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Which gauge matters?

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Abstract

I present a new interpretation of the Aharonov–Bohm by identifying an overlooked physical field: the current field. The interpretation is non-arbitrary, completely local, deterministic, and separable. I then consider the connection between theoretical methodology and fundamental ontology by considering Earman's call to consider gauge matters. I conclude that we should be clear about which gauge matters we consider, and question the tight connection he draws between the methodological and ontological roles of gauge fields.

Keywords: Gauge theory; Semiclassical electrodynamics; Intrinsic formulations; Aharonov–Bohm effect; Vector potential

0. Introduction

I would like to try, in a constructive way, to miss what is widely regarded as the point of Aharonov and Bohm's (1959) discovery of the action of electromagnetism far from the electromagnetic fields. I will ultimately propose that there is nothing particularly puzzling about the effect beyond the obvious fact that it shows that there is more to electrodynamics in the quantum theory than we would have thought from the classical theory.[1] Aharonov and Bohm themselves claim that the fact that there can be such an effect should not be

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[1]An anonymous referee points out that Brown & Pooley (2001) claim that a purely classical effect analogous to the AB effect arises in Weyl's theory unifying electromagnetism and general relativity, in the so-called second clock effect. Similarly, John Stachel (for example, in discussion in Cao (1999, p. 313)) identifies a classical analog of the AB effect arising from "the gravitational field of an infinite rotating massive cylinder". Without going into

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surprising. In their 1961 follow-on to the paper where they discuss the effect, they point out that any number of results in quantum theory confirm the view that force has there become superfluous (Aharonov & Bohm, 1961). They cite, for example, the Bohr–Sommerfeld quantization rules for atoms, and how these are explained in terms of standing wave phenomena which obviously (insofar as they imply discrete energy levels) are not derivable from the concept of force (p. 1512). Their claim there is that it should then come as no surprise that similar modifications in our expectations concerning electrodynamics must be made.

The basic problem for interpretation is that there exists in electrodynamics an entity that is apparently ill defined, or underspecified by the totality of facts about the world. This quantity, the gauge potential[2] of electrodynamics, was once thought to be no more than a mathematical trick but has become an object of concerted study by physicists and philosophers of physics since Aharonov and Bohm showed that in the hybrid theory of quantum mechanics of particles plus classical electrodynamics of fields, the gauge potential is not so obviously dispensable, nor, apparently, is dispensing with it free of costs. I want to show that it is dispensable and with minimal cost. Thus I hope to undermine the story of the necessity of viewing the gauge potential as real by illustrating how the gauge freedom[3] of the electromagnetic part of the theory is eliminable in semiclassical electrodynamics. While in some sense this is already well known, it is not overtly known. I want to correct this oversight, and also to explore some implications for philosophy of physics and perhaps physics as well of recognizing and reflecting on the dispensability of the gauge potential. I will suggest that we replace the gauge potential with the gauge invariant quantity I will call the "current field" and identify *that* as the appropriate vector potential for electrodynamics.

In the first section, I discuss the status of the gauge field **A** in the classical theory and explain the sense in which it is dispensable there. I follow this with a brief summary of the Aharonov–Bohm (AB) effect. I then present a brief overview of the current state of attempts to unpack the philosophical significance of the effect. While by no means exhaustive, the overview does indicate that there is a broad consensus that deep metaphysical adjustments of some sort are necessary.

I then try to display the AB effect as the result of intrinsic processes arising from the presence and disposition of charges alone in such a way that it becomes plausible to think that charges are the only entities in classical (i.e., non-quantum) electrodynamics, and that all fields arise therefrom. The gauge freedom is shown to be a mathematical artifact. While there is nothing new in the semiclassical theory of electrodynamics, it *is* true that in that theory a proper analog of a classical electrodynamical property[4] is found to interact with quantized charges in a way that its analog does not interact with classical charges. So I will

(*footnote continued*)
prolonged discussion, I will merely point out that whatever these effects may show (and I reserve judgment) they do not immediately suggest that the *actual* AB effect in Minkowski spacetime is of non-quantum origin.

[2]Note that I use the expression "gauge potential" to refer to what is standardly called the "vector potential". It is precisely the equivalence of these potentials, standardly assumed in discussions of gauge theory, that I intend to challenge here.

[3]"Gauge freedom" indicates that certain quantities are not fixed by the theory itself, nor by any observations, but can be assigned values arbitrarily.

[4]Here "property" is to be understood loosely as an assignment of geometric objects (scalars, tensors, spinors, etc.) to spacetime points. Little in what follows relies importantly on how this expression is understood.

claim that we have an old property (the current field) with a new effect, or rather an old property that is only now seen to be efficacious. Note that even if one rejects my claim that my analysis of the AB effect is metaphysically neutral, the analysis still requires significantly less in the way of novel ontology. There is nothing non-local, or indeterministic, or non-separable about the solution that I propose. That alone should recommend it.

Many efforts have been made in recent years to unpack the philosophical significance of the AB effect. The particular focus has, actually, been on the proper interpretation of gauge fields given that the naive interpretation of electrodynamics—according to which the only quantities of ontological interest in the theory are **B** and **E** (or $F^{\mu\nu}$)—has been decisively refuted by the AB effect. Thus, so the story goes, any interpretation of gauge theories that does not take seriously the gauge potential itself (where taking seriously is taking as more than a convenient calculation device) must, like the naive interpretation of electrodynamics, be abandoned. So with the naive view out of the way, several competing views on the proper interpretation of gauge theories have sprung up. There are roughly four suggestions on the table for how to interpret the gauge potentials employed in these theories: the real gauge potentials approach;[5] the fields only approach; the holonomies approach; the fiber bundle approach.

I will not here address what I take to be a key interesting feature of the AB effect, the various connections between classical, semiclassical, and quantum field theoretic accounts of electrodynamics (and other gauge theories) that a satisfactory analysis of the effect would illuminate. I am entirely sympathetic to the suggestions made at various places by, for example, Belot (1998) and Leeds (1999), that such connections, and what they reveal about how to interpret the claims of physical theory, are at the heart of what we do, and should do, as philosophers of physics. And yet discussion of such issues will be reserved for the future. Instead I will here restrict myself to the no less important ancillary task of illuminating the correct way to understand the effect semiclassically. I suspect that a more outré account than the one I give of why we should reject the potential but accept the current field will suffice in the case of QED as well, but I will not argue for that claim here.

I conclude with a brief discussion of Earman's suggestion that gauge does and should matter to philosophers of physics.

1. The Aharonov–Bohm effect

Before beginning discussion of the AB effect itself, I will first rehearse the role of the gauge potential **A** in classical electrodynamics, pointing out that there we can dispense with it if we are so inclined.

1.1. Classical electrodynamics

The standard story of classical electrodynamics is that there are real quantities **B** and **E** and **j** and q that are responsible for all electrodynamical phenomena and that there are other quantities **A** and ϕ that serve no "real" function. **A** and ϕ are, according to this

[5]There is a naive and clownish version of this: the "One True Gauge" interpretation. If it is not obvious, let me say that I will be endorsing something that seems a little like the "One True Gauge" interpretation. Perhaps I can make it clear, in what follows, why the take I provide on it is not so clownish after all.

story, merely calculation devices with no physical significance. It is an interesting mathematical fact about the form of the magnetic field, for example, that the field is derivable from a potential. That is, the fact that $\nabla \cdot \mathbf{B} = \mathbf{0}$ implies that there is some quantity $\mathbf{A}$ such that $\mathbf{B} = \nabla \times \mathbf{A}$. It then follows that $\oint \mathbf{A} \cdot \mathbf{dl} = \oint \mathbf{B} \cdot \mathbf{da}$, that is, that the line integral around a loop is equal to the flux through the area bounded by the loop. Now even this simple situation gives rise to a significant question: how is it that the information about the state of the field goes from the interior of the solenoid out to the path over which the integral is taken? For the structure of the theory, the fact that the only forces generated in the theory involve products of the form $q\mathbf{E}$ and $q\mathbf{v} \times \mathbf{B}$, prevents the measurement of any effect associated with the bare field $\mathbf{A}$. There is an even more troubling worry about $\mathbf{A}$ though. Because of the way $\mathbf{A}$ appears in the theory, we can self-consistently add to it the gradient of an arbitrary scalar function, and change no predictions of the theory. Facts of this sort prompt a fictionalism about $\mathbf{A}$. We simply deny that there is such an entity and regard $\mathbf{A}$ entirely as a mathematical trick. That is one possibility. Another possibility though is that we take seriously another field, the Liénard–Wiechert (LW) potentials which are completely definite and defined purely in terms of intrinsic, local properties (the light cone structure and the relativistically invariant 4-current). This field, $\mathbf{C}$, satisfies $\mathbf{B} = \nabla \times \mathbf{C}$ (in any choice of frame). While some other mathematical quantities do as well, we have no ontological interest in those quantities because they are just math tricks according to this interpretation. Admittedly in this regime there is little incentive to adopt the LW interpretation of the vector potential either. But the possibility remains.

1.2. Semiclassical electrodynamics

In semiclassical electrodynamics, the fields and currents are still described classically, but there are quantum-mechanical charged particles that can couple to the fields. Imagine that a beam of electrons has been split in some way, but in such a way that one is not in position to measure whether any particular electron is in any particular component of the beam, and so the two parts of the beam will interfere if they are brought together again. One normally thinks of the two slit diffraction set-up here. Then we direct the parts of the beam in different directions around a very long solenoid. When the solenoid carries current the interference fringes are shifted (in general) from the case when the current is zero. The explanation of the effect, and indeed the stimulus for the prediction, is that the electron wave functions of the two parts of the beam have changed relative phases due to their separate interactions with the vector potential of the magnetic field inside the solenoid.

In general in quantum theory, the change in a particle's phase is given by the action integral over its path of travel. Thus in the AB effect, there is a term $\int \mathbf{A} \cdot \mathrm{d}\mathbf{x}$ for the component of the beam going one direction and a term $\int \mathbf{A} \cdot \mathrm{d}\mathbf{x}$ for the component in the other direction. Because the sense of rotation differs between the cases, the integrals are of opposite sign. And thus the total phase difference for the two components is $\oint \mathbf{A} \cdot \mathrm{d}\mathbf{x}$.

There is also a dual effect: we can direct the two components of the beam into Faraday cages. Once they are well inside we can hook the cages up to a battery of potential V during their travel through the cages. As they emerge we unhook the battery. What results is again a difference in phase between the two components, this time the total phase difference is given by Vt, for t the time spent in the cage.

This is all old news. But I emphasize that we know something novel has happened because in each case the behavior of the electrons has changed even though they never experience any electromagnetic fields. What the novel feature of the situation is remains unclear, and philosophers who have studied the effect have diverse accounts of that novelty. I turn now to a very hasty canvas of some of these accounts.

2. Philosophers' diverse consensus

There are by now many excellent treatments of the AB effect and gauge theories more generally in the philosophical literature. These reveal a great variety of possible responses to the AB effect, and there is no firm consensus about which to prefer. Nevertheless, there is a clear consensus in the philosophical community that the AB effect forces us to adopt some novel metaphysics that goes beyond quantum mechanics alone. That is, philosophers almost uniformly believe that in addition to whatever metaphysical adjustments are necessary for accounting for the world described by the quantum physics of particles coupled to the classical physics of fields, the AB effect teaches us that we must make further adjustments. It is necessary, according to these philosophers, to change radically our conception of what it is to be a property, or to introduce further non-determinism into physics, or to allow for the non-local action of a classical field, or to allow quantities with no definite value to count as good physical quantities. There is broad agreement that some metaphysical intuitions about what kinds of thing there are, or what kinds of property there are, must be given up. Before introducing my metaphysically deflationist account of the effect I outline four important approaches in order to illustrate the kinds of conceptual change these views require and by way of contrast make clear that a view eliminating the gauge potential, **A**, has much to recommend itself. These approaches are: real gauge potentials; fields only; holonomies; fiber bundles.

2.1. *Real gauge potentials*

One option is to bite the bullet and take the gauge potential as real. The idea is to suppose that initial data for electrodynamics includes a specification of the actual value of **A**. Although unknown, perhaps, there is still some particular value of **A** associated with the total state of the system. The problem with this approach is that it is not deterministic, in contrast with our widely held views about electrodynamics, because even given such an initial state nothing in the theory specifies how **A** is to evolve. Here is a non-determinism above and beyond whatever might be required in quantum mechanics itself. Maudlin's (1998) "One True Gauge"—where there are spies that report both on the state of the **B** field and on the state of the slits in the apparatus—is such an approach. Maudlin ridicules that approach for being ad hoc and completely unmotivated. While the system itself is deterministic on such an approach, our theory is not since no observable facts could ever reveal the true state of **A** nor its rule of evolution. (As I show below, my approach will have only a superficial resemblance to this one since my approach is physically motivated, and comes with an evolution law given by electrodynamics itself.)

2.2. *Fields only*

In classical electrodynamics we can ignore the gauge potential entirely and work exclusively with field quantities **E** and **B**. While most view the AB effect as establishing that we must treat the gauge potential as real in semiclassical electrodynamics, we can if we like stick with just **E** and **B**. Since knowledge of the magnetic field suffices to specify the phase change on a complete circuit of the solenoid by the electron, then we clearly do not *need* any new ontology in semiclassical electrodynamics. The price we pay for such minimalism, however, is that we must allow the magnetic field to act non-locally—it acts on the electron at a place where it itself vanishes.

This interpretation is troubling not only because it requires non-locality, but more importantly because of the character of that non-locality. The **E** and **B** fields were introduced in the first place to avoid having magnets and charges act non-locally. To find now that these fields are themselves acting non-locally is extremely puzzling to say the least. While not the only interpretation that appeals to non-local action, this one is not widely adopted. (Sakurai, 1994 seems to have an interpretation like this in view in his quantum mechanics textbook.)

More popular are the holonomies and fiber bundle approaches (and some think they are more or less the same (e.g., (Wu & Yang, 1975)) while others (e.g., Healey, 2001)) think they are strikingly different.

2.3. Holonomies

The holonomies approach cleverly avoids the gauge dependence of the theory by substituting new field quantities for the gauge potential.[6] To each closed curve of space, γ, assign a quantity $h(\gamma) = e^{i\oint_{\gamma} \mathbf{A}\cdot d\mathbf{x}}$, the holonomy around γ. One can then formulate electrodynamics using only these variables and the electric field. No indeterminism arises here since the holonomies have integrated away all gauge dependence. However, we must introduce a significant change in how we understand properties: Belot (1998) suggests that electromagnetic predicates are no longer assignments of values to points, but to regions, and concludes that the approach is non-local since specifying the state of a region will require knowing the value of all holonomies in space (544); Healey (1997) agrees that we must either see non-locality here or see that the properties that arise on this approach are what he calls "non-separable" since the holonomies view implies that there are physical processes occurring in some spacetime region that are not supervenient upon an assignment of qualitative intrinsic physical properties to that region. Either account of the demands of the approach implies rethinking the metaphysics of properties in electrodynamics (and physics more generally).

2.4. Fiber bundles

A trivializable fiber bundle[7] $\langle E, B, \pi, G\rangle$ consists of a manifold E (the total space), a manifold B (the base space), a continuous map π from E to B such that, $\forall x \in B, \pi^{-1}(x)$ is homeomorphic to a space F (the typical fiber), and a single element group G (the structural group of the bundle) of homeomorphisms from F onto itself. That is, for a trivializable bundle, the homeomorphism $\phi_{k,x} \circ \phi_{j,x}^{-1} : F \to F$ is the identity map for all $x \in U_j \cap U_k$, where U_j, U_ks are open sets of F. We also require that $E = B \times F$. Define a section of the

[6]For more extended discussion consult Belot (1998), Healey (2001, 2004), and Wu & Yang (1975).

[7]I am only concerned with these both for simplicity, and since the fiber bundle of electrodynamics is trivializable.

bundle to be a smooth curve $\chi : B \to E$ such that $\chi(x) \in \pi^{-1}(x)$. In the case of electrodynamics the base space is physical space and the typical fiber is $U(1)$.[8]

Following Leeds (1999) we can understand semiclassical electrodynamics in the following way: choosing a gauge for the electron wave function amounts to a specification of a section χ such that $\Psi(x) = \psi e^{i\chi(x)}$. We can then think of the action of the gauge potential in a particular gauge as governing the parallel transport of the electron (via its contribution to the canonical momentum) in the base space and rotation of its phase in $U(1)$. That is, moving an electron along in physical space changes its phase by $\int \mathbf{A} \cdot d\mathbf{x}$, and in particular moving it around a closed path, γ changes its phase by $\oint_\gamma \mathbf{A} \cdot d\mathbf{x}$. We can then think of the various elements of the gauge family $\{\mathbf{A}\}$ as arising from different sections of the bundle, each of which is understood as a choice of electron gauge.

But, says Leeds, if we understand this approach literally by thinking of the different "fiber elements as representing different properties of electrons" we must say that "distinct wave functions in the same gauge will represent different states, even if they differ only by a constant phase" (pp. 613–614). And this apparently flies in the face of widespread consensus that such quantum states are equivalent. So this interpretation itself will require more in the way of interpretation. Beyond that issue the interpretation suffers as well from non-locality or non-separability in Healey's sense. For the particular vector $\mathbf{A}$ that arises on a given electron gauge still is determined only by considering *all* closed paths. How much to shift the electron phase at a given point is fixed once the fiber section is specified, but whether the connection on the bundle is trivial or not requires global information.

2.5. Novel metaphysics

The above remarks are necessarily too compressed and hasty for a full introduction to the issues that arise in trying to interpret electrodynamics in the light of the AB effect. I do hope to have indicated, however, that all of these approaches introduce metaphysical novelties that go beyond the actual data in the AB effect itself. It should be clear what I am asked to give up both when I take seriously the potential in semiclassical electrodynamics and when I do not take it seriously. While it may not seem troubling on the face of it to accept non-locality, given that there is some non-locality or other involved in quantum mechanics itself, I think this is decidedly an extra worry. Similarly the idea that properties of a classical field are somehow definable only in terms of the facts about all of space also seems quite troubling. The idea of such an holistic account of properties is contentious even in the case of quantum mechanics itself, with some claiming that such a view is incompatible with relativity theory. Similarly, the choice between non-locality and indeterminism is unwelcome. I think it is worth seeing if there is some other option. What follows is, I think, the least metaphysically troubling view of the AB effect to date:[9] least troubling because there is no arbitrary fixing of a gauge, there are no non-local interactions, there are no holistic properties, there is no indeterminism. There is only an assignment of properties (vectors) to spacetime that arises purely from intrinsic electromagnetic quantities.

[8] See Chouquet-Bruhat, DeWitt-Morette, & Dillard-Bleick (1982) for details.

[9] Work in progress shows that there is an even more minimal approach that, while completely local, involves only the electric and magnetic fields.

3. The trivial response: A is physical

Given all the conflicted and conflicting accounts of how to understand the reality of the vector potential, I suggest we see how far we can get by retreating to the position widely adopted pre-AB effect: no gauge dependent field is real. That is, there's nothing that a gauge dependent quantity does that cannot be done equally well by something else. We have seen one way of adopting this position: the holonomies view. We also saw that adopting the holonomies view entailed abandoning what are often taken to be key features of physical properties; we had to abandon locality, or local revelation, or local supervenience. It is not clear of course that older intuitions about physical properties cannot be abandoned in light of new physical results. On the other hand it is not mindless metaphysical conservatism to demand strong reasons for giving up locality, or separability, or determinism, or local revelation. Especially where there are many competing views about what to give up that rely on strong intuitions about the importance of one or the other of these requirements, I think it is best to be cautious.

I wish now to take an even stronger line than merely denying the reality of gauge dependent quantities. I will eliminate all *reference* to gauge dependent quantities. My tame suggestion along these lines is that we recognize the primary role that current plays in the AB effect and replace our differential equation for the vector potential, $\mathbf{B} = \nabla \times \mathbf{A}$, with a sum over definite integrals that expresses a field arising from the current itself. This I will call the "current field". That, in substance, is my suggested interpretation of electrodynamics, what follows is a clarification and response to some possible objections.

3.1. The current field

If my suggestion is adopted, then some terminological confusion is sure to arise. So I will reserve the term "vector potential" for the quantity that is causally responsible for the AB effect. In earlier discussions, that quantity is defined by $\mathbf{B} = \nabla \times \mathbf{A}$,[10] but here it will be given by a different quantity to be introduced below. The argument below is meant to establish that exactly one member of the gauge family defined by $\mathbf{B} = \nabla \times \mathbf{A}$ is the true vector potential, in the sense of being the true causal agent. I thus reserve "**A**" for any arbitrary member of the gauge family, "vector potential" for the quantity at issue here, and "**C**" for the current field, the quantity that I *define* below and *argue* is the true vector potential.

I will define **C** as the LW potential. Begin with the LW potential of a single charged particle with 4-velocity $V^{\alpha}(\tau)$ and charge e as follows (see, e.g., Jackson, 1975, pp. 608–612, 654–656): The 4-current of the charge is

$$J^{\mu}(x) = ec \int \mathrm{d}\tau\, V^{\mu}(\tau)\delta^{(4)}[x - r(\tau)]. \tag{1}$$

[10]In earlier discussions there is disagreement about how **A** plays its causal role, but it is clear that it is the quantity defined by $\mathbf{B} = \nabla \times \mathbf{A}$.

The retarded[11] Green's function is given by

$$D_r(x-x') = \frac{1}{2\pi}\theta\left(x_0 - x'_0\right)\delta\left[(x-x')^2\right], \tag{2}$$

where θ is the step function (= 0 for times in the future and = 1 for times in the past) and δ is Dirac's delta function. Then the LW potential of a single charge is

$$C^\mu(x) = \frac{4\pi}{c}\int \mathrm{d}^4x' D_r(x-x')J^\mu(x'). \tag{3}$$

Thus $C^\mu(x)$ is non-zero for a single charge on the past light-cone of x. After suitable manipulation this can be put into the form,

$$C^\mu(x) = \left.\frac{eV^\mu(\tau)}{V\cdot[x-r(\tau)]}\right|_{\tau=\tau_0}, \tag{4}$$

where τ_0 is given by $[x-r(t_0)]^2 = 0$ and we impose the retardation condition $x_0 > r_0(\tau_0)$. The above derivation is really no more than a precise way of saying that we define a vector-valued field at every point (except that of the charge itself) that is given by the registration of the charge on the past light-cone of that point. Notice that the current field of a single element of current is a vector field given by the value of the charge divided by the distance from the observation point to the charge and pointing in the direction of the current.

The reason I focus on the LW potential is that it is purely intrinsically and locally definable in terms only of the charge and the geometric structure of spacetime.[12] The vector field defined in this way is completely gauge invariant since it results from a definite integral over gauge invariant quantities. I am unaware of any other "gauge" that involves neither extra geometric structure that is not part of Minkowski space nor a non-local assignment of values to points in spacetime. For example in the Coulomb gauge we must employ both the instantaneous Coulomb potential as well as the transverse current. Both of these quantities are non-local, and locality in the theory is preserved only because their non-local contributions, so to speak, cancel each other out. The standard story of gauge freedom suggests that we cannot choose a gauge non-arbitrarily, and that we can see thereby that nature itself cannot make such a choice. Beyond the metaphorical imagery, the claim seems to be that any quantity that arises arbitrarily in a theory is not in nature, but only in our theory. I accept this story in its broad outlines and suggest that only the LW potential of a single charge arises from a non-arbitrary analysis of what is real in a world accurately described by classical electrodynamics.

Now however, summing the LW potential over all charges in the causal past of any point gives rise to another gauge invariant vector field. I will call this the current field **C**.[13]

[11]Because this is not an paper on the interpretation of quantum mechanics, and since the status of the transactional interpretation of quantum mechanics is not clear, I will consider only the retarded solutions here. I do not think this restriction will have any interesting effect on the claims I am making. Whether the future is influencing the past in this way has little to do with the question of what is causing the potential—it will be the charges in either (or both) case(s).

[12]Indeed spacetime need not be Minkowskian. All that is used in the definition is the light-cone structure connecting the charge and the observation point.

[13]There is an issue here concerning incoming radiation in the infinite past. However, this radiation acts purely through its electromagnetic field. That is, the electromagnetic field tensor alone is sufficient to characterize fully electromagnetic radiation. Gauge freedom actually makes a kind of sense here since the charges giving rise to that

Thus the full current field of the solenoid is

$$\sum_{i=0-n}^{\text{Charges}} C_i^{\mu}(x) = \sum_{i=0-n}^{\text{Charges}} \frac{4\pi}{c} \int \mathrm{d}^4x' D_r(x-x') J_i^{\mu}(x'),$$

where the sum is over all charges in the past of the observation point.[14]

On the present suggestion we can, if we like, view **C** as the "reality" underlying the AB effect. **C** is numerically identical to just one member of the family of vector fields defined by $\mathbf{B} = \nabla \times \mathbf{A}$. Those other quantities, defined by replacing $\mathbf{A} = \mathbf{C}$ with $\mathbf{A} + \nabla\lambda$, will be mere mathematical artifacts of little interest. Why did we think they were important for understanding the ontology of electrodynamics? We have here an interesting question in the history of science. A rough and ready answer is that because of an accident of history that defined the vector potential as any field **A** satisfying $\nabla \times \mathbf{A} = \mathbf{B}$, and therefore originally masked its reality, we were unable to see that just one member of the family is numerically identical to a real, causally efficacious,[15] non-arbitrary, local quantity that I call **C**. That confusion has had, in my view, important negative consequences for our understanding of electrodynamics. We now should recognize, I claim, that $\mathbf{B} = \nabla \times \mathbf{A}$ is merely *true of* the vector potential not *the truth about* it.[16] On this tame view we would not be eliminating the vector potential but rather reserving the expression "**C**" for the real quantity of interest. Insofar as it is numerically identical to one member of the gauge family defined by $\mathbf{B} = \nabla \times \mathbf{A}$ we might be seen misleadingly to be "fixing a gauge". But the correct view, I claim, is that only here have we finally found the *physical* field underlying the AB effect—the current field defined by summing over all charges of the retarded Green's function of the 4-current.

One might well wonder what differs between this version and a version that allows the **B** field to interact with the electron even though it is never at the location of the electron. But note that the value of **B** at the location of the electron is zero. Thus the fact that the **B** field registers on the past light cone of the electron is irrelevant, and so the **B** field cannot act alone—we would require spies of the kind Maudlin describes to suss out the value of the field there and report it, and then the electron would have to act as though the **B** field were non-zero right there. But this isn't what happens with the new field. The new field (or newly taken seriously field) that I've described just is how the value of the 4-current[17] on the past light-cone of the electron registers at the position of the electron. And the interaction of the electron's wave function with that field is just the 4-d inner product. The new field behaves, in this respect, just as we thought the vector potential was behaving. For, as is obvious, it is numerically identical to one member of the gauge family. Yet it is not a gauge dependent quantity, for it does not arise as a solution to the differential equation of before, but rather from the definite integral equation I have outlined. And since the field arises

(*footnote continued*)
radiation are not even in our universe, and so the current giving rise to the potential would be observable only through the incoming radiation.

[14]Of course one could sum over all charges in spacetime and rely on the Green's function to restrict to the past light-cone. I prefer to make explicit that we need refer only to the past light-cone in the first place, and that the Green's function merely restricts us to the surface of the cone rather than its full interior.

[15]The causal efficacy here is quite strange, as Healey has urged to me. But see below.

[16]Naturally it is the full truth about the gauge potential, but the point at issue is whether the gauge potential and the vector potential are best viewed as identical.

[17]*That* value, moreover is in principle observable from the electron's location.

physically as the sum of the contributions from the individual charges, there will be no principled issues about adapting the interpretation to the field theoretic setting. We will continue to maintain that the interaction between charges is the origin of relative phase changes, and that total changes will arise as the sum of the changes from individual interactions.

It might be instructive to recall that the magnetic field itself arises in very much the way the current field does. We consider an element of current and, roughly speaking, differentiate twice with respect to distance from the element of current. The complication is figuring out which direction the field is pointing, and that is given by a vector normal to the plane defined by the current's direction and the distance vector to the observation point. As is well known, in the case of an infinite or very long solenoid, the contributions from the various current elements cancel in the exterior region. Clearly it would not make sense to ask how the magnetic field registers at the observation point; the magnetic field *is* an answer to a question about how the current registers at the observation point. The salient difference between the magnetic field and the current field is that only the latter is non-zero outside of the solenoid because of the way the various contributions add up. Here is more indirect evidence that the current field is the quantity of interest—it arises in parallel with, and in much the same way as, the magnetic field itself.

Is my choice of interaction the only "gauge" that does not require non-local specification of a function throughout spacetime? Surely there are global properties of spacetime (the Minkowski metric for example), but these seem very different from fields that must involve non-local specifications from the very beginning in order to be themselves definable. Now it may be that some gauge, numerically identical to $\mathbf{C}$, is specifiable in such a way that it will involve such quantities. But as we have seen $\mathbf{C}$ is definable purely locally, and so it does not require such non-local specifications while on the other hand such choices as, e.g., $\nabla \cdot \mathbf{A} = \mathbf{0}$ do.

I am no advocate of a return to a naive machianism that requires all of physics to be framed in directly observational terms. Yet there is something highly artificial about suggestions that trade in locality (only in a non-observable way) to eliminate granting reality to a quantity that is gauge dependent and hence indefinite (but that too only in a non-observable way). Part of the artificiality must amount to the fact that quantities of this sort trade heavily in objects and properties that are unobservable in principle, and that are, moreover, defined in terms of other objects and properties that are unobservable in principle.

On the other hand, we are well justified in appealing to the action of the charge–current distribution in the past of the observation point. And indeed, we are in a much better position to measure that than the $\mathbf{E}$ and $\mathbf{B}$ fields themselves since *those* are always given in terms of the force on or velocity of some charge distribution or other. We do in fact help ourselves to the values of the 4-current in the rest of our electrodynamics in order to underwrite most of our standard explanatory stories; normally j_μ is considered a good physical quantity.

3.2. Objections

3.2.1. Topology

Kennedy (1996) offers a perspective on the vector potential similar to mine, at least in that he is groping toward an account that takes due notice of the role played by the charge–current density.[18] He uses Hodge decomposition to break the potential uniquely into two components. One is the locally definite harmonic subcomponent (harmonic means it is closed and its dual is closed), and the other, the exact component, which contains all the gauge freedom of the potential. He shows that the AB effect arises purely from the harmonic subcomponent. Thus, he argues, the AB effect is non-topological—i.e., is not to be understood via Wu and Yang's non-integrable phase factor, nor via holonomies. While it is natural to think that the effect is topological because the potential may always be set to zero at any point, Kennedy points out that it is only the total potential that can be set to zero; the harmonic component is unaffected by a gauge transformation, and the exact component must change in order for the entire potential to vanish.

While I think Kennedy is on the right track, his view has at least two problems. First he does not motivate a connection between the mathematics and the physics. In particular he fails to indicate why one should be concerned with closed versus exact forms. One might well ask why we should think that the harmonic part of the potential is responsible for the phase changes when they clearly arise from the expression $\mathbf{A} \cdot d\mathbf{x}$. Despite the uniqueness of the decomposition, I think what is missing from his account is an explicit connection between the harmonic component and the current. Second, and this point is related, the uniqueness of the Hodge decomposition fails for certain manifolds.[19] As Healey (1999) points out, the failure of the decomposition theorem for semi-Riemannian manifolds entails either abandoning local action for the harmonic potential, or abandoning claims to the effect that it is a real, physical quantity (pp. 306–307). The main issue is that when changing the current in the solenoid, one must include the time development of the electromagnetic field (non-zero for exterior regions when the current is changing) and so must take into account the semi-Riemannian character of Minkowski space. For static fields I can entertain the fiction that I am isolated in Euclidean 3-space, but for dynamical fields I cannot. Absent a unique decomposition, Kennedy cannot non-arbitrarily choose to identify one component of the potential as real and the other as a mathematical fiction. Insofar as I join Kennedy in rejecting the topological character of the effect, I appear to have a similar problem. This appearance is misleading however. My account does not rely on particular facts about the topological structure of the space. Instead it appeals directly to facts about charge–current distributions that are independent of topology. In the simple case of manifolds with a unique Hodge decomposition, the current field and the harmonic component of the gauge potential coincide. But there is no a priori reason to think that they coincide in general, and in particular the current field is non-arbitrary, uniquely defined, and appropriately behaved even when the current is changing.

There is a yet more powerful reason for taking the AB effect to be topological. One can perform the following thought experiment: excise from space the z-axis. How are we then to quantize the electron we scatter in the AB experiment?[20] That is, how will we know that we have correctly identified the correct quantization routine before knowing how electrons actually behave on scattering around the excised region? For all we can tell in advance, the electron may pick up a phase on circumnavigating the "hole". We thus will need to specify facts about path integrals around all spacetime loops before we can specify all the properties in spacetime. Another way of putting the worry considers a non-trivial

[18] He is explicit about this only near the end of his discussion, so it plays little role in his actual account.

[19] I am indebted to Healey for urging me to make this point explicit, and for the example.

[20] See Belot (1998, pp. 547–548) for further discussion of this issue.

potential, $\mathbf{A} \neq \mathbf{0}$ that satisfies $\nabla \times \mathbf{A} = \mathbf{0}$ everywhere. This is possible since the Poincaré lemma fails for such a spacetime.[21] So, given that $\mathbf{A}$ is non-zero, the integral around the excised region will be non-zero, and there will be a shift in the interference fringes. In this situation there is no solenoid and no current, and thus the effect is not due to the current field.

The example is misguided. The AB effect was discovered because Aharonov and Bohm noted that quantum mechanics predicts that the vector potential will exert an influence on the wave function of the electron in the absence of a magnetic field at the location of that influence. We are thereby moved to modify our electrodynamical ontology in order to account for this influence. We next observe that the topology of spacetime could similarly exert an influence on the wave function of the electron if there were a non-trivial field with everywhere vanishing curl. But how is this story connected to the electromagnetic effect? It is true that we can derive $\mathbf{B}$ from the curl of a potential. But does that show that every non-vanishing function with vanishing curl is to be identified with the vector potential even when the magnetic field vanishes *everywhere*? I do not see why it should. Indeed the fact that a change in the topology of space makes such a field possible suggests that we should not; one is clearly associated with the charge–current, and the other with the structure of space itself. Perhaps Einstein's and Weyl's and others' convictions that physics just is geometry could motivate one to adopt such a view. It is even possible, I suppose, to *demonstrate* that Einstein and Weyl were correct, if they were. However, the argument that the (hypothetical) topological effect is to be identified with the electromagnetic effect does not establish such a claim, it rather *relies* on it.

Here is a much simpler way to understand the topological thought experiment: topology affects the momentum operator, it affects how wave functions are moved around in spacetime. Those who think that the magnetic potential is best understood as a connection field should have no objection to that claim. Here we have a quantity that arises independently of any electromagnetic situation—the non-trivial connection field—whose very existence is crucially dependent on the topology of spacetime. Why not suppose either that we should adopt a new quantization scheme when faced with a new topological situation, as Belot suggests, or that there is a new connection field on spacetime and conclude that on either suggestion the feature of interest arises from the topology? I think all parties to the debate should be content with one or the other of those suggestions. But why then associate the topological situation with the electrodynamical situation? Why not instead suppose that the canonical momentum relies on two separate components: spacetime features such as topology and electromagnetic features such as the current? The importantly distinct feature of the electromagnetic AB effect that arises from a solenoid is that we can control it; I can turn the solenoid on and off, I can increase the current, etc. The brute fact that some region or other of spacetime displays an AB type effect due entirely to the topology of spacetime is in no way suggestive of the appropriate way to understand the electromagnetic effect. Solenoids are not excisions of spacetime points. Solenoids are objects that carry current, and current is moving charge. I claim that it is the current that gives rise to the AB effect, and that claim is independent of, and does not fail in the light of, the existence of some other cause that also shifts the phase of an electron.

[21]This way of putting it is due to a referee's suggestion. I am grateful for the formulation, and the suggestion that I address the concern directly.

3.2.2. Gauge freedom again

One thing that is wrong here is that the theory *still* has gauge freedom. That is, once we have solved for the wave function of the electron using the current field, we are still allowed to perform a local gauge transformation. If we change the phase of the wave functions with a position dependent quantity we must transform the momentum operator in the Lagrangian. That is obvious. So does that not involve a change $\mathbf{A} \to \mathbf{A} + \nabla\lambda$? And does *that* not show that there is no way to avoid the old-style gauge potential? No. All the change in the Lagrangian shows is that the wave function is sensitive to the form of the derivative operator. But we would need a separate argument to establish that the current field itself should be involved in that sensitivity. The $\nabla\lambda$ that arises from a phase change in the wave function is irrelevant to the question of the causal efficacy of the vector potential now re-conceived as the current field. The function that appears in the derivative operator appears there *only* because of the arbitrary change we have introduced into the phase, the position dependent phase factor. If one is not antecedently convinced that the three different features involved in the canonical momentum (electrodynamics, topology, arbitrary phase transformation) are essentially the same, their formal similarity is not sufficient to lead to such a conviction.

In the literature on the AB effect the following analogy seems to exert a powerful effect on the imaginations of all involved: in electrodynamics we can define a family of vectors, $\{\mathbf{A}|\mathbf{B} = \nabla \times \mathbf{A}\}$ parameterized by $\nabla\lambda$. In quantum mechanics the momentum operator is the gradient. But in quantum mechanics with electrodynamics, the canonical momentum includes the magnetic potential. In order now to adopt a phase invariant interpretation of quantum mechanics, we must augment the canonical momentum with a the gradient of the scalar change in phase, $\nabla\phi$. The formal analogy between $\nabla\lambda$ and $\nabla\phi$ seems to cry out for interpretation. Similarly the fact that a non-trivial topology violates the Poincaré lemma and allows a field that is formally identical to the vector potential seems to cry out for interpretation. But the proper interpretations are easily found. The vector potential changes phases, certain topological features of spacetime (may) themselves shift phases, a change in representation may shift phase.[22] But it is the momentum operator that does all of this, so all of these features must be exercised through something that is formally analogous to *that*.

The preceding discussion is just another way of endorsing a number of different criticisms of the "gauge argument" that have appeared in recent years. Teller (2000), Martin (2002), and Batterman (2003), for example, all point out an important ellipsis in a standard argument for the existence of electromagnetism. The standard argument goes like this: in order to preserve invariance of quantum mechanics of a charged particle under a position dependent phase factor, one must move from the mechanical momentum to the canonical momentum. But the new momentum operator has a factor that looks just like the classical electromagnetic gauge potential. Thus we have derived electromagnetism from gauge invariance. As many have noted, nothing in the argument assures us that there ever will be a non-zero electromagnetic field. All we pick up by the argument is the *form* of the canonical momentum. I want to go a bit further and claim that even in the case of non-zero magnetic field there is no reason to link the quantity identified by the gauge argument with

[22] See Leeds (1999) for an extended discussion of the connection between our choices of representation of the wave function and gauge invariance.

electrodynamics directly. As I hope to have made plausible, that formal analogy does not uniquely dictate an interpretation of the various components of the canonical momentum.

3.3. The reality of the current field

What is missing from *all* the accounts of the AB effect I know about is a recognition that a definite integral over an quantity known to exist on spacetime produces a strong prima facie contender for a real quantity. Then when we have reason to believe that there is some real quantity or other hanging about—because we see effects—we can look over the crowd of mathematically similar prospects and rule out those that are not strong contenders because they lack a direct connection to real quantities. Consider the gravitational potential in Newtonian gravitation theory.[23] There is no mathematical advantage in taking the gravitational potential to be $\Phi = \int(\rho/r)\,\mathrm{d}\tau$ instead of $\Phi = \int(\rho/r)\,\mathrm{d}\tau + k$ for some constant k. We do not worry about the significance of Φ because it has no observable effects. But if it did, and we had to regard it as *somehow* real, then we would look for a unique, intrinsic quantity to identify as the gravitational potential. The most natural view would be that $\Phi = \int(\rho/r)\,\mathrm{d}\tau$ and those other quantities were solutions to a differential equation ($\mathbf{F}_g = \nabla\Phi$) that is now outdated as a *definition* of Φ, but is still suited for figuring out the gravitational field given a value for Φ.

In the case of the vector potential we do have a good contender for a physically real field, something built out of the electromagnetic current and the unique light-like path from any element of that current to the observation point (i.e., the electron). The quantity we could imagine ourselves observing has a quite similar character to other clearly real electromagnetic quantities. Some effect at a distance x from a source takes place at time $t = x/c$ later. I see no reason not to claim that the electrons are measuring the retarded current. True, we have access to their measurements only after they make a full orbit of the solenoid; so they are not good measuring *devices*. But the components of the electrons' wave functions do "report" on the strength of the solenoid (modulo factors of 2π) once for each trip *they* take around the solenoid, and there is no difficulty understanding this in terms of changes in phase at each point of the path if we adopt, for example, Feynman's sum over paths version of quantum mechanics or Liu's (1994) wave packet approach. Moreover, the retarded current appears to be the only quantity straightforwardly definable only in terms of quantities that are clearly real in the theory.

I do not have a good a priori argument for why we should think of the retarded current as itself a field that can interact with the electron, and I suspect there is not one. A posteriori however there is, I think, good reason for treating the current field as real given that it provides a local, deterministic, separable account of the AB effect without ever mentioning the $\mathbf{B} = \nabla \times \mathbf{A}$ relation. So its independent existence is established the way we establish the existence of any other quantity of interest, by a combination of experiment and intuition. Experimentally we know that something numerically identical to one of the vector potentials is causing the AB effect. Our intuition tells us that its the charges.

One might object at this point that we have good reason to *deny* reality to the retarded current since it lacks a number of features some take to be important for real fields. For

[23] David Malament also noticed the connection to between the gravitational potential and the story I am trying to tell here. I am grateful to him for pointing it out.

instance it carries no momentum or energy.[24] Thus we have a strong presumption against considering this to be one more field among many that acts in a standard way. And thus is belied my claim that I have introduced nothing qualitatively new.

But what is really new here is not the field itself (an assignment of a property to each point of spacetime definable in terms of intrinsic features of the field in the causal past of those points). What is new is its efficaciousness, and that efficaciousness *is* a peculiarity of quantum mechanics. What is not new, on the other hand, is the character of the field's interaction. As do other real fields (the electric and magnetic fields for example) it does obey Healey's demand of separability (as defined above). An assignment of intrinsic, accessible properties (the value of the retarded current as measured at the location of the electron) suffices to account fully for the infinitesimal changes in the value of the electron's phase. It is strange that the quantum phase is sensitive to fields that do not bear momentum, but it is *only* strange. To admit the reality of the retarded current field, we need violate no apparently well grounded *principles* governing physical systems; local revelation, synchronic and diachronic locality are preserved. Instead we need merely recognize a feature peculiar to quantum as opposed to classical matter: that matter can interact causally with some fields that carry no energy.[25]

There is definitely something peculiar about finding out that the phase of the electron interacts with the current in the way that it does. But note that this interaction seems to be the least peculiar of the various suggestions, because only the present proposal is completely local, separable, and deterministic. All of the extant interpretations of the origin of the AB effect require some novel change as a result of the electron's passage around the solenoid. Versions that deny the sensibleness of speaking about the phase of the electron will require that the phase change differ from one direction around the solenoid to another, and also require some non-local interactions or non-separable properties that give rise to these changes. Versions that assert the sensibleness of speaking about the phase of the electron will require non-local, or non-separable interactions. And both require in addition that the vector potential have an influence on a measurable quantity. Only the present proposal introduces *only* the latter novelty. Yes, in quantum mechanics there is something new: the phase of the electron wave function interacts with the field of the 4-current. That is a truly novel and interesting feature of quantum mechanics. But it's merely a new effect of an old property. It is not even a new property much less a new kind of property.

Thus the AB effect is deflated.

4. Gauge and theory

Earman (2002a, 2002b) has urged us to attend to gauge matters. But which gauge matters? There are two senses to the question: (1) I have discussed the one "gauge" that matters in the AB effect, and have demonstrated that the AB effect follows from local facts about the distribution of 4-current on spacetime. I presented an outline of an intrinsic formulation of the effect, and suggested that it is the unique formulation involving only the current and the light-cone structure—two quantities that are clearly "real" in

[24]This objection is due to Healey, and I am grateful to him for mentioning it.

[25]In future work I will show how a failure of distributivity of the interaction between the electron and the flux from the individual current elements in the solenoid, can eliminate even this apparent strangeness.

electrodynamics. So it matters which "gauge" we pick. (2) For the second sense in which gauge matters, I suggest below that there are two senses of gauge that should be distinguished: one involves ontological/metaphysical matters, and that one has no bite; the other involves methodological/epistemological matters, and I offer one (deflationary) proposal for a methodological response to the discovery of "gauge freedom" in a physical theory—find the sources. For ontological/metaphysical issues, gauge matters not at all. For methodological/epistemological issues, there is some place for gauge matters.

Noether's first theorem establishes that *unless* there is a conserved current we will not have form invariance of the Lagrangian under change of variables. That by itself would seem to indicate that the conserved charges are causally involved in the change of phase of the quantized electron, and not that some mysterious gauge field both is a real (because influential) and is not a real (because not fixed by the equations of motion) quantity that must be interpreted in order to bring explanatory closure to the situation. This contra-positive version of the Noether's theorem, I believe, has considerable force in indicating that we should look to conserved quantities in formulating gauge theories.

The connection to quantization of the Hamiltonian framework employing potentials is more obscure than Earman (2002a, 2002b) would have it. We can applaud his commitment to taking seriously the views of physicists on these matters. And yet "their" views are not entirely univocal since there has been some controversy among physicists about how to understand the effect. Consider the views of just two, DeWitt and Belinfante, concerning the relevance for quantization of local, potential formulations of the theory. Immediately after Aharonov and Bohm discovered their effect, De Witt and Belinfante proposed theories that would account for it without explicitly invoking the vector potential.

DeWitt (1962) objects that there is a straightforward "demonstration that quantum mechanics *can* be formulated solely in terms of field strengths" (p. 2189). And after he does so by redefining the wave function Ψ he concludes: "Non-relativistic particle mechanics as well as relativistic quantum field theories with an externally imposed electromagnetic field can therefore be formulated solely in terms of field strength, *at the expense*, *however*, *of having the field strengths appear non-locally in line integrals* (p. 2190). He notes that we get just the right commutation relations on the spacelike hypersurface where these integrals are taken, and points out that though the anticommutator of Ψ and $\bar{\Psi}$ does not vanish for all spacelike separation, that's true of causal propagators entering into real physical effects anyway (p. 2190).

Belinfante (1962) then generalizes DeWitt's result. Since DeWitt's theory admits a gauge transformation even though formulated solely in terms of field strengths, Belinfante says it should not be called "gauge invariant" (p. 2833). If we average over all possible redefinitions of Ψ in DeWitt's framework we get back the radiation gauge. And we can then produce an electrodynamics of gauge independent quantities alone.

DeWitt and Belinfante both conclude that there is no need to afford any reality to the vector potential.

Aharonov and Bohm (1962) reply, but not by objecting to the mathematical claims of DeWitt and Belinfante. As far as I know there is nothing wrong with DeWitt's or Belinfante's analysis, so we can if we like adopt these gauge invariant suggestions. The Aharonov and Bohm reply instead concerns the *theoretical status* of moves like the above. They suggest, but do not prove, that only in the case where a local, potential-based theory is available are the non-local versions self-consistent.

DeWitt (1962) responds to Aharonov and Bohm in same issue by disputing the significance of a local formulation as tied to a complete set of commuting observables. He says that the real significance of such a formulation is that it allows for a causal description of effects. Since potentials are not observable, but only their group invariants are, they provide themselves an overcomplete *not* complete set of commuting observables and do so only through expanding the Hilbert space in a nonphysical way (p. 2191). In any case the complete set of commuting (anti-commuting) operators is easily obtained in DeWitt's framework (p. 2191). He concludes that quantum electrodynamics is not given by the requirement of locality, but by experiment. The real controversy concerning potentials is over the question of local versus non-local theories (p. 2191).

Belinfante (1962) goes so far as to suggest that, in terms of looking for a "theory based on a complete set of commuting *observables determining a* Hilbert space", the gauge-independent theory is superior to conventional versions (p. 2836, his emphasis).

The views of DeWitt and Belinfante seem to call into question the tight connection between the gauge features of electrodynamics and its proper quantization. These views are especially relevant since DeWitt is no fringe player here, but rather a founder of attempts to canonically quantize gravity. Two points are in order: first consider the case of quantum gravity and the current interest in the loop quantization program (similar in many respects to the holonomies approach to electrodynamics). It is not the gauge nature of gravity that makes the loop quantization program appealing. That was present as well in the old variables approach to canonical quantization, and we have seen what a dismal failure that version of the theory turned out to be. What is interesting about both canonical approaches (metric formulation and connection formulation) is that they continue to take seriously both the dynamical character of gravity and the quantum mechanical character of all dynamical fields. They are also interesting for showing how two versions of the same classical theory (differing in their formulations, or the answer to the question of what the theory is about) can lead to entirely divergent theories under quantization (Mattingly, 2005). Second, formulations of theories in terms of potentials that lead to gauge motivated worries are irrelevant to the actual quantization of classical theories if DeWitt is correct that (perhaps non-local) formulations without potentials are always possible.

What now of our ambitions to use what we have learned about gauge theories as a result of our investigation of the AB effect? Here I think the question is much more interesting, and concomitantly more difficult. I would like to make two suggestions, one perfectly general and one slightly more focused. First, by all means we should make whatever epistemological hay we can out of the gauge field analysis we have been undertaking for the last 45 years. The loop formulation of quantum gravity, for example, is quite an interesting case of the fruitful use of a holonomies version of gauge theory. And while I do not regard the ontology of that formulation as of particular significance, I am mindful of DeWitt's (1962) suggestion that there might be a deep theorem relating nonlocal and local versions of field theories. And certainly the holonomies model of electrodynamics is interestingly linked to the loop formulation of quantum gravity; and that version is, as far as I know, the only one in which we have managed to produce a solution to the Wheeler–DeWitt equation. And so methodologically, the gauge principle (whatever that might be) and the attention paid to gauge over the last few decades have produced important rewards. So, let a thousand interpretations—that is, methodological heuristics—of gauge theories bloom. But do not get too caught-up in the ontological claims and demands of these interpretations.[26]

The second point is also methodological and yet it has its own metaphysical foundation. All things being equal, an intrinsic formulation of a theory is to be preferred. These formulations are able to bring us into closer contact with the actual assertions of the theory. I do not claim that these formulations will bring us into closer contact with reality but these formulations *will* make it clear what count as the basic commitments of the theory. In the version of electrodynamics I have outlined, there are no quantities that are indeterministic, no quantities that give rise to non-local effects, no quantities that are gauge dependent. Of course there is the local gauge freedom of the quantum wave function. But that is quantum mechanics, not electrodynamics. How sharply can we separate these two? I do not really know. But if our suspicion is that quantum mechanics is a constraining rather than constituting theory, we can separate them at least far enough to underwrite my point about electrodynamics: it is one theory in classical, semiclassical, and quantum versions. Only the new interaction with the wave function separates these versions.

I would like to end with a few words about how in general we should view the gauge freedoms that we see in many of our best physical theories. Earman (2002a) suggests that we take as empirical "confirmation of the gauge interpretation of GTR dictated by the Dirac constraint formalism" the success, or at least progress, of programs employing that interpretation (p. 21). It seems to me that such a move would be short-sighted. He claims that he is not offering an appeal to authority, but it is difficult to see what force the success of a theoretical program exerts on our interpretive efforts. Naturally we do not want a return to the bad old days of philosophy attempting to dictate to physical theory, and especially in a way that ignores the results of that theory. But a large part of the issue for philosophy of physics seems to be understanding what it is exactly that the results of the theories we have really are. Let me contrast Earman's assessment with that of DeWitt. (Now I hope that I am not myself merely offering an argument from authority, but I take what DeWitt says rather as useful advice from someone who has sat on both sides of the canonical quantization program.) Thus: "There may be a theorem... that any non-local theory formulated solely with observables, which satisfies certain causality requirements, also has a local form related to the non-local form... But would this imply that the potentials... have themselves a special physical significance? Not obviously" (p. 2191). Similarly he says that the fact that two of Maxwell's "equations, from a purely mathematical viewpoint, proved to be statements of the necessary and sufficient conditions that the field strengths be expressible in terms of potentials is certainly interesting, but may well be more a consequence of the demands of causality than of any physical significance

[26]A caution about the methodological utility comes, though, from considering what role precisely gauge theory has played in theory construction. There is no doubt much to say on this topic, but one should note that canonical gravity is not an unbridled success. Rather a particular choice of the variables to use in formulating the theory (connections rather than metrics) is responsible for much of its success. As noted above, canonical quantization, in its metric formulation, was for many years an embarrassing failure. To be sure, gauge theory looms large for the founders of this approach, but curiously the major stumbling block is the lack of a dynamics. Then on the other side is Feynman's quantum electrodynamics. He says in the three foundational papers (Feynman, 1949a, 1949b, 1949c) that to understand what is going on in the physics, it is crucial to consider the *interactions between charges*. Certain technical problems are best solved, he believes, in the Hamiltonian framework, but for foundational issues we turn to Lagrangian formulations.

to be attributed directly to the potentials'' (p. 2191). I suggest that something is to be learned about physical significance by formulating, insofar as is possible, all gauge theories purely intrinsically.

References

Aharonov, Y., & Bohm, D. (1959). Significance of electromagnetic potentials in the quantum theory. *Physical Review*, *115*(3), 485–491.

Aharonov, Y., & Bohm, D. (1961). Further considerations on electromagnetic potentials in quantum theory. *Physical Review*, *123*(4), 1511–1524.

Aharonov, Y., & Bohm, D. (1962). Remarks on the possibility of quantum electrodynamics without potentials. *Physical Review*, *125*(6), 2192–2193.

Batterman, R. (2003). Falling cats, parallel parking, and polarized light. *Studies in History and Philosophy of Modern Physics*, *34*(4), 527–557.

Belinfante, F. (1962). Consequences of the postulate of a complete commuting set of observables in quantum electrodynamics. *Physical Review*, *128*(6), 2832–2837.

Belot, G. (1998). Understanding electromagnetism. *British Journal for Philosophy of Science*, *49*(4), 531–555.

Brown, H. R., & Pooley, O. (2001). The origin of the spacetime metric: Bell's 'Lorentzian pedagogy' and its significance in general relativity. In C. Callender, & N. Huggett (Eds.), *Physics meets philosophy at the Planck scale* (pp. 256–272). Cambridge: Cambridge University Press.

Cao, T. (Ed.). (1999). *Conceptual foundations of quantum field theory* (pp. 298–309). Cambridge: Cambridge University Press.

Chouquet-Bruhat, Y., DeWitt-Morette, C., & Dillard-Bleick, M. (1982). *Analysis, manifolds and physics*. Amsterdam: Elsevier.

DeWitt, B. (1962). Quantum theory without electromagnetic potentials. *Physical Review*, *125*(6), 2189–2191.

Earman, J. (2002a). Thoroughly modern McTaggart. *Philosopher's Imprint*, *2*(3), 19–23.

Earman, J. (2002b). Gauge matters. *Philosophy of Science*, *69*(5), S209–S220.

Feynman, R. (1949a). The theory of positrons. *Physical Review*, *76*(6), 749–759. Reprinted in Schwinger, J. (Ed.). (1958). *Selected papers on quantum electrodynamics*. New York: Dover.

Feynman, R. (1949b). Space–time approach to quantum electrodynamics. *Physical Review*, *76*(6), 769–789. Reprinted in Schwinger, J. (Ed.). (1958). *Selected papers on quantum electrodynamics*. New York: Dover.

Feynman, R. (1949c). Mathematical formulation of the quantum theory of electromagnetic interaction. *Physical Review*, *80*(3), 440–457. Reprinted in Schwinger, J. (Ed.). (1958). *Selected papers on quantum electrodynamics*. New York: Dover.

Healey, R. (1997). Non-locality and the Aharonov–Bohm effect. *Philosophy of Science*, *64*, 18–41.

Healey, R. (1999). Is the Aharonov–Bohm effect local. In T. Cao (Ed.), *Conceptual foundations of quantum field theory* (pp. 298–309). Cambridge: Cambridge University Press.

Healey, R. (2001). On the reality of gauge potentials. *Philosophy of Science*, *68*, 432–455.

Healey, R. (2004). Gauge theories and holisms. *Studies in History and Philosophy of Modern Physics*, *35*, 619–642.

Jackson, J. (1975). *Classical electrodynamics*. New York: Wiley.

Kennedy, J. (1996). The Aharonov–Bohm effect, Hodge decomposition, and non-locality. Unpublished manuscript.

Leeds, S. (1999). Gauges, Aharonov, Bohm, Yang, Healey. *Philosophy of Science*, *66*(4), 606–627.

Liu, C. (1994). The Aharonov–Bohm effect and the reality of wave packets. *British Journal for Philosophy of Science*, *45*(4), 977–1000.

Martin, C. (2002). Gauge principles, gauge arguments, and the logic of nature. *Philosophy of Science*, *69*(3), S221–S234.

Mattingly, J. (2005). The structure of scientific theory change. *Philosophy of Science*, *72*, 365–389.

Maudlin, T. (1998). Healey on the Aharonov–Bohm effect. *Philosophy of Science*, *65*(2), 361–368.

Sakurai, J. (1994). In S. Tuan (Ed.), *Modern quantum mechanics, revised edition*. Reading, MA: Addison-Wesley.

Teller, P. (2000). The gauge argument. *Philosophy of Science*, *67*(5), S466–S481.

Wu, T., & Yang, C. (1975). Concept of non-integrable phase factors and global formulation of gauge fields. *Physical Review D*, *12*(12), 3845–3857.