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For a New Department of Energy Lab to Examine Laser Fusion for Energy

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Abstract

This paper gives a summary of a talk by the author at the mini-conference entitled *Progress in Making IFE-based Concepts a Reality* at the APS-DPP meeting in Atlanta in October, 2024. It argues principally for a new DoE lab to examine the potential opportunity of laser fusion for civilian energy, by direct drive, with an excimer laser. This work is motivated mostly by the demonstration of a burning plasma in an indirect drive configuration, by the Lawrence Livermore National laboratory with its NIF laser. Also, it concludes by briefly gives some impressions of the mini conference.

Keywords

Laser Fusion, Fusion Breeding, Excimer Lasers

1. Introduction

On the Tuesday, October 8, 2024, of the recent American Physical Society Division of Plasma Physics (APS-DPP) conference in Atlanta, Dr. Kruse of the Lawrence Livermore National Laboratory (LLNL) hosted a mini conference called *Progress in making IFE* (Inertial Fusion Energy) *based concepts a reality*. It was mostly a conference of scientists participating in a new Department of Energy (DoE) effort on laser fusion. This author is not a participant in this effort, but was invited to the mini conference, nevertheless, as he had done work in the area. I gave a talk at it entitled: *Fusion*, *it is time to color outside the lines* [1], which is the title of a paper making these points in much more detail [2]. I also made many of the same points in a book, published by the Generis publishing company entitled: *Mass delusions*, *how they harm sustainable energy*, *climate policy*, *fusion and fusion breeding* [3]. Reference 2 is backed up by 117 references, the work of hundreds of scientists at many different labs, universities and companies. Sections 4 and 5 of Reference 3, the portion dedicated to fusion, is backed up by 87, so this paper hardly relies on the author's work alone, as one might think from the few references here. The real reference list is that in [2] and [3]. The reference list of this manuscript list is kept brief only for considerations of the length of this paper, which is intended to be rather short, certainly compared to [2]. Since [2] and [3] are already in the literature, with [2] published open access, I will be brief in my discussion of it here.

The main point is that the NIF laser at the (LLNL) achieved a burning plasma about 20 years before ITER hopes to do, and at much lower cost. The laser beam is a match, like the spark plug in the cylinder of a car. It does not burn the fuel; it only ignites it. The alpha fusion burn is analogous. The laser compresses and heats a small part of the target until it starts a tiny fusion reaction, generating 14 MeV neutrons, and 3.5 MeV alpha particles. The geometry of the target is such that the neutrons escape, but the alpha particles are reabsorbed locally, heating the nearby parts of the target to fusion conditions, initiating a burn wave. In a typical explosion, the expanding gas cools as it expands, as thermal energy is converted to kinetic energy. However, LLNL has diagnostics showing that as the exploding target expands, for a while it heats, until ultimately as the expansion proceeds it then cools.

2. How to Optimize This Amazing Accomplishment

To this author, this is an inflection point, which means that American Department of Energy (DoE) fusion effort cannot simply proceed with business as usual but must consider carefully alternate strategies. Specifically, the strategy proposed here, is that the American DoE should set up a new National lab, with the goal of examining and exploiting this amazing development. Its goal should be to use it to develop sustainable energy for the civilian economy. Since the goal of this proposed lab, and that if LLNL (*i.e.* nuclear stockpile stewardship) are so different, this lab should not be LLNL. Before this, ITER seemed to be the only reasonable path, but LLNL's amazing achievement changed this completely. The talk asserted that 100 years from now, it could well be regarded as one of the main experiments of the 21st century, and that it is Nobel Prize worthy. It seems that because of this triumph, or as just described, this inflection point, the US Department of Energy should shift its main attention from magnetic fusion energy (MFE) to IFE.

In the fusion program, there is a precedent for such an abrupt shift. About 60 years ago, the Princeton Plasma Physics Laboratory (PPPL), was working on stellarators, which had very poor confinement. When they learned of the advances the Russians made with tokamaks, nearly immediately, they jumped ship and put all their efforts in tokamaks. This action was amazingly successfully on their part. From about 1970 to about 2005, PPPL led the world in fusion.

References [1] and [2] made the point, and make it again here, that the US DoE should shift its main effort to laser fusion, based on the LLNL results. This is just what the Princeton lab did about 60 years ago but based on Russian tokamak results. Of course, as pointed out in [2], I have no expectation that the grand pooh bah's at the DoE will read this, or [1] or [2], and immediately set up the new DoE lab. Hopefully this might help to initiate discussion among many interested parties and help to convince them that this is an optimum way for the US fusion program to proceed. Perhaps this, as well as [1] [2] and [3] can be an initial value in what hopefully will become an exponentially increasing interest in the concept. This could well be the best way for the United States to recover its long-lost leadership of the entire fusion program. After all, if MFE proves to be the way to go, the rest of the world will do it. But we are the only ones able to do laser fusion at this point, and at least to this author, laser fusion now seems to have all the advantages over MFE. I briefly discussed this in my talk [1] and documented it much more thoroughly in [2] and [3].

3. Why the American Fusion Program Should Now Emphasize Laser Fusion over Magnetic Fusion

Here we give list of 12 reasons why, in view of the LLNL result, this author makes the case that the American fusion program should now favor laser fusion over magnetic fusion. These assertions are backed up by at least 40 references cited in [2].

1) Tokamaks do not know how to drive the current steady state at an acceptably low power as shown by experiments in EAST and KSTAR.

2) Recent calculations have indicated that you may be able to run at high duty cycle by oscillating the current back and forth. There is no experimental confirmation of this plan. It may work, it may not. Furthermore, what happens when the current goes to zero and there is no MHD equilibrium? Surely what is left of the plasma will immediately hit the wall.

3) Tokamaks at fusion relevant parameters have not proved that they can run at sufficiently long time without disruptions as shown in experiments in both TORE SUPRA and JET.

4) Tokamaks, and any MFE scheme have no experimental experience on what to do with the alpha particles. Maybe you must get rid of some of them? Maybe all of them? Maybe you can control their heating in some way? Maybe they build up the pressure until a disruption results? In MFE alphas are a nuisance, the project without them would be happier if they did not exist, but they do.

5) By contrast IFE loves them and knows exactly what to do with them and has demonstrated it. They initiate a burn wave, something not possible in MFE.

6) Tokamaks probably have a big problem with recycling. The wall is hit by neutrons, radiation, fast ions, fast neutrals. Who knows what will come back into the plasma.

7) IFE is all over in a few nanoseconds, it takes microseconds for fast particles to hit the wall and come back. The reaction will be long finished by then, *i.e.* no recycling.

8) Tokamaks (and any MFE) have water cooled components near the energetic

plasma. These occasionally spring leaks when bombarded by the plasma [4], and the vacuum chamber fills with water! This will be only more difficult when there are also lots of 14 MeV neutrons.

9) Laser fusion has no components closer to the plasma than many meters.

10) ITER admits that it is only a step toward a DEMO, a smaller, cheaper, more powerful higher Q device. Who knows how many tens of billions it will cost, who knows how many decades, and who knows if it is even possible.

11) ITER has been plagued by delays and cost over runs. First plasma now expected in 2034, 18 years late. DT experiments are not scheduled to start until 2040, 15 years late, assuming no more delays or cost overruns.

12) NIF has also been plagued by delays and cost overruns, but nothing like ITER. It was approved in 1995 for $1.1B to be completed in 2002 and produce a Q = 10 fusion burn ~3 years after that. It was completed in 2009 for $3.5B and produced a fusion burn Q = 2.5 fusion burn in 2024.

As this author sees it, the problems MFE faces are of a fundamental nature. This means it must do things which have never been demonstrated and for which there is no experimental experience. Some examples: There have been no experiments on driving steady state current in a tokamak at fusion relevant power; there has been no demonstration that disruptions can be sufficiently minimized in fusion relevant plasmas, nobody has performed an experiment demonstrating what to do with the alphas; and nobody has demonstrated how to control recycling.

The problems laser fusion faces are more of a technical nature. This means the basic requirements have been demonstrated, they just must done bigger and better. First, as we will see, their Q might be within a factor of 2 of relevance for energy production! We can track and hit missiles with missiles over thousands of miles, certainly we can track and hit a fusion target as it wobbles along across 10 meters. Then we need the rep rated pulse power of sufficient average power to drive the laser. The accelerator and klystron programs have developed rep rated pulse power with about 1% of what is needed for laser fusion. The program needs to develop a Megajoule rep rated laser with sufficient bandwidth. These milestones have all been met, but need to be upgraded, some by an order of magnitude or two.

4. Other Considerations Regarding Laser Fusion, Direct vs Indirect Drive

The laser fusion effort at LLNL is not supported by fusion energy, but by DoE NNSA which is concerned with nuclear weapons which are generated by X-ray driven implosions (*i.e.* stockpile stewardship). The LLNL experiment uses what is called an indirect drive configuration. The target is enclosed in a small container, called a hohlraum, made up of heavy metals, typically gold or uranium. However, the target does not touch the walls of the hohlraum, it is supported in the hohlraum by other means. The laser focuses on the inner walls of the hohlraum creating a blackbody plasma with a temperature of 250 - 300 eV. The X-rays emitted by this black body illuminate, compress and heat the target.

NNSA's interest is only in X-ray driven implosions, and has little or no interest ultraviolet driven implosions, laser efficiency, laser average power, laser bandwidth, rep-rated pulse power, the effect of laser plasma instabilities, tracking and engaging a fast-moving target in a not so gentile environment, manufacturing targets cheaply in mass… All of these are vital for laser fusion for civilian energy, and they are all serious challenges. However, to this author, they seem easier to overcome, as to him they are more technical: and the challenges to MFE more fundamental. Accordingly, this author has suggested that the proper course of action for DoE is to set up a completely different laboratory to examine and optimize recent results for energy rather than nuclear simulation. This new lab should not be LLNL, as the goals and scientific challenges of the weapons program and the civilian power program are so different; the labs should be separate.

The talk [1], and [2] and [3] in much greater detail made a few other points. Hohlraums now cost many thousands of dollars now and contain very expensive materials like gold and uranium. Surely mass manufacturing can bring their price down but consider indirect drive for the civilian sector. Say we use a 1 MJ laser pulse and find a gain of 100, giving 100 MJ of fusion energy. Converting it to electricity gives about 30 MJ, or ~10 kWhrs, worth about a dollar! Can the price of the hohlraum and whatever supporting material it needs be reduced to less than about a dime, or even quarter? It seems like a stretch. What can you get for a dime these days?

For energy, rather than nuclear weapon simulation, direct drive seems to be the way to go. Hence this new lab should concentrate on direct drive, as its goal will be energy for the civilian sector. In a direct drive configuration, the laser is focused directly on the spherical target, and it is imploded by ultraviolet light, rather than by X-rays. Also, with indirect drive, every shot destroys something pricy, the hohlraum, to produce something very cheap, a few kWhrs of electric energy. Another problem is that as the hohlraum and target is shot in, surely on a slightly uncertain, wobbling path, its position *and* its orientation must be exactly aligned with the laser light. This is a not nearly as serious an issue with direct drive and spherical targets, where there is no concern with target orientation. Furthermore, as LLNL readily admits ([2] ref 94, [3] ref 55 in Section 4), only about 10% - 15% of the laser light is absorbed by the target (the target absorbs the X-rays produced on the hohlraum wall). Hence, since their maximum gain, *i.e.* Q, at the time of this writing is ~2.5 [5], their Q for direct drive, assuming that ultraviolet works as well as X-rays may be as high as 25! Also, for a rep rated system for energy, the reaction chamber must be prepared between shots. In direct drive, one only must clean up the residue of the target; indirect drive, it must clean up the residue of the target and the much larger high Z hohlraum. But while this new lab will focus on direct drive, of course LLNL will continue with its indirect drive, NNSA supported project. Possibly their approach will ultimately be the best way to go, and this new lab should follow its work carefully (and of course visa versa).

While the LLNL result is certainly the tallest poll of the tent for reorganization of the fusion effort around laser fusion, there are important supporting results also from two other labs. First, the Naval Research Lab (NRL) has decades of experience of developing excimer lasers, both KrF and ArF ([2], see Ref 87). These definitely have advantages over glass in that they have shorter wavelength and zooming capability. They are most likely also have an advantage over glass of higher efficiency and higher average power capability, since they use a flowing gas, and there is no glass to cool down between shots. Furthermore, preliminary NRL calculations indicate the ArF laser could have sufficient bandwidth ([2], see Ref 87). Other laser types, for instance a diode-pumped, solid-state pump laser, like LLNL's Mercury project might be in the running, and this was discussed in the NRL/LLNL led HAPL program ([2], see reference 100 there). However, this author believes that excimer lasers are the way to go.

Since both nuclear weapons and the LLNL burning plasma are driven by X-ray implosions, some have argued that this is the only viable approach to laser fusion. However, the University of Rochester Laboratory for Laser Energetics (URLLE) has produced what they call a hydrodynamically equivalent implosion with their 30 kJ ultraviolet OMEGA laser ([2], see Refs 97 and 98). This is an implosion, just like a Megajoule X-ray driven implosion, but with shorter scale length, shorter time, and obviously achievable with much less energy. The shorter scale length does mean higher gradients. The hydrodynamically equivalent implosions URLLE achieved may be stabilized by these gradients against laser plasma instabilities, whereas an actual size target, with smaller gradients may not be. However, as mentioned earlier, if the laser can have sufficient bandwidth, and an ArF laser may have advantages here, it can most likely stabilize the instabilities. This is the principal reason potential bandwidth is an important consideration. Thus, it may be that the URLLE direct drive experiment makes a convincing argument that an ultraviolet driven implosion could rather quickly reach a Q of 25! This is the Q that the best LLNL target achieved if one considers only the radiation (X-rays in the LLNL case) impinging on the target. To this author, the LLNL burning plasma result, enhanced by the NRL work on excimer lasers, and by the URLLE results on hydrodynamically equivalent ultraviolet driven implosions; strongly emphasize the case that the Department of Energy should support a new lab, based on direct drive and excimer lasers, to develop laser fusion for civilian power.

5. Fusion Breeding and Some Other Considerations

Another point made in my talk is that the DoE should no longer persist in its decades long policy of ignoring fusion breeding, that is using fusion neutrons to breed fuel for thermal nuclear reactors. Assuming the world continues to build many thermal nuclear reactors, availability of mined uranium will become a serious issue in the next few decades. Fusion breeders can fuel these reactors, fission breeders cannot, as described in (1 - 3 and other references therein). Basically, it takes 2 fission breeders, at maximum breeding rate, to fuel a single thermal reactor of equal power. But a single fusion breeder can fuel 5 or 10. Also, since the demands on the pure fusion reactor are much greater than those on the fusion breeding reactor, fusion breeding provides an insurance policy if the calculations of fusion gain, prove to be optimistic, as they nearly always have. For instance, a steady state (or high duty cycle) tokamak like ITER, assuming it is successful, could be fine as a breeder ([2], see refs 50-57, and [3]); that is ITER could be an end itself. However, for pure fusion, according to the ITER web site 'THE DEMO' is what will be needed for commercial fusion. This DEMO must have higher gain, higher power, and yet be smaller, and cheaper. Hence ITER is only an initial steppingstone to who knows what DEMO, at who knows what cost, and after who knows how many decades, assuming the DEMO can be accomplished at all.

For the laser fusion case, it is argued in [2] that a gain of 50 with a 7% efficient laser could be viable for fusion breeding, but not for pure fusion, given the various inefficiencies of the laser and the generation of electricity. The Q of most recent LLNL results, interpreted as they might be (optimistically) for direct drive, could already be halfway there! Second, even if the IFE gain's optimistic estimates prove to be correct, fusion breeding can greatly lower the cost of fusion provided electricity, even neglecting the additional fuel it provides for thermal reactors [2]. Fuel for these thermal reactors could finally become "too cheap to meter!".

In addition to [2], the author has examined fusion breeding for decades. His first paper on it was in 1999 ([2], Ref. 49), in 2009 he derived the limits under which tokamaks operate and showed that breeding falls within these limits, while pure fusion has difficulty doing so ([2], Ref. 50). Between 2014 and 2022, He has published 4 papers on the subject in extremely high-quality open access journals ([2], Refs. 52 - 55), journals published by Springer, the Cell network and IEEE. These 4 papers had 222 combined references, some of which were repeated from one paper to the next.

Finally, [2] had a section on digressions. These are brief, preliminary discussions of matters that could be important. There, a new type of cylindrical target chamber which could have many advantages was introduced. It would have the ability to use as a blanket one or more flowing liquids with free surfaces; or liquids flowing in pipes; or a solid blanket, which could be removed and reinserted very easily. It also pointed out that if fusion, or fusion breeding, does reach a point where it would be necessary to build many fusion reactors quickly, availability of tritium, even for the first commercial reactor, will be a serious problem. The digression in [2] suggested a way to solve this problem of tritium for the first one or two commercial reactors using the world's thermal reactors and then using a reaction in the fusion blanket which could allow exponential growth of tritium supply. Note that it is likely that in the life of a fusion economy, there could be at least 3 different blankets that the reactor might need to use. First there is a blanket for the exponential growth of the tritium supply. Second there is a blanket for pure fusion, where there are already enough fusion reactors, and the extra growth of tritium is the last thing anyone wants (tritium is an important component of the most powerful nuclear weapons). Third there is a blanket for fusion breeding, where some thorium is mixed in. A point source for the fusion reaction, enhanced by the cylindrical blanket just mentioned is likely the only way that a fusion blanket, especially a solid one, can be rapidly changed as needs change. It is very, very difficult to see how a tokamak or stellarator can support these multiple blankets on any kind of reasonable time scale.

6. It Is Unlikely That the Privately Funded Fusion Start-Up Will Deliver Power to the Grid Anytime Soon

Also discussed in ([2] and [3]) is the author's assertion that the new (and not so new) privately funded "fusion start-ups", which promise fusion for the grid in about a decade, are extremely unlikely to deliver on those promises. In [2] there are 10 separate citations where a variety of fusion experts, including the author, mostly retired and financially independent, debunk the claims of fusion to the grid in a decade. In fact, the probability that these "startups" will supply any net power to the grid in the next decade or so, is about the same as the probability that the NY Mets will put me in right field instead of Juan Soto.

Googling "How long does it take to build a 1 GWe nuclear power plant?", one first sees the AI assessment. Here it is:

Building a 1 GW nuclear power plant typically takes between 5 and 10 years on average, depending on factors like location, regulatory environment, and design complexity, with some countries like South Korea and China potentially building them faster than others; however, some projects can take significantly longer due to delays and complications.

Heck, here is AI's assessment of how long it takes to build a 1 GWe coal fired powered plant:

Building a 1 GW coal-fired power plant typically takes between 3 to 7 years to complete, depending on factors like location, construction complexity, and regulatory processes; with most estimates falling within a 4 - 5 years timeframe.

These are the times it takes to build power plants where the science and technology are well known! Is it credible that a fusion pilot plant, where the science and technology are far from known, can be built and hitched up to the grid as fast? This author emphatically believes that the answer is NO!

But some fusion 'startups' think they can do it even faster. For instance, Helion Energy has contracted to sell Microsoft 50 MW of electric power in 2028! (50 MW for a microsecond perhaps?) However, unlike many of the recent skeptics of the potential of fusion, this author has never wavered in his confidence that fusion, and/or, fusion breeding, is likely to not only be successful, but might well be the salvation of future civilization.

Furthermore, this author is willing to go out on a limb and say that if the laser fusion lab proposed here, is established and funded, after a few decades, it will be the first organization to provided fusion as a power supply for the world. Perhaps it will even be able to keep the long-broken promise that fusion (or in this case, perhaps fusion breeding) will become a reality in 35 years.

7. Ways to Fund This New Lab

Hopefully new support can be found for this new laboratory. This would certainly be justified by the recent results just mentioned. However, considering that the US government budget deficit is now in the trillions, it may not be possible. If not, the support should come from switching a major part of the MFE support to laser fusion. After all, no federal research program is guaranteed eternal life. As knowledge advances, and as needs change, the supported federal research programs must change with it.

It is simply impossible for the author, a single person in his study, with little access to any more than Google to come up with any kind of precise estimate of what the cost of this new lab ought to be. Clearly to come up with a good estimate would take a study done by dozens of experts with access to all sorts of data. The only data point this author has, is the total support of the DoE fusion project, and this ought to be a reasonable estimate for making an initial estimate. Currently, MFE is supported at ~$500 M per year and in addition there is ~$200 M per year for the US support of ITER. As ITER is an important international project, which may prove yet that the tokamak approach is viable for commercial fusion, or fusion breeding; this support should remain, especially as ITER is now mostly built. However, using this guidance, a reasonable initial estimate is that ~$350 M per year should be switched to support this new lab. In providing this support, it should recognize that getting commercial fusion power is certainly a multi decade project; nobody will have it in the next 5 or 10 years. After all, NIF was approved in 1995 for $1.1 B, to be completed in 2002. It was finally completed in 2009, for ~$3.5 B! How can the much larger project of setting up an appropriate rep rated laser and pilot power plant possibly be done more quickly and cheaply?

On September 26, 2023, I wrote to the president of Princeton University, the ultimate boss of its PPPL lab and suggested that he propose this change to DoE, just as his predecessors did 60 years ago. Princeton has a nearly unique opportunity here. It not only has a great deal of expertise and infrastructure in fusion, but it also has an endowment of ~$40 B and could easily sweeten the pot for DoE by shaking loose some $100 - 200 M. This sounds like a lot of money, but it is much less than the daily fluctuations, in the various markets, of the value of Princeton's endowment. This would provide a rep rated laser with perhaps 100 - 200 kJ shots (an intermediate energy before the final multi Megajoule laser is built), some new infrastructure, and some new hires for the lab. With this laser it would start to do rep-rated experiments on tracking and hitting fast moving spherical targets, among other tasks. For this *small* investment Princeton might be able to persuade the DoE to choose it for a new lab, and in doing so recover its position as the world leader in fusion. This sounds worth it to me. While I am unwilling to circulate the letter, I did give a brief description of it in the open literature [6].

Certainly, other labs, LANL, ORNL… could also make their cases. It is also worth mentioning URLLE. It has done a great deal of work in laser fusion, but it is not a national lab, and it has used only glass lasers and has never investigated excimer lasers. The question is whether it could become a DoE national lab and separate from the University of Rochester in the same sense as PPPL has separated from Princeton University. Of course, the University of Rochester has an endowment which is tiny compared to Princeton's. In any case, issues like these are far above my pay grade.

8. Conclusion and Summary

In conclusion, I will begin by summarizing some of the other plenary talks at the mini-conference, as they summarize the issues and problems at hand, scientific and financial. The first 3 talks were given first by Dustin Froula of URLLE [7], the leader of a consortium on laser plasma instability (LPI) research; the second was by Carmen Menoni of Colorado state [8], the leader of a university consortium on laser issues; the third by Tammy Ma of LLNL [9] on a coordinated national plan. What struck me about these talks is that the DoE does not seem to take this effort seriously. Compared to what is needed, the support for these projects is minimal, to say the least. Consider Carmen Menoni's consortium. I spoke to her and learned her support was ~$16 M over four years, split among 5 universities. I forgot Dustin Froula's numbers, but they were comparable. Yet each had viewgraphs showing far too many tasks for any of us to absorb, and a timeline leading to a fusion pilot power plant in 2035. Considering that it took 14 years just to build NIF, another 10 to get a burning plasma, all costing at least two orders of magnitude more than what is available in these consortia; the plan is nothing if not extremely optimistic. This author feels that what the DoE needs to do is to rock the boat and quickly set up a crash program. Increasing the budget by a million or two every year, so as not to ruffle too many feathers, will at best, delay the development of laser fusion by decades, and at worst kill it completely. That is the author's motivation for his recommendation that the DoE set up a new lab as quickly as possible.

This author finds it difficult to keep the following thought out of his mind: namely that the main part of the DoE is offering table scraps just to quiet down some pesky nuisances, without rocking the boat. However, to make "*IFE based concepts a reality*", we do not need table scraps, we need the entire banquet. In other words, several hundred millions of dollars for yearly support for a new lab, for a period of decades is what is required.

Furthermore, even if the DoE came up with the necessary support, is it really a good idea to have the support split among a couple of dozen completely independent organizations and tell them to work together? Isn't it better to have a single organization responsible for managing the entire project, like NNSA does with LLNL? If we have learned anything from ITER, with all its delays and cost overruns, it must be that having 7 different independent leaders, each with its own culture and manufacturing strategy is a serious mistake. Instead of having the IFE wagon pulled by 100 cats, let's get a horse. This horse is the new DoE lab proposed here.

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Conflicts of Interest

The author declares no conflicts of interest regarding the publication of this paper.

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