The Antiphysical Review

Founded and Edited by M. Apostol **167** (2010)

ISSN 1453-4436

Of lasers, plasmas and atomic nuclei as seen from Magurele-Bucharest, April 2010
Some friendly comments on a Research Project

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Most of the case studies included in this Research Project belong to author A and collaborators. We present here some critical comments on these contributions, as well as on others'.

1. A Th-beam is accelerated by a high-intensity laser and directed upon a Th target. It is supposed that Th nuclei fission, light or heavy fragment fuse back, substantial neutrons are released which may help producing neutron-rich nuclei. None of these assumptions are supported by scientific, experimental or theoretical evidence. The strong point of motivation would be the origin of heavy elements, allegedly unknown yet, which depend critically of their neutron content. Nevertheless, there is no mistery: too many neutrons make the nuclei highly unstable, while heavy nuclei with a moderate neutron content are metastable. Even if the experiment succeeds, there will be no other strange nuclei, except those commonly known. This research yields nothing.

2. A high-intensity laser pulse is sent on a thin foil. The laser pulse disrupts the sample, and takes away energetic ions and electrons. The light pressure on the ions is sufficient to overcome the cohesion energy of the sample (a few eVs per atom) along the rim of the focused pulse. According to the proposers, no numerical simulation or modelling explains the phenomenon, and only an experimental investigation would clarify the matter. As we see, the matter is nothing misterious, it is already both clear and clarified. It is only the inconsistencies of faulty numerical simulations and modelling which obscure the physics and make it appear misterious. But these do not warrant a research project.

3. Classical, well-known, stopping power formulae are compared for ionization and radiation processes (Bethe, Bloch, etc). It is claimed that "new laws" would work for the stopping power in rarefied plasmas. This is incorrect. The basic reasoning leading to the well-known classical formulae holds in the new condition of a rarefied plasma, leading of course to different formulae, but no "new law". It is not clear whether the authors know the derivation of these classical formulae (admitedly notoriously difficult and controversial for many indeed), nor whether they propose a research for learning themselves this derivation. In addition, the new formulae bring nothing interesting. The motivation of this research is untenable.

4. A high-intensity laser pulse strikes a thin foil and pushes forward a "mirror" of electrons, with a high reflectivity, on which a second laser strikes and would produce intense, hard gamma rays, presumably coherent. This is a very interesting idea, highly imaginative. The only problem, which in fact is a hard fact, is that those "mirror" electrons do not hold together, nor even the ejected sample fragment (ions included), which anyway is too thin for a useful yield of gammas. This research is pure fantasy, of an exquisite beauty. In addition, the reflectivity of an electronic sheet diminishes drastically, and depend oscillatory on the incidence angle.

5. A high-intensity laser beam may create a standing electric field of high intensity: cca $10^{15}V/m$ at most. There are some indefinite suggestions that electron-positron pairs could be created from vacuum in much stronger fields $\sim 10^{18}V/m$ (Schwinger), or very high intensities $\sim 10^{29}w/cm^2$ (Sauter), none attainable yet. The authors believe that a superimposed hard gamma rays beam may catalyze the pair production. This would be true probably only for gamma-gamma scattering, whose cross-section is enormously small in this energy range. Moreover, apart from unattainable critical parameters as those above, even if the scenario goes, the yield is about one pair per day. This research is at the very limit of reality, it is pure fantasy. Two more contributions (by different authors) discuss further the same point: with wrong equations, employed in an improper context, and conclusions hidden in an uncontrollable numerical code. The presumed vacuum birefringence estimated by the authors themselves is of the order of 10^{-4}: even so, it certainly does not matter at all.

6. Classical Compton effect is suggested to be studied in the new context of high-intensity fields. Apart from the red-shift, a blue shift (inverse Compton scattering) and a change in the electron mass are envisaged. These effects, if real, are extremely sensitive to the experiment parameters, and in any case very low to deserve any serious attention. This research brings nothing new, except possibly for some wrong physics.

7. Conversion electrons may be used to measure the lifetime of the nuclear levels, as in atomic physics. Above the emission threshold these lifetimes are very short, below that threshold the lifetimes are longer. This is presented as something new. It is not new at all. It is old, trivial and obvious. Ultrashort gamma pulses may take pictures of the decaying process, or photonuclear reactions. This has already been done for chemical reactions, orders of magnitudes longer. The pictures are some black spots on a white background, or white spots on a black background. This zebra tells nothing.

8. Laser beams are scattered off accelerated electrons to get high-intensity gamma rays, which in turn can be used to investigate nuclear reactions. Gamma rays are of high resolution, sharp nuclear states will be identified. They are irrelevant, as being practically inaccessible.

9. Gamma rays to be used for studying the fluctuations of highly excited nuclear states. The authors look for support of chaotical or random matrix theory predictions. Nucleons behave statistically, not as chaotical classical or quantum (via random matrices) dynamical systems. Consequently, fluctuations have noting to compare with the predictions of dynamical systems. This research has no object. Quite characteristically the authors want to "predict the strong component of the many very weak, unobservable transitions..." (p. 56). To predict the unobservable could indeed be a big feast!

10. Inelastic electron scattering: "Here the transfer of larger spins compared with the gamma beams is possible" (p. 59). Electron spin is 1/2, gamma "spin" is 1. The authors write up without any understanding of what they are writing up.

11. Parity violation mix nuclear states and produce MeVs doublets. The authors want to probe them by gamma rays. The required accuracy however is not yet attainable. This is another research at the border of the impossible and non-existence.

There follow a few proposals mainly by author B and collaborators.

12. Monoenergetic and brilliant gamma rays used for exciting pygmy electric dipole resonance in deformed nuclei. The motivation is to test some "modern calculations" (p. 62). However, in these "modern calculations" there is nothing interesting. Leaving aside the high density of states, which impedes seriously upon the measurements. This proposal has one page of text and one page of References. The authors resort to others, as one can see.

13. The emission of particles is notoriously difficult. There exists a surprising classical result for atoms (Kramers). The authors want to study such emission processes (photo-response) for nucleons. The motivation is a "tremenduous increase of insight into nuclear structure in the continuum..." (p. 64). Nor the "tremendous", neither the "insight" can be seen. The proposal is bombastic and hazardous.

14. Brilliant gamma rays will increase the sensitivity of the nuclear resonance fluorescence. This test method will get improved eficiency, for detecting rare, trace isotopes or isomers. Right, but this is a technique, not a research.

15. Multiple nuclear excitons (again author A): A gamma or X-ray laser is suggested, by pumping gamma nuclear resonant states ($100keV$, Fe), which, in the conception of these authors appear as delocalized excitons extending over many nuclei: the delocalization may help pumping. Which is wrong. What we gain in delocalization over many we lose in delocalization from each. The lasing mechanism is entirely different. The authors speak either of excitons, or polaritons, rather indistinctly, and References 2,4,8,10-13 are not cited in the text. In addition, this idea was around for some time, the only original contribution of the authors being the useless novelty of excitons.

16. Neutron capture cross-sections to be measured by inverse gamma-neutron reactions (author B), because the heavy elements are too many in comparison with some theoretical predictions, though not so many according to some other theoretical predictions. And the cross-sections would serve in some theoretical predictions, which, very likely, will oppose other theoretical predictions, etc, etc. There is nothing clear in these studies. It is recommendable that the authors suggest new experiments only after they would have an acceptable and minimal "theoretical" understanding of the matters they speak of.

17. Entirely same reasoning as above is transferred over from neutrons to protons (author B). Same as above applies.

18. An amusing proposal comes from some Japanese friends (p. 73): they want to detect "clandestine" materials by improved nuclear resonace fluorescence, with intense gamma rays. The authors "stress the political importance of the project" as it will help detecting nuclear waste isotopes. Only that the intense gamma rays facility must be carried along to the strategic material. Or viceversa.

19. Sharp (and intense) gamma or X- rays could be used in crystalography (author A again and finally): yes, I agree. But where is the science? Similarly for thermal neutrons, produced by gamma-neutron reactions. A bit more moderately for positrons produced by gamma-pairs reaction.

My conclusion is that the present Research Project is an Anti-Scientific Case of ill, verge-cutting Physics, of a highly speculative nature, which, unfortunately, warrants no consideration.