Review of: "The Compton Wavelength Is the True Matter Wavelength, Linked to the Photon Wavelength, While the de Broglie Wavelength Is Simply a Mathematical Derivative"

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I have some issues with this manuscript. This is on the one hand connected to strongly overselling of points, which are common knowledge. Furthermore, there is an excessive number of self-citations and partial misinterpretations of physical concepts. I will limit my detailed critique to the first two sections as this is where my main area of expertise lies.

The first point of critique covers the discussion of the Rydberg constant. First of all, one has to keep in mind that the Rydberg constant is a collection of other constants to make formulas more compact and readable. Thus, sentences such as “the Rydberg constant is never needed to predict these [atomic transitions]” is misleading. Of course, such expressions can be reformulated and other constants can be chosen, but what is the benefit? The underlying constants are the same, no matter how they are arranged. Such statements are even more questionable as the author replaces one composite constant by another one – the Compton wavelength, which combines Heisenberg’s constant, the speed of light, and the mass of the electron.

Section 3 shows some conceptual misunderstandings of the topic, leading to an incorrect picture. The first one is the statement regarding the experiments by Davisson and Germer [Davisson and Germer, Nature 119, 585-560 (1927)]. The author claims that it was the “wavelike properties that was confirmed, not the prediction of wavelength from his formula”. This is not correct. In their study Davisson and Germer illuminated a single crystal of Nickel with electrons and changed the acceleration voltage of the electrons, that is, their velocity. The authors saw the diffraction maximum changes with velocity exactly as prediced by de Broglie. This is an unambiguous proof that the wavelength depends on velocity. This approach is used ever since in countless experiments. Changing the wavelength of a particle by tuning its velocity is common practice in electron microscopy and in diffraction experiments using atoms [Estermann and Stern, Z. Phys. 61, 95-125 (1930)], neutrons [von Halban and Preiswerk, C. R. Acad. Sci. Paris 203, 73-75 (1936)], and polyatomic molecules [Arndt et al., Nature 401, 680-682 (1999)]. In all these experiments the de Broglie wavelength is clearly observable: it determines the spacing of the diffraction fringes and the achievable resolution in a microscope. Thus, its effect can be clearly extracted from an experiment, which is the reason why it’s commonly used. And again, exchanging one composite wavelength (de Broglie) by another one (Compton) does not change the underlying physics.

The author furthermore ponders on what happens as soon as the velocity is zero. It’s true that the wavelength approaches infinity as velocity goes to zero. However, at this point we have to briefly recapitulate how the wave properties of a massive particle are evoked: by exploiting Heisenberg’s uncertainty principle. Typically, the position of a particle is strongly measured to induce a sufficient momentum uncertainty. In an electron microscope this is achieved by emitting electrons from just a few atoms at a tip. For more massive particles, often slits are used. The strong confinement of Delta x leads to a large value of Delta p. This momentum uncertainty then determines how fast the wavepaket spreads. So, slowing an electron to v=0 does not mean that the electron is immediately distributed over the whole universe. The other point is that such a low velocity has to be prepared and that the systems has to be completely unperturbed. For a dilute gas of Rubidium atoms, the lowest temperature achieved yet is in the low pikoKelvin regime, see for instance [Deppner et al., Phys. Rev. Lett. 127, 100401 (2021)]. The corresponding velocities are in the µm/s-regime. Thus, preparing something at 8.3E-31 m/s (25 orders of magnitude smaller!) as stated in the manuscript is unrealistic. Moreover, as soon as the particle interacts with another one, its position is usually measured and the region over which it was delocalized is reduced. This is coined partial decohernece. Regarding molecule interference, this is, for instance, reviewed in [Arndt et al., Experimental Decoherence in Molecule Interferometry. In: From Quantum to Classical, Springer (2022)]. Thus, the discussion confuses several things and concepts. I strongly encourage the author to reassess the literature with respect to this topic. A very useful starting point is the excellent review by Cronin et al. [Cronin et al., Rev. Mod. Phys. 81, 1051-1129 (2009)].