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A. R. P. Rau

Chitra Rangan
University of Windsor

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Reply to "Comment on 'Photodetachment in combined static and dynamic electric fields'"

A. R. P. Rau

Department of Physics and Astronomy, Louisiana State University, Baton Rouge, Louisiana 70803

Chitra Rangan

Physics Department, University of Michigan, Ann Arbor, Michigan 48109

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While distortion of the initial negative-ion state by a strong static electric field can have observable effects, the effect attributed by the authors of the preceding Comment [Phys. Rev. A **64**, 037401 (2001)] to a cross term between the detaching laser field and the static field is spurious, an artifact of their procedures. Other points of dispute are also clarified.

On the whole, the preceding Comment [1] misleads, while not confronting clearly the main issues of disagreement between both groups of authors. There is, perhaps, only one point of clarification at the end. There are three topics: photodetachment in the presence of a static electric field, Volkov wave functions, and the ponderomotive potential.

Photodetachment in combined fields. The original papers [2] inspired by the H^- experiment dealt with the usual one-photon $E1$ process of a weak radiation field of amplitude E_0 with the added influence of a static field E_s on the outgoing electron. Two assumptions were clearly stated; the initial negative ion state was undistorted by E_s (note that the energy due to 140 kV/cm at distances less than $5a_0$ is less than 1% of the H^- binding energy) and there was no residual interaction between the detached electron and the atom left behind ("rescattering"). The Gao-Starace (GS) paper [3] claimed that there were further effects due to a cross-term between E_s and E_0 in Eq. (9) of Ref. [3], abbreviated Eq. (GS.9), which lead to a second term in Eq. (GS.64). This is what our paper (RR) [4] questioned because, regardless of the formalism, it makes no physical sense. In GS, this term was attributed to a strong laser field. Now in the preceding Comment, it is attributed to the strong static field's effect on "both the initial bound state and the final state." First, that is at odds with GS's use of the same unperturbed negative ion wave function in Eqs. (GS.24–25) as do RR and other previous treatments. The continuous reference in the preceding Comment to its Ref. [10] only misleads because that paper did include a distortion of the initial state by E_s, but GS did not. Second, there is nothing in GS Sec. V B about the strength of E_s so that such a term would seem to occur whether E_s is weak or strong. Particularly in the former case, but actually as long as E_0 is small, the approximation can be made at the start in the wave functions in Eq. (GS.9), by replacing exponentials by the first term beyond unity in their expansion. Upon doing so, the cross-term's $(E_s E_{0z}/\omega^3)\cos\omega t$ has no electron operators, and hence, cannot give a nonzero transition matrix element between different states of an atom. By contrast, similar terms involving E_0 alone in Eqs. (GS.7–9) give $(\vec{E}_0\cdot\vec{p}/\omega^2)\sin\omega t$ whose matrix element does involve that of the operator $\vec{p}$, leading to the usual

expression for photoabsorption as given by the Fermi-Golden Rule of first-order (in E_0) time-dependent perturbation theory.

The preceding Comment incorrectly states that we simply ignored the second term in Eq. (RR.9). We did include it in our numerical integrations to show that GS's results follow from that cross-term above, but not if that singularity in the phase is removed with the replacement of $\cos\omega t$ by $\cos\omega t - 1 + \frac{1}{2}\omega^2 t^2$. It is of some significance that we were able to reproduce both the GS and the earlier results within a single scheme, thereby pointing to the specific source of the disagreement.

Note also a recent paper [5] that considered a pulsed laser field, also finding no such cross-term. Therefore, our caution of what we see as a spurious effect in their S matrix, and now quasienergy, treatments remains. We have no problem in seeing that rescattering or distortion of the initial state by a very strong E_s can lead to physical effects not included in the original treatments, but these are not due to the cross-term under dispute.

The preceding Comment advances as proof of gauge independence that length and velocity forms give the same result. This was not the point of dispute as explicitly stated in RR at the bottom of the left column on page 2. Our point was that *two alternative vector potentials* for the same field $E_0\sin\omega t$ give different expressions in Eqs. (GS.7–9), in particular of the disputed cross-term and, therefore, of its effects. To us, this is reason for caution, whereas the preceding Comment simply dismisses vector potentials with nonharmonic terms from use in the S matrix formalism. We have not encountered before such strictures on what wave functions or gauge potentials cannot be used in the S matrix.

Volkov functions. The preceding Comment claims that we questioned by implication the Volkov wave function. We did not question the basic validity of Volkov's solution for a free electron in an electromagnetic field, either in RR or in the paper which is Ref. [9] of the Comment. What we have shown is that there are alternative Volkov solutions for alternative phases or forms of the electric field, as is indeed easily verified. Further, the solutions can be built on alternative "starting forms," whether plane waves as in Volkov's original work or on Airy functions if so desired (more natural for applications involving an additional E_s).

The preceding Comment's first paragraph has the strange criticism that we "insist that the phase of the laser field should be such that the laser field reduces to a static field in the zero-frequency limit." Surely, that is a truism, that whatever the phase, the zero-frequency limit is a static field? What we say is simple: any treatment such as GS's, with a field $E_s + E_0 \sin \omega t$, should give in the limit of zero-frequency the results valid for static field E_s alone, whereas with a field $E_s + E_0 \cos \omega t$ should reduce to that for the static field $E_s + E_0$. The preceding Comment also states "it is unclear why the wave function must have the correct limit at $\omega = 0$," "the physics of finite and zero-frequency fields are very different: one involves quantum objects, photons; the other involves a static potential." "The expectation that one can pass from one situation to the other simply by taking the zero limit of the parameter ω is incorrect." Whether in a semiclassical or second-quantized field-theoretic treatment of radiation-matter interaction, surely ω can take any value from zero upward. It is one thing for the authors to use (and point to others who do so) different formalisms for $\omega = 0$ and nonzero, but to make that a virtue, or claim that there is no consistent way of handling the limit and that it is even incorrect to seek it, is unacceptable.

Whether one treats radiation classically or as a quantum field, one should be able to develop a formalism capable of handling the zero-frequency limit. We have aimed to do so, providing such consistent functions for use in semiclassical radiation-atom problems. The wave functions behave properly for all ω, reducing appropriately to plane waves or Airy functions without extraneous, leave alone singular, phase factors. It is such a factor that makes their wave function exhibit "rapid oscillations" that the authors say "should be treated properly." That is what our procedure implements through a treatment that does not suffer from such singularities, but vanishes in the $\omega \to 0$ limit. Here is a prescription for dealing with all values of ω consistently. Likewise, a fully quantum QED treatment of both radiation and atom should also give expressions that remain meaningful whatever the value of ω. Note that in such a treatment, an A^2 term does not enter explicitly and only integer multiples of ω occur in excitation and/or absorption as they should (see below).

Ponderomotive potential. The preceding Comment also charges that we question the "well-known ponderomotive potential," citing two experiments. We have no problem with these experiments but question some of the interpretations of the ponderomotive potential in GS. With regard to multiphoton processes, in a consistent photon picture, the energy $\epsilon_f - \epsilon_i$ of an atomic transition can only equal an integer multiple of $\hbar \omega$. Yet, their energy-conserving δ function includes the ponderomotive term that does not satisfy this requirement. In this case, there can be no argument that this is a strong laser field effect, but their result violates a basic aspect of the photon picture. This points to possible conflicts between implicit assumptions that have been made. The Comment mentions that GS assumed an adiabatic switching of the time-dependent field, but clearly the forms used of the vector potential or electric fields do not have explicit switch-on and/or off factors. This might be the difficulty because it was pointed out some years ago [6] that such formulations and the subsequent interpretations of the ponderomotive potential should ensure that, at both asymptotic limits of $t = -\infty$ and $t = \infty$, the electron is out of the laser field so as to avoid inconsistencies. Formulations using the gauge potential $\vec{A}$ need to pay attention to these questions.

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