Hidden Variables

As we saw in the last lecture, classical physics, while it may not necessarily *demand* the concepts of objectivity and determinism, at least appears to be *compatible* with them. In particular, let's consider the paradigm of an apparently "random" process in classical physics: Brownian motion. It is clear, first of all, that there is no particular problem in treating the position $x(t)$ of the Brownian particle itself as an "objective" property. On the other hand, this position (or the associated velocity) appears at first sight to behave *indeterministically*, in that it is impossible to predict, from its position and velocity at any given time, what it will do in the next few instants. The standard explanation of this state of affairs is that the particle is being continually bombarded by individual molecules, which we cannot see individually, and the details of whose motion we therefore do not and cannot reasonably know. We therefore have to specify, not the actual force, but the *probability* of a force $F(t)$. Nevertheless, so long as we are doing classical physics, there seems no compelling reason to doubt that these molecules do have definite positions $x_i(t)$, hence definite trajectories, and that these trajectories are determined by the intermolecular forces according to Newton's laws; and thus, the forces $F(t)$ exerted on the Brownian particle are perfectly definite. Hence, the perfectly probabilistic description that we are forced to give of Brownian motion is merely a result of our *ignorance*, and is fully compatible with an underlying "level of reality" that is both *objective* and *deterministic*.

Contrast the situation in QM according to the "standard" interpretation (by which I mean the kind of interpretation given in most textbooks, usually a more or less sophisticated version of the Copenhagen interpretation). According to this view, the most complete description that can ever be given of a quantum system, *even in principle*, is by the specification of its state vector $|\psi\rangle$; there simply *is* no other "reality". As in the case of Brownian motion, we can calculate (if we know $|\psi\rangle$) the *probability* of different outcomes of a given measurement; but unlike the case of Brownian motion, the need to work in terms of probability is not simply a result of our ignorance – there is nothing to be ignorant *of*! There is, by hypothesis (or fiat?), no underlying "level of reality" that could restore the property of determinism. Moreover, as to "objectivity", we have seen that, e.g., in the Young's slits experiment, the question "which slit did this particular electron go through?" appears to have no answer in the absence of measurement, even though by changing the experimental setup we can easily force the electron, as it were, to answer it.

Many people, over many decades, have found this feature of QM (or rather of its "standard" interpretation) unsatisfactory, and have postulated that in some sense or other there is an underlying level of reality that possesses (almost always) the property of objectivity and (usually, though not always) the property of determinism. In such a view (with both properties), the need in standard QM to use a probabilistic description is, just as in the case of Brownian motion, simply a consequence of our ignorance of the details of this underlying reality. The variables describing this level of reality, which are by hypotheses currently not accessible to us, are called "hidden" variables (or param-

eters), and an approach that has this character is called a "hidden-variable" theory or interpretation (see below for the distinction). Historically, many people in the very first years of QM (the late 20s) speculated about such theories, but a severe damper on this speculation was put already in 1932, when von Neumann, in his famous book *Mathematical Foundations of Quantum Mechanics*, gave what was widely interpreted as a proof that no HV theory could reproduce the experimental predictions of QM (by that time well verified). Von Neumann's "proof" was criticized as early as 1935 by the Austrian philosopher of science Grete Hermann on what, in retrospect, seem valid grounds; but it held almost undisputed sway in the physics community until 1951, when David Bohm produced a detailed HV theory that he demonstrated explicitly would, under appropriate assumptions, reproduce all the experimental predictions of QM for a single nonrelativistic particle – something most physicists had believed von Neumann had shown to be impossible! Thereafter, interest in HV theories intensified and analysis of them led to a number of striking results, which showed that if indeed the world can be described by a HV theory it must have some very peculiar and counterintuitive properties. By far the most spectacular such result is Bell's theorem (1964), which deals with the properties of correlated two-particle systems. In this lecture I shall review the question of HV theories as applied to simple "one-particle" QM, and in the next go on to Bell's theorem and related matters.

Let us start with some definitions and distinctions. For simplicity, I initially discuss the general setup of an HV approach that possesses not only the property of objectivity but also that of determinism. We imagine that the system in question possesses a "hidden" variable (or more likely a set of such variables) that we generically label λ (this notation is standard). This set of variable(s) evolves deterministically in time. When a measurement of some physical quantity A is made, the value of λ *uniquely determines* the outcome of the measurement. Note that this must be true for any variable: thus, if A, B, C are different physical quantities (irrespective of whether or not it is possible to measure them simultaneously), a knowledge of λ must determine the outcome A_i, B_j, C_k of any one of them. In general, of course, we do not know the value of λ for any particular system; so we are forced to adopt an "ensemble" description and assign a *probability* $p(\lambda)$ that a system drawn at random from the relevant ensemble has a particular value of λ. This probability assignment is of course conditional on our knowledge, and in particular, a measurement of A that gives some definite and known result a_i may force us to assign the system to a new ensemble with a different $p(\lambda)$ ($p(\lambda|a_i) \neq p(\lambda)$). (E.g., in the extreme case where there is only one value of λ that produces the value a_i of A that is observed, we clearly have to conclude that $p(\lambda) = 1$ for this λ and 0 for all others). However, in addition, it is not excluded that the measurement process itself may change the λ's and hence the relevant probability distribution.

If the HV approach is to give predictions consistent with those of standard QM, either overall or within a given domain (e.g., for nonrelativistic single-particle experiments), it is necessary that whenever the ensemble in question is such that we can assign a QM state vector $|\psi\rangle$ to it, we can also assign an HV probability distribution $p(\lambda)$ that will replicate the QM predictions, for the measurement of any physical quantity A, of getting

the value a_i; i.e.,

$$p(\text{any } \lambda \text{ such that } A(\lambda) = a_i) = p_{\rm QM}(a_i)$$

All plausible HV theories satisfy this important constraint, at least under "usual" conditions; see below.

A few distinctions:

(1) "Theories" vs. "interpretations": It is essential to distinguish these two notions. Suppose that a given HV approach is such that *under all conceivable circumstances* it gives experimental predictions that are identical to those of QM. Such an approach will then in an obvious intuitive sense be equivalent to QM for practical purposes, and should perhaps be called an "interpretation" of QM rather than a separate theory. Note that the above condition implies, inter alia, that in such an "HV interpretation", at least if deterministic, it must be impossible in principle to produce an ensemble with a *unique* value of the HV λ; for, by hypothesis, such an ensemble would give unique outcomes even for the measurement of two quantities, such as position and momentum, which in QM do not commute, and therefore satisfy an indeterminacy relation. While HV "interpretations" of QM are of some interest to philosophers of science, most practicing physicists would probably tend to regard them as artificial and rather uninteresting. Below I shall use the term "HV approach" to cover both HV theories and HV interpretations.

(2) Deterministic vs. indeterministic HV approaches: What we have described above is a deterministic HV approach. However, determinism per se is not fundamental to the notion of a hidden variable approach; in principle, one could also consider approaches in which either (a) the evolution of HVs in time can be described as a stochastic (random) process, or (b) the value of λ does not uniquely determine the outcome a_i, of the measurement of A, but only gives the probabilities for getting the different possible a_i's (or (c): both (a) and (b)). Most of the considerations developed in this lecture and the next actually apply equally to deterministic and indeterministic HV approaches.

(3) Within the class of HV theories, one sometimes distinguishes between "theories of the first kind", which give experimental predictions different from QM only when the HVs have some kind of nonequilibrium distribution (hence, generally speaking, only in circumstances that have to be very specially engineered); and "theories of the second kind", which give predictions different from those of QM even under "equilibrium" conditions, and thus in situation that are relatively easily obtained in the laboratory. Bohm's original (1951) theory was of the first kind (subsequent versions, however, have tended to reduce the "theory" to an "interpretation"); the work of Bell (1964, see next lecture) showed that all local HV theories must be of the second kind, and hence was of much more "urgent" experimental interest.

(4) Finally, there is an important distinction between "contextual" and "noncontextual" HV theories. I postpone this until later in this lecture, when the motivation

for it will be clearer, but just indicate here that, crudely speaking, an HV theory is said to be "noncontextual" if the answer to a question does not depend on what other questions one is asking simultaneously with it. (You might well ask why *contextual* HV theories should be even worth considering...)

Let us start with a brief and schematic overview of Bohm's 1951 approach, which comprises, depending on the assumptions made, both a "theory" and an "interpretation". Start with the "interpretation": In his original papers, Bohm confines himself to the simple case of a single nonrelativistic particle ensemble that standard QM described* by a complex scalar wave function $\psi(x)$, which satisfies the time-dependent Schrödinger equation. He agrees with this description, but, first, unlike the CI, regards the wave described by $\psi(x)$ as a real physical wave; and second, makes two additional assumptions, as follows:

(1) Each particle of the ensemble has a real position x. The probability of finding a particle drawn at random from the ensemble at position x is proportional to $|\psi(x)|^2$.

(2) If we write $\psi(x)$ in terms of its amplitude and phase, i.e., $\psi(x) = |\psi(x)|^{i(\phi(x))}$, then the momentum of a particle at x is given by the formula:

$$p(x) = \hbar \times \text{slope of } \phi(x) \text{ at } x = \hbar \times d(\phi(x)/dx)$$

and its velocity is given as usual by $v = p/m$. Note that the momentum (and hence the velocity) depends on the behavior of $\psi(x)$ not just at, but near, x.

Assumption (2) has an interesting consequence: we can use it, together with the time-dependent Schrödinger equation (which, remember, we are continuing to assume to be true) to work out the rate of change of the momentum (i.e., mass times acceleration) and compare the result with Newton's second law (this requires some nontrivial algebra). If in the latter, we set the force equal to the slope of the "real" potential ($F = -dV/dx$), we do not get agreement. However, if we add to $V(x)$ an extra "quantum potential" $V_q m(x)$, given by

$$V_q m(x) = (-\hbar^2/2m\rho(x)) \times (d^2\rho(x)/dx^2)$$

where $\rho(x) = |\psi(x)|$ is the square root of the probability density, then we indeed exactly recover Newton's second law! Moreover, we see that unless the numerator vanishes in $V_q m(x)$, it is automatically infinite wherever $\rho(x)$ is zero, and thus repels the particle from such spots, so that the interpretation of $\rho = \psi^2$ as a probability density is consistent. (More generally, the particle will tend to be repelled from regions where the $\psi(x)$ is small.)

Bohm shows that with these assumptions, all the predictions of QM regarding the behavior of a single particle in the absence of measurement are reproduced. Moreover, the results of the so-called "projection postulate" are reproduced if we assume that once

*For notational simplicity, I will assume motion in only one dimension.

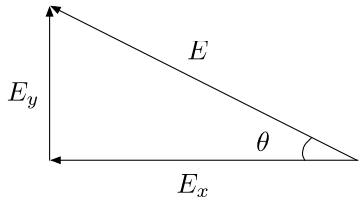
the particle is measured to be at x, the wave function everywhere else vanishes or can be ignored (more on that later).

In his 1951 papers, Bohm acknowledged that such an "interpretation" of QM might not be of universal interest, and remarked that by modifying one or more of the postulates (1) or (2), it could be tuned so as to make predictions that, at least under some conditions, differ from those of standard QM, thereby converting it into a genuine alternative theory. There are a number of possible modifications; for example, we can assume that the relation $p(x) = \hbar \times d(\phi(x)/dx)$ holds only when the distribution of the hidden variables (in this case, the particle position) is in equilibrium, and that the relaxation to equilibrium takes a nonzero time; Bohm tentatively estimated that this time might be of the order of $\hbar/k_BT$, which at room temperature is about 10^{-13} seconds. Thus, if one could do an experiment where the distribution were thrown out of equilibrium by a measurement, and a second measurement done less than 10^{-13} later, one might anticipate a deviation from the behavior predicted by QM. Some years later, in 1967, such an experiment was in fact done, but failed to show any deviation, thus refuting this particular version of the BdB theory (but not, of course, the whole idea). Nowadays, most of the minority of physicists who call themselves "Bohmians" seem to prefer to treat the Bohmian approach as an interpretation of QM rather than a genuine alternative theory.

In order to analyze the HV question in more detail, it is useful to be able to discuss a system that is simpler than (e.g.) an electron in a Young's slits diffraction apparatus. In the latter, the question "where is the particle?" has a continuum of answers; it is advantageous to be able to deal with an experimental setup where any relevant question has only two possible answers. Such a setup can be realized if we deal with the measurement of the *polarization* of photons (light). Recall that in classical electromagnetic theory, an EM wave (let's say, for definiteness, in the visible region of the spectrum) involves the propagation of an *electric field* that is always in the plane perpendicular to the direction of propagation. It is convenient to represent this electric field by a vector – i.e., intuitively, by an arrow in the plane whose direction represents the direction of the field and whose magnitude is proportional to the strength of the field. Now, it is found experimentally that there are certain materials (e.g., calcite crystals) that not only are *anisotropic* – i.e, have different "directions" associated with them that are not equivalent (in practice, the different crystal axes, the unit "cell", might, e.g., not be cubic) – but also have the following property with respect to an incident light wave: when the electric field of the light wave is *parallel* to a particular "special" axis of the crystal, the wave (i.e., its energy) is fully transmitted; whereas when it is *perpendicular* to the special axis, the wave (energy) is not transmitted at all, but rather either reflected or absorbed in the crystal (for our purposes it is convenient to assume reflection). (As usual, this is of course something of an idealization.) Such a crystal is called a *polarizer*. As we know in QM, the energy of the wave comes in "chunks" (photons); we would therefore expect intuitively that at the quantum level, a photon whose electric field is parallel to the "special" axis would always be transmitted, while one with electric field perpendicular would always be reflected, never transmitted. This is indeed found experimentally to be the case.

So far, so good. But what if the electric field of the light wave (in CM) or the photon (in QM) is neither parallel nor perpendicular to the "special" axis, but is in an intermediate direction? The experimental facts are these (we suppose for definiteness that the wave is propagating in the z-direction [normal to plane of paper] and that the "special" axis lies in the x-direction):

(a) In classical physics (which, remember, is a very good approximation for a sufficiently strong beam), we find the fraction of the energy that is transmitted as follows:



Resolve the total electric field into its "components" along the x and y-axes. Then the fraction of the energy that is transmitted is simply $E_x^2/E^2 (= \cos^2\theta)$ and the fraction reflected is correspondingly $E_y^2/E^2 (= \sin^2\theta)$. This fundamental result is called *Malus's law*. It is rather plausible intuitively: recall that the total energy is proportional to the *square* of the wave amplitude – i.e., of the electric field, E^2 – and by Pythagoras' theorem we can write $E^2 = E_x^2 + E_y^2$. Thus, it seems rather natural to associate an energy (proportional to) E_x^2 with the x-component and one proportional to E_y^2 with the y-component; i.e., to look at the actual wave as simply the "sum" of two waves corresponding to E_x and E_y, respectively (note: no interference [at least in the situation specified]!). If we assume that the x-component is totally transmitted and the y-component not at all, we immediately get Malus's Law.

A further important observation is that that part of the wave that is transmitted appears to be totally polarized in the x-direction – e.g., it will be completely reflected by a second polarizer in which the "special" axis is set parallel to y. (Correspondingly, the part of the wave that is reflected will be completely polarized in the y-direction.) Again, this looks rather natural if we separate the beam into its components as above.

(b) Now let's suppose that we attenuate the beam to the point where QM considerations become important; i.e., we have to describe it as a collection of single photons (and can actually see these arriving as flashes on some final screen, etc.). What happens when a single photon, with an electric field that is in some intermediate direction, impinges on the polarizer? Let us explicitly assume, for the sake of the present discussion, that we *actually detect* whether a given photon is transmitted or reflected; this is the analog, in the Young's slits experiment, of "shining a flashlight" to determine which of the two slits the electron went through. Under

these circumstances, it is found experimentally that each individual photon is either transmitted "in full" or reflected "in full";[†] we never get a fraction of a photon traveling on! (How do we know this? In the last resort by observing a flash on [say] the screen corresponding to transmission and nothing on that corresponding to reflection; and if necessary, by measuring the energy deposited.) If we measure the total number transmitted for a large ensemble we find that the *probability* of transmission (= the fraction actually transmitted, in the limit $N \to \infty$) is given by E_x^2/E^2 and the probability of reflection by E_y^2/E^2. Hence, QM transmission probability = the fraction of energy transmitted classically (it is clear that this correspondence is necessary if the QM and classical results are to agree for strong waves containing a large number of photons). Just as in the classical wave, we find that if a photon is transmitted then it will subsequently behave as if it were completely polarized in the x-direction (e.g., it is certain to be reflected by a 90°-rotated crystal).[‡]

The advantage of using this example is that we can analyze the behavior of the QM system (photons) entirely in terms of "yes/ no" questions (transmitted/ reflected). Moreover, by rotating the crystal axis we can generate a whole series of *different* "yes/ no" questions (e.g., "are you polarized ↑ or →", "are you polarized ↗ or ↖", etc.). Note carefully that in this setup, it is *impossible to ask two different questions simultaneously* (although, of course, we can perfectly well ask them "successively").

It is convenient to the single-photon results as follows: we know from the original experiments that if the photon is completely polarized along the x-axis ($E_x^2/E^2 = 1$), it will certainly be transmitted, whereas if polarized along the y-axis ($E_x^2/E^2 = 0$), it will certainly not be. Thus, it is tempting to regard the polarizer as "measuring" the quantity E_x^2/E^2; since for any polarization, each individual photon is either transmitted or not, we would then say that the only possible outcomes of such a measurement are 1 or 0. Clearly, the argument should apply whatever the orientation of the polarizer axes.

We are now in a position to consider the "informal" version of von Neumann's so-called "anti-HV" proof, illustrating it with the photon example. How would a (deterministic) HV theory work in the context of this setup? We would have to suppose that each photon that is incident on the polarizer is described by some hidden variable λ (or set thereof) in such a way that the outcome of the "yes/ no" question is determined, for any orientation of the polarizer axis. Thus, if the polarizer is set with its special axis

[†]However, it should be strongly emphasized that if we do *not* detect the transmission/ reflection, but rather arrange for the two beams to recombine, we can observe interference similar to that seen in the Young's slits experiment. In the context of the present discussion, we do not need to consider this setup explicitly; cf., however, the end of this lecture.

[‡]The alert reader may ask how we would actually establish this experimentally, given that the act of detection normally destroys the photon altogether. Actually, all we can say rigorously on the basis of the "transmission" data *alone* is that in a situation where to reach the relevant detector, a photon has to pass successively two "crossed" polarizers (i.e., two polarizers rotated by 90° relative to one another), no photons are detected; in principle, this is compatible with the hypothesis that they were all reflected at the first polarizer. We can, however, refute this hypothesis by an explicit measurement of the reflected beam.

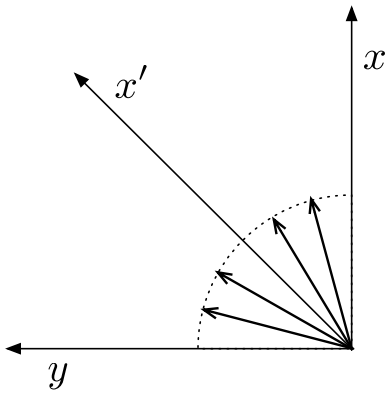
parallel to the x-direction, the HV λ should give a unique prediction for the value of E_x^2/E^2, and this value should be either 1 or 0. On the other hand, if the special axis is in the x' direction (say for definiteness at 45° to the x-axis) then equally λ should determine that $E_x'^2/E^2$ should take one of the values 1 or 0. But, the informal von Neumann argument goes on, this is impossible. For suppose that $E_{x'}^2/E^2 = 1$ (i.e., the electric field is parallel to the $x'(45°)$-axis). Then by simple trigonometry (Pythogoras!) the value of E_x^2/E^2 must be $^1/_2$, which is neither 1 nor 0. In fact it is easy to show using this argument that we cannot simultaneously arrange to get only the results 0 or 1 for *any* pair of axis that are "truly" different (i.e., not related to one another by a 90° or 180° rotation). Hence, hidden variable theories are impossible.[§]

Once the von Neumann argument has been applied explicitly to this simple example (as it was not in its original formulation), it is fairly easy to see where its fallacy lies. Consider the statement "If the value of $E_x'^2/E^2$ is 1 (i.e., the electric field lies along the x'-axis), then the value of E_x^2/E^2 must be $^1/_2$". Needless to say, if the quantities $E_{x'}$ and E_x are interpreted as the corresponding components of a classical electric field (vector), this is just a statement of simple trigonometry and is undeniably true. However, this is *not* the implicit definition of the statement "the value of $E_{x'}^2/E^2$ is 1" in the above anti-HV argument. Rather, we have implicitly *defined* the *meaning* of "$E_{x'}^2/E^2 = 1$" or "$E_{x'}^2/E^2$ was measured to be 1" on a particular photon *operationally*, by the statement that that photon passed (or would have passed) a polarizer set in the x' direction. Once we go back to this operational definition, it is clear that the statement "this particular photon would pass (i.e., be 100% transmitted) through a polarizer set at x'" in no way logically entails the statement "this photon would be only 50% transmitted through a polarizer set at x".

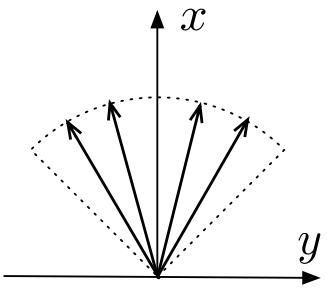
Indeed, for the special case of x' and x set at a relative angle of 45°, it is almost trivial to construct an HV theory that will reproduce the QM results: let the "hidden variable " in this case be the direction of a fictitious vector (short arrow) in the xy-plane, and when the photon in question is polarized along the x' direction (i.e., has passed an x'-polarizer) let these arrows be uniformly distributed in a fan of total angular extent 90° centered on the direction x'.

The result of a subsequent encounter with a polarizer set in the x-direction is then simply specified as follows: if the hidden variable (short arrow direction) of the photon in question is closer in direction to x than y, than the photon passes; otherwise, it is rejected. It is clear by construction that a given photon always either passes in full or is rejected in full, and moreover that the probability for each of these outcomes is 50% – in agreement with the QM predictions (and with experiment). It is only slightly more complicated to construct an HV theory that will reproduce the Malus's law prediction (probability of passage $\propto \cos^2\theta$) for arbitrary relative settings of the polarizers. However, it is essential in such a model that, if we want to reproduce the quantum mechanical results, every time the photon passes (or fails to pass) a polarizer, the distribution of hidden variables readjusts itself appropriately, e.g., in the simple "45°" example above,

[§]The above argument is formulated, for simplicity, explicitly for a *deterministic* HV theory. However, it is easy to see that introducing indeterminism does not change the conclusion.



if the photon subsequently passes an "x-set" polarizer then the distribution of the short arrows must change to a cone of angle 90° centered on the *x-axis*. (See graph on the next page.)



Why, however, should we necessarily *want* to reproduce the QM results in their entirety? An interesting speculation, in an HV theory, is that the above rearrangement of the hidden variables indeed corresponds to some kind of "equilibrium" state, but that it takes a finite time, say τ, for the relevant equilibrium to be achieved. If this is so, then a measurement of *different* polarizations with an intervening time interval $\leq \tau$ should give results different from QM. An experiment of this type was performed by Papaliolios (1967), with a time interval of about 10^{-13} seconds (space separation between polarizers $\approx 10^{-13}$ cm), and the QM results were found; this implies that in any such theory τ must be less that about 10^{-13} seconds.

A similar but technically rather more complicated argument applies to von Neumann's more general formal argument against HVs: in essence, it too begs the question by implicitly assuming that the algebraic relations that hold between quantities defined in classical terms also hold between the "values" of these quantities when operationally defined and "measured" in a context where QM is necessary. However, it should be stressed that both in the above "toy model" and in the more general argument, the dubious assumption is always about the relation of the results of two or more measurements *that cannot be simultaneously performed* (so that in QM language, the corresponding operators do not "commute" and in fact obey an indeterminacy relation similar to that of position and momentum).

A much more intriguing result, which follows from a pure mathematical theorem of Gleason (1957), relates to the possible outcomes of experiments that *can* be simultaneously performed. To discuss this it is necessary to go beyond the simple type of "yes/no" experiment discussed above, where for a given polarizer setting only two mutually exclusive outcomes are possible, and to consider the case of questions that have three (or more) possible answers. Such "questions" cannot be realized using single photon beams and polarizers, but can be formulated, e.g., with certain kinds of atoms. We may think of these atoms as possessing something similar to a "polarization" (actually it is an angular momentum) that can be arbitrarily directed in 3D space. We can then imagine that we measure the components of polarization along three axes that are mutually perpendicular in 3D space. Under appropriate circumstances, QM predicts (and experiment shows) that the component along 2 of these axes will be 1 and that along the third 0.¶ Thus, if we define a "characteristic direction" (let's call it 'c') that is in an intuitive sense perpendicular to the polarization, any experiment will tell us that c lies along *one and only one* of the three mutually perpendicular axes (and this is just what QM predicts).

Suppose now that we want to set up an HV theory to describe this kind of experiment. We then require that any given value λ of the HV should uniquely determine the result of the measurement, for *any* orthogonal triad of axes we choose to measure along. Moreover, it seems that if we choose any particular axis – call it x – then the answer to the question "does the characteristic direction lie along x?" should be either yes or no: this means, in particular, that if we ask the question "x or y or z?" and the answer is x, then the answer to the (alternative) question "x or y' or z'?" must also be x.

Now, the question of supplying an HV theory that has these properties is exactly isomorphic to the following task: color a grapefruit (or other spherical object) red and blue, in such a way that, of any orthogonal triad of points on its surface, exactly one is colored red and the other two blue. Note that in 2D the corresponding problem (involving one red and one blue arrow) is totally trivial: e.g., color successive quadrants of the circle blue and red. But in 3D (or more) *this task is impossible*!

Thus, we reach the conclusion that even in this very simple case (involving only one-particle systems), any HV theory that is to reproduce the results of QM (or experiment) must have the very surprising character that the answer to the question "x or not-x" must depend, in general, on what the choices subsumed under "not-x" are – i.e., the answer to "x or y or z?" may be "x", while, the answer to "x or y' or z'?" may be (e.g.) y' and thus not x! HV theories having this peculiar characteristic are called *contextual* (and those lacking it "noncontextual"): the result of a measurement of a particular quantity – in this case the component of c along x – depends on which other quantities are measured simultaneously with it.

Are contextual HV theories so weird that we should a priori exclude them from consideration? Again, one has to remember that one is not talking about a relationship between abstract algebraic quantities, but in the last resort about quantities that are

¶The meaning of this statement, of course, must (and can) in the last resort be defined operationally, as in the simple case analyzed above.

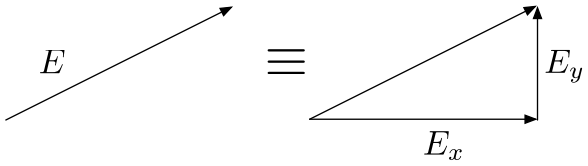
operationally defined. And the two questions "x or y or z?" and "x or y' or z'?" are incompatible – it is impossible to set up an experimental apparatus that will simultaneously answer both. E.g., in principle, it might be possible to determine the answer to the question "x or y or z?" for an excited nucleus in an anisotropic crystal by using the fact that, because of the electric quadruple coupling to the crystal electrons, these states have slightly different energies and therefore the frequency of the γ-ray emitted when the nucleus decays to the groundstate (assumed unique) will be slightly different. However, to ask the question "x or y' or z'?" one would have to, at a minimum, rotate the crystal, and there seems no logically compelling reason that under the new conditions the answer to the question "x or not-x?" must be the same as before. Thus, it is not clear that contextual HV theories, however counterintuitive they may look at first sight, are excluded by any totally compelling a priori consideration.

Alas, worse is to come. Recall that the above discussion has dealt with the possibility of mimicking the QM results by an HV theory in a simple setup where it is only the transmission or reflection of the photon that is detected. We briefly noted earlier that if we chose instead to let the "transmitted" and "reflected" waves recombine (and manipulated them appropriately), we would see the effects of interference between them, and we might well suspect that it would be difficult to reconcile this phenomenon with any simple HV theory of the Bohm type. This is indeed true, but it turns out that a much more dramatic contradiction can be established if we are prepared to consider not one photon, but two correlated ones. This is the subject of the next lecture.

Appendix: Probability amplitudes for polarization of a photon

In the body of this lecture, we were able to discuss the probabilities of transmission/ reflection of a photon by a polarizer without explicitly introducing the idea of a "probability amplitude" in this context. However, since we have already introduced this idea in, e.g., the context of the Young's slits experiments, and it will become essential in the two-photon case to be discussed in the next lecture, it is convenient to re-derive the results concerning single photons in this language.

We assume that a photon with polarization (electric field) along the x-axis ("vertical" axis) is described by a state vector $|x\rangle$ (or, as it is sometimes convenient to write it, $|\uparrow\rangle$), and similarly a photon with polarization in the y-direction ("horizontal") is associated with a state vector $|y\rangle$ (or $|\rightarrow\rangle$). Suppose now that we have a photon that we know to be polarized in some arbitrary direction in the xy-plane (say $\nearrow$ or x'). What state vector should we associate with it? Well, of course, we can perfectly well invent a state vector $|x'\rangle \equiv |\nearrow\rangle$), but the interesting question is, how do we express this in terms of $|\uparrow\rangle$ and $|\rightarrow\rangle$? There is a simple prescription: Decompose the electric field in the x' direction into its components in the x and y directions:



and define $\varepsilon_x \equiv E_x/E$, $\varepsilon_y \equiv E_y/E$. (Evidently, if the angle made by the direction x' with x is θ, then $\varepsilon_x = \cos\theta$ and $\varepsilon_y = \sin\theta$.) Then the expression of the state vector $|\nearrow\rangle$ is simply $|\nearrow\rangle = \varepsilon_x|\uparrow\rangle + \varepsilon_y|\rightarrow\rangle = \cos\theta|\uparrow\rangle + \sin\theta|\rightarrow\rangle$. Thus, the "probability amplitude" for the photon to "have" polarization in the $\uparrow$ direction is $\varepsilon_x = E_x/E$, and the corresponding probability that it has this polarization is the square of this, $\varepsilon_x^2 = E_x^2/E^2 = \cos^2\theta$, as stated in the body of the lecture. In this way, we derive Malus's law (for the QM case) and the other results quoted above.