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Inadequacy of Hydrodynamical Theories of the Aharonov–Bohm Effect

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A recent argument suggesting that inaccessible fields cannot produce observable effects is fallacious. Potential induced phase changes may take place at times for which no fields exist, accessible or inaccessible, or at locations arbitrarily far removed from regions containing fields. Hydrodynamic formulations cannot be viewed as workable and properly posed alternatives to Schrödinger type theories.

The Aharonov–Bohm phenomenon (Aharonov and Bohm, 1959; Roy, 1980) continues to be a source of interesting and stimulating articles in the literature; our considerations stem from two of these. In the most recent, Roy (1980) has stated an interesting proposition to the effect that "I show under standard continuity conditions that no effect of inaccessible fields can exist if the vector potential satisfies a condition proposed here. The condition is satisfied in actual experiments on Aharonov–Bohm effect."[1] These conclusions are fallacious and do not follow from the propositions stated by Roy. In the second paper[2] Casati and Guarneri (1979) propose a modification to the hydrodynamical treatment of quantum theory so as to make the flux condition proposed by Strocchi and Wightman (1974) assume a local form. We point out that in general neither form of the condition can be satisfied in any realistic sense and argue that one must question if the hydrodynamical theories can be viewed as acceptable replacements for a Schrodinger-type theory.

[1]There is a defect in logic in this sentence, which should read, "The condition can be..." The condition is not inherent to the experiment and need not of necessity hold. We assume this revision.

[2]This paper and Roy (1980) give extensive references to recent papers relating to the hydrodynamical viewpoint.

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The conclusions of Roy depend upon all points x^μ accessible to the beam (these defining a region $\mathscr{A}$) being pathwise connected in $\mathscr{A}$ to infinity by a single-valued and differentiable field of paths $z^\mu(x^\kappa, \xi)$, $z^\mu(x^\kappa, 0) = x^\mu$ and $z^\mu(x^\kappa, -\infty) \to$ spatial infinity. As DeWitt and Mandelstam argued (DeWitt, 1962; Mandelstam, 1962; see also Belinfante, 1962; Aharonov and Bohm, 1962), such paths sample the field adequately so that a path-dependent $A^\mu(x)$ can be expressed in terms of $F^{\mu\nu}(z(x, \xi))$ as an integral over ξ. Roy then argues that subject to his conditions (4), the following proposition holds: If there exists a single-valued and differentiable path $z^\mu(x, \xi)$ lying in $\mathscr{A}$ for every x in $\mathscr{A}$ such that the above conditions hold, then physical effects on the particles are completely determined by field strengths in $\mathscr{A}$ alone.

Even though this proposition is technically correct, it has little relevance to real Aharonov–Bohm type of experiments; the point is that all accessible (to the beam) points x in the above must also be accessibly connected in $\mathscr{A}$ to spatial infinity by some suitable differentiable (in both x and ξ) field of paths $z^\mu(x, \xi)$. According to Roy this was supposedly the case in the actual experiment, but, as we shall argue shortly, this is in fact quite impossible. In any case, it could be arranged so this possibility is obviously excluded; for example, we might provide a spherical return path for the magnetic flux large enough to allow the whole experiment to be performed in the interior field-free region, electron gun, detectors and all. Simply stated, there is then no way to find the accessible fields (which are all vanishing) or accessible potentials (which do not vanish) as expressions in the accessible fields alone. DeWitt–Mandelstam-type paths $z(x, \xi)$ would have to pass through the confining sphere, sampling the field therein. Since it is obvious that in this case standard quantum theory predicts an unchanged Aharonov–Bohm-type result, the argument of Roy in no way minimizes the Aharonov–Bohm phenomenon.

In reality, the Roy argument fails to apply in a much more serious sense. This relates to the fact that the field of paths $z^\mu(x, \xi)$ is restricted by the requirement that the path-dependent potential be differentiable and single valued—one is not free to choose the set $z^\mu(x, \xi)$ arbitrarily, but they, too are subject to conditions of single-valuedness and differentiability. Stokes' Theorem (which is actually a primitive definition in terms of which the differential operator curl is a derived concept—see any text on differentiable manifolds) and the vanishing of the fields at infinity imply that if x and x' are infinitesimally close in the field-free region $\mathscr{A}$, then paths $z(x, \xi)$ and $z(x', \xi)$ in $\mathscr{A}$, joined by, say, a straight line segment from x to x' and a curve at infinity, must enclose no flux, for the path-dependent potential vanishes at all points along both these paths. Stokes' Theorem must hold (DeWitt, 1962; Mandelstam, 1962; Belinfante, 1962; Aharonov and Bohm, 1962) if a vector potential is to be defined at all.

If we consider the actual experimental setup, or one involving a solenoid with a flux return or keeper, or a toroidal field, we see that we cannot define a differentiable path-dependent potential at neighboring points by paths in $\mathscr{A}$ linking the flux return and, if we attempt to define such a potential for all x by avoiding such linkage (passing in the same sense about the flux return), we are forced to link the central flux region. For obvious topological reasons and in view of the source-free character of the magnetic field, this a circumstance that cannot be avoided; we have no choice but to choose $z^\mu(x, \xi)$ for some x so as to pass through regions containing flux. This precludes the possibility of taking the paths attached to all x contained in $\mathscr{A}$ as themselves lying totally in $\mathscr{A}$. Roy failed to notice these matters in his argument with the result that his conclusions, far from applying to the experimental situation, have no validity so long as flux closure holds. His specific suggested path fields $z^\mu(x, \xi)$ [as in his (7) and (8)] would intersect any conceivable flux return, so the potentials in the accessible region $\mathscr{A}$ cannot be obtained in terms of the field in $\mathscr{A}$. Nor can any other choice of the $z^\mu(x, \xi)$ achieve the end he desires. The proposition offered by Roy has little if any relevance to Aharonov–Bohm-type experiments.

Idealized configurations such as infinitely long solenoids contradict the realistic constraint of flux closure so that conclusions based upon path-dependent potentials (which reflect the breakdown of realistic global field topology) are then of no relevance to any real experimental configuration. Unfortunately, Roy's remarks relating to a finite solenoid configuration contribute nothing new since his proposition is not applicable, the fields being accessible in part and certainly unavoidable by any field of paths $z^\mu(x, \xi)$.

Now consider the "electrostatic" version of the experiment discussed by Aharonov–Bohm in which an electron beam is split and allowed to pass through two conducting cylinders. During the passage through these and while the particle packet is well within the field-free screened interior, the total charges (and hence Coulomb or Lorentz gauge potentials) on the cylinders are lowered in one case and raised correspondingly for the other, then returned to zero charge before emergence of the particles. Though the beams are never in a non-vanishing-field region, hence feel no forces, an observable relative phase discrepancy between the two beams develops.

We consider this same experimental procedure in the gauge in which $A^0(x,t) = 0$ for all (x,t). This gauge has some interesting properties and was considered early in the history of quantum electrodynamics by Pauli and Heisenberg (Heisenberg and Pauli, 1930) and by Oppenheimer (1930); we only need consider the external fields as classical (unquantized). As always $\mathbf{E} = -\operatorname{grad} A^0 + (1/c)(\partial \mathbf{A}/\partial t) = (1/c)(\partial \mathbf{A}/\partial t)$ and $\mathbf{B} = \operatorname{curl} \mathbf{A}$ with the spatial components of **A**, in this gauge, giving both **E** and **B**. The curiosity, in relation to the Aharonov–Bohm discussion, lies in the fact that, inasmuch as $\mathbf{E}=0$ inside the cylinders always, then $\partial \mathbf{A}/\partial t=0$ in this region for all t. Since we may choose $\mathbf{A}=0$ before the process began, it follows that no potentials at all exist in the region within the cylinders at any time. Clearly, vanishing potentials inserted into the Schrödinger equation give no effect whatsoever, not even a phase effect, so that it would seem that we have established a contradiction bringing the predicted relative phase shift into question. That this is not the case follows by examination of the character of the solution potentials in this gauge.

The potentials can be obtained from Lorentz gauge potentials A_L^{μ} by using the gauge function $\Lambda(x,t)=-c\int_\tau^t A_L^0(x,t')\,dt'$, where a turn-on time τ for the external charges is required in general to define the integral; τ may be taken in the distant past and is of no concern here. An explicit expression for the potentials may be obtained from Λ or, by Green's function methods, directly from the field equations, giving, analogous to the usual Lorentz gauge-retarded potentials,

$$A^i(x,t)=\int d^4x'\left\{\frac{\delta(R^0-R)}{R}j^i(x')-\frac{R^i\delta(R^0-R)}{R^2}j^0(x')\right.$$
$$\left.-\frac{(1-h(R^0-R))R^i}{2R^3}j^0(x')\right\},\qquad R^0>0$$

where $R^0=x^0-x^{0\prime}$, $R=|\mathbf{x}-x'|$, $R^i=x^i-x^{i\prime}$ and $h(x)=-1$, $x<0$ or $+1$, $x\geqslant 0$. Hence, thanks to the $\delta(R^0-R)$ factor, the first two terms are of the usual retarded character (the first is the Lorentz gauge potential) but the last term is peculiar to this gauge and involves an integral over the total past history of all charge densities $j^0(x')$ at x' preceding intersection of x' with the past light cone drawn from (x,t). This last term is thus of a cumulative nature; furthermore, in most near field macroscopic cases, certainly in the instance at hand, this last is the dominant term.

The explanation for the apparent contradiction is then easily seen. As a result of the R^i factor in the last term for $A^i(x,t)$, the **A** inside the cylinders will indeed be very small, but outside the cylinders an **E** field of substantial size will exist and, since $\mathbf{E}=(1/c)(\partial\mathbf{A}/\partial t)$, so will a significant **A** field. However, this potential does not simply return to zero once the cylinders are discharged as one might anticipate on the basis of Lorentz gauge—rather, the potential simply stops changing in time, leaving the accumulated resultant.

This resultant persists indefinitely, and, as the electrons emerge from the cylinders, enters by way of the Schrödinger equation into a change in the relative phase. That this phase change is as expected is trivially verified (as must be, due to gauge invariance). An amusing way to see this is to notice that, after the fields are turned off, then $\partial\mathbf{A}/\partial t = 0$ and $\mathbf{A} = \text{grad}\,\Lambda(x)$. If $\Lambda(x)$ does not vary rapidly over the beam packet, then the phase shift will be

$$\Delta\varphi|_a^b = \frac{e}{\hbar c}\int_a^b \mathbf{A}\cdot d\mathbf{x} = \frac{e}{\hbar c}\int_a^b \text{grad}\,\Lambda(x)\cdot d\mathbf{x} = \frac{e}{\hbar c}[\Lambda(b) - \Lambda(a)]$$

If b is taken as the point of detection, where the relative phases become observable, and a the exit of the cylinder, we see the result is as expected. Notice that $\Lambda(x, t)$ acts as an effective "phase potential" during the time after turn off when it is time independent, but $\Lambda(x, t)$, even for those times, is not a phase function. That is to say, although $d\varphi = (e/\hbar c)\text{grad}\,\Lambda\cdot d\mathbf{x}$, this does not integrate to a function $\varphi(x)$ independent of previous path, so that the phase $\varphi(x)$ of the packet located about x is not in general $\Lambda(x)$; though $\Lambda(x)$ is single-valued, it must be pieced onto equal-phase initial conditions as a double-valued function. One beam runs up the $\Lambda(x)$ phase surface, while the other runs down it—starting from the same phase. And all this happens after the charge has been removed and while the packets are in field-free space outside the cylinders. Indeed, the phase change takes place at a time when there are no fields anywhere, accessible or inaccessible, and when this condition has persisted for times long in comparison with the times for field propagation over distances comparable to the dimensions of the apparatus. Thus any argument that the phase change, viewed as an "effect" in itself, is due in a local causal sense to fields, inaccessible or not, is pointless.

If one persists in viewing the wave function phase as intrinsically observable, which it is not, and hence in viewing a change of phase as an "effect," then in this gauge the effect resulting from charge on the cylinders takes place long after the "causal" condition has ceased to exist—these imposed conditions remain encoded for eventual effect only in the nonobservable potential. Causal constraints apply to observables, but one cannot in general construct a continuous system of observables carrying the causal evolution; indeed, this would effectively define a classical, not a quantal system evolution.

Similar considerations apply in the magnetic Aharonov–Bohm experiment and bear directly upon the position espoused by Strocchi and Wightman (1974), recently amplified upon by Casati and Guarneri (1979). We may consider the electrons as confined to a region $\mathscr{A}$ in which the fields vanish and in which the potential is locally—though not globally—a gradient. If we choose a thin semi-infinite slab with the wisker (or its extension) as its only boundary, with the interior of the slab the region R

and exterior R', we may position R as we please by nonintersecting deformation of the slab (keeping the whisker as an edge) and choose a gauge function $\chi(x)$ such that, in the new gauge, $\mathbf{A}(x)=0$, all x in $R' \cap \mathscr{A}$. If we join $\chi(x)$ on one side of the slab to its value on the opposite side by some smooth function, then $\mathbf{A}(x) \neq 0$ in $R \cap \mathscr{A}$ and the total effect on the wave function in this gauge will take place in $R \cap \mathscr{A}$. We may, by positioning the slab (a nonphysical construct), place R in the left-hand beam with no phase change on the right, or in the right-hand beam with no phase change on the left. Once again we see that the location at which the phase change takes place is of no physical significance whatsoever. In fact, by running R upstream in the beam a distance l and then bending the slab through, say, the left-hand beam, we can put the "effect" an arbitrary distance from the field region in the whisker.

Further, since the pair $(\mathbf{A}, \psi)$ uniquely generates the set $(\rho, \mathbf{j}, h)$ of hydrodynamical (density, current, and stress tensor) field quantities, we may investigate in detail the change in $(\rho, \mathbf{j}, h)$. The overall effect of passing through R at l is a constant phase change $e\Phi/\hbar c$; such a phase change leaves $(\rho, \mathbf{j}, h)$ invariant—there is simply no way of encoding such a change in $(\rho, \mathbf{j}, h)$ prior to the detection region. In the region of eventual observability, $(\rho, \mathbf{j}, h)$, as computed from $(\mathbf{A}, \psi)$ will reflect the phase shift, but tracing $(\rho, \mathbf{j}, h)$ as obtained thru the mediation of $(\mathbf{A}, \psi)$ back to times before beam overlap will, for both right- and left-hand beams, result in $(\rho, \mathbf{j}, h)$ independent of the flux. While the Schrödinger theory presents a well-defined continuous evolution problem, the hydrodynamical theories clearly do not. The set of field equations (2.14)–(2.16) of Strocchi and Wightman (1974) do not "provide the required manifestly gauge-invariant local substitute for the Schrödinger equation" as they suggest. It is their contention that by addending the nonlocal integral condition $\int_L (\mathbf{j}/\rho)\cdot d\mathbf{x} = e\Phi L/mc$ for all closed paths L, Φ_L being the enclosed flux, a criterion is provided to select from the manifold of solutions to the hydrodynamical equations an acceptable solution. That this is not a well-defined procedure becomes immediately apparent upon considering the obvious query—how does one satisfy such a condition before the electron gun is turned on (considering the whole process, which we may, including the switching on of the gun, as one long time-dependent procedure)? For such times the electrons are localized in the gun filament and for essentially all other locations $\mathbf{j}$ and ρ both vanish, so that the left-hand side of the proposed condition for all L is simply undefined.

Even if we could accept the integral condition we would thereby spoil the local character of the theory, and DeWitt and Mandelstam have already shown that a nonlocal, fields-only theory is possible; thus Casati and

Guarneri propose an alternate local substitute [their (2)] for this integral condition. To be effective it most hold everywhere (in which case it implies the integral condition) and it thus fails to be defined for the same reason. Strocchi—Wightman recognize that a problem exists to the extent that $\mathbf{v} = \mathbf{j}/\rho$ must be defined even in inaccessible regions; this gives rise to the contention that a tail, however small, of ψ penetrates into these regions thereby defining $\mathbf{v}$ everywhere. As we have now seen, this contention can be stretched to the point of being wholly incapable of realistic implementation.

The basic origin of the difficulty inherent in such hydrodynamic theories is clear enough—the condition that some region $\tilde{R}$ be inaccessible at some stage in the process is that $\rho = 0$ in $\tilde{R}$ (more precisely, that $\psi = 0$) whereas the flux condition is given in terms of $\mathbf{v} = \mathbf{j}/\rho$. Thus, the nicely regular (and experimentally realizable) $\rho = 0$ condition (or limit) of the theory translates into a highly singular condition in hydrodynamical theories.

The inadequacy of hydrodynamical theories is even more obvious in the electrostatic case where, by mediating a solution into $(\rho, \mathbf{j}, h)$ through $(\mathbf{A}, \psi)$, we see that in $A^0 = 0$ gauge $(\rho, \mathbf{j}, h)$ cannot show any change at times when ρ is localized inside the cylinders (which can be made as impregnable as we please), so that any purported tail penetration into field regions outside the cylinder is in fact totally ineffective for time periods as long as we wish to make them (one need only alter the length of the cylinder). Any significant "tail-wagging-dog" effect is locked out of $(\rho, \mathbf{j}, h)$, for these cannot change without a corresponding change in $(\mathbf{A}, \psi)$, which does not take place. The change in $(\rho, \mathbf{j}, h)$, as in $(\mathbf{A}, \psi)$, only takes place much later when no fields are present. Unless one is prepared to accept some sort of catastrophic onset of phase alterations triggered somehow by beam overlap and governed somehow by evolution equations of such bizarre character as to make a realistic evolution prediction unworkable, one must accept that $(\rho, \mathbf{j}, h)$ can only be found by mediation of the truly basic theory, the $(\mathbf{A}, \psi)$ theory.

Just prior to beam overlap the $(\rho, \mathbf{j}, h)$ complex reflects negligible, if any, encoded data of field presence. That a major field-dependent effect is immediately thereafter observed is in the nature of a catastrophic event and, as usual, indicates an improper choice of variables of description, a projection, onto a representation space, with singular Jacobian. A proper choice of variables, $(\mathbf{A}, \psi)$ specifically, avoids this catastrophe. Alternatively, one may assert that the hydrodynamical theories are intrinsically incapable of presenting initial-value problems as properly posed problems; we know of no attempt to establish that the $(\rho, \mathbf{j}, h)$ theories present a well-posed initial value problem and the attitude seems to be that, since the $(\mathbf{A}, \psi)$ theory is well defined and since a mapping into $(\rho, \mathbf{j}, h)$ is given explicitly, then the resulting formalism must inherit the requisite status. As we have argued here, this fails to be the case due to the singular character of the connecting map.

The mathematical concept of "well-posed" is not equivalent to "logically consistent" alone but also requires that small changes in input data, loosely speaking, give small changes in output results. In the usual $(\mathbf{A}, \psi)$ theory, the question as to whether ψ is exactly vanishing in some region, or only so small as to be unascertainable, is of little consequence. The $(\rho, \mathbf{j}, h)$ theory is certainly logically consistent; we contend that it is apparent that such a theory is not well posed.

Leaving aside various assertions in the literature based upon outright error (Bocchieri and Loinger, 1978, 1979; Bocchieri *et al.*, 1979, 1980)[3], there is no basis for doubting the correctness of the predictions of the standard $(\mathbf{A}, \psi)$ theory, and, as a well-posed theory, these results are insensitive to the usual idealizations (impenetrable solenoids or tubes, infinitely long solenoids, etc.), whereas the $(\rho, \mathbf{j}, h)$ approach depends in a crucial way upon such physically inconsequential distinction.[4] Logically speaking, it is precisely the seemingly innocuous role of wave-function–field overlap in contrast to the large-scale result which is the Aharonov–Bohm effect; or, in the terms used here, the Aharonov–Bohm effect is the well-posed character of theory couched in terms of potentials contrasted with the ill-posed character of theory expressed in terms of fields. It is our contention that the ill-posed character of $(\rho, \mathbf{j}, h)$ theories is so severe a burden that they cannot be viewed as workable alternatives to standard theory, no matter the logical equivalence of the two views in the strict sense.

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[3]These fallacious arguments have been fully and correctly refuted by A. Zeilinger, 1979; Bohm and Hiley, 1979; Klein, 1979; and Rowe, 1980.

[4]As we have seen, moreover, idealizations which contradict flux closure cause problems for DeWitt—Mandelstam type path-dependent potentials.

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