Re: The time it takes to emit one photon

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- *From*: "nightlight" <nightlight@xxxxxxxxxxxxxx>
- *Date*: Fri, 23 Sep 2005 19:56:29 +0000 (UTC)

> DOUBLE−SLIT
>
> This is called interference. If you think this experiment
> can be explained without the notion of photons and their
> quantum−mechanical behavior, I would like to hear your
> explanation.
>

The classical EM field produces the same interference pattern of intensity variation across the screen. Therefore the interference pattern alone would be entirely unsurprising to Maxwell. How would you surprise him? A simple detector which thresholds the classical field intensity values (of the incident field superposed with the 1/2 hv vacuum fluctuations per mode) into 1−bit approximation (above threshold: 1=trigger, below threshold: 0=non−trigger) will give you exactly the counts that you will observe.

In order to surprise Maxwell with this experiment you would need to place an array of detectors at the screen and show that triggers of detectors show subpoissonian trigger statistics (e.g. enhanced mutual exclusivity of 2 points A and B so that probability P(AB) of coincident triggers at A and B is smaller than what independent coincident triggers p(A)*p(B) imply). Otherwise (for poissonian or superpossonian trigger statistics) he could easily explain the discrete single triggers as result of simple thresholding of the photocurrents at the detectors (i.e. they get approximated with 1−bit precision; that is precisely what pulse analyzer−discriminator, PAD, the electronic component of a photodetector does; see Bykov papers [1] on the origins and nature of the photo−detection discreteness).

To see why you need additional exclusivity, consider a simple (but very close) analogy. Arrange onto a rectangle a list of all cities buying lottery tickets so that the average ticket sales viewed across the rectangle form an interference−like pattern, say, matching exactly your double−slit interference pattern. The probability of a guess is such that on average n tickets wins per week (a lottery cycle). For any given week there will be k winners with probability given by Poisson distribution:

p(k,n) = [n^k/k!] exp(−n) ... (1)

For example, if the odds are tuned so that on everage 1 player wins per week (n=1) you will have odds of no winners p(k=0,n=1) = 1/e = 36.8%, odds of exactly 1 winner as p(k=1,1) = 1/e = 36.8% and the odds of more than 1 winner as p(k>1,1) = 1−2/e = 26.4%. The ratio of single to multiple wins is 1.4, which doesn't seem very exclusive. But if you reduce the average number of wins tenfold, hence n=0.1, you will have the odds of no winners p(k=0,.1) = 1/e^.1 = 90.5%, odds of 1 winner as p(k=1,.1) = .1/e^.1 = 9.05% and odds of more than 1 winner p(k>1,.1) = ..46 %. The ratio of single to multiple wins is now 19.3, which is quite like "one particle at a time with inefficient detectors" (the 90.5% losses). For general n, the odds of a single win/trigger are p(k=1,n) = n/e^n and of multiple triggers p(k>1,n) = 1 − (1+n)/e^n. The ratio of multiple to single triggers is then:

R(m/s) = [e^n − (1+n)]/n ... (2)

In the low intensity limit (n−>0) this ratio goes as: R(m/s) −> n/2 −> 0, i.e. it approaches the perfect particle like exclusivity, _provided_ you don't count the no−win/trigger cases (as the Glauber's QO subtraction conventions do with the photo−detections in coincidence experiments). If you now check the often cited paper [2] Grangier et al.(GRA), their Fig 2. (p.176), you will see that they use precisely this property of the ratio of single to multiple triggers in the low intensity limit as the "proof" of the nonclassical behavior, where for classical case they use all data, thus R(m+k0/s+k0) −> 1 for their "classical" case as n−>0 (since it is dominated by k0, the no−detection cases; see also [3] for discussion and errata of GRA paper). The AJP−2004 paper [4] also uses this same ratio (with the same strawman "classical" limit of Rc −> 1). Note that GRA & AJP paper use only two detectors per "screen" and GRA also verifies that the two beams, when their paths are extended to intersect, do interfere with nearly perfect visibilty (sharpness). The "non−classical" exclusivity in their setup corresponds to the double−slit setup detectors placed behind one or the other slit, which supposedly will detect "almost always" a single photon, and "almost never" two or more photons (i.e. the R −> 0 as n−>0).

There is nothing in the low intensity limits in the double−slit (or their more practical variant, the beam−splitter) experiments which would surprise Maxwell or Poisson or Laplace (or Pascal and Newton, even Archimedes) in the slightest. What is happening, the counts being actually observed, exactly parallels what is happening in the lottery analogy above. Despite half a century of attempts (since the 1956 Hanbury Brown & Twiss controversy), the Quantum Opticians haven't shown a single experiment in which the ratios of multiple to single triggers drops below the classical (poissonian) value RHS of eq. (2) (cf. [4.c] to see the experimental perfection crowning the half a century of pursuit of this non−existent effect, the keystone of the Quantum Optics magic photon dogma −− behind the curtain, the holy of the holies of

Quantum Optics phenomena rests on nothing more but a cheap magic trick).

It is important to note that the Glauber's QED photo−concidence predictions (via his filtered−correlation functions Gn() cf. [5]) explicitly _require_ these subtractions (in the QO coincidence experiments) of no−detections _by definition_ of his Gn(). Check his primary derivation in his 1964 Les Houches lectures [5] (the sacred tablets of the QO, and nearly as hard to find nowdays as the tablets of Moses), in particular his transition from eq. (5.2b) to eq. (5.5), where he simply drops from the full n−detector+EM field evolution, all the terms "we" (??) are not "interested in" (why?), such as all no−detections, 'missing' and 'excess' detections, generally, any count of photo−absorptions m different than the number of detectors n). These subtractions/filtering (which are explictly non−local!) at the level of formalism are operationally mapped to standard Quantum Optics subtractions/rejections of the counts, such as of no−detections, unpaired singles, accidental coincidences etc (depending on the number of detectors and the type of Gn() sought). These terms are dropped not because they are small, but because "we" are not "interested in" them.

The sleight of hand the Quantum Opticians do when they trot out their quantum magic show (all their numerous "observed" non−classicalities, including their "observations" of Bell inequality violations and photon anticorrelations) to physicists is to extract (non−locally!) Glauber's Gn() from the actual counts (using the standard QO subtractions, to match the Glauber's _definition_), then they pretend that these Gn() functions are what is supposed to happen in the "ideal" case. They conveniently forget that the "derivation" of the coincidence "counts" Gn() in [5] is _not a derivation of what counts are supposed to be observed_ at all (which is predicted by the full dynamics in eq. (5.2b) in [5]), but merely Glauber's definition of a "signal function" Gn(). The Gn() is indeed practical and useful (in the engineering sense) by virtue of retaining only the terms with the incident "signal" field photons being absorbed precisely specified number of times at specified locations, thus it concentrates and amplifies the effects of the incident field (through the non−local QO subtractions). By pretending that these QO subtractions are due some kind of temporary technological imperfections (instead of being a fundamental step, as it is obvious in [5] pp. 84−86, in the "derivation" of Gn() and its experimental reconstruction from the obtained counts), the Quantum Opticians mislead the physicists in what has been observed, feeding the mass misperception among physicists on what the genuine empirical facts are.

In his "derivation" of Gn() (which is just a round about, obfuscated definition of his Gn()), Glauber drops most of the terms from (5.2b) describing the actual evolution (i.e. what is supposed to happen) of the n detectors in EM field, for no other reason (such as, say, showing that the dropped terms are negligible) beyond declaring that "we" (Quantum Opticians, engineers?) are not "interested in" them. The

Quantum Opticians (especially in popular & pedagogical accounts) start with the filtered eq. (5.5) in [1], or an equivalent, declare it as actually describing what is supposed to happen with n detectors, implying that the dropped terms (which are rarely, if ever nowdays, mentioned) from the genuine evolution (5.2b) are result of some kind of present technological imperfection which are not supposed to be there, once they perfect the detection technology (e.g. the so−called "ideal detector" which will show "loophole free" B.I. violations).

The excelent example of this QO sleight of hand is the Ou & Mandel 1988 experimental "demonstration" of the Bell Inequality violations [6], in particular when they write down their eq. (4), supposedly describing the QED/QO "prediction" as to what will happen with the 4 counts in the Bell EPR setup (for PDC source), which is a G2(x1,x2), and they cite (as their reference [17]) the pair of Glauber's 1963 Phys. Rev. papers as the source for the equation (the [17] in [6] is the same as parts of Glauber's lectures [5], minus the key derivation in [5] pp. 84−88). While the operational interpretation may not be obvious from the Ou−Mandel's ref [17], due to the absence of the key "derivations", it is plain in [5] that the Ou−Mandel's eq. (4) _does not_ describe what is happening with the 4 detector counts in Bell EPR setup, but it merely means that one can extract such function from the actual counts by applying the Glauber's subtraction scheme (as applied in his transition from (5.2b) to (5.5) in [5], where (5.5) implies, as a special case, Ou−Mandel's eq. (4)). In addition to Glauber's non−local subtractions implicit in G2() of their eq. (4) in [6], the mere usage of the particular G2() (referring only to a pair of events on the opposite sides of Bell EPR setup) discards (also non−locally!) the triple and quadruple events (requiring G3() and G4() terms, since there are 4 detectors) as well as the other G2() functions, such as G2() terms for double detections (+ and −) on the same side of the Bell EPR setup. These _additional_ defects (failures to model what is happening on the four detectors) are partially masked out by their use of the caricature incident EM field state eq. (2) in [6], which contains only a single PDC pair of D−photons (free field photons), thus it would yield 0 contributions for G3, G4 and other G2's, even though the number of pairs in any detectable in any sampling window is Poissonian (being just a 'sparsed' out Poissonian distribution of the PDC laser pump, which has Poisonian number of incident D−photons, cf [7]). The "single pair" of eq. (2) in [6] is not merely a poor caricature of their PDC source output, but it is an even worse caricature of any QO coincidence measurements (cf. [8]).

None of these two kinds of data adjustments/rejections, the one from the original use of G2() in eq. (4), and the others from their use of particular G2() only (in part via their choice for the input state eq.(2)), is result of an imperfect detection technology. The imagined "perfect detectors" which would generate _actual_ counts to match what their eq. (4) "predicts" is "supposed to happen" on the four detectors, would need, by definition of G2() and their choices in (4), to perform not just the Glauber's non−local subtractions (implied in any G2()), but would need the results database of supplementary experiments (for accidentals and other rejections) to help each "perfect detector", the G−detector, decide whether to declare trigger or no−trigger. Except for subtracting their own local noise (which could be legitimately built into a detector electronics), all the other subtractions & data rejections required for G−detector are purely non−local by definition.

In short, there is _no QED prediction_ that the counts collected in a Bell EPR setup will violate Bell Inequalities. The best you will find is the 'QO sleight of hand' kind of "proof" shown by Ou & Mandel in [6], which only weakly pretend to predict what is happening on the four detectors. Most of what you will see, though, are the QM toy derivations, which are at best the crude caricatures of the QED "derivation" in [6] (which in turn is based on [5] for the key eq (4) in [6] and on the toy input state (2) in [6]). The exactly same bare factual situation (the double absence of any observed effect and of any genuine QED prediction of such effect) holds for the double−slit and beam−splitter experiments (cf. [4.c] for a discussion).

Physicists, such as Gerard 't Hooft (cf. [9]), exploring the next fundamental layer of physical laws, which he (similarly to another great physicist too far ahead of his time, Stephen Wolfram) suspects are based on local, discrete cellular automata dynamics, has to divert his creative efforts to puzzle out how to fit in the fake pieces which just don't fit, the allegedly observed (by Quantum Opticians) non−local QM phenomena, which have in fact never been observed. It is a great waste of time and creativity for physics. Our grandchildren will laugh at us for getting bamboozled for this many decades by a small mutual back−patting society of pretentious 'engineers' with a gift of gab (such as Glauber, Mandel) and an unquestionable talent for slick magic tricks and showmanship (Clauser, Aspect, Grangier, Chio,...).

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{ Sept 22, 2005, 00:05 EST }

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    - *From:* Eugene Stefanovich

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