Reciprocity, complementarity and minimal action

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ABSTRACT. It is shown that the quantum uncertainties can be derived directly from the limits imposed on action, Et=pλ=h., without introducing further, more complex premises. The weird predictions entailed by such premises, namely the Fourier treatment of E=hν leading to ΔEΔt≥h, which have caused the scepticism of all concerned since the first appearance of the Aharonov-Bohm attack on ΔEΔt≥h, are completely avoided and this uncertainty, as presently interpreted, complies fully with ordinary experience. A distinction is drawn between a 'particle' associated with a wave and *action* thus associated, rendering wave-particle duality idle as a basis of complementary reasoning. *Reciprocity*, a concept introduced by Bohr but kept behind the scenes, is reinstated and understood as a duette of complementary "counter-happenings" at either end of a quantized unit of action, the E,*p* end, on the one hand, the *t,q* end, on the other, and is shown to be the mediating ling between quantum wholeness and complementarity.

1 Quantized Action

In Olivier Darrigol's historical account of de Broglie's assumption, which account is a great deal more than historical, we read:

Einstein's form of the quantum condition fitted well with de Broglie's idea that action played the role of a *phase.* In his seminar Langevin called the action variables "the cyclic *periods"* of the action integral, and so did Leon Brillouin and Louis de Broglie. This denomination implied that Langevin regarded action as the argument of a *periodic* function.[1]

The meaning of this is that in QM the least possible exemplification of a dynamical quantity, E or *p*, can only be recorded *over a period.* A wave period. If this period is expressed in terms of the frequency, ν, we obtain

E=hν and so Et=h. If it is expressed in terms of the wavelength, λ, we obtain p=h/λ and so pλ=h, as two alternative expressions of minimal action, =h.

The idea, stated in more precise terms discloses that the basic quantum relation, E=hν or E≈ν, actually signifies that E, exactly like ν, can no longer be defined *at* an instant d → 0, as was classically assumed, but only over a period t>0, whose boundary instants $\{t_1, t_2\}$ are here given by the frequency of this period, ν. Correspondingly, *p* also cannot be determined *at* a point location, dq→0, but only over a *distance,* whose boundary points $\{q_1 q_2\}$ are given by the wavelength of this period, λ.

It is precisely because of this fact, i.e. precisely because E cannot be defined *at* an instant dt→0, but can only be mapped over a period, that the resulting product of action Et is not arbitrarily reducible to a dimishing value Et→0, but can only be determined over a *limiting* such period and not less, that the product Et is thereby rendered a *quantum* of action, *h*, instead. Clearly, for the case that E could be recorded *at* an instant, taking this instant as narrow we might want it to be, thereby leading to a t→0, the pro-duct Et would be a vanishing quantity and no quantum of action would result.

2 *ΔEΔt≥h*[1]

Since 1961, the first time that the Aharonov-Bohm (A,B) papers appeared, [2,3] which revealed a number of irresoluble problems regarding ΔEΔt≥h, essentially made official by P. Bush's paper of 1990, [4] physicists have been beating their heads to understand exactly what the Energy-Time Uncertainty (ETU) actually was. There was no operator for time in quantum formalism, yielding an Et≠tE comparable with pq≠qp, and time itself, related according to A,B to the *apparatus,* was in no way involved with the energy state, hence could be determined independently. In consequence, no uncertainty should ever be expected between the values of energy and time [3, p.B1417]. In Busch's opening words, thirty years later: "Today one finds physicists claiming that there *is* no energy-time un-certainty relation at all". [4, p.1.]

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[1] The derivation which follows appear self evident and, to an extent, even trivial. If it does, it will do no harm to its validity. Yet how really *unlike* the *standard* method it is, will already begin to announce itself, when the Fourier case is discussed, in Section 3. And when the standard method is fully revisited, in Section 6, the two methods, the standard and mine, will be shown to *conflict*. The very same applies to ΔpΔq≥h, coming next.

True enough, the problems are there and I have myself listed a number of them, [5, 6, 7] but these were to a certain extent *extrinsic* to the principles of QM, mostly relating to the knowledge of time in *general,* whereas A,B's initial point was that the ETU also had problems *intrinsic* to the theory. In their words "there is therefore no reason inherent in the principles of *quantum theory* why the energy of a system cannot be measured in as short a time as we please". [2, p.1652].

A close look at the analysis I have above given, deducing a quantum of action of dimensions Et from the fact that E must be mapped over a *period,* rather than *at* an instant, hence not 'sooner' than a whole Δt, directly contradicts A,B's quoted conclusion, though of course it anything but contradicts action quantization. Whatever other problems the ETU may face, and they are indeed several and serious, that it is not *inherent* in the principles of QT is clearly not one of them. For that energy can only be defined over a period, rather than *at* an instant, thus leading to a *minimal* product Et, whose time component can never be arbitrarily reduced but has instead to always stay a $t>0$, is what quantized action is all about.

And the ETU relation is a direct and immediate consequence of this principle alone, no further premises added. This is how, roughly, the argument is supposed to go:

If there is such a thing as a shortest time permissible, i.e. a time *limit,* imposed on the conditions warranting the very manifestation of E, this being a time limit of dimensions $\Delta t=\{t_1,t_2\}$ or ν, then any subsequent *narrowing* of this interval, of the order, say, $\Delta t' = \{t_1,t_2\}/2$, can only mean that the overall energy determination will only be *reciprocally* affected, and therefore reciprocally inaccurate. For if we need *at least* a time length of dimensions Δt, if we are to determine the energy within an accuracy $\Delta E=n$, (where *n* is sufficiently small to stand for a high E approximation), then, all other conditions being the same, at half the time formerly allowed, i.e. within $\Delta t/2$, we can only expect to end up with an uncertainty $\Delta E=2n$, if the action product itself, of which E and *t* are the components, is to always remain *constant,* (i.e. *h).* And so on, reciprocally, for any other, proportional diminution of Δt. (NB: "Reciprocally" is the key to this argument. See below.)

In other words, if energy can never be defined within an instant $t\rightarrow 0$, and hence is only to be defined over the boundaries $\{t_1,t_2\}=\Delta t$, as is dictated by the assumption that the act-ion unit which is the product of the components E and *t* is to be kept constant (or mini-mal) at all times, then the optimal definitions of both these action components, E and *t,* cannot themselves be any better than the said limiting product, Et. And therefore that the joint errors in the definitions of these two action components, E and *t,* can at best be equal, or if not then greater than this limiting product Et. Hence, in symbols, $\Delta E\Delta t \geq Et$. But Et=h. So $\Delta E\Delta t \geq h$.

Rather than saying that this interpretation of the ETU is the consequence of action quantization, namely, the restriction that E can never be defined *at* $t \rightarrow 0$, I am almost tempted to say that, if only in a slightly more complex fashion, it essentially *is* the action quantization, if E is not to be defined *at* a $t \rightarrow 0$. Whatever other problems the ETU is known to face, that it is not inherent in quantum principles clearly does not appear to be one of them. In fact, to accept Et=h and deny $\Delta E\Delta t \geq h$ would in the context of the above analysis directly lead to a contradiction.

3 The Fourier Analysis

How all this relates to measurements, is an altogether different problem. I do agree with A,B that, due to known conditions raising certain constraints when it comes to time de-termination in *general,* it may appear a "mystery" [8] how we are to subsequently interpret this Δt. Indeed, in certain cases the restriction that Δt, will appear curious.

But in my opinion, this results from the practice of deriving $\Delta E\Delta t \geq h$ by just relying too heavily on the Fourier Analysis. My far more simple discussion, however, suggests that perhaps we shouldn't rely on it all that much. According to A,B the ETU establishes a "relation between the error in the measurement of energy and the *duration* of the measurement process". [2, p.1651.) On one particular reading of it, this interpretation may relate with mine, above; however, on another, it may just as well go wildly astray.

For as the standard Fourier reasoning is known to go, "the *more* wave cycles are counted" [9], the more accurate the energy determination will become. Thus, this reasoning goes, in the case of counting the cycles of a harmonic wave, when E can be made precise to the extent that the frequency, *v,* also is, we must in fact wait till we count indefinitely many cycles, or even all cycles there *are*, out of fear(!) that the next cycle might well come about just a little too sooner or just a little too later but nonetheless sufficiently so to destroy ideal periodicity. Whereupon it is evident that all eternity shall have to pass, before we can make sure of the 'exact' energy value. Thus Δt would grow to infinity.

That this is an extravagant claim, is plain to all to see. And certainly false. If at the end of, say, two minutes of having counted exactly periodic cycles, we cut the Gordian knot and retrodictively say that at *any sharp* instant *t,* of the total period elapsed, T, (=two minutes), the system had *exactly* E, we would only be saying the self evident, yet now offering accuracies way below the limit of the quantum. That then physicists may resort to their usual tricks of trying to convince us that the quantum uncertainties only hold predictively, but not also retrodictively, as for instance in Orfanopoulos, where "the possibility of an accurate quantum retrodiction of a system is *opposed* to a description of its future",[10] is an idea inviting suspicion, not persuasion. (For an identical passage see also Heisenberg.[11])

The quantum uncertainties exist because and only because of the quantum of action. And, I gather, if the quantum of action exists at all, it also exists retrodictively. For if it exists at all, then it has existed as soon as the physical counterpart of what we call act-ion was first initiated in the universe. Which was certainly long before I began my counting up to two minutes. And hence it was itself *there,* throughout the entire period of my having counted up to two minutes, when I finally cut the Gordian knot. Clearly, then, refuge to "retrodictions" is not the answer.

It would therefore appear that at *any chosen* instant *t* of these two minutes=T, i.e. at any point sharp instant tεT, the system had *that* energy, an accuracy, that is, so high that it flatly contradicts the standard reading of the ETU, according to which the error in the measurement of energy is *proportional* to the duration of the measurement. It is any-thing but that and A,B were perfectly right to criticize it. For a measurement of a mere couple of minutes duration should be just *too short* on Fourier standards to give any satisfactory account of the energy. Which, however, it has.

On the contrary, it would fully confirm Dr.Orfanopoulos's claim, that there *can* be accurate quantum retrodictions after all. The difference being, however, that on this other interpretation, these retrodictions would themselves be accurate only to the extent that the quantum itself was *absent* from the process! Which is hardly a better alternative, since QM itself would then be contradicted in either case.

But on the basis of my own account of the ETU, which slides past the Fourier Analysis, none of all this would ever happen. For my derivation of $\Delta E \Delta t \geq h$ was based on quantized action alone, namely, on the assumption that no definition of energy is possible, unless an *entire* period Δt has elapsed, and not based on the usual Fourier practice of "counting cycles" all the way to eternity before (allegedly) making sure, when, in fact, we already should have, long before that. And then the ETU would have been retrodictively contradicted, exactly as Dr.Orfanopoulos has observed.

But in my derivation the ETU stays just as strong as ever without having to count infinite "cycles" to define it, and just as strong as ever, even if taken retrodictively and just as strong as ever, even if we *have* precisely defined the energy in no more than two minutes. For in my proposal, for any instant of time *narrower* than that *limiting* period Δt, contained within these two minutes, no definition of energy would have been possible in any event and hence it would be false to say then, that at *any chosen* instant $t \rightarrow 0$ from within these two minutes, =T, the system had *that* energy! It would, but then again only just *after* that critical period, Δt, had first elapsed at least once. And then at any time narrower than *that* period, the energy would indeed be indefinable, even retrodictively, thus fully satisfying quantum requirements –and common sense- in the past as much as in the future, even if I have contemptuously declined to measure E beyond the trivial span of two insignificant minutes.

All this, without having to count cycles to eternity, to make sure that $\Delta E \Delta t \geq h$ holds, if at all. For a different way of putting that (rather absurd) idea, is that if we must count to eternity to only then obtain an accurate E for an infinite Δt, we will never on those standards count enough to be any wiser than day one, when we first started, as concerns the energy value, so as to then proceed to affirm that we *have* thereby completely lost *t,* as the ETU demands, and so be at all able to say that the ETU is true in the first place!

4 ΔpΔq≥h and the "Asymmetry"

What is expressed in terms of the frequency of the wave period, necessary for the definition of action as $E=h\nu$ or $Et=h$, can also be expressed in terms of the wavelength resulting from the same wave period, available for the definition of action as $p=h/\lambda$ or $p\lambda=h$. Then, on the basis of a reasoning quite similar to that which led to the ETU, the momentum- position uncertainty (MPU) can thereby be derived.

If momentum, by strict analogy with the energy before, can only be defined *over* a distance and therefore not *at* a point location $\Delta q \rightarrow 0$ (see also Zeno's arrow paradox! [12]), hence only over a distance of dimensions $\Delta q=\{q_1q_2\}$ $(=\lambda)$, it equally follows that any at-tempt at its definition within spatial boundaries narrower than those already specified, will be as inaccurate, as the distance itself employed for this attempted definition is taken shorter. Same as before, if we need at least a distance of dimensions $\{q_1q_2\}=\lambda=\Delta q$, if we are to determine the momentum with an accuracy $\Delta p=n$ (where *n* is sufficiently small to stand for a high momentum approximation), then, all other conditions being the same, at half the distance formerly allowed, i.e. at $\{q_1q_2\}/2$, we can only expect to end up with an uncertainty $\Delta p=2n$, if the action product itself, of which *p* and *q* are the components, is to always remain *constant,* i.e. *h.*

So, in general, if momentum cannot be defined *at* a space point, but is only to be defined over the spatial boundaries $\{q_1q_2\} = \Delta q$, as is dictated by the assumption that the product pλ is to be kept constant (or minimal) at all times, then the optimal definitions of both the action components, *p* and *q,* cannot themselves be any better than the limiting product pλ. And then, consequently, the joint inaccuracies in the definitions of *p* and *q* can at best be equal, or if not then greater, than this limiting product pλ. Hence, in symbols, $\Delta p\Delta q \geq p\lambda$. But $p\lambda = h$. So $\Delta p\Delta q \geq h$.

Countless things have been said to this day about the notorious lack of *symmetry* between the two basic uncertainties, most of them correct and to the point. An introduction to them was given in Section 3. Why is there no such thing as an $Et \neq tE$, when there is such a thing as a $pq \neq qp$, or why is there no operator for time, when there is one for position –which is the same exact thing-, or why is time only a *parameter* in QM, whereas position is an "observable", which is the most intriguing one of all.

The reason is, of course, that positions have to be *observed* to be known, but times not. Some would be tempted to retort that times too have to be observed, if to be known, and this is what having to rely on clocks is all about. But this is quite superficial. Clocks are not ways of observing time. Clocks are ways of *keeping* it. Time as such we can never ob-serve. Time, we just keep. And this is what makes all the difference.

Clock indications, on the face of it, appear to be more trustworthy than our "inner sense" of time is. This, besides, is their reason of existence. As a rule, yes; but not always and not when it counts. For clock indications are trusted, provided they do not *conflict* with our inner sense of time. Whenever they do, it is not clocks but our inner sense which takes over. When the clock in my room has run full circle, both its hands returning back to "12" –or zero- I am never deceived into thinking that time has returned to zero point or gone backwards. If my clock stops, I am never tempted to suppose that time has stopped with it. And if earth were to enter a vast, cosmic magnetic field which plays havoc with all our clocks, we will say that it is our clocks that have gone mad, not time.

Hence, vagueness and all not withstanding, our inner sense of time always takes precedence over clocks in fundamental matters. Hence, in essence, it is the "inner sense" of time, which is primary, clocks themselves only being copies just affording greater accuracy, and this essence is certainly nowhere near being an "observable". Position, how-ever, is. There can be no question of an "inner sense", as to *where* an object is. It is, quite simply, where I *see* it. Whence, of course, their much mentioned asymmetry.

But, and as the previous line of reasoning reveals, this asymmetry is *extrinsic* to QT. QT as much as any other theory cannot change facts about the world we already know to be true independently of its truth. Red will be a colour different than green and cats will be animals different than elephants, no matter what QM has to say about anything. In consequence, whatever problems may emerge, when the two basic uncertainties are ex-tended beyond the pure axioms of QM, and enter the domain of macro, or even micro, experience, this will still not go to show that the ETU and the MPU are asymmetric in *quantum mechanics.* Take a look at the interpretation given to the ETU and then at the one given to the MPU one. You will simply discover that, by merely putting *p* and *q* in the place of E and *t,* and by mere replacing the minimal product Et=h by the minimal product pλ=h, and by substituting temporal intervals of duration $\{t_1,t_2\}$ for spatial intervals of dimensions $\{q_1q_2\}$, I have otherwise used the self same, identical words and reasoning in either. The two deductions are symmetrical across the board.

And when it comes to asymmetries extrinsic to the theory, those, namely, which relate directly to the idiosyncracies of a time as opposed to a place determination, even then I no longer see any reason for such a deep going discrepancy, as so many have surmised. True enough, some of the admissions forced upon us by a much too strict application of the Fourier analysis of E=hν, will appear extravagant, as is no doubt the contention that we must needs wait for an eternity, only to make sure that the energy value is what it was all along, if not indeed right from moment one. For be that as it may, still we can never abandon the self evident truth that energies, come what may, can only occur in the world *at* a time or, if not, then *over* a time, but, either way, in a *limited* time. The Fourier "straitjacket", as Hilgevoord and Uffink have called any attempt to squeeze the ETU into the MPU one, [13] does indeed lead to such curiosities which, extrinsic to QM or not, do make some of its demands look a bit like the proverbial eyebrow raisers. And then the re-mark that they are after all extrinsic, will hardly patch up the gaps or lower the eye-brows.

But in my proposal, neither was the ETU 'squeezed' into the straitjacket of the MPU, or its noncommutative formalism, -in fact if anything, then the converse-, nor therefore did it lead to exact energies occurring only over infinitely long waiting, severy testing one's patience, and one's patience not just as concerns the energy value *alone,* I might add. In my proposal, energies and/or momenta are simply both grafted over a wave *period*, exactly as the theory says they must, which manner of speaking, however, by definition implies a ***finite*** period, and not the weird infinity foisted upon QM by too much reliance on Fourier reasoning. For unless the period is finite, *no wave* *of any sort will ever result*. And then the very statement of the principle of action quantization, will itself be impossible, with all that that entails.

So even empirical demands extrinsic to QM, such however that must at all costs be satisfied, even if extrinsic, are anything but threatened or violated by my proposal. Which, if they were, would threaten QM itself instead. The self evident demand that energies must needs occur *at* a time or, if not then at least *over* a time, but in any case within a *finite* time -if to occur at all, is fully respected and vindicated, *and* the ETU was derived in (tautological) accordance with the action principle. Therefore the two derivations are perfectly symmetric even in that latter, extrinsic respect.

5 Wave-Particle Duality

It should be equally clear and equally certain that the interpretations of the ETU and the MPU I have previously given, neither say a word about wave-particle duality (WPD) nor even require or presuppose it in any fashion. For it is not *particles* in my discussion, which are 'wave-like'. It is action, which is.

One may indeed be tempted to assume, together with L. de Broglie to whom the thesis to follow is usually attributed, that the momentum of the particle is *"associated"* with a wave of wavelength λ. My initial, brief account of de Broglie's conception of action, as given by Derrigol (Section 1) speaks quite differently. In any event, if de Broglie did speak about such "association" he shouldn't have. $p=h/\lambda$ speaks of no particles and allows none such. Indeed, what $p=h/\lambda$ actually says is this: that in order that I may obtain an accurate momentum value, *p,* I must correlatively obtain a precise *wavelength* value, λ, if to be at all able to do it, by thus giving to the fraction h/λ a precise value on the whole, since h itself is independently known with accuracy.

Now why do people continue to read a "particle" in the contents of the relation in question or, better, in the contents of its *p* member itself, when it is by means –and only by means- of a *wave* that this *p* can be at all determined, I neither know nor comprehend. All the more so, since use of the "wave picture", achieved by giving a unique and so an accurate value to λ, can only mean a harmonic, *plane* wave which therefore extends infinitely in space, rendering any *position* account, and therefore any thought about "the particle", no less than contradictory to the initial assumption. Is not indeed the very logic of those who insist on speaking about waves and particles, in the first place, the logic claiming that any consistent and strict application of either of these concepts by definition excludes any simultaneous application of the other? This is what WPD reasoning is all about. Why then, pray, and in what precise sense is the momentum *p* of the relation in question the momentum of the particle? So far as I can see, on the very logic of those who thought it all up, when the wave picture is applied –and for a unique λ it is applied in its strictest possible sense- the particle is thereby *excluded!* Therefore *p* cannot be the momentum of any "particle".

There's no denying that de Broglie's quantum relation is, to say the least, *ambiguous*. Even without my former remark within the picture, on its own and in all good faith considered, it is literally impossible to unfathom. Leon Rosenfeld, for example, has spoken thus of the matter:

The energy and momentum, E and *p,* are concentrated in the *particle*, and the frequency and wave number, ν, σ, are defined by the wave [14]

What is overlooked, however, is that energy and momentum are *themselves* defined by the wave. Which makes all the difference. More than a decade later we have an improved version, by C.A.Hooker, even if its improvement lies mainly in its added ambiguity:

There is a deep going connection between the particle-like energy and momentum and

the frequency and wavelength of the corresponding wave, namely, $p=h/\lambda$ and $E=h\nu$ [15]

But does this say that, due to the "deep going" connection, properties normally attributed to waves, such as frequency and wavelength, are now ascribed to particles, because such properties are now employed to determine energies and momenta? Or does it say the contrary, namely, that since properties normally attributed to particles, such as energies and momenta, due to the deep going connection, are now ascribed to *waves,* since waves are in fact employed for their determination? I feel I have to stop this questioning immediately, for it has made me dizzy. And suffice it to say, that, either way, energies and momenta are identified by the *wave.* Namely, by something *other* than the particle. Though, of course, not other than the wave.

Granted that $p=h/\lambda$ is a bit on the side of ambiguity, due presumably to this elusive *"p",* which somehow implies a *mv;* but, surely, $E=h\nu$ is not comparably ambiguous, since the energy, E, featuring in it, could be, say, thermal energy and hence not necessarily involving mass and velocity. Is there any doubt, as far as $E=h\nu$ goes, that, given *h's* constancy, the energy E *is* the frequency? Clearly not. $E=h\nu$ is just as frequently expressed as $E\approx\nu$. There is even an old and by now forgotten paper by D.M.Mackay which from start to finish speaks of the "empirical *identification* of energy with frequency"[16]. But then the energy at least belongs to the wave; not to the particle. So this much of the two previous contentions of Rosenfeld and Hooker is directly contradicted, at least as concerns the energy part of them.

But energy and momentum are considered to be the dynamical variables, when taken together, which, by contrast with positions and times, therefore forms a group of their own. Hooker himself, about a hundred and fifty pages later, but still in the same work, now does exactly the same with wavelength and momentum:

If the quantum relation p=h/λ holds, then the momentum, *p,* can only be determined in the latter, *plane wave* case, where the spatial location, *q,* is completely indeterminate. [15, p.219.]

And since it is the concept of spatial location, i.e. that of *position*, which normally relates to a particle (waves and, in particular, plane waves of the sort here discussed do not allow of any position whatsoever), when the spatial location is completely indeterminate, the particle, to which alone it corresponds, also is. But the momentum, by contrast, is not at all "completely indeterminate" in Hooker's last passage. It is completely determinate instead. Hence the *p* of de Broglie's quantum relation is not the *p* of a particle, though what it is precisely in this connection I am the last person responsible for an answer. In any event, the closest we can ever get to this puzzle, is given by a much better account, belonging to M. Bunge:

In the particular case of entities endowed with mass (such as electrons) one group of variables, i.e. position, describes the *corpuscular* aspect, while the group complementary to these, e.g. momentum, describes *–as can be seen from de Broglie's relation-* the *wave* aspect. [17]

We have an identical interpretation by Bohr:

The very definition of the energy or momentum of the photon, given by the product of Planck's constant, and the frequency and wave number of the radiation, directly refer to the characteristics of a *wave*.[18]

My sentiments exactly on both accounts. In consequence, if *p* (or "mv", whatever that latter might mean in this twisted connection) is the wave property, then, since this one was the *sole* reason and the sole initial candidate for introducing a "particle" in the first place –out of virtually nowhere, quite honestly- if it is to be subsequently identified with the *wave*, there is simply no room for the particle. De Broglie's p=h/λ makes no reference to particles of any kind, however much its own author, and many others besides, would have liked it to do so. And it was only by an act of fiat that it was ever put there.

On the force of this remark, it is equally clear that my rather simple argument about how to derive ΔpΔq≥h directly from p=h/λ makes no reference to particles of any kind and, in fact, none such to waves either. For "waves" in quantum jargon are not *just* waves at all, are they? They are the other side of "particles". And if p=h/λ says nothing about particles, it most certainly says nothing about their "other side", the waves, either. All that p=h/λ says is that, since there is always a distance $\{q_1q_2\}$=_ required for specifying the momentum, and not just a unique space point as was(?) classically assumed,[2] then momentum in QM cannot be conserved *locally.* Now whose momentum is this, that we, or rather QM, is presently talking about, I quite frankly do not know.

One thing however I do know. That if momentum can only be conserved nonlocally, it cannot then be the momentum of a particle. For particles have to be local, if anything has. Whereupon the claim that a *p* which is conserved nonlocally is the momentum of a particle, whose latter's momentum can only be conserved *locally*, is no less than to contradict p=h/λ. If the conception of an incorporeal momentum sounds weird to the reader,

he should perhaps enquire whether this is my fault or the theory's and whether the duality of waves and particles itself, which is hereby avoided, is any less weird than that. I think otherwise on both issues. And, besides, philosophers have for some time now being discussing colours just as incorporeally, when they divorce them from their necessary correlative, the surface, and connect them with "after images" and the like. If they can, so can I.

6 Reciprocity Versus Complementarity ? Conclusion

For a brief period of time Bohr had entertained the idea of giving up his all prevalent term "complementarity" (CTY) and adopt the term "reciprocity" instead. For some reason or other the secondary term did not really register with him and so was abandoned for the sake of the standard term soon after. But in the context of my present approach, which has itself slightly departed from past accounts of mine of CTY, I will attempt to revive it in ways consonant with my present account of the quantum uncertainties, to

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[2] In fact, this was not even *classically* assumed, to be exact. "Momentum is formally defined in terms of a limiting process between two *distinct* positions, at least classically, since it is a derivation of position. This means that within strict logic momentum is not defined at *a* place but only over an *interval*". [C. H. Hooker, private communication, 15 August 1983.] According to the author, the very structure of Zeno's paradoxes is due to his timely realization that ve locities can never be defined at a space point, without employing quantal premises to show this. This fact spells the weird suspicion very much like the MPU is just about foretold in the content of classical principles.

which the abandoned term, “reciprocity”, seems to fit better. The presence of reciprocity is announced in the *Introductory Survey,* where Bohr writes:

In view of the *reciprocal symmetry* peculiar to the use of classical concepts in this symbolism, the writer [himself] in this article has preferred the term “reciprocity” to the word “complementarity”, used to denote the relation of mutual exclusion characteristic of the quantum theory with regard to the application of various concepts and ideas.[19]

The passage referred to in the main article goes like this:

The contents of relations (5) [n: $E\tau=I\lambda=h$, where E and I are energy and momentum respectively –op.cit., p.57] may be summarized in the statement that according to the quantum theory a general *reciprocal* relation exists between the sharpness of definition of the space-time and energy-momentum vectors associated with the individuals. [19, p.60]

The reciprocal relation between sharpness in the respective definitions of space-time, on the one hand, and energy-momentum values, on the other, is precisely what my own derivations *straight* from the quantum hypothesis have revealed, when due to the insurmountable limit imposed on action, its two alternative products, $Et=pq(=p\lambda$ –see Appendix), drive their components to just this sort of symmetric mutual exclusion spoken of in the two passages. And this symmetric reciprocity is due to nothing else but the *indivisibility* of the elementary quantum of action itself (which is as it should be):

*Complementarity is a term suited to embrace the features of *individuality* of quantum phenomena. [20]

*The fundamental postulate of the *indivisibility* of the quantum of action [...] forces us to adopt a mode of description designated as complementary. [19, p.10]

*The universal quantum of action reveals a feature of *wholeness* in atomic processes. [18, p.80] Because of all the previous,

*This principle [Planck’s quantum of action] symbolizes, as it were, the peculiar *reciprocal symmetry* relation between space-time description and the laws of conservation of energy and momentum. [19, p.94]

In other words, the dynamical-to-kinematical concept CTY results directly from our inability to make the quantum smaller, no additional premises added. This connects with my analysis of the ETU and the MPU in the following obvious way: The pairs of classical conjugate concepts, E,t and p,q, cannot be any better defined each, than what their respective minimal products, $Et=pq=h$, will make *room* for. Any further decrease of “error” (not the best term, but it’s all there is) in either one, action-wise related classical quantity, will be *balanced out* by a reciprocal counter-increase of the “error” in the other, act-ion-wise classical quantity, related to the former. To complete the argument

The quantum of action imposes *restrictions* on the description of the state of the systems by means of space-time coordinates and momentum-energy quantities. [20, 89]

The word “balance” is here as essential as reciprocity itself is. One thinks of such things in this connection as scale arms, say, rising upwards, when their other end tilts down-wards, in a single, undivided move. In this specific context, the tilting at the one end *is* the rising at the other (if only to put it too crudely). Other, similar cases, also suggest themselves. Suppose I’ m holding a rigid, wooden ruler and I use it to point downwards. The other end will *eo ipso* rise upwards. Now suppose that instead of the rigid, wooden ruler I am holding a plastic one, made of particularly soft rubber. Its one end will go down, *without* forcing its other end to balance out by rising up. Lack of rigidity makes the rubber ruler react in these circumstances, as if essentially *two* things. I.e., as *sub-divisible.* Hence its two ends become *unconnected,* and can therefore behave independently of one another.

The arm of the scale and the wooden ruler are indivisible wholes; the rubber ruler not. The latter offers arbitrary freedom of movement concerning what is tried at either end, without ever affecting the other. The two former do not. They inseparably *link* the fates of their “polarized” components, the two ends, in inverse fashion, so that whatever happens at either end of the undivided unity will be evened out by a reciprocal and inverse *counter-happening* at the other, to keep the undivided unity going. My two analyses of the deep connection between the principle of minimal action, on the one hand, and the two quantum uncertainties, on the other, reproduce the exact same effect, of reciprocal and inverse influence upon their own components. E with t and p with q appear to penetrate into one another far too deeply to allow their separation and disentanglement, when their respective products eventually come to yield an ultimate, indivisible, *atomic* unit of act-ion. They too, in a strange sense, somehow become (a two-ended) *one* thing, whereas, if their respective products were vanishing quantities, they’d be just so many isolatable and mutually independent states, just like the two ends of the rubber ruler. The fact that they are neither, is what their CTY is all about.

In this discussion reciprocity appears to be just a step *behind* CTY, which latter is the last one to take. Therefore the question of their connection becomes inevitable. In my treatment it seems that the reciprocal counter-happenings at either end of the action cluster *lead* to CTY rather than *are* CTY. Although, of course, they appear to be at least logical equivalents in this context, even if not semantically identical. So one may safely say that where there is reciprocity there will be CTY. But the converse of this does not seem to be true. And there *is* CTY without reciprocity, to Bohr's own apparent admission. Bohr's discussion in [19, p.57-60] now suggests that CTY is the result of "the *super-position* principle" whose physical meaning is "a limitation of the wave fields in space-time" produced as is known out of "the interference of a group of elementary harmonic waves" (ibid.). This reasoning is concluded below in the following way:

Here the complementary character of the description appears, since the use of wave groups is necessarily accompanied by a lack of sharpness in the definition of period and wavelength, and hence also in the definition of the corresponding energy and momentum as given by relation (1). [19, p.59; relation (1) is Et=pλ=h.]

Notice the sudden *contrast* with my method –and his *own,* besides. What Bohr now speaks of is *Fourier* logic or, what is the same thing, wave packet logic. That is to say, the known general rule for wave localizability; a small size, even a position-size wave packet can be shown to be the product of the superposition and then the mutual interference of a number of harmonic waves, its so-called harmonic components, which number, how-ever, is particularly great. In fact, for small size wave packets almost infinite.

In consequence, the energy and momentum values become exactly as incalculable, as there are more and more plane waves in mutual interference entering in the characterization of the wave packet resulting from such interference. For then there'd be almost infinitely many frequency and wavelength values to equally choose from. So what is thereby gained in terms of space-time location of the packet (or "group"), will be reciprocally(?) lost to energy and momentum conservations, themselves possible only for a *unique* wave-train, of a unique frequency and wavelength. Whence, of course, their CTY.

I have used the word "reciprocity" once again, just now, in this other connection, namely, that of the wave-packet logic. But now observe the overall discrepancy with my previous use of it. In visualizing balance and reciprocity in my own account I have confined myself to the employment of *one, single* wave train, or rather the period of one, and used this period *alone* to derive ***both*** sets of conjugated "unsharpnesses" on its basis.

And *my* way of proceeding to the two quantum uncertainties (essentially Bohr's, of course), was by simply taking that elementary wave period as *indivisible.* Whereupon the two alternative pairs of conjugated quantities, Et and pq, to be defined *upon* the spatio-temporal width of this period –and upon no *smaller* width than that- had to lose in sharpness at "the one end", e.g. E, or *p,* exactly as much as they had gained at the counter end, *t* or *q,* *because* the period in question was the minimal available. There has been no other premise involved, as the word "counter *end"* itself testifies.

I never said that I require *one* wave for determining E and *p, many* waves for determining *t* and *q,* alternating thereby between one *phenomenon* as opposed to another. I have only alternated between one *end* as opposed to the *other end*, of a single, self same and self contained phenomenon, inversely (or reciprocally) folding and unfolding upon its own*,* indivisible self. As was, I trust, amply exemplified with the reciprocal and indivisibly interconnected counter-happenings at the two ends of my chosen paradigms, the scale arm or the wooden ruler, both rigid and inflexible enough to merit the term "a single, un-divided whole". The reciprocally balanced, complementary counter-happenings proceed-ed in that argument directly from the compact singularity, whose restrictive nature gave them rise. CTY was then the consequence of *that* situation.

Now observe the structure of Fourier reasoning (more closely connected with QT than with Bohr's actual doctrine) once again, as outlined by Bohr himself: I need *one* wave, for defining E and *p, many* waves for defining *t* and *q.* Whence, of course, these two respective definitions will be as mutually exclusive as "one" is to "many". Isn't this the whole logic behind the argument? But then, not only is there a profound discrepancy between this reasoning and mine, in that it employs *two different types of phenomena,* to my having employed just one. It, in addition, displaces the quantum of action almost entirely, and delivers its conclusions on the self-evident, *tautological* truth, that I cannot simultaneously obtain many of this, of which I must also obtain only one. Which is a truth of logic not a truth of physics.

"Many" though clearly opposed to "one", nevertheless yields a case of mutual exclusion *without any help from the quantum.* One could retort here that help from the quantum is most definitely warranted in Fourier reasoning, since $\Delta\nu\Delta t\geq 1$ combined with $E/\nu=h$, does in fact yield exactly what we need, i.e. $\Delta E\Delta t\geq h$. In fact, it does. But then, in fact, it also yields that the "one/many logic" which, by any standards, is ***sufficient*** for leading to mutual exclusion, -everyone can see that!-, is in fact *not* sufficient for leading to mutual exclusion, since this mutual exclusion must also include and comparably rely upon the quantum of action, *equally* sufficient to the purpose. Whereupon we would –inconsistently- end up with one sufficient condition too many.

To put the point in the way I have done in an earlier work of mine, we are here essentially involved with *two* types of mutual exclusion (or incompatibility). A *conditional* type and an *unconditional* type. [21] The "one/many" type of incompatibility is unconditional. But this is not the only type there is.

When the bandit threatens me by presenting me with the proverbial dilemma: “your money *or* your life!”, he is also leading these two, hitherto compatible possessions of mine, to a conflict. But then all this *on condition* that I am faced with this specific dilemma, which can be removed, if the conditions producing it are removed, i.e. if the bandit is arrested immediately after. Then I can go back to having my money *and* my life, just like always.

To then say, in view of this distinction, that within the Fourier reasoning, leading to $\Delta E\Delta t\geq h$ from $\Delta\nu\Delta t\geq 1$ and $E/\nu=h$, E and *t* are incompatible both: (a) because it incorporates the “one/many” logic *and* (b), because it also incorporates the quantum, is no less than saying -and not counting the previous contradiction- that within Fourier reasoning E and *t* are both; unconditionally incompatible, as entailed by the “one/many” relation, *and* also incompatible on *condition* that action is quantized, which however it doesn’t *have* to be. And so both conditionally incompatible and unconditionally so.

This is the result of piling up too many items, any one of which could equally well full-fill the task on its own, and force them to collaboration, if only to accomplish the very thing which any item on its own would have accomplished quite handsomely and without any help from the others. The quantum of action above all the rest. And in all this, though *some* CTY is still obtained, *the* CTY is not. Meaning by *the* CTY, this special kind resulting from reciprocally balanced, counter-happenings at the polar ends of a single, undivided entity, which simply resists our own attempts at subviding it. That is to say

The demand for unrestricted *divisibility* on which classical description rests is clearly incompatible with that feature of *wholeness,* typical of quantum phenomena which involves that any definable *subdivision* requires a change of the experimental arrangement giving rise to *new* individual phenomena. [20, p.99]

That is to say, the reciprocal *counter*-happenings at either end of the indivisible action unit, earlier referred to. This is because

Any consistent use of the concept of the quantum of action refers to phenomena *resisting* such an analysis. [18, p. 94.]

I simply fail to see how the indivisibility argument (1st passage), or the *nonanalyzability* argument (2d passage), can ever be coherently applied to the Fourier ‘reflection’ of CTY. It cannot be applied to “many”, for “many” is subdivisible by definition, if anything is. And it cannot be applied to the contrast itself *between* “one” and “many”, for these are distinct and separable by definition, if anything is. And to be applied to “one” alone, i.e. the harmonic wavetrain, furnishing the determinations of the dynamical quantities at the exclusion of the kinematic, would destroy the symmetry and the reciprocity altogether, for the converse reasoning (or the converse counter-happening), which in my account is al-ways perceivable with the 'corner of our eye' within the self same procedure, would now have to be completely shut out of the picture. And all reciprocity would be gone.

Time to conclude, by returning to what *I* take authentic quantum reasoning to consist of, and assess the link between reciprocity and CTY in the cases in which they are *both* there, as of course they should be. It seems to me that the former, reciprocity, stands closer to the *object.* For it emanates *from* the object, and proceeds from its own nature, rather than being something *applied* to it. Whereas CTY, a *descriptive* situation, stands closer to *us.* Reciprocity reflects the ontology of the quantum and CTY its epistemology. CTY would then be the descriptive result of what is essentially a deeper reality, indivisibility, and reciprocity the more immediate revelation of its presence.

7 Appendix

I have been assuming throughout that the minimal space region, over which momentum has to be defined, as in pλ=h, is equivalent with the pq product of the corresponding un-certainty, to permit the argument to apply. So that pλ=pq. This is indeed the case, as is shown below:

1. E=hν	the Planck-Einstein relation
2. ν=U/λ	from wave theory
3. E=hU/λ	from 1 & 2.
4. U=s/t or U=q/t	for the special case of an object moving on any of the coordinate axes.
5. $E=\frac{h\frac{q}{t}}{\lambda}$ or $E=\frac{hq}{\lambda t}$	from 3 & 4.
6. But h/λ=p	de Broglie's law
7. $E=\frac{pq}{t}$	from 5 & 6
8. Et=pq	from 7

But Et=h, so pq=h. Then, since pλ=h, pλ=pq.

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[21] Antonopoulos, C. “Indivisibility and Duality; A Contrast. Essay on the Logical Relation Between the Classical Variables in Quantum Mechanics”. *Physics Essays,* 7, 2, 1994, pp.187-8. The logical aspect of the situation is fully treated there, so I will not enter into it again. But the physical arguments are considerably different to those advanced presently. In that essay I had relied on energy *discontinuity,* to draw a contrast between indivisibility and duality, mostly borrowed from the early quantum theory. Here I confine myself to action quantization which, although now referring to a wave period, is still shown to conflict with duality.

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