a half-silvered mirror into two components and then these components were detected separately through two photomultipliers. Owing to the kind of the source used, one would expect only single-photons to arrive at the mirror. As a result, *no* systematic coincidences would be observed by considering the standard concept of the photon developed in the old quantum theory – a small, invisible and localized particle. Hanbury Brown and Twiss observed the contrary, however. They detected a significant correlation between photons after being split by the mirror. In other words, two photons were detected at the same time at the two detectors.[1] How could one then conciliate the HBT results with the canonical concept of the photon? As remarked later by Hanbury Brown, "if you insist on thinking of light as a stream of independent particle like ping pong balls, which is what most physicists… prefer to do, then it is impossible

[1] See the previous chapter: I. Silva and O. Freire Jr., "The Concept of the Photon in Question: The Controversy Surrounding the HBT Effect circa 1956-1958," Historical Studies in the Natural Sciences 43, no. 4 (2013): 453-91.

to see how the arrival times of these particles can be correlated."[2] The controversy surrounding the HBT experimental results, which seemed to go against the prediction of quantum theory, revealed the way in which physicists interpreted the concept of the photon at the time, revisiting a fifty-one year old concept.

The other development that also provided new conceptual insights on the concept of the photon, the quantum theory of light proposed in 1963 by Roy J. Glauber (1925- ), is the focus of this historical analysis. Glauber applied quantum field theory methods to optics to explain what it would happen if the new light source – the laser – was used in a HBT-kind experiment. This was the first step towards constructing his sophisticated quantum theory of coherence. It is striking why quantum electrodynamics (QED), thus quantum field methods, which study the interaction between charged particles and photons, had not been applied to the optical domain before. An insight on this delay can be gained from regarding Glauber's theoretical physics training during his doctoral studies under one of the "men who made it [QED]" Julian Schwinger (1918-1994).[3] Other physicists, such as Emil Wolf (1922- ) and Leonard Mandel (1927-2001), who were also involved in discussing the problem of coherence had backgrounds in either classical optics or in cosmic-ray physics, respectively. The other reason why QED methods were applied in optics only in the early 1960s seems to be related to the establishment of the field in the physics community as well highlighted by the physicist and historian of science Silvan S. Schweber:

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[2] Robert Hanbury Brown, *Boffin: A Personal Story of Early Days of Radar: Radioastronomy and Quantum Optics* (New York: Taylor & Francis Group, 1991), on 121.

[3] Silvan S. Schweber, *QED and Men Who Made it: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton, NJ: Princeton University Press, 1994).

> Although the fruitfulness of quantum field theory had been recognized during the thirties – the successes of quantum electrodynamics (QED), Fermi's theory of beta decay, Yukawa's meson theoretic explanation of the nuclear forces, Pauli's proof of the connection between spin and statistics all attested to the power of this form of theorizing – the inability of these theories to make quantitative predictions in agreement with experimental data – except for quantum electrodynamics in the lowest order perturbation theory – and their universal failure in higher orders perturbation due to the divergence difficulties had raised serious doubt about the validity of the approach. The success of renormalization theory in quantum electrodynamics, and its extension to other field theories, made plausible the assertion that quantum field theory was the natural framework for the synthesis of the quantum theory and the theory of special relativity."[4]

The renormalization methods were developed between 1946 and 1951, through the contributions of Freeman Dyson (1923- ), Richard Feynman (1918-1988), Julian Schwinger (1918-1994), and Sin-Itino Tomonaga (1906-1979), as a result of "the divergence difficulties of quantum electrodynamics." Renormalization might be described as a technical procedure to avoid infinite results regarding perturbative calculations in quantum field theory, which contributed therefore to "clarify the conceptual basis of quantum field theory and to establish its consistency." In the early 1960s, QED was what Thomas Kuhn would call "normal science," and was beginning to be applied to other fields as in the case of optics with Glauber. [5]

[4] Schweber, *QED and Men Who Made it* (ref. 3), *xi*.

[5] Ibid., 595. See also Thomas Kuhn, *The structure of scientific revolutions* (Chicago, IL: University of Chicago Press, 1996).

Besides Glauber's training in QED which definitely helped in his theoretical achievements related to the quantum theory of light, the advent of a new light source – the LASER (for Light Amplification by Stimulated Emission of Radiation) – also opened paved the way for new developments. As is well-known, the research which culminated in the creation of the laser benefited from military expenditure after the World War II. The historian of science Joan Lisa Bromberg explores the relationship among scientific research, military and industry applications in the laser era in the US between 1950 and 1970, discussing the role played in the idealization of the laser by Charles Townes of Columbia University and his collaborator and brother-in-law Arthur L. Schawlow at Bell Telephone Laboratories, and by Gordon Gould, a graduate student at Colombia University. Gould had previously discussed the issue with Townes and was working for Inc. TRG (an American company which used military expenditures to support weapons technology research). The idea for developing the laser device was to put atoms into a narrow cavity with mirrors at each extremity (only one would be partly silvered), called the Fabry-Péron etalon, and hence the reflected rays would stimulate further radiation – atoms to radiate, producing thus the laser beam. Even though the idea of an optical maser, an acronym for Microwave Amplification by Stimulation Emission of Radiation, was born between 1957 and 1958, it was only in 1960 that the first pink ruby laser came into operation, constructed by Theodore H. Maiman at Hughes Laboratories.[6] The main features of the laser are its high coherence and monochromaticity. As soon as the laser was operating, Glauber asked the question: "What was a laser beam?" He felt alone in wondering, as Glauber would later recall, "[i]t's rather strange there was never a discussion [about] what a light beam is. And, no attention

[6] Joan Lisa Bromberg, *The Laser in America (1950-1970)* (Cambridge: The MIT Press, 1993). See also Mario Bertolotti, *Masers and Lasers: An Historical Approach* (Bristol: Adam Hilger Ltd., 1983).

was paid to the question, absolutely not." Describing the laser beam through the coherent states and explaining quantum-mechanically the HBT results, Glauber's work contributed significantly to the renaissance of optics which had been "stagnated for two decades," and to shed a new light on the concept of the photon.[7] It seems that both his scientific background working with QED methods and the end of doubt regarding the foundations of quantum electrodynamics with renormalization theory helped Glauber to be the one to propose the quantum theory of light.[8] As is well discussed by Forman in the case of the "Inventing the Maser in Postwar America" by Townes, "the historical reconstruction of the route taken by an individual scientist to a conceptual innovation or experimental discovery has always been based on the ascription to the scientist in question of peculiar, possibly unique, educational background, research experiences, and intellectual orientations."[9] Concerning Glauber and the quantum theory of coherence, these idiosyncrasies might also have played an important role.

Although Glauber's quantum theory of light became a landmark in the development of quantum optics and is one of the events which culminated in what Bromberg called "mid-

[7] Helge Kragh, *Quantum Generations: A History of Physics in the Twentieth Century* (Princeton, NJ: Princeton University Press, 1999), on 390.

[8] In Bromberg's talk paper, she asked some questions related to that issue: "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?" I would say yes as briefly discussed before, but further investigation on those questions is necessary. In: Joan Lisa Bromberg, "Modelling the Hanbury Brown – Twiss Effect: The Mid-Twentieth Century Revolution in Optics," available in http://quantum-history.mpiwg-berlin.mpg.de/news/workshops/hq3/hq3_talks/22_bromberg.pdf. Accessed on 18 Aug 2013.

[9] Paul Forman, "Inventing the Maser in Postwar America," Osiris 7 (1992): 105–34, on 127.

twentieth century revolution in optics," this chapter focuses on analyzing the contributions of his conceptual insights to discussions about the concept of the photon, and not on the creation of such a new field.[10] The chapter is organized as follows. The first section is dedicated to presenting Glauber's 1963 theoretical achievements on defining coherence in terms of the quantum theory. In section two, I briefly discuss the controversy surrounding the necessity, or otherwise, of quantizing the electromagnetic field in optics between 1963 and 1964. The third section focuses on the concept of the photon which emerged from Glauber's conceptual framework. Finally, there is an epilogue in which I discuss the experimental achievements of the 1970s and 1980s that demonstrated the corpuscular nature of light. Unfortunately, I did not have access to primary source materials regarding Roy J. Glauber, Emil Wolf and Leonard Mandel, limiting this chapter. However, the use of original papers, interviews and secondary literature contributed significantly to tracing the story of the concept of the photon after the 1960s.

**Quantum Electrodynamics in Optics**

The American theoretical physicist Roy J. Glauber (1925- ) received the Nobel Prize in Physics 2005 for his contributions to the development of the quantum theory of coherence.[11] Currently, he is the Mallinckrodt Professor of Physics at Harvard University and the Adjunct

[10] Bromberg, *Modeling the HBT Effect* (ref. 7), 4. Bromberg also highlighted the importance of analyzing Glauber's contributions to quantum optics, which will be our focus elsewhere. In: Joan L. Bromberg, "Device Physics vis-à-vis Fundamental Physics in Cold War America: The Case of Quantum Optics," Isis 97, no. 4 (2006): 237-59.

[11] The Nobel Prize 2005 was shared; and the other half was awarded jointly to John L. Hall and Theodor W. Hänsch. See, "The Nobel Prize in Physics 2005," available at http://www.nobelprize.org/nobel_prizes/physics/laureates/2005/.

Professor of Optical Sciences at the University of Arizona. Glauber commenced his studies at Harvard University in the spring of 1941, the same year in which the Japanese government attacked on the U.S. Naval Base at Pearl Harbor in Hawaii. Because of the war, there was considerable change in institutional dynamics in the US and in students' academic lives.[12] Looking back to that time, Glauber recalled how activities at Harvard had changed during the war:

> The entire school then began operating during the summer and accelerating its course programs with the thought of providing as much education as possible before the young men left for the armed forces. In the meantime Harvard's dining halls lost their graciousness and were transformed into the cafeteria-style mess halls they have been ever since. The draft age, then 21, was presently lowered to 18 and the university began losing students in large numbers […] It was with the war thus cracking the whip that I managed to assimilate most of the courses of a graduate school education by the time I turned 18 in September 1943.[13]

After attending some graduate courses, Glauber felt ready to start doing war work and sent an application to the National Roster of Scientific Personnel which was giving "scientific training and try[ing] to place people accordingly." That same year, Glauber was called to be part of a secret project related to the creation of the atomic bomb, the Manhattan Project.[14] On

[12] About how the US institutions changed as result of the Second World War, see Charles Dorn, *American Education, Democracy, and the Second World War* (New York: Palgrave Macmillan, 2007).

[13] Glauber, *Autobiography* (ref. 11).

[14] About The Manhattan Project, consult: Richard Rhodes, *The making of the atomic bomb* (New York: Simon & Schuster, 1986); Cynthia C. Kelly, *The Manhattan project: the birth of the atomic bomb in the words of its creators,*

discovering the principal aim of the project immediately after arriving in Chicago, Glauber initially thought of declining to be part of it. However, he reconsidered and accepted the challenge by thinking about the possibility of the same nuclear weapon being constructed by the German government. Glauber thus joined the Theoretical Division of the Manhattan Project, as one of the youngest researchers, and solved problems associated with neutron diffusion. Still in Los Alamos, Glauber met Julian Schwinger who would later become his adviser at Harvard. "The principal reason for my remaining at Harvard", as recounted later by Glauber, "was the addition of Julian Schwinger to the faculty," with whom Glauber was "immediately so impressed with his knowledge and his incredibly informative lecturing style that I felt he was unique among teachers and would be the ideal thesis advisor as well." [15] Schwinger is the Nobel prize-winning American theoretical physicist widely known for his work on renormalization methods.[16] Upon returning to Harvard University, Glauber received his doctorate degree in 1949 working on quantum field theory. After his Ph.D studies, he was invited by Robert Oppenheimer to work at the Institute for Advanced Study in Princeton and then went to Zürich to work in Wolfgang Pauli's research group. Glauber was hired as a temporary professor at Caltech to substitute Richard Feynman who had gone to spend a year in Brazil. At the time, he was interested in research on neutron diffraction by molecules. In 1952 Glauber was invited to be part of the

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*eyewitnesses, and historians* (New York: Black Dog & Leventhal Publishers, 2007); Cynthia C. Kelly, *Remembering the Manhattan Project perspectives on the making of the atomic bomb and its legacy* (New Jork: World Scientific, 2004).

[15] Glauber, *Autobiography* (ref. 11).

[16] Schweber, *QED and Men Who Made It* (ref. 3).

Physics Department at Harvard University, and there continued his research on nuclear physics.[17] He would change his research program only in the 1960s.

During one of the regular meetings among professors and students at Harvard, Glauber attended a talk given by his colleague Nobel laureate Edward M. Purcell (1912-1997), also a Harvard University Professor in Physics, about photon temporal correlations observed in 1956 by Hanbury Brown and Twiss. Explaining the reason why a correlation was detected in the HBT experiment, Purcell considered a semi-classical model for the electromagnetic field and the formula for the relaxation time of radiofrequency noise obtained during wartime. At the end of the talk, Glauber was convinced that Purcell's theoretical approach was evidently consistent and hence there might no longer be any questions left unanswered. This would change with the development of the laser beam, however.[18] Influenced by the laser physicist Saul Bergmann, who worked at Research Department at American Optical Company, Glauber turned his attention to the issue of coherence. According to Joan Bromberg, "Bergmann wanted to understand the relationship between the work of Hanbury Brown and Twiss and the properties of laser light, and he invited Glauber to help in this project as a consultant to the company."[19] In his first article on "Photon Correlations," Glauber acknowledged Bergmann for having pointed out the problem and for the financial support given by the American Optical Company. Despite the fact that he was always interested in nuclear physics and quantum field theory, Glauber was familiar with the problem of coherence as a consequence of the theoretical and experimental research which was

[17] Glauber, *Autobiography* (ref. 11).

[18] Interview of Roy J. Glauber by Indianara Silva on 25 Jun 2012, Lyman Laboratory, Department of Physics, Harvard University, Cambridge, MA.

[19] Bromberg, *The Laser in America* (ref. 5), 109.

being discussed and performed at Harvard. Endeavoring to solve the problem given by Bergmann, Glauber asked himself the question: "How can Hanbury Brown and Twiss' results, and the completely coherent character of laser light, be embodied in a fully quantum-mechanical theory?"[20] The answer to this question could be achieved, according to Glauber, by "find[ing] a way of describing the light beams and what their structure might be." Through the idea of coherent states of the harmonic oscillator proposed by Erwin Schrödinger (1887-1961), Glauber envisioned the possibility of constructing a fully quantum description of light based on the coherent states. Schrodinger's states "never had any practical value," as emphasized by Glauber, and were only used as an illustration to "show the complementarity of momentum and position." Although such states had already been developed in a mathematical structure sense since 1926 with Schrodinger's theoretical formulation, "nobody paid attention to at all was the fact that these [states] are eigenstates of the annihilation operator" of photons in the electromagnetic field.[21] This insight led Glauber to the quantization of the electromagnetic field in optics, shedding new light on the meaning of coherence.

Until the early 1960s, the fields of optics and quantum field theory developed without considerable connection between them. Glauber claimed that "for reasons which are partly mathematical and partly, perhaps, an accident of history, very little attention of the insight of quantum electrodynamics has been brought to bear on the problems of optics." Although the classical or semi-classical studies of photon behavior could be "informative," there was the

[20] Ibid.

[21] Glauber's interview (ref. 16).

possibility of “overlooking quantum phenomena which have no classical analogs.”[22] The reason for this could lie, according to Glauber, in the fact that “optical experiments to date have paid very little attention to individual photons,” and as those experiments dealt with ordinary light intensities, “it is not surprising that classical theory has offered simple and essentially correct insights.” That is, if the system were dealing with a chaotic source, classical optics would be able to explain any experimental results. However, if the system were dealing with a single photon at a period of time, this would require an explanation based on quantum theory. Therefore, there would be no way of constructing a theoretical approach for the single photon experiments, other thanincluding QED into optics. “Observ[ing] that the quantum theory is fundamentally necessary to the treatment of these problems is not,” as highlighted by Glauber, “to say that the semi-classical approach always yields incorrect results,” but rather that “[t]here is ultimately no substitute for the quantum theory in describing quanta”.[23] He was completely convinced of the need to embrace elements of quantum theory into optics.

After analyzing the photon correlations in the HBT experiment, Glauber concluded that “if they [coherent states] were used systematically to describe the laser beam […] there would be no Hanbury Brown and Twiss effect whatsoever.” After making such a prediction, he then decided to publish it. Before even sending the paper to the Physical Review Letters (PRL), the fastest way to publish, he decided to phone the Editor of the Journal and inquire if the journal

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[22] Roy J. Glauber, "Coherent and Incoherent States of Radiation Field," *Physical Review* 131, no. 6 (1963): 2766-88, on 2766.

[23] Roy J. Glauber, "The Quantum Theory of Optical Coherence," *Physical Review* 130, no. 6 (1963): 2529-39, on 2529; Roy J. Glauber, "Photon Correlations," *Physical Review Letters* 10, no. 3 (1963): 84-86, on 85.

would be able to publish "such a thing;" since the PRL rarely published work on optics or quantum theory. The Editor asked Glauber to read the first paragraph of his paper and so he did:

> In 1956 Hanbury Brown and Twiss' reported that the photons of a light beam of narrow spectral width have a tendency to arrive in correlated pairs. We have developed general quantum mechanical methods for the investigation of such correlation effects and shall present here results for the distribution of the number of photons counted in an incoherent beam. The fact that photon correlations are enhanced by narrowing the spectral bandwidth has led to a prediction of large-scale correlations to be observed in the beam of an optical maser. We shall indicate that this prediction is misleading and follows from an inappropriate model of the maser beam."[24]

The Editor of Nature then said: "[L]ook I would never publish that… I could sense in this first paragraph that you are critical of what these guys have said and you can't just suggest… like that. If they were wrong, you're gonna come out and say it." Glauber recounted later that "I didn't want any fight and certainly not with Wolf… So, I suffered from it… [and] I said the suggestion was misleading… and sent the thing in."[25] Glauber's first paragraph indeed clearly indicated that he was criticizing the model for an optical maser source proposed by the physicists Wolf and Mandel who were part of the optical community. According to Wolf and Mandel's

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[24] Glauber, *Photon Correlations* (ref. 21), 84; I did not have access to Glauber's manuscript, and so this paragraph comes from his 1963 paper. But it seems that Glauber changed only the part with "misleading," as he recounted later.

[25] Glauber's interview (ref. 16).

prediction, it would be expected to observe HBT correlations in a maser beam.[26] Wolf is a Czech-American physicist who graduated in mathematics and physics in 1945, and did his doctorate in physics at Bristol University in 1948. He also received his D.Sc. in Optics from the University of Edinburgh in 1955. Since 1959 he has been a Physics Professor at the University of Rochester, having also been the president of the Optical Society of America in 1976.[27] Wolf is widely known for the publication of his classic optics book in 1959, Principles of Optics, in collaboration with Max Born. Since the mid-1950s, Wolf's interests were in classical theory of coherence and the HBT experiment.[28] The German-born American physicist Mandel completed his bachelor's degree in mathematics and physics in 1947 and his doctorate in nuclear physics in 1951, both at the University of London. From 1964 on, he became a Physics Professor at the University of Rochester. Mandel changed his research focus from nuclear physics to optics because of the HBT experimental results. Wolf and Mandel enjoyed lifelong scientific collaboration publishing several papers in the field of optics and in 1995 they also published the book "Optical Coherence and Quantum Optics."[29]

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[26] L. Mandel and E. Wolf, "Correlation in the Fluctuating Outputs from Two Square-Law Detectors Illuminated by Light of Any State of Coherence and Polarization," Physics Review **124**, no. 6 (1961): 1696-1702.

[27] JOSA, "Biographical Notes," Journal Optical Society America 66, no. 1 (1976): 82.

[28] Bromberg, *The Laser in America* (ref. 5), 107.

[29] G. S. Agarwal and Z. Y. Ou, "Leonard Mandel (1927-2001) - Obituary," *Nature* 410, (2001): 538; H. J. Kimble and E. Wolf, "Leonard Mandel - Obituary," *Physics Today* 54, no. 8 (2001): 62; Marlan O. Scully, Robert J. Scully, and H. Jeff Kimble, "*Leonard Mandel 1927–2001: Biographical Memoirs*," vol. 87 (Washington, DC: The National Academies Press, 2006).

Despite being aware that his first article would probably stir up a controversy as it conflicted with Wolf and Mandel's correlation prediction for a laser beam, Glauber submitted the paper to the Physical Review Letters (PRL). After a short while, he received negative referee reports. The reports claimed, according to Glauber, that "this paper should not under any circumstances be published… [and] that first of all, Mandel and Wolf didn't make any mistake because the existence of non-Gaussian light was unknown when they wrote the paper," and second, "if you publish it, it would only lead to an extreme controversy." By non-Gaussian light, they meant a laser beam. Glauber thus contacted the Editor of the PRL and highlighted that "with this stack of war I don't see how you can do anything other than publish it." At the beginning of 1963, the article was published and a widespread and heated debate commenced soon after.[30]

Writing on the history of the laser in the US, Bromberg indicated the stage of research in optics in the 1960s through Wolf and Mandel's papers:

> To this point, many physicists and engineers outside the specialty had only a limited acquaintance with optical coherence theory. Mandel and Wolf were obliged to point out that coherence was not equivalent to monochromaticity;[31] therefore, equations that had been applied to laser beams simply because they were valid for monochromatic light had to be examined anew. Conversely, laser light could serve as a touchstone, to show which "established" results within the old coherence theory had to be reinterpreted or

[30] Glauber's interview (ref. 16).

[31] Monochromaticity refers to an electromagnetic radiation of a single wavelength, that is, of a single color.

extended. Wolf's early papers also demonstrate that the basic question of why a laser light beam is coherent was still unanswered.[32]

The definition of coherence was not clear even among laser physicists. As discussed by Bromberg in the case of the inventor of the maser Charles Townes, for instance, "[t]he word coherence is mentioned only once in this paper [Infrared and Optical Masers published in 1958]. When it is, in the phrase "a long train of coherent waves", it means a wave with very small frequency spread, that is, coherence is taken to mean monochromaticity."[33] The historian of science Paul Forman had also highlighted that "[n]owhere is the word "coherent" used in this notebook entry; Townes seems at pains to avoid it."[34] After analyzing the scientific trajectory of other laser physicists, such as Joseph Weber, Robert H. Dicke and Israel R. Senitzky, and discussing the problem of coherence underlying their contributions, Bromberg concluded that "[c]oherence was complicated by the multiplicity of definitions and intuitive understandings that had accumulated around the term."[35] It was Glauber's studies which explained why a laser is a coherent light beam in terms of a complete quantum-mechanical approach in which both the radiation field and matter are quantized.

In his first paper on "Photons Correlations," Glauber discussed the reason why the HBT experiment had detected a correlation between photons. Until then, all the physicists who had tried to explain the HBT results, such as Mandel, Wolf, and Purcell, represented the electric field

[32] Bromberg, *The Laser in America* (ref. 5), 108.

[33] Joan L. Bromberg, "Coherence and Noise in the Era of the Maser," *forthcoming*, 2013, on 8.

[34] Forman, *Inventing the Maser in Postwar America* (ref. 32), 122.

[35] Bromberg, *Coherence and Noise in the Era of the Maser* (ref. 33), 23.

in a light beam as a classic Gaussian stochastic process, depending only on its frequency-dependent power spectrum.[36] In contrast, however, he highlights that "[b]eams of identical spectral distributions may exhibit altogether different photon correlations or, alternatively, none at all."[37] Because of the limitation of the classical or semi-classical approaches, Glauber suggests analyzing the problem using a different approach based on quantum field theory, in which a steady light beam would be described as independent from the spectral distributions. In his explanation of the HBT experimental results, Glauber states that as long as incoherent mixtures or superpositions of coherent states were part of the HBT-type experiment, a correlation between photons would be observed as a result. That is, non-HBT correlations will be detected only if there is a single photon in a mode. If there are two or more photons in a mode of the electric field, on the other hand, the light beam will be incoherent and consequently photon correlations will be observed. An incoherent light beam is described by Glauber as "a statistical mixture of all the excitation states available for each mode excited." In the classical limit, an incoherent light may be understood as modes of the electric field when they are conceived as forming a continuum.[38]

From 1963 on, Glauber published more papers detailing a new concept for coherence based strictly on quantum theory, taking a major step forward by introducing coherent states into

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[36] A Gaussian process is composed of random variables whose finite number has a Gaussian distribution (a continuous probability distribution – a function which provides the probability of obtaining a number that fluctuates between any two real numbers). See also R. F. Fox, "Gaussian Stochastic Process in Physics," Physics Report 48, no. 3 (1978): 179-283.

[37] Glauber, *Photon Correlations* (ref. 21), 85.

[38] Ibid., 85-86.

optics.[39] By doing so, Glauber was introducing the conceptual framework for what would later be a new discipline, quantum optics. For mathematical convenience, he describes the expression for the oscillating electric field $\boldsymbol{E}$ as two complex conjugate terms: $\boldsymbol{E} = \boldsymbol{E}^{(+)} + \boldsymbol{E}^{(-)}$, in which $\boldsymbol{E}^{(-)} = (\boldsymbol{E}^{(+)})^*$. The negative frequency $\boldsymbol{E}^{(-)}$ – the creation operator – and the positive frequency $\boldsymbol{E}^{(+)}$ – annihilation operator – had the property of changing the state of the electric field. The creation operator is related to the photon emission and the annihilation one to the photon absorption. These operators have two properties: first, if one applies the operator $\boldsymbol{E}^{(-)}$ to a state of photons $|n\rangle$, it will produce a state of $|n+1\rangle$; and second, when the operator $\boldsymbol{E}^{(+)}$ is applied to a state of photons $|n\rangle$, however, the final state will be $|n-1\rangle$. If one keeps applying the $\boldsymbol{E}^{(+)}$ to an $n$-photon state until there is no longer any photons left, there will be a "state in which the field is empty of all photons", that is, the vacuum state: $\boldsymbol{E}^{(+)}(\boldsymbol{r}t)|vac\rangle = 0$, in which $\boldsymbol{r}$ is the position and $t$ the time. If one applies the $\boldsymbol{E}^{(-)}$ operator to the $|vac\rangle$ state, conversely, it will produce a state of a single photon.[40] Comparing the observed variables between the classical and the quantum approaches, Glauber claimed that while the classic al electric field may be measured only if there is no manner of distinguishing whether or not photons are absorbed or emitted, in a quantum domain the detection process lies in the absorption of quanta and hence it is the annihilation operator which describes what is measured.[41]

[39] R. G. W. Brown and E. R. Pike, A History of Optical and Optoelectronic Physics in the Twentieth Century, in Twentieth century physics, edited by Laurie M. Brown, Abraham Pais, Sir Brian Pippard (Bristol; Philadelphia: Institute of Physics Pub.; New York: American Institute of Physics Press, c1995), on 1440.

[40] Glauber, *The Quantum Theory of Optical Coherence* (ref. 21), 2530-31.

[41] Roy J. Glauber, Quantum Theory of Coherence, in Quantum Electronics, Proceedings of the Third International Congress, edited by N. Bloembergen and P. Grivet (New York: Columbia University Press, 1964), on 111.

In this context, Glauber considered a photon detection situation in which a photon would be detected at a position ***r*** and time *t* and consequently the field would change from an initial state $|i\rangle$ to a final state $|f\rangle$. As the initial state of the field might be affected by uncontrollable random parameters, since it depends only on the characteristic of the light source used in a counting experiment, Glauber introduced the density operator $\rho$ of the radiation field into his definition for the correlation function for the complex fields[42]

$$(1)\quad G^{(1)}(\mathbf{r}t,\mathbf{r}'t')=\mathrm{tr}\{\rho E^{(-)}(\mathbf{r}t)E^{(+)}(\mathbf{r}'t')\}$$

where the symbol *tr* (trace) represents the sum of the diagonal matrix elements. If one assumes the diagonal form, where ***r****t* = ***r'****t'*, and that detectors are located at the positions ***r*** and ***r'***, Eq. (1) gives the probability of detecting photons at the points ***r*** and times *t*.[43] In addition, if an experiment were arranged to superpose the fields $\boldsymbol{E}^{(-)}$ and $\boldsymbol{E}^{(+)}$, the expression (1) would then be represented in terms of interference parameters.[44]

Glauber thus generalized Eq. (1) and defined the *n*th-order correlation function for the electromagnetic field at different space-time as[45]

$$\begin{aligned} G^{(n)}{}_{\mu_1\cdots\mu_n,\mu_{n+1}\cdots\mu_{2n}}&(\mathbf{r}_1t_1,\cdots\mathbf{r}_nt_n,\mathbf{r}_{n+1}t_{n+1}\cdots\mathbf{r}_{2n}t_{2n}) \\ &=\mathrm{tr}\{\rho E_{\mu_1}{}^{(-)}(\mathbf{r}_1,t_1)\cdots E_{\mu_n}{}^{(-)}(\mathbf{r}_n,t_n) \\ &\quad\times E_{\mu_{n+1}}{}^{(+)}(\mathbf{r}_{n+1},t_{n+1})\cdots E_{\mu_{2n}}{}^{(+)}(\mathbf{r}_{2n}t_{2n})\} \end{aligned} \qquad (2)$$

[42] Glauber, *The Quantum Theory of Optical Coherence* (ref. 21), 2531-32.

[43] Brown and Pike, *A History of Optical and Electronic Physics* (ref. 38), 1440.

[44] Glauber, *Quantum Theory of Coherence* (ref. 41), 112.

[45] Glauber, *The Quantum Theory of Optical Coherence* (ref. 21), 2533.

After defining such a correlation function, Glauber turned to "sharpening the meaning of coherence." He highlighted the optical concept for coherence which had been used to date: "In physical optics the term is used to denote a tendency of two values of the fields at distantly separate points or at greatly separate times to take on correlated values. When optical means are used to superpose the fields at such points (e. g., as in Young's two-slit experiment) intensity fringes result." Glauber gave two reasons as to why such a concept should be reviewed. First, the classic concept of coherence was only applied to experiments in which field intensities were measured, that is, quantities which are quadratic in the field strengths. As a result of the development of non-linear optics, there would be a significant increase in the number of experiments carried out to measure the fourth or higher powers in the field strengths. Hence, a concept of coherence able to extend to them would be necessary. The second concerns the optical maser. The classical framework of optics was used to describe the light beams of narrow spectral bandwidth created by the maser as coherent, but "the sense in which the term is used has not been made adequately clear." The problematic aspect is that, as highlighted by Glauber, "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"[46] since the classical definition could be applied only to stationary fields.

Afterwards, introducing the normalized forms of the higher order correlation functions

$$g^{(n)}(x_1\cdots x_{2n})=G^{(n)}(x_1\cdots x_{2n})/\prod_{j=1}^{2n}\{G^{(1)}(x_j,x_j)\}^{1/2}, \qquad (3)$$

Glauber presented a set of conditions for coherence given by

[46] Ibid., 2534.

$$|g^{(n)}(x_1 \cdots x_{2n})| = 1, \quad n = 1, 2 \cdots \tag{4}$$

In other words, fields might be described as fully coherent only if they satisfy the infinite succession of conditions contained in Eq. (4). Glauber therefore stated that "[t]he definition of coherence which has been used to date in all studies of physical optics corresponds only to first-order coherence" when $|g^{(1)}(\boldsymbol{r}t, \boldsymbol{r}'t')| = 1$. In this case, for classical experiments there will be a lack of second and higher order coherence in the coherent fields produced prior to the development of the maser. With the creation of the maser this device, it "may produce fields which are coherent to all orders" by working with ideal stability.[47] As Glauber considered ensemble averages, instead of time averages as in the case of classical optics, he was able to construct a new concept of coherence which deals with non-stationary fields.[48]

Glauber then analyzed a situation in which a field has an *m*th-order coherence, for $j \leq n$, satisfying all coherence conditions $g^{(j)}(x_1 \dots x_j, x_j \dots x_1) = 1$, and calculated the corresponding values of the correlation functions $G^{(j)}$ through factorization, obtaining

$$G^{(j)}(x_1 \cdots x_j, x_j \cdots x_1) = \prod_{i=1}^{j} G^{(1)}(x_i, x_i) \tag{5}$$

This means that *j*-fold delayed coincidences, detected by an ideal detector, depend on the detection rates of individual counters, leading to an important conclusion: "[i]n 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 events." That is, if the

[47] Ibid., 2534.

[48] Bertolotti, *Masers and Lasers* (ref. 6), 220.

field satisfied the coherence condition (Eq. 4) to an *m*th-order, non-HBT correlations, or anti-correlations, would be observed. Conversely, Hanbury Brown and Twiss detected two photons arriving at the same time at two different detectors as a result of the characteristics of the light used. They used a thermal source, which even being coherent in the first-order, did lack the second-order coherence.[49] Individual events would therefore be detected only if a field had second and higher order coherence, which characterizes it as completely coherent.

In order to guarantee that fields would remain coherent independent of the system of axis, Glauber demonstrated that the correlation function $G^{(n)}$ satisfied the factorization form given by

$$G^{(n)}(x_1\cdots x_n, x_{n+1}\cdots x_{2n}) = \mathcal{E}^*(x_1)\cdots\mathcal{E}^*(x_n)\,\mathcal{E}(x_{n+1})\cdots\mathcal{E}(x_{2n}) \tag{6}$$

which also satisfied the set of conditions (Eq. 4). He also derived that these functions $\mathcal{E}_\mu(\mathbf{r}t)$ are eigenstates of the operators $\boldsymbol{E}^{(+)}$ and $\boldsymbol{E}^{(-)}$, that is, $\langle|E^{(-)}(\mathbf{r}t) = \langle|\mathcal{E}^*(\mathbf{r}t)$ and $E^{(+)}(\mathbf{r}t)|\rangle = \mathcal{E}(\mathbf{r}t)|\rangle$. These functions $\mathcal{E}_\mu(\mathbf{r}t)$ are thus the complex eigenvalue.[50]

Now, returning to the question which led Glauber to the construction of his quantum theory of coherence: “How then would one describe the delayed-coincidence counting measurement of Hanbury Brown and Twiss?” Considering two sensitive detectors in space-time ($\boldsymbol{r}_1 t_1$) and ($\boldsymbol{r}_2 t_2$) and the fact that one is dealing with the absorption process, the annihilation

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[49] Glauber, *The Quantum Theory of Optical Coherence* (ref. 21), 2535.

[50] Ibid., 2535-36.

operators $E^{(+)}(\boldsymbol{r}_1 t_1)$ an $E^{(+)}(\boldsymbol{r}_2 t_2)$ have to be taken into account. In this case, the amplitude required for the field makes a transition from the state $|i\rangle$ to a state $|f\rangle$ is[51]

$$\langle f|\ E^{(+)}(\boldsymbol{r}_2 t_2) E^{(+)}(\boldsymbol{r}_1 t_1)\,|i\rangle \tag{7}$$

By squaring, summing over the final states $|f\rangle$, and averaging over the initial states $|i\rangle$, the correlation function will be given by

$$G^{(2)}(\boldsymbol{r}_1 t_1 \boldsymbol{r}_2 t_2 \boldsymbol{r}_2 t_2 \boldsymbol{r}_1 t_1) = \mathrm{Trace}\{\rho E^{(-)}(\boldsymbol{r}_1 t_1) E^{(-)}(\boldsymbol{r}_2 t_2) E^{(+)}(\boldsymbol{r}_2 t_2) E^{(+)}(\boldsymbol{r}_1 t_1)\} \tag{8}$$

Writing Eq. (8) according to the parallel factorization form of $G^{(2)}$, for simplifications $x_j = \boldsymbol{r}_j t_j$, one obtains

$$G^{(2)}(x_1 x_2 x_3 x_4) = \mathcal{E}^*(x_1)\mathcal{E}^*(x_2)\mathcal{E}(x_3)\mathcal{E}(x_4) \tag{9}$$

Despite the HBT correlations not having this kind of factorization, as Glauber discussed, such a definition becomes "useful" to show that if they were present in the HBT experiment, Eq. (9) would be given by

$$G^{(2)}(x_1 x_2 x_2 x_1) = G^{(1)}(x_1 x_1)\ G^{(1)}(x_2 x_2) \tag{10}$$

which means that second-order coherence relies on the average intensities which are detected separately. This is quite different from what HBT observed – a correlation between the time of arrival of photons. Eq. (10) led Glauber to the conclusion: "Ordinary light beams, that is, light from ordinary sources, even extremely monochromatic ones as in the Hanbury Brown-Twiss

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[51] Roy J. Glauber, "Nobel Lecture: One Hundred Years of Light Quanta," available at http://www.nobelprize.org/nobel_prizes/physics/laureates/2005/glauber-lecture.html, accessed on 19 May 2013, on 88.

experiment, do not have any such second order coherence."[52] That is, although Hanbury Brown and Twiss used a monochromatic source in the classical optical sense, the characteristic of such a source still lacks second-order coherence. Chaotic sources, as in the HBT-type experiment, only have first-order coherence. That is why correlated photons were detected. As the laser beam is a complete coherence source, satisfying the conditions (Eq. 5) for all coherence orders, if this beam were used in HBT-type experiment, correlations would *no* longer be expected between photons. The detection of a photon in one detector would then be statistically independent from the detection of another one in the other detector, as described in Eq. (10).

In the optical concept for coherence the fields defined as coherent turned out therefore to be those of the narrowest spectral bandwidth. That is why, as stated by Glauber, "there has been a natural tendency to associate the concept of coherence with monochromaticity," which may only be applied to stationary light fields. Considering Eq. (6), however, the "[c]oherence conditions restrict randomness of the field rather than their bandwidth."[53] In this sense, coherence is not associated with the frequency spectra of the field, but rather with statistical properties of the field.

After proposing this new meaning for coherence, Glauber constructed a complete quantum-mechanical approach for the field of photon statistics by defining a coherent state and the *P* representation. In quantum electrodynamics, the field is represented through a particular set of quantum states which means "the presence of a precisely defined number of photons." Whereas the QED methods deal with more than a few photons at a time, the field in classical

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[52] Ibid.

[53] Glauber, *The Quantum Theory of Optical Coherence* (ref. 21), 2536.

optics describes light beams in terms of states in which the number of photons presented is large and uncertain. Glauber thus proposed that "any state of the field may be represented simply and uniquely in terms of [coherent states]." Even though they are not orthogonal, coherent states form a complete set of states and are represented by[54]

$$|\alpha\rangle = e^{-\frac{1}{2}|\alpha|^2} \sum_n \frac{\alpha^n}{(n!)^{1/2}} |n\rangle \tag{12}$$

in which $\alpha$ is an arbitrary complex amplitude and $|n\rangle$ is the photon number state. Such states $|\alpha\rangle$ are fully coherent states of the field mode and the eigenstate of the photon annihilation operator.

Glauber derived the Gaussian description of the density operator for a single mode as a function of the coherent states

$$\rho = \int P(\alpha) |\alpha\rangle\langle\alpha| d^2\alpha \tag{13}$$

which is called the P representation of the density operator, or a quasi-classical distribution function such as the Wigner function.[55] As the projection operator $|\alpha\rangle\langle\alpha|$ is not orthogonal, P (α) may not always be understood as a probability distribution. Glauber highlighted the role of such

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[54] Glauber, *Coherent and Incoherent States of Radiation Field* (ref. 10), 2766-67.

[55] The Wigner distribution function was developed in 1932 by Eugene Wigner, from which "statistical information is transferred from the density operator to a quasi-classical (distribution) function." In: R. F, O' Connel, Wigner Distribution, Compendium of Quantum Physics: Concepts, Experiments, History and Philosophy, edited by D. Greenberger, K. Hentschel and F. Weinert (London: Springer, 2009), on 852.

a representation, “[t]his form is one which brings to light many similarities between quantum electrodynamics calculations and the corresponding classical ones.”[56]

Moreover, Glauber calculated the probability of detecting a single photon at $\boldsymbol{r}$ between $t_0$ and $t$ by examining the photodetection process[57]

$$p^{(1)}(t) = \sum_{\mu,v} S_{\mu v} \int_{t_0}^{t} G^{(1)}\,(rt', rt')dt' \tag{14}$$

in which $S_{\mu v}$ is the frequency- dependent sensitivity function.

Before moving to the next section, I would like to highlight how the American physicist Marlan O. Scully (1939- ), widely known for his work on quantum optics, summarized Glauber’s contributions to quantum optics: “[T]here’s a story about Charles Townes and Niels Bohr concerning the line-width of the laser, and why it is so narrow... Other people were trying to understand how it is, from a photon point of view, that you get coherent radiation. You go from a photon picture, which is the antithesis of coherence, to a coherent beam of light. A deep philosophical question, that Roy Glauber helped us to understand. He pointed out that it was a difficult problem that we wouldn’t answer until we got a more careful theoretical analysis of the quantum nonlinearities of the laser.”[58] Glauber had therefore explained the HBT experimental results in terms of a full quantum theory of light and defined the quantum concept of coherence by quantizing both the electromagnetic field and matter. In the new meaning of coherence, a

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[56] Glauber, *Coherent and Incoherent States of Radiation Field* (ref. 20), 2776.

[57] Glauber, *Quantum Theory of Coherence* (ref. 40), 81-82.

[58] Interview of Marlan O. Scully by Joan Lisa Bromberg on 15-16 June 2004, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.

field is coherent only if it has second and higher order coherence. That is why the laser is completely coherent whereas chaotic sources only have fields with the first-order coherence. With Glauber's theoretical contributions, coherence was no longer related to monochromaticity, but rather to the statistical properties of the field.

### The Birth of the Controversy (1963-1964)

It was at the Third International Conference on Quantum Electronics, held in Paris in February of 1963 and sponsored by the Office of Naval Research, the International Radio Union, and the Fédération Nationale des Industries Électroniques, when the first debate between Glauber and the Rochester group took place. Despite the fact that Wolf did not consider himself part of the quantum electronics community, he was invited by the Dutch-American physicist Nobel laureate Nicolaas Bloembergen (1920- ), one of the organizers of the conference, to present a paper on coherence; as was Glauber. The discussion between Wolf and Glauber was recorded and then transcribed for the proceedings of the congress. Wolf later recalled some delicate matters which were not published in the volume, for example, "I remember… Glauber accusing me that I have set optics 50 years back by using instead of…That for some reason has not been reprinted but it was part of the big argument we had at that point."[59] In other words, Glauber seemed to suggest that Wolf was taking a step backwards by working with semi-classical, or classical, approximations to deal with the issue of coherence in a laser beam. From

[59] Interview with Emil Wolf by Joan Lisa Bromberg on 23 Sept 1984, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.

Glauber's point of view, quantum electrodynamics was the only way of providing a satisfactory definition for coherence.

After Wolf's talk, Glauber suggested he examine the problem of coherence through a quantum-mechanical approach. In answer, Wolf pointed out that it would be possible to deal with the problem in terms of the quantization of the radiation field, "but this may not be easy to do." He also claimed that classical and semi-classical approaches provided reasonable approximations in the explanations or predictions of experiments. He dissented from Glauber's view concerning the need to quantize the electromagnetic field in the case of a laser beam by claiming that:

> One should also bear in mind that the classical theory arouse from an attempt to undertand certain type of phenomena with light from thermal sources. Of course, as new problems arise, the theory has to be extended and this is precisely what is now being done with the help of higher order correlation fucntions. But my guess is that for maser light classical theories will be even more useful than for thermal light.[60]

Then commenting on Glauber's presentation, Wolf highlighted three points of disagreement. It was a mistake to assume that the classical theory of coherence could only be applied to monochromatic light beams, according to Wolf. Due to some mathematical simplifications and getting "closer to experiment", Wolf also claimed that the classical theory usually considered the average time of the light intensities, instead of the ensemble averages. He also emphasized the difference between his own concepts and Glauber's theoretical proposal by concluding that

[60] E. Wolf, in Quantum Electronics, Proceedings of the Third International Congress, Paris, edited by N. Bloembergen and P. Grivet (New York: Columbia University Press, 1964), on 34.

"[f]or non-stationary fields one would naturally use the ensemble average – one does not need a quantum mechanical treatment to do that." Furthermore, Wolf highlighted that there was some similarity between Glauber's and Mandel and Wolf's papers presented at the conference. For instance, the use of the analytic signals for the theory of coherence by Wolf and Mandel corresponded to the separation of the positive and negative frequency proposed by Glauber. Last but not least, Wolf recognized the sophistication of Glauber's structure for a light beam by "defin[ing] complete coherence in terms of an infinite sequence of correlations." Thinking about it in terms of an experiment, however, Wolf stated that as it would be necessary to perform "an infinite number of experiments" to characterize some beams of light as coherent or not, and thus Glauber's coherence definition seemed  impractical. Wolf added that "I think one must wait to see how useful some of these definitions will for the analysis of experiments."[61]

In answer to Wolf's comments, Glauber pointed out that Wolf's classic theory of correlations dealt only with statistically stationary fields in time with clearly arbitrary frequency spectra. Regarding Wolf's optical definition of coherence, Glauber stated that it would "only be satisfied by fields which are monochromatic or at worst quasi-monochromatic", however. The "limitation" of Wolf's approach, according to Glauber, was the fact that he used the time-average of the field intensity to explain the concept of coherence, while Glauber considered the ensemble-average. Glauber thus decided to develop a theory of coherence which could be applied to "fields with all sorts of time variations." Even though ensemble averages could be defined both in classical and quantum approaches, Glauber reported that "Prof. Wolf is aware of the fundamental sense in which ensemble-averages are unavoidable in quantum statistics."

[61] Emil Wolf, in R. Glauber, in Quantum Electronics, Proceedings of the Third International Congress, Paris, edited by N. Bloembergen and P. Grivet (New York: Columbia University Press, 1964), on 118-119.

Concerning the correspondence between the two theoretical constructions, Glauber agreed with Wolf that their expressions for the first-order correlation function would indeed be similar in the classical limit. To Wolf's criticism of the number of experiments required to verify whether or not a light beam was coherent, Glauber admitted his set of conditions for a field to be completely coherent was "indeed rather idealized". However, he totally disagreed with the statement that his theoretical construction had "no practical" application in terms of experimentation. Although some fields had successfully satisfied the coherence conditions, theoretically speaking as in the case of maser beams, Glauber highlighted the fact that "[t]he higher order correlation functions are not quite as easy to measure in the optical region, but they may be found through the techniques of non-linear optics and, perhaps more effectively, through the study of multiple quantum transition in high field."[62]

Shortly after the conference, R. J. R. Hayward, who worked for General Electric Company in Wembley, published a report highlighting the principal issues discussed. As the laser had provided a coherent light beam, according to Hayward, physicists turned their attention to the concept of coherence. In that context, the principal "uncertainty" question about coherence, which had divided the community of physicists in the 1960s, was whether "a semi-classical description [is] sufficient or a fully quantum mechanical approach [is] necessary."[63] This question would be revisited in the 1970s as we will see in the following section.

In the same PRL issue as Glauber published his first article on correlation between photons, and after the Paris conference, the Rochester group's answer appeared. Henceforth, the

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[62] Glauber, *Quantum Theory of Coherence* (ref. 40), 119.

[63] R. J. R. Hayward, "Third International Symposium on Quantum Electronics," Br. J. Appl. Phys. **14,** no. 7 (1963): 411-13.

controversy, or as dramatically described by Glauber, war broke out. Even before the conference, the journal had requested Wolf to be a referee on Glauber's first 1963 paper, and if Wolf's memory served him correctly, "we certainly didn't block it."[64] In contrast, Glauber claimed that he received negative referees.[65] Unfortunately, I have not yet had the chance to consult the Physical Review Letters archives to know more about it. Mandel and Wolf admitted that Glauber was right, although they disagreed with his description of a laser beam, by recognizing that "our remarks were indeed misleading" because they would not apply to a maser light operating in one or a few modes.[66] Wolf and Mandel had suggested in 1961 that lasers could be described as Gaussian random processes. However, such a classical description could be considered only if the fields had many modes.[67]

On returning to Rochester after the conference, Wolf and Mandel invited the Indian physicist E. C. George Sudarshan, also at Rochester University, to analyze the problem of coherence for a laser beam. While Mandel and Wolf were unfamiliar with quantum field theory, Sudarshan was "quantum mechanically adept" and played a fundamental role in what came after; conversely, according to Glauber, "Wolf was ever converted to quantum theory."[68] Sudarshan thus constructed a quasi-classical representation of quantized fields, which had some analogies

[64] Wolf's interview (ref. 59).

[65] Glauber's interview (ref. 16).

[66] L. Mandel and E. Wolf, "Photon Correlations," Physical Review Letters 10, no. 7 (1963): 276-77, on 276.

[67] Wolf's interview (ref. 59).

[68] Glauber's interview (ref. 16).

with Glauber's P representation.[69] After this, the controversy intensified and a part of it is still going on.[70] According to Wolf, "[t]here is the question still even now of priorities but they arrived at it independently around the same time." Yet, Glauber totally disagreed with that. In 2005, when Glauber received the Nobel Prize, the issue of priority came to the fore. Some physicists publicly expressed their disappointment over the fact that Sudarshan did not share the Prize with Glauber.[71] Sudarshan himself sent a letter to the Nobel Committee in which he decried the Committee's decision stating that:

> I am therefore genuinely surprised and disappointed by this year's choice. It would distress me and many others if extra scientific considerations [because he was an Indian, according to Sudarshan] were responsible for this decision. It is my hope that these glaring injustices would be noted by the Academy and modify the citations. Give unto Glauber only what is his.[72]

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[69] E. C. G. Sudarshan, "Equivalence of Semiclassical and Quantum Mechanical Descriptions of Statistical Light Beams," Physical Review Letters 10, no. 7 (1963): 277-79.

[70] Although the controversy between Glauber and the Rochester group is historically fundamental to understanding the development of quantum optics, a detailed discussion of this issue is beyond the scope of this dissertation.

[71] L. Zhou, C. S. Writer, Scientists Question Nobel, The Harvard Crimson, accessed January 2013, http://www.thecrimson.com/article/2005/12/6/scientists-question-nobel-a-group-of/ . More news in magazines about the Nobel in Physics 2005, see: Top scientist says Nobel jury 'racist', http://www.gulftoday.ae/portal/6af72fbc-32a1-40e3-8457-8a2f8406ed9e.aspx; Nobel jury racist, says physicist, http://www.highbeam.com/doc/1P3-2084858581.html; Indian physicist cries foul over Nobel miss, http://www.hindustantimes.com/News-Feed/Science/Indian-physicist-cries-foul-over-Nobel-miss/Article1-213327.aspx; When the Nobels are handed out, some get left out, http://www.3quarksdaily.com/3quarksdaily/2005/12/when_the_nobels.html;

[72] E. C. G. Sudarshan letter to the Nobel Committee, in Frontline 22, no. 24, 2005.

He also tried to clarify the difference between the two theoretical achievements. The development of “coherent states as basic entities to describe optical fields certainly goes to Glauber,” however, “the possibility of using them to describe ‘all’ optical fields (of all intensities) through the diagonal representation is certainly due to Sudarshan.” Such scientific priority dispute is not part of this study, but we highlight its importance to understand the construction of the field of quantum optics.[73]

Comparing the citation dynamics through the Web of Science between Glauber’s 1963 paper, “Coherent and Incoherent States of Radiation,” and Sudarshan’s 1963 one, “Equivalence of Semiclassical and Quantum Mechanical Descriptions of Statistical Light Beams,” it can be seen that the former had been cited 3,895 times from 1963 to early January 2013, while the latter, 1,020 times. This significant difference suggests that Glauber’s paper has had greater repercussion in the scientific community than Sudarshan’s.

The controversy between the Rochester group led by Wolf and Glauber was an extremely delicate issue, as highlighted by Bromberg,

> [E]ven 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 Review of Modern Physics, Mandel and Wolf lay out the history of Glauber’s work in

[73] Detailed analyses on that subject will be done in another article.

> such way as to imply that some of it was formulated as an analogy with work of their own."[74]

As described by Mario Bertolotti, writing a history of the maser and laser, the first controversy started in 1963 would end by 1965. An expressive illustration of its end was the fact that Mandel and Wolf included the quantum theory of coherence proposed by Glauber in a theoretical review of the theory of coherence. "With time, the quarrel died down," according to Bertolotti, and "[t]he elegant construction of the quantum theory of coherence was, from that moment on, accepted unconditionally."[75] The end of the controversy requires detailed investigation. The publication of Wolf and Mandel's review containing Glauber's quantum theory of light does not mean that they accepted Glauber's conceptual framework. The French physicist Alain Aspect in a 2010 interview claimed that "Mandel changed his mind, but in the beginning he was sitting on the side of Wolf" by mentioning about how necessary the quantization of the electromagnetic field was. [76] Did Wolf thus change his mind on that issue? During a 1984 interview, Wolf expressed his point of view:

> Mandel, of course, I think, I'm not sure, you should ask him, but I think many of the experiments he's doing to this day about finding limits of classical, semi classical methods, he was influenced also by this controversy. Because it turns out, after all this, there are very very few experiments for which the full coherence theory quantum is needed. But he is much more competent to talk about that. Sudarshan would probably say

[74] Bromberg, *Modelling the Hanbury Brown – Twiss Effect* (ref. 7), 13.

[75] Bertolotti, *Masers and Lasers* (ref. 6), 228; See also L. Mandel and E. Wolf, "Coherence Properties of Optical Fields," Reviews of Modern Physics 37, no. 2 (1965): 231-87.

[76] Interview of Alain Aspect by Olival Freire Jr. on 16 Dec 2010, France.

> the same as I'm telling you now, that practically every problem in optics, which turns up in optics, can be treated by classical or semi classical methods. Mandel is a little more cautious in this and he can produce of course examples which can be, and he's right, his recent experiments — but there are about two experiments, two kinds of experiments.[77]

From 1967 on, physicists started applying Glauber's theoretical construction to other problems. While working at the University of California, Y. R. Shen, for example, studied the interaction between a non-linear light and matter in terms of Glauber's theoretical structure.[78] Mario Bertolotti and colleagues, who worked at Università di Roma, also applied Glauber's coherence definition to the investigations on the structure of matter.[79] P. Di Porto and collaborators discussed the second and fourth order statistical properties of the radiation field when it is scattered by particles in a turbulent field.[80]

Glauber's quantum theory of coherence was therefore widely acknowledged to be consistent. In 1967 there was the first International "Enrico Fermi" School of Physics dedicated to quantum optics, with Glauber as the director and lecturer. In the preface to the proceedings, Glauber highlighted that "[t]he term quantum optics which gives our course its title is new enough to need some explanation; [i]nstead of confining ourselves to describing the way in which light propagates, we consider explicitly the way in which it is radiated, its interaction with

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[77] Wolf's interview (ref. 63).

[78] Y. R. Shen, "Quantum Statistics of Nonlinear Optics," Physical Review 155, no. 3 (1967): 921-31.

[79] M. Bertolotti, B. Crosignani, P. Di Porto, and D. Sette, "Photostatistics of Light Scattered by a Liquid," Physical Review 157, no. 1 (1967): 146-49.

[80] P. Di Porto, B. Crosignani, and M. Bertolotti, "Statistical Properties of Light Scattered by Particles Suspended in a Turbulent Fluid," J. Appl. Phys. 40, no. 13 (1969): 5083-88.

matter as in scattering experiments, and finally the way in which it is detected."[81] By the end of the 1960s, quantum optics had become a new fruitful and promising field of research.[82]

## Reflections on the Concept of the Photon

In the preface to Glauber's book, "Quantum Theory of Optical Coherence: Selected Papers and Lectures," Scully remembers an episode which occurred in the Les Houches Summer School of 1964 at the University of Grenoble:

> [M]any scientists in Les Houches were using the word "photon" even when they referred to an effect whose explanation did not rely on the quantum theory of radiation. This misuse of the word "photon" annoyed [Willis] Lamb and he introduced a licence which entitled its owner to use the word "photon." Scientists without licence were not allowed to even mention photons. Roy was one of the very few colleagues who received such a licence from Lamb.[83]

Thus, a question arose: Which concept of the photon was the American physicist 1955 Nobel laureate Lamb referring to? In particular, which concept of the photon emerged from Glauber's

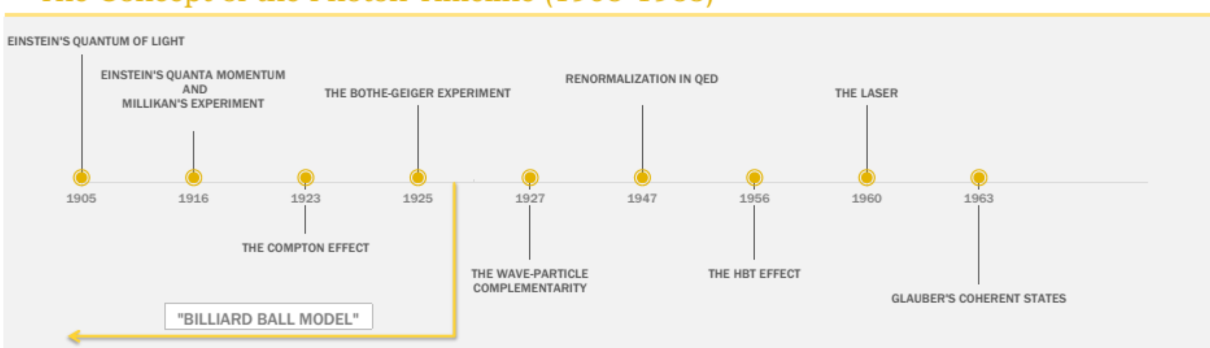
[81] Roy J. Glauber, in Proceedings of the International School of Physics "Enrico Fermi," (New York: Academic Press, 1969), on xv.

[82] The Brazilian physicist Moysés Nussenzveig was one of the first to include Glauber's quantum theory of light in his lectures and a textbook on quantum optics, see Climério da Silva Neto and Olival Freire Jr., Herch Moysés Nussenzveig and the quantum optics: consolidating disciplines through summer schools and textbooks, Revista Brasileira de Ensino de Física 35, no. 2 (2013): 2601-11.

[83] Marlan O. Scully, *Preface: Quantum Theory of Optical Coherence* (Weinheim: Wiley-VCH Verlag GmbH & Co. KGaA, 2007), on xv.

theoretical constructions? Considering the following timeline of the mainstream events which contributed to how physicists came to understand the concept of the photon, it is evident that such a concept had not been settled by the 1930s as one may think. In contrast, from the 1950s on, new episodes provided a deeper insight into the conceptual framework of photons, such as the HBT experiment and Glauber's coherent states.

The Concept of the Photon Timeline (1905-1963)




To cut a long story short, the first years of the development of the concept of the photon started between 1905 and 1916 with Albert Einstein's publications on the quantum of light. Einstein proposed that radiation was composed of indivisible particles – quanta – which carry energy *hv* and momentum *hv/c* in a defined direction, with which he successfully explained the photoelectric effect. Such a hypothesis did not convince Max Planck, Max von Laue, Wilhelm Wien, and Arnold Sommerfeld, for example, since optical phenomena – interference and diffraction – required a wave interpretation of light based on the well-established Maxwell equations. In an attempt to refute Einstein's light quanta, Robert A. Millikan found himself confirming Einstein's equation for the photoelectric effect experimentally in 1916. Another empirical confirmation came from Arthur H. Compton in 1923, after having previously spent years trying to interpret the interaction between X-rays and matter in terms of classical or semi-classical approaches. He included elements of quantum theory into his theoretical treatment of the problem by considering a kind of collision between a quantum of light and the electron, obtaining thus significant agreement between his experimental data and light quanta hypothesis. Yet, Niels Bohr, Hendrik Kamers and John Slater proposed a new theoretical framework to explain Compton's observations, known as the BKS theory, based on a semi-classical approach in which radiation was considered classically and matter was treated quantum mechanically. The dispute between the BKS theory and the Compton Effect came to an end in 1925 with an experiment carried out by Walther Bothe and Hans Geiger, in which they confirmed Compton's experimental results by using a method of coincidence. Nevertheless, a dilemma remained: How to conciliate the wave and the quantum interpretations of light? In 1927 Bohr tried to solve the wave-particle duality by introducing a new concept called complementarity, according to which, light may exhibit either the wave characteristic of light or the particle one depending on the experimental apparatus, but never both. Thus, in the case of interference and diffraction, the wave interpretation of light come to the fore, while in the photoelectric effect and Compton Effect are the particle characteristic.[84]

Later in the 1930s, quantum theory interpreted radiation as indivisible quantities, photons, whose energy and momentum were conserved during their interaction with matter. Such a

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[84] Olivier Darrigol, "A simplified genesis of quantum mechanics," Studies in History and Philosophy of Modern Physics 40, 151 (2009); Stephen Brush, "How ideas became knowledge: The light-quantum hypothesis 1905-1935," HSPS 37, 205 (2007); Helge Kragh, *Quantum generations: A history of physics in the twentieth century* (Princeton, NJ: Princeton University Press, 1999); Max Jammer, *The conceptual development of quantum mechanics* (New York: McGraw-Hill, 1966); Bruce R. Wheaton, *The tiger and the shark: Empirical roots of wave-particle dualism* (Cambridge, UK: Cambridge University Press, 1983); Roger H. Stuewer, *The Compton Effect: Turning Point in Physics* (New York: Science History Publications, 1975); Thomas S. Kuhn, *Black-body theory and the quantum discontinuity, 1894–1912* (Chicago: University of Chicago Press, 1978).

concept of the photon was widespread at the time, and it survived in the physicist's ken even after the development of more sophisticated theoretical studies such as Bose-Einstein statistics and quantum electrodynamics. To illustrate, the traditional concept of the photon might be found in two classic textbooks on quantum theory used to train physicists: Atomic Physics by Max Born and The Principle of Quantum Mechanics by Paul Dirac. In Born's book, after defining the light quanta, he characterized them as "fly[ing] through space like a hail of shots with the velocity of light."[85] Dirac went beyond Born to claim that photons "appear to have just as real existence as electrons, or any other particles known in physics."[86] Photons were thus interpreted, as described by Born and Dirac, as material particles such as electrons and neutrons.

In 1964, Lamb was not refereeing to the canonical concept of the photon. He would later remark sarcastically that "[t]alking about radiation in terms of particles is like using such ubiquitous phrases as "You know" or "I mean" which are very much to be heard in some cultures. For a friend of Charlie Brown, it might serve as a kind of security blanket."[87] Lamb gave Glauber a licence to use the word 'photon' since Glauber's coherent states provided new insights on its conceptual nature. However, even before Glauber's studies, the traditional concept of the photon was doubted in 1956 through the experimental results performed by Hanbury Brown and Twiss. Again, they observed a correlation between the arrival times of two photons in the detectors. Interpreting photons as indivisible particles, however, how could the HBT experimental results be possible? If one considered this concept of the photon, Hanbury Brown and Twiss should have observed an anti-correlation between photons since, as highlighted by

[85] M. Born, *Atomic Physics* (Darien, Conn., Hafner, 1970), on 82.

[86] P. A. M. Dirac, *The Principles of Quantum Mechanics* (Clarendon Press, Oxford, 1958), on 2.

[87] W. E. Lamb Jr., "Anti-Photon," Applied Physics B 60, no. 2-3 (1995): 77-84, on 84.

Dirac's textbook, "a fraction of a photon is never observed."[88] The understanding of photons as material particles constrained some physicists from looking forward, even after the development of the Bose-Einstein statistics. As highlighted by Lamb, "I think the confusion in people's minds about the nature of the photon and the electromagnetic field, stimulated emission, is -- the confusion is pretty widespread." It was Edward Purcell who made the step forward in the analysis of the HBT effect. Purcell interpreted photons as bosons, instead of material particles as electrons. In this case, photons from a thermal source, chaotic, tend to arrive at the mirror in bunches because of Bose-Einstein statistics. The HBT effect demonstrated what is nowadays called bunching of photons.[89]

Unlike Purcell's semi-classical approach, in which photons are bosons, Glauber shed new light on the concept of the photon. By answering what the definition of a photon is in quantum optics, he claimed: "Well, it is a dilemma… What is a photon? Is it a point particle? No. Is it a wave packet? Well, maybe… So, what is it? To me, it is mostly now just an excitation of a quantum state… I can't easily make pictures out of them, but I know how to do mathematics using the creation and annihilation operators."[90] The concept of the photon has become much more complex and complicated than a crude point particle interpretation of it. If one keeps this simplistic interpretation in mind, it is very difficult to understand, for example, the HBT experiment and Glauber's coherent states. The billiard ball model for a photon had a long life, between 1905 and 1956, because it gave physicists "a security blanket" in which it was much

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[88] Dirac, *The Principles of Quantum Mechanics* (ref. 86), 82.

[89] Silva and Freire Jr. (ref. 1); Interview of Willis Lamb by Joan L. Bromberg on 7 March 1985, Niels Bohr Library & Archive, American Institute of Physics, College Park, MD USA.

[90] Glauber's interview (ref. 16).

easier to make pictures of it.[91] However, after the 1960s the photon clearly became a mathematical tool from which it is far from straightforward to create images of it: How could one make picture of the creation and annihilation operators by defining what a photon is? In this context there is no good picture for a photon, but rather a very sophisticated mathematical formalism to represent the modern concept of the photon. In quantum optics, one knowsthat a photon is not an indivisible particle. By discussing the photon concept in terms of Glauber's theoretical achievements, Scully mentioned that "[h]e could connect a light quantum in the field with a click in the detector." In the detection experiments, for example, every time a single photon is absorbed the state of the field changed to $|n-1\rangle$ and the detector registered it. Thus, the creation and annihilation operators may change the state of the radiation field.

In the 1970s, the concept of the photon again became a subject of discussion. Feeling the need to reflect about what a photon might be, Scully and his colleague Murray Sargent III tried to bring up "a logically consistent definition of the word "photon" – a statement far more necessary than one might think for so many contradictory uses exist of this elusive beast."[92] If one thinks that after the development of quantum theory in the 1920s the photon concept became a well settled unity question in physics, this is a misconception. Indeed, reflections about this complex and controversial concept have entered the twenty-first century.[93] Physicists still try to

[91] The expression "security blanket" was used by Lamb in 1995 (ref. 83), 84.

[92] Marlan O. Scully and Murray Sargent III, "The Concept of the Photon," Physics Today 25, no. 3 (1972): 38-47, on 38.

[93] For an example of how physicists have put their energy into discussing the concept of the photon, see C. Roychoudhuri, A. F. Kracklauer, and K. Creath, eds., *The Nature of Light: What Is a Photon?*(Boca Raton; London; New York: CRC Press Taylor & Francis Group, 2008).

define or draw conclusions about the nature of photons, as in the case of Scully and Sargent III. Even before in 1961, the principal physicist working on Apollo Program's Lunar Laser Ranging Experiment, Carrol Alley wrote that “[w]e don’t know what a photon is,” according to the historian of science Bromberg.[94] Glauber also claimed with witticism that “I don’t know anything about photons, but I know one when I see one.”[95] All these same feelings concerning the nature of light show that the conceptualization of the object – photon – is far from straightforward. Scully and Sargent III also tried to suggest a concept for a photon by claiming that “the photon is a quantized excitation of the normal modes of the entire system,” a similar definition to Glauber’s. Scully and Sargent III therefore concluded that “[t]he photon concept as contained in the quantum theory of radiation provided the basis for explaining all known electromagnetic phenomena,” and highlighted an important aspect that remained in physicists’ mind for years: “‘the fuzzy-ball’ picture of a photon often leads to unnecessary confusion.”[96] Such a picture of a photon has constrained physicists from interpreting the HBT experiment and, if one keeps on embracing it, it would be extremely complicated to understand the modern concept of the photon which lies at the roots of the quantum theory of light.

**Epilogue**

[94] Interview of John F. Clauser by Joan L. Bromberg on 20 May 2002, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.

[95] Roy J. Glauber, in Marlan O. Scully, *Preface* (ref. 83), on *xv*.

[96] Scully and Sargent III, *The Concept of the Photon* (ref. 92), 47.

Glauber's quantum theory of light, while mathematically consistent, was not enough to convince some physicists about the indispensability of its use in optics. In fact, the question – was the quantization of the electromagnetic field indeed necessary in optical domain? – remained in the air from the mid-1960s on. By performing an experiment, the American physicist John F. Clauser tried to solve the dispute between the quantum and semi-classical predictions for the photoelectric effect.[97] By the time Clauser published his experimental results, the American physicists Nobel laureate Willis E. Lamb Jr. and Marlan O. Scully had discussed this phenomenon without considering the concept of the photon. Indeed, Lamb and Scully demonstrated that "the energy dependence of the ejected photoelectrons obeys the Einstein relationship even for a *classical* radiation field illuminating *quantized* atoms." They thus concluded that, in the photoelectric effect, radiation was described as a classical field and the detector (atomic electrons) was quantized, and hence "[t]he introduction of the photon concept is neither logically implied by nor necessary for the explanation of the photoelectric effect."[98] The debate about the necessity or not of the quantization of the electromagnetic field intensified significantly when the American physicist Edwin T. Jaynes, in collaboration with M. D. Crisp and C. R. Stroud, developed a neoclassical radiation theory (NCT), a more sophisticated version of the semi-classical approach. In both approaches, neoclassical and semi-classical, the electromagnetic field is described through the classical Maxwell equations and consequently

[97] John F. Clauser, "Experimental Distinction between the Quantum and Classical Field-Theoretic Predictions for the Photoelectric Effect," Physical Review D 9, no. 4 (1974): 853-60.

[98] W. E. Lamb, Jr. and M. O. Scully, "The Photoelectric Effect without Photon," in *Polarisation Matière Et Rayonnement*, edited by Societé Française de Physique (Paris: Presses Universitaires de France, 1969).

there is *no* need to consider the quantized fields in order to explain experimental results.[99] Even though Lamb and Scully had explained the photoelectric effect semi-classically, they were defenders of the validity of QED by dealing with fundamental problems regarding lasers.[100] Conversely, Crisp and Jaynes claimed that "in spite of the labors of two generations of theorists in improving the formulation of the theory and developing more powerful methods of calculation, present quantum electrodynamics contains many mathematical and logical difficulties… every calculation one encounters divergent and/or ambiguous integrals… ."[101] Jaynes's point of view was summarized later by Scully, by citing one of Jaynes's quotes, "[p]hysics goes forward on the shoulder of doubters, not believers. And I doubt that quantum electrodynamics is necessary."[102] The only way of resolving such a theoretical dispute was, according to Jaynes, to "[find] feasible optical experiments in which the differences between QED and semi-classical theory could be reduced to issues of fact rather than faith."[103] Clauser thus decided to perform an experimental test capable of distinguishing particles from waves, or QED from semi-classical predictions, concerning the photoelectric effect. "The most conspicuous difference between particles and waves is," according to Clauser, "that only particles may be localized." As up to that time no

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[99]M. D. Crisp and E. T. Jaynes, "Radiative Effects in Semiclassical Theory," Physical Review 179, no. 5 (1969): 1253-61.; C. R. Stroud, Jr. and E. T. Jaynes, "Long-Term Solutions in Semiclassical Radiation Theory," Physical Review A 1, no. 1 (1970): 106-121.

[100] Bromberg, *Device Physics Vis-À-Vis Fundamental Physics in Cold War America* (ref. 10), 243.

[101] Crisp and Jaynes, *Radiative Effects* (ref. 99), 1253.

[102] Scully's interview (ref. 83).

[103] E. T. Jaynes, Is QED Necessary?, in Proceedings of Second University of Rochester Conference on Coherence and Quantum Optics, edited by Leonard Mandel and Emil Wolf (New York: Plenum, 1966), on 22.

experimental apparatus was capable of displaying photons as localized particles, he noted the importance of such experimental results by claiming that "[t]hey are related to experiments which seek to determine whether or not nature may be viewed objectively… [i]t seems reasonable to assume that photons objectively exist, propagate, and in so doing carry information independently of external observers."[104] Clauser compared the predictions of the QFT and CFT for the case in which a single photon was falling on a half-silvered mirror. For the QFT, only *one* photoelectron would be detected in two separate atoms, while in the CFT there would be a probability of detecting two photoelectrons at the same time interval. Due to this clear distinction between the two predictions, by carrying out an experiment of that kind, it would be possible to verify the potentiality of one of the two theories. This could be achieved by considering that "the usual particle interpretation of photons… [in which a] particle must be either transmitted or reflected … [b]oth may be done simultaneously only by a wave." In 1974 Clauser embraced the same interpretation of the concept of the photon, as most physicists did in the late 1950s, which is a simplistic interpretation of it considering Glauber's quantum theory of light. This illustrates how complicated and delicate the photon concept is. Discussing the previous experimental results found previously and separately by A. Ádám, L. Jánossy and P. Varga, and by R. Hanbury Brown and R. Q. Twiss, Clauser pointed out that "none provides the desired distinction."[105] According to Clauser,

> That a photon is not split in two by a beam splitter is certainty "old hat," and it may seem surprising that we have gone to the effort to test this prediction experimentally. What is in

[104] John Clauser, "Localization of Photons," in *Coherence and Quantum Optics*, edited by Leonard Mandel and Emil Wolf (New York-London: Plenum Press, 1973), on 825.

[105] Clauser, *Experimental Distinction* (ref. 97), 853-54.

fact much more surprising is that evidently no such experimental test has heretofore been performed, and such tests are clearly of great importance.[106]

After performing a photon splitting experiment, using highly efficient photodetectors and an atomic cascade as a light source that emitted photons in pairs, Clauser observed *no* coincidence rates between photons confirming thus the QFT prediction for the photoelectron effect. He concluded his paper highlighting that "[t]he importance of experimentally demonstration phenomena which required a quantization of the electromagnetic field has been emphasized recently by a number of suggestions that such a quantization is unnecessary," and that "[m]any standard effects have thus been challenged as not providing definitive proof for the necessity of this quantization."[107] Clauser was referring to the NCT and the explanation of the photoelectron effect without photons, respectively. His results showed the contrary, however, that it was necessary to quantize the electromagnetic field.

Another experiment also brought to the fore the quantum nature of light and consequently the concept of the photon, which brought a new phenomenon to physics – the antibunching effect. In April 1972, the physicist David Stoler from the Polytechnic Institute of New York submitted a paper about "Photon Antibunching and Possible Ways to Observe It" to the Physical Review Letters. The opposite effect, photon bunching, was detected in 1956 by Hanbury Brown and Twiss and is widely known as the HBT effect. There are two ways of explaining it based on the quantum-mechanical or classical approaches: in terms of a photon clustering or of the stochastic process of radiation in natural light sources, respectively. Contrary to the HBT effect, "the

[106] Ibid., 855.

[107] Ibid., 858 and 859.

quantized character of the electromagnetic field is indispensable to a correct interpretation, namely, the negative Hanbury Brown-Twiss effect, also called the photon-antibunching effect or the anti-correlation effect." As claimed by Stoler, it was Glauber who predicted this effect and suggested that it could not be explained on the basis of an unquantized electromagnetic field. In this context, Stoler asked himself the question: "Why has thus this anticorrelation effect (ACE) not been observed?" In order to make it possible to display the effect, it would be necessary to create a state with a definite number of photons – an *n*-quantum state; however, "such states offer little hope of experimental realization." Stoler argued that it was much easier to demonstrate mathematically how to generate states with that characteristic than to produce them in the laboratory in order to observe the anticorrelation effect. As long as the variance of the photon number $(\Delta N)^2$ is less than the average photon number $\langle N \rangle$, $\Delta \equiv (\Delta N)^2 - \langle N \rangle < 0$, there will be a state of a single mode capable of displaying the ACE. Considering an arbitrary state in which $\Delta$ is positive or zero, one has to find an operator that by acting on this state will increase the value of $\langle N \rangle$, while the variance $(\Delta N)$ remains unchanged. The operator that has such a property is the phase operator $E_+ = a^\dagger (a^\dagger a + 1)^{1/2}$. Thus, Stoler demonstrates that by defining $|\psi\rangle = E_+ |\varphi\rangle$ for an arbitrary state $|\varphi\rangle$, one may obtain the conditions in which the states will possess the ACE as follows: $\langle N \rangle_\psi = \langle N \rangle_\varphi + 1$ and $(\Delta N)^2_\psi = (\Delta N)^2_\varphi$. Stoler also discussed how it would be possible to observe the antibunching effect. In order to create a state with $\Delta < 0$ experimentally, it would be necessary to have a device capable of conducting $\Delta$ to a negative value during some interval of time since the initial state of the field had $\Delta$ nonnegative. The degenerate parametric amplifier is the device, as suggested by Stoler, which "can generate states having the sort of correlations required to produce the ACE during a portion of its operation."[108]

[108] David Stoler, "Photon Antibunching and Possible Ways to Observe It," Physical Review Letters 33, no. 23

The physicists H. J. Carmichael and D. F. Walls, working at the University of Waikato, also predicted and discussed the photon-antibunching phenomena, highlighting that as antibunching was a purely QED prediction, by observing it one might test QED vis-à-vis the semiclassical approaches.[109] It was only in 1977 that the first observation of the antibunching effect was published. The experiment was carried out by two of Mandel's graduate students, H. Jeff Kimble and Mario Dagenais, who were working at the University of Rochester. Kimble, Dagenais, and Mandel highlighted the importance of detecting the antibunching effect, "its observation would provide rather direct evidence for existence of optical photons, unlike positive correlations effects that have a semi-classical explanation." In other words, if the antibunching of photons were observed, there would be no doubt about the necessity of the quantization of the electromagnetic field. In Kimble and his colleagues' experiment, an atomic beam of sodium atoms was used which was prepared by optical pumping to guarantee a pure two-level transition, then these atoms were irradiated by a dye laser, and finally the laser beam was divided by a beam splitter. After performing such an experiment and comparing the agreement between the quantum field theory and observations, Kimble, Dagenais, and Mandel stated that "[t]he quantum nature of the radiation field and the quantum jump in emission, which are of course inextricably connected, are therefore both manifest in these photoelectric correlation

(1974): 1397-1400, on 1397.

[109] H. J. Carmichael and D. F. Walls, "Proposal for the Measurement of the Resonant Stark Effect by Photon Correlation Techniques," Jornal of Physics B: Atomic and Molecular Physics 9, no. 4 (1976): L43; H. J. Carmichael and D. F. Walls, "A Quantum-Mechanical Master Equation Treatment of the Dynamical Stark Effect," Journal of Physics B: Atomic and Molecular Physics 9, no. 8 (1976): 1199.

measurements."[110] Photon antibunching was also observed in Munich by G. Leuchs, M. Rateike, and H. Walther, according to Walls.[111] The condition required to display photon antibunching is that in which the intensity correlation is given by $g^{(2)} (0) \sim 0$ with $\tau \sim 0$ for a single mode field using a coherent light, a characteristic of the quantum nature of light, in which "the atom emits a photon at time $t$ and is unable to radiate again immediately after having made a quantum jump down to the lower state." In the case of the HBT experiment, when a chaotic light is used, the theoretical value of the intensity correlation is $g^{(2)} (0) = 2$ which generates the positive correlation or photon bunching. The physicist Peter Knight from the University of London pointed out that "[w]hereas most optical coherence and correlation experiments have an adequate semiclassical interpretation, antibunching requires quantisation of the emitted fluorescence and the idea of a 'quantum jump.' "[112] Both Kimble and co-workers and Leuchs et al.'s observations evidenced the quantum nature of light and so the necessity of using the quantum theory of light to explain them.[113]

The experimental results obtained by Clauser and Kimble et al. revealed the non-classical effects in the statistical properties of light. Nevertheless, "there has still been no test of the conceptually very simple situation dealing with single-photon states of the light impinging on a beam splitter," since Clauser and Kimble had used an attenuated light source. Such a test could verify the prediction of quantum theory experimentally, according to which, one would observe a

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[110] H. J. Kimble, M. Dagenais, and L. Mandel, "Photon Antibunching in Resonance Fluorescence," Physical Review Letters 39, no. 11 (1977): 691-95, on 692 and 694.

[111] D. F. Walls, "Evidence for the Quantum Nature of Light," Nature 280, no. (1979): 451-54.

[112] Peter Knight, "Observation of Photon Antibunching," Nature 269, no. 5630 (1977): 647.

[113] Walls, *Evidence for the Quantum Nature of Light* (ref. 111), 454.

significant anticorrelation between photons after traversing a beam splitter since “a single-photon can only be detected once!”[114] In the late 1970s, a physics course given by the French physicist Claude Cohen-Tannoudji (1933- ), whose principal aim was to answer if one could “dispense the concept of the photon at least in the optical domain,” inspired the French physicist Alain Aspect, well-known for his extraordinary experiment on Bell’s inequalities, to perform an experiment of that kind.[115] As remarked later by Aspect, “Cohen-Tannoudji was giving his lecture at Collège de France and it was describing the experiment of Kimble, Mandel and Dagenais, photon antibunching, and listening to this I got the idea that maybe we could do a source of single-photon.” While Aspect was sharing such a possibility with his colleagues, they surprisingly and uncannily highlighted “single-photon?! … we don’t even understand what you mean.” Yet, Aspect’s colleagues stated that the only person who could help to create a single-photon source was Cohen-Tannoudji himself. In a meeting with Cohen-Tannoudji, Aspect then explained his idea of carrying out an experiment with single-photons. Either because Aspect’s idea was unclear or because Cohen-Tannoudji did not pay enough attention to it, the idea did not go forward. Approximately five years later, however, Aspect suggested a doctoral project on performing single-photonexperiments to Philippe Grangier who accepted the challenge. Influenced by his work on Bell’s inequalities, Aspect found another inequality to be violated, the Cauchy-Schwarz inequality, but now in the context of another experiment.[116] The Cauchy-

[114] P. Grangier, G. Roger and A. Aspect, “Experimental Evidence for Photon Anticorrelation Effect on a Beam Splitter: A New Light on Single-Photon Interferences,” Europhysics Letters 1, no. 4 (1986):173.

[115] C. Cohen-Tannoudji, Emission et détection de rayonnement: approches semi-classiques et approches quantiques, Lecture, Collège de France, Paris, 1979-1980, on I-5.

[116] Aspect’s interview (ref. 76).

Schwarz inequality states that the probability of detecting classical coincidences is always greater than 'accidental coincidences.' If such an inequality were violated, and the anticoincidence probability greater than classical coincidences, it would then show the anticorrelation effect, a non-classical phenomenon. In this vein Grangier with G. Roger and Aspect designed an experiment in which a two-photon radiative cascade was capable of emitting pairs of photons with different frequencies $v_1$ and $v_2$, the atoms were excited through two single line lasers at different frequencies. In the Grangier-Roger-Aspect (GRA) experimental set up, the detection of the first single-photon with frequency $v_1$ activated the two photomultipliers to detect $v_2$ during a time interval. By doing this they could be sure that only a single photon would be split by a mirror since the first photon would be captured in order to open the gate. The photomultipliers were able to detect only single-photons after being split by a mirror. The guarantee that GRA's experiment was dealing with single-photons concerned the fact that "[d]uring the gate, the probability for the detection of a photon $v_2$, coming from the same atom that emitted $v_1$, is much bigger than the probability of detecting a photon $v_2$ emitted by any other atom in the source."[117] This was the closest to an ideal single-photon state. Grangier, Roger and Aspect performed two similar experiments using an atomic cascade as a light source and a triggered detection process for the second photon of the cascade. In the first experiment in which single photons were arriving at the beam splitter and then detected through two photomultipliers, they observed that "the light emitted after each 'triggering' pulse has been shown to exhibit a specifically quantum anticorrelation behavior," which agreed with the quantum theory of single-photon states. The second one was a Mach-Zehnder type-interferometer, but now operating with single-photons, in which a light source was split by a half-silvered mirror and its components

[117] Grangier, Roger and Aspect, *Experimental Evidence for Photon Anticorrelation Effect* (ref. 114), 174-175.

were reflected by two mirrors; then the light passed a second half-silvered mirror and finally was detected through two photomultipliers. After carrying out this experiment, interference fringes with a visibility of approximately one hundred per cent were observed.[118] With these experiments, Grangier, Roger and Aspect brought to the fore once again the wave-particle duality of light in 1986. They concluded their article by proposing two ways of interpreting their results. "[I]f we want to use classical concepts, pictures, to interpret these experiments," as claimed by GRA, "we must use a particle picture for the first one (the photons are not split on a beam splitter), since we violate an inequality holding for any classical wave model. On the contrary, we are compelled to use a wave picture (the electromagnetic field is coherently split on a beam splitter) to interpret the second (interference) experiment." That is, they were suggesting the use of complementarity to evidence the corpuscular nature of light or the wave one in each experimental setup, but not both natures in the same experiment.[119] It is important to highlight that "the problem of incompatible description arises only if we insist on using classical concepts such as waves or particles." Conversely, if one considers quantum optics, "there is a unique description of light" based on the coherent states,[120] and the use of classical pictures is totally inadequate.

[118] Ibid., 177.

[119] Ibid., 178-179

[120] A. Aspect and P. Grangier, "Wave-Particle Duality for Single Photons," Hyperfine Interactions 37, no. 1-4 (1987): 1-17, on 15.

In addition, it should be noted that at the same time other physicists were developing a source of single states. This was the case of Chung Ki Hong and Mandel in 1986.[121]

As phenomena such as the photoelectric effect, spontaneous emission and the Lamb shift could be explained with suitable approximation through semi-classical and neoclassical theories, physicists questioned the need to use the quantum theory of light to elucidate them. Even the HBT effect might be dealt with by considering the classical description of the electromagnetic field. In the 1970s and 1980s, this scenario changed with the observations of Clauser, Rochester's group, and Grangier, Roger and Aspect. Their experimental results demonstrated the strictly quantum character of light. Clauser and the French group's experiments violated the semiclassical inequality, called Cauchy-Schwartz inequality, for photon anticorrelation experiments. Clauser demonstrated that the detection of two photons was separated in time with the use of an atomic cascade. As for Grangier, Roger and Aspect, they observed an anticorrelation between single photons after traversing a beam splitter. Their light source was "the first excited state of the quantized radiation field, containing only one quantum of energy."[122] In the case of Rochester's group, it was demonstrated that photons from a single sodium atom are detected separately in space, giving rise to the antibunching effect. These two experiments became a turning point in physics. Now, if one asks: "Is QED necessary?" Or even, is the concept of the photon necessary? The immediate response is that it is not only necessary, but indispensable. Besides the peculiar differences between their experimental apparatus, the kind of source used also played an important role. As discussed by Sulcs,

[121] C. K. Hong and L. Mandel, "Experimental realization of a localized one-photon state," Physical Review Letters 56, no. 1 (1986): 58-60.

[122] Philippe Grangier, "Experiments with single photons," Seminaire Poincaré 2, (2005): 1-26.

> Quantum opticians think of light as being ‘classical’ or ‘quantum.’ A ‘quantum light’ beam contains a small, well-defined number of clearly separated photons… It is difficult to find in the literature explicit sufficient conditions for a ‘quantum’ source, but most quantum opticians define it in terms of sub-Poisson counting statistics. Super-Poisson light and bunched photons are kinds of ‘classical light’… The optical experiments performed prior to that of Grangier et al. (1986)… made use of kinds of light sources which would nowadays be classified as ‘classical’… Classical light also included all high intensity beams including coherent ones from a laser, and all other kinds of light in which the number of photons is indefinite.”[123]

It seems that as the photon concept has become such a fundamental tool in modern physics physicists even nowadays still work on fundamental questions related to the nature of the light. Since 2003 The International Society for Optics and Photonics (SPIE), for example, has organized conferences to discuss what a photon is. The physicists C. Roychoudhuri, A. F. Kracklauer, and K. Creath, after selecting several papers published in conference proceedings and journals, also published the book titled The Nature of Light: What is a Photon? . “This book is an attempt,” according to the editors, “to rekindle active interest by both aspiring scientists… and practicing scientists in the nature of light – an unresolved issue in the field of physics.” Roychoudhuri, Kracklauer and Creath continue, “[m]any fundamental issues pertaining to light

[123] Sue Sulcs, “The Nature of Light and Twentieth Century Experimental Physics,” Foundations of Physics 8, (2003): 365-91, on 370-371.

persist; they should be explored and understood, hopefully *inter alia* opening up many new applications."[124]

It seems that *a* definition for the photon remains open even in the twentieth-first century, although in Quantum Optics it is described as an excitation of a quantum state. Acknowledging the conceptual difficulty underlying the concept of the photon, Albert Einstein wrote in 1951 that "[a]ll these fifty years of conscious brooding have brought me no nearer to the answer to the question, 'What are light quanta?' Nowadays every Tom, Dick and Harry thinks he knows it, but he is mistaken."[125] Rephrasing Einstein's quotation, I conclude this chapter by stating that even more than one hundred years after its birth, the concept of the photon is *still* a black-box in the vanguard of modern and contemporary physics.

[124] C. Roychoudhuri, A. F. Kracklauer, and K. Creath, eds., *The Nature of Light: What Is a Photon?*(Boca Raton; London; New York: CRC Press Taylor & Francis Group, 2008), on xiii.

[125] Albert Einstein to Michele Besso [Dec. 1951], in Everett Mendelsohn (ed.), Transformation and Tradition in the Sciences: Essays in Honor of I. Bernard Cohen (Cambridge University Press: Cambridge, 2003).

## Understanding Scientific Practice: Some Considerations

This conclusion reflects on the following question: What can we learn about scientific practice through the controversies presented in the preceding chapters? I would like to begin with the scientific collaboration between the engineer and experimenter Robert Hanbury Brown and the theorist Richard Quentin Twiss. Both had worked on radar during WWII before they started working together in the 1950s. One of the most significant lessons from wartime was how fruitful and successful a research program could be when both theorists and experimenters were put together to solve a specific problem. It was the case of the atomic bomb and radar, fruit of the scientific interaction between theorists, experimenters, engineers, and instrument makers.[1]

Hanbury Brown had certainly learned the lesson. By the time he decided to construct an intensity interferometer, Hanbury Brown had envisioned how to proceed in terms of experimental apparatus. "Hanbury was a superb and imaginative engineer, a natural astronomer, and a true visionary."[2] His idea of putting tapes on the antennas (spaced at a very long distances) to record the signals separately so as to correlate the signals demonstrated his experimental skills and inventive thinking. Even though he knew that he had the skills to build the new kind of interferometer, Hanbury Brown needed to be certain that it would be sensitive enough to measure the angular diameter of the radio stars. He could have read up on the mathematics underlying what he wanted to construct. Acknowledging the importance of scientific

[1] Peter Galison, *Image & Logic: A Material Culture of Microphysics* (Chicago: The University of Chicago Press, 1997), on 781.

[2] Bernard Lovell and Robert M. May, Obituary: Robert Hanbury Brown (1916–2002), *Nature* **416**, 34 (2002).

collaboration instead, Hanbury Brown started looking for a theorist who could work on the idea of his interferometer. He knew his limitations regarding mathematics and the new instrument would require a very sophisticated mathematical approach. The collaboration between Hanbury Brown and Twiss then started.

While Hanbury Brown and his graduate students were working on building the new interferometer, Twiss worked on mathematics of the new apparatus. They created a scientific network surrounding the new instrument. Hanbury Brown and Twiss were working at different institutions in distinct cities; the former in Manchester, and the latter in Baldock. The distance between the two cities is approximately 293 Km. In order to maintain the collaboration, Twiss visited Jodrell Bank quite often and was always in correspondence with Hanbury Brown. They established what the historian and philosopher of science Peter Galison would call a "trading zone" – a site in which engineers, experimenters, and theorists, even from different subcultures, can collaborate.

In his book "Image and Logic: A Material Culture of Microphysics," Galison introduced the idea of a "trading zone" by analyzing "the culture of experimental science in the twentieth century and the complexities of its interaction to the wider cultural spheres of theory, industry, warfare, professional, identity, and philosophical inquiry" related to elementary particle physics.[3] The notion of a trading zone was created to criticize Thomas Kuhn's definition of incommensurability, according to which different communities from successive theories have their particular way of explaining and describing phenomena, unable to dialogue with one another. However, Galison defends the idea that even though distinct communities – theorists, experimenters, instrument makers, and engineers – have peculiarities in their way of thinking

[3] Galison, *Imagine & Logic* (ref. 1), 62.

and how to proceed in a particular situation, there is a zone in which it is possible for them to dialogue and work in collaboration.[4] Furthermore, Galison criticizes the logical positivists who claimed in the 1920s and 1930s that "unification underlies the coherence and stability of the sciences," and the antipositivists who defended that "disunification implies instability" in the 1950s and 1960s. In contrast, Galison argues in favor of the disunification of science – physical sciences can not be seen as a homogeneous community – a unity, but rather as composed of many subcultures each with their own peculiarities and beliefs.[5]

As discussed by Galison, "[e]ach subculture has its own rhythms of change, each has its own standards of demonstration, and each is embedded differently in the wider culture of institutions, practices, inventions, and ideas," but "[t]he culture they partially construct at the junction is what I have in mind by the "trading zone." "[6] In the case of Hanbury Brown and Twiss, they were able to create a "zone of exchange" by working together on the intensity interferometer. Hanbury Brown constructing the apparatus; Twiss developing the theory. Each contributed to the other's subculture – the traditions of experimenting and theorizing – without losing their distinct identities and practices. After the collaboration, Hanbury Brown did not become a theorist, nor did Twiss become an experimenter. In contrast, they did know how to explore the skills, ideas, inventions, and the knowledge of each other to construct the new interferometer. The success of the intensity interferometer, working properly in a turbulent medium, motivated Hanbury Brown and Twiss to go further. Using the same theoretical

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[4] Galison, *Image & Logic* (ref. 1), 783.

[5] Ibid., 781-782. See also, *The Disunity of Science: Boundaries, Contexts, and Power*, Peter Galison and David J. Stump (eds.), (Stanford, California: Stanford California University Press, 1996).

[6] Galison, *Imagine & Logic* (ref. 1), 798.

principles and adapting the experimental apparatus slightly, they thought of constructing an interferometer capable of working at visible wavelengths. After publishing the results of a laboratory test to verify whether or not the same theoretical model could be used, a controversy began.

During the controversy, experimenters and theorists around the world established a new trading zone by trying to understand and interpret the HBT results. The different scientific background of Edward Purcell, Eric Brannen, Peter Fellgett, and Richard Sillitto, coming from distinct subcultures of physics, resulted in different explanations and interpretations of the results. For instance, Purcell beautifully explained the HBT experiment by considering quantum statistics and described photons as bosons, confirming thus the validity of the HBT observation. Purcell established a zone of exchange between two communities – physicists and astronomers – creating a meeting point where physicists could understand the HBT experiment in a different way. Looking at Purcell's equation and the HBT one, even though they are formally very different from each other, they represent the same phenomenon. The significant difference between the theoretical formulations is a result of the protagonists' training and scientific background. After learning about Purcell's equation in the HBT experiment, Twiss himself tried to derive the same expression and exchanged ideas and practices with Purcell through correspondence.

The case of Purcell seems very similar to Schwinger's analyzed by Galison. "In short, the war forced theoretical physicist – such as Schwinger – to spend day after day calculating things about devices, through these material objects, liking their own prior language of field theory to the language and algebra of electrical engineering."[7] Purcell linked the language of

[7] Galison, *Imagine & Logic* (ref. 1), 824.

quantum statistics to the language of radio engineering since HBT's used the same principle of their radio interferometer in the laboratory test. Twiss wrote to Purcell saying that initially he tried to use concepts from quantum theory as Purcell did, but he was not certain about which concept of the photon should be considered. It was Rosenfeld who helped him by giving a "sort of language" based on complementarity. Hanbury Brown and Twiss then interpreted their results as a wave phenomenon. This illustrates what Galison said: "even specialties within physics cannot be considered homogeneous communities," with their own way of solving or interpreting even the same phenomenon.[8] In this context, "[i]nterpretations could conflict, or could come to consensus, but this intermediate set of linguistic and procedural practices bound experiments, instrument makers, and theorists in collaboration."[9] Collaborating with Purcell, even at a distance, Twiss tried to learn a different way of looking at the HBT results. On 29 November 1956, he wrote to Purcell, "[t]he reason for my writing you is not very serious, but I cannot get the same answer as you do in deriving the "coherence length" for light with a rectangular bandwidth."[10] When Twiss realized that he did not find the same theoretical formulation to the HBT problem as Purcell, Twiss immediately decided to open a line of collaboration with him in order to acquire new theoretical practices.

Unlike Purcell who helped Hanbury Brown and Twiss to solve the controversy, Fellgett criticized their results by considering the thermodynamics conceptual scheme. His arguments against the HBT results were based on Fellgett's previous studies on fluctuations in a body of

[8] Ibid., 782.

[9] Ibid., 805.

[10] Twiss to Purcell, on 29 Nov 1956, Edward Purcell's Personal Archive, Harvard University Archive, Cambridge, MA.

emissivity in an enclosure. In response to Fellgett's criticism, Twiss studied thermodynamics to be familiar with Fellgett's arguments. The disagreement between Fellgett and HBT was so intense that Clark Jones, who was part of the subculture of thermodynamics as Fellgett, created a scientific network to discuss their results by circulating a report among physicists with the principal differences between their formulations. Fellgett, Clark Jones and Twiss, exemplifying the dynamics of scientific practice, solved the controversy publishing a paper together in 1959, even though Fellgett and Twiss had fought strongly with each other. The examples of Hanbury Brown, Twiss, Purcell, and Fellgett collaborating with each other illustrate the disunified traditions of experimenting and theorizing surrounding the HBT experimental results.

The discussions about the concept of the photon brought different interpretations of it to the fore. As Galison emphasized, "[l]ike two cultures distinct but living near enough to trade, they can share some activities while diverging on many others." He continues, "[w]hat is crucial is that in the local context of the trading zone, despite the differences in classification, significance, and standards of demonstration, the two groups can collaborate."[11] This is clear with the concept of the photon. Each subculture of physics had its own way of representing the photon – which did not have the same meaning for an experimenter, a theorist, or an engineer. Hanbury Brown and Twiss described light as waves, which would never cause trouble working in the field of radioastronomy or astronomy; conversely, Jánossy described photons as small, indivisible and localized particles; Purcell represented photons as bosons, while Sillitto described photons as wave-packets; and Glauber defined a photon as an excitation of a quantum state. An experimenter would thus use a very different concept for the photon from his fellow theorists, and vice and versa. Although there was a "global" distinction among these definitions for a

[11] Galison, *Imagine & Logic* (ref. 1), 803.

photon, “there remains a localized zone of activity in which a restricted set of actions and beliefs is deployed.”[12] In the case of the HBT experiment, the possibility of establishing a trading zone in which different protagonists from distinct subcultures of physics and astronomy contributed to the strength of the wider culture of science, such as the development of the field of astronomy and of quantum optics. The controversy surrounding the HBT results illustrates thus Galison’s arguments: “It is the disorder of the scientific community – the laminated, finite, partially independent strata supporting one another; it is the disunification of science – the intercalation of different patterns of argument – that is responsible for its strength and coherence.”[13]

Let’s now turn our attention to the case of Glauber and the Rochester group. The dynamics of scientific practice is quite a delicate issue as there is a scientific priority dispute and accusations of using the other’s achievements improperly. Unfortunately, without checking the personal archives, it is difficult to reflect on scientific practice. The accusations and disputes require a sociological analysis. Bromberg has highlighted this: “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

[12] Galison, *Imagine & Logic* (ref. 1), 813.

[13] Ibid., 844.

unnecessarily aggressive as Wolf thought him?"[14] It was a very different situation from the HBT controversy. The protagonists brought the controversy elegantly to an end by writing papers together to demonstrate their agreements on the HBT results, and by exchanging correspondence acknowledging the results as an effect in physics. As far as I know, Bromberg too, this kind of waving the scientific white flag never occurred in the context of Glauber and Wolf.

As Glauber later remarked, "[Wolf and Mandel] never admitted making a mistake… they just insisted they turned into a different argument." Looking at their interviews, there is clearly a mutual feeling of resentment . While Glauber saw Wolf as the Emperor of Optics, "Wolf himself became rather imperious figure having done this [by publishing the Principles of Optics with Max Born] and saw himself as dominating optics completely," Wolf suggested that Glauber's attacks were a way of being publically known. "I think extremely highly of Glauber's work, but I think he got to be very well known by what I would consider — well, to put it mildly, not exactly gentlemanly types of procedures. He would have got the same publicity eventually because he was very good without all these attacks."[15]

Wolf turned to the problem of coherence in the early 1950s when he was working on the book "Principles of Optics" with Max Born. As Wolf later remarked,

> One day I received a letter from Born in which he asked me why the manuscript was not yet finished. I wrote back saying that the manuscript is almost completed, except

[14] Joan Lisa Bromberg, "Modelling the Hanbury Brown – Twiss Effect: The Mid-Twentieth Century Revolution in Optics," available in http://quantum-history.mpiwg-berlin.mpg.de/news/workshops/hq3/hq3_talks/22_bromberg.pdf. Accessed on 18 Aug 2013, on 15.

[15] Interview of Emil Wolf by Joan Lisa Bromberg on 23 Sept 1984, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA.

> for a chapter on partial coherence on which I was still working. Born replied at once, "Wolf, who apart from you is interested in coherence? Leave the chapter out and send the manuscript to the printers."[16]

Looking at his scientific trajectory, it was obvious that Wolf would be the individual who would construct the quantum theory of coherence. He was not. There is no natural tendency in the scientific dynamic. Glauber, who was outside the field of coherence and even optics, was the one to do it.

A competitive struggle then started between Glauber and Wolf when Glauber openly criticized Wolf and Mandel's model for a maser, intensifying when Glauber received two negative referees at the Paris conference (according to Glauber, it was Wolf who gave them). It was not a struggle between one renowned physicist, Wolf, and one new entrant in the profession, Glauber. Actually, considering the Web of Science Databases, between 1945 and 1960, Wolf had published thirty papers, while Glauber twenty-six. The number of citations of Wolf's paper each year, considering that same time period, was approximately 107 times; in comparison, Glauber's paper was cited 144 times. Both Wolf and Mandel had the same kind of recognition as theoretical physicists in terms of citations and the number of papers published at the time. All Wolf's published articles were related to the optical field as well as the publication of his classic book on optics with Born. He was also part of The Institute of Optics at the University of Rochester. All these facts contributed to making Wolf a distinguished physicist with high prestige in the optical community. "In the 1920s, Rochester was undoubtedly the optical center of the United States," where there were the optical companies such as The Bausch & Lomb

[16] Emil Wolf, "Early days of coherence theory and the first Rochester conference on coherence," Journal of the European Optical Society - Rapid publications, v. 5, 10044s, 2010, on 159.

Optical Company, The Eastman Kodak Company, Wollensak, Ilex, Folmer Graflex, among others.[17] The creation of The Institute of Applied Optics in 1929, which had its name changed to The Institute of Optics in 1939, was financially supported initially by the companies Kodak and Bausch & Lomb. The interest of these companies were evident: "Since there was no college or university in the country offering specialized courses or university in applied physics, it was natural that these Rochester companies should consider the desirability of forming an optics department at the University, to supply their own needs for optically trained personnel."[18] By the time that Wolf had become a faculty of the Rochester University in 1959, The Institute of Optics had granted 155 bachelor's, 65 master's, and 19 doctor's degree. Hence, Wolf was at the most prestigious research center dedicated exclusively to optical sciences.

According to the sociologist of science Pierre Bourdieu, Wolf was dominant in the field due to his prestige, recognition, and contributions to optics. He was thus the one who had scientific authority. Conversely, Glauber was the newcomer – who was the competitor by challenging the dominant and who had come from *another* field, quantum field theory and nuclear physics. Bourdieu defends the idea that "[t]he pure universe of even the "purest" science is a social field like any other, with its distribution of power and its monopolies, its struggles and strategies, interests and profits, but it is a field in which all these *invariants* take on specific forms."[19] In this context, the dispute between Wolf and Glauber was a competitive struggle for

[17] R. Kingslake and H. G. Kingslake, "A History of The Institute of Optics," Applied Optics 9, no. 4 (1970): 789-796, on 789.

[18] Ibid.

[19] Pierre Bourdieu, "The specificity of the scientific field and the social conditions of the progress of reason," *Soc. sci. inform.* 14, no. 6 (1975): 19-47, on 19.

scientific authority – "a particular kind of social capital which gives power over the constitutive mechanisms of the field, and can be reconverted into other forms of capital, owes its specificity to the fact that the producers tend to have no possible clients other than their competitors."[20] As social as any field, Bourdieu defines the scientific field as

> the locus of a competitive struggle, in which the *specific* issue at stake is the monopoly of *scientific authority*, defined inseparably as technical capacity and social power, or, to put it another way, the monopoly of *scientific competence*, in the sense of a particular agent's socially recognized capacity to speak and act legitimately (i.e. in an authorized and authoritative way) in scientific matters.[21]

If Wolf did try to block Glauber's paper - one needs to look at the journal's archives to be certain - he was trying to use his scientific authority (prestige and recognition) to stop what would come next, a controversy or competitive struggle. In the scientific field, the competitors have to embrace "antagonistic strategies" compared to their rival's work in order to solve problems that are at stake.[22] This is a way of showing their intellectual competence. In fact, Glauber and Wolf solved the problem of coherence differently to demarcate their scientific capital and to acquire power in the field. While the newcomer Glauber was defending the necessity of quantizing the electromagnetic field to define coherence, the dominant Wolf claimed that "Glauber's definition of higher coherence, complete coherence, higher complete coherence is useless," then using a

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[20] Bourdieu, *The specificity of the scientific field* (ref. 19), 23.

[21] Ibid., 19.

[22] Ibid., 29.

semi-classical approach for coherence. Glauber used what Bourdieu defines as a “subversion strategy,”

> infinitely more costly and more hazardous investments which will not bring them the profits accruing to the holders of the monopoly of scientific legitimacy unless they can achieve a complete redefinition of the principles legitimating domination: newcomers who refuse the beaten tracks cannot “beat the dominant at their own game” unless they make additional, strictly scientific investments from which they cannot expect high profits, at least in the short run, since the whole logic of the system is against them.[23]

That is why Glauber introduced a new sophisticated mathematical structure – QED methods into optics – for a problem that Wolf had been working on classically since the 1950s. Glauber knew that he had the scientific capital to face the Rochester group: under-graduation and graduation at Harvard University, he had been part of the Manhattan Project, worked with quantum field theory, had become a Professor at Harvard University, and had published as many papers as Wolf, so as good a theoretical physicist as Wolf. If Glauber wanted to achieve recognition in the optical field, he had no choice other than come out with a distinct way to solve the problem of coherence, which would differ him from Wolf’s work.

As Wolf did not have the same scientific training as Glauber regarding the quantum field theory, Wolf’s strategy to was to invite Surdarshan, who was familiar with QFT, to work on coherence. Wolf certainly did want to maintain his scientific capital. What happened was not only a scientific struggle, but also a struggle for scientific authority, to use Bourdieu’s terms. The scientific field, “as the locus of political struggle for scientific domination,” is a social

[23] Bourdieu, *The specificity of the scientific field* (ref. 19), 30.

characteristic of the scientific practice. [24] Competitive struggle is part of any field, and it is no different in science. Scientific production has also benefitted from such struggles. If Glauber had accepted the scientific authority coming from the negative referees, there would not have been any controversy; and the birth of quantum optics would maybe have been deferred.

I would like to discuss another issue. Following the debates about the concept of the photon, two philosophical points of view have come to the fore: realism and anti-realism. The viewpoint of realism defends the idea that all theoretical entities – such as photons, electrons, and protons – indeed exist in nature; whereas anti-realism describes these entities as "fictions, logical constructions, or part of an intellectual instrument for reasoning about the world."[25] The creation of a mental picture for a photon, as a small, indivisible and localized particle, constrained physicists to interpret the HBT results, as in the case of Jánossy, Brannen, and Ferguson. Instead of thinking about the ontology of the photon, Purcell embraced mathematical arguments of quantum mechanics and described photons as bosons. Acknowledging the difficulty in creating a picture for a photon, Glauber claimed that at least he knew how to do mathematics with the annihilation and creation operators.

Another meaningful illustration of realism vis-à-vis anti-realism regarding the photon is from Alain Aspect. His explanation of the single-photon experiment used two different and alternative approaches:  on the one hand, pictures such as particles and waves; on the other, the mathematical structure of the single-photon states. If one insists on using a picture for a photon, it will then be necessary to embrace complementarity and consequently treat photons as a

[24] Bourdieu, *The specificity of the scientific field* (ref. 19), 22.

[25] Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983), on 27.

classical particle. However, if one does not care at all about creating images for this entity, the photon becomes only a mathematical tool. It seems then that if one wants to understand the modern concept of the photon from Glauber's quantum theory of light, it will be necessary to consider a photon as a logical construction representing an excitation of a quantum state. It is quite impossible to create a picture capable of reproducing the photon in such a way.

The discussions on what the photon is, or how to represent it, are still open in the physics community. The physicist David Kinkelstein of Georgia Institute of Technology claimed that "[f]rom the point of view of experience, "What is a photon?" is not the best first question. We never experience a photon as it "is." For example, we never see a photon in the sense that we see an apple, by scattering diffuse light off it and forming an image of it on our retina. What we experience is what photons do. A better first question is "What do photons do?" After we answer this we can define what photons are, if we still wish do, by what they do."[26] Revisiting the concept of the photon, the physicists Ashok Muthukrishnan from Texas A&M University, Marlan O. Scully of Princeton University, and M. Suhail Zubairy at Quaid-i-Azam University highlighted that "subsequent developments required us to envision the photon as an intrinsically quantum mechanical entity," and defined photons by saying that "[a] photon is what a photodetector detects."[27] The authors used Richard Feyman's words, "nobody knows, and its best if you try not to think about it," to claim that "[t]his is a good advice if you have a picture of

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[26] David Finkelstein, What is a Photon?, in C. Roychoudhuri, A. F. Kracklauer, and K. Creath, eds., *The Nature of Light: What Is a Photon?* (Boca Raton; London; New York: CRC Press Taylor & Francis Group, 2008), on 23.

[27] Ashok Muthukrishnan, Marlan O. Scully, and M. Suhail Zubairy, The Concept of the Photon – Revisited, in C. Roychoudhuri, A. F. Kracklauer, and K. Creath, eds., *The Nature of Light: What Is a Photon?*(Boca Raton; London; New York: CRC Press Taylor & Francis Group, 2008), on 37 and 39.

a single photon as a particle. On the other hand, if you think of the photon as nothing more or less than a single quantum excitation of the appropriate normal mode, then things are not so mysterious, and in some sense intuitively obvious."[28]

It seems therefore that physicists have learned how to be anti-realistic regarding the photon. Otherwise, it would be far from straightforward to understand or interpret the theoretical and experimental achievements in modern physics considering its modern definition.

[28] Ibid., 53.