STATEMENT ON THE 2024 NOBEL PRIZE IN PHYSICS

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Since most of the physics academia[1] has a decidedly negative opinion of the 2024 Nobel Prize in Physics, which was awarded to Hopfield and Hinton for their works with statistical mechanics, Energy Based Models, Boltzmann Machines and Hopfield Networks, I have decided to give a very impartial judgment and verdict on this fiasco. Note that I work in high energy physics theory as well as machine learning, so you should consider this as unbiased as it gets from someone that understands both sides. The TL;DR is: **The 2024 Nobel Prize in Physics is not irrelevant and at the very least, is partially justified.**

Before getting anywhere close to why this verdict is as reasonable as it gets, we must first ask what constitutes a physics result, and what constitutes an *applied math* result. Machine learning at heart is purely a statistical problem: the model optimization itself for *any* objective, does *not* have to be a complicated problem and could be modeled fairly easily. How complicated this means is very subjective, but I would roughly say that the level of complexity goes up till standard stochastic differential equations, information theory, or statistical techniques used in say diffusion models, generative autoregressive models, etc. However, there are many involvements of results from physics into this field, primarily (of concern here) from statistical mechanics. For instance, assigning a Boltzmann probability distribution in a generative model would give us EBMs, where we rely on many aspects of *canonical ensemble learning*, such as the partition function, the energy function, free energy, etc. As of writing this, there are many papers and models that actually use these results. However, at this, it is important to note that *this physics is a useful framework and not a foundational construct.* Most of machine learning is still made of applied mathematics.

To get back to the question of what constitutes a physics problem differently from an applied math problem, it would be that in applied mathematics, we are often little concerned whether or not the dynamics are modeled precisely in real life. In that sense, while stochastic PDEs are very practical, they are not *physical* unless we have physical constraints. When we say we are working on a machine learning model, we mean the former, not the latter. So machine learning models at heart are at the very least applied mathematical objects. Good. Now what allows one to say that these models could *also* be physics results?[2] In the case of Hopfield and Hinton, their results used statistical mechanics excessively; for that matter, this is one of the things I am interested in right now! There are many links to Ising models, quantum spin networks, etc. So in some sense, they do constitute *something* that can at the very least be vaguely considered physics. Whether this physics is "pure" physics or not is not a relevant argument here, considering that

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[1]Particularly the subset which is comprised of theoretical high energy physicists.

[2]In the agreed category of those machine learning models that *use* these physics results.

most of the Nobel Prizes in Physics go to applied physics results. So the argument that people presented, that Ed Witten or Juan Maldacena should *also* receive the Nobel Prize, is not correct.

Here is a fair comparison to illustrate why the Nobel Prize wasn't unjustified. Ed Witten was awarded the Fields medal for his work on the positive mass theorem in 1981, which is – strictly speaking – not a pure mathematics result. However, the domain in which he was awarded the Fields medal is very clear. If one can win a Fields medal for working in physics, one must also accept that winning a Nobel Prize in Physics for machine learning is justifiable.

Let me just make one thing clear here: I am **not** supporting this. I am not saying that Nobel Prizes in Physics could be awarded for things merely linked to physics, even if there are good results that are being considered. But all the same, I am saying that this is not unjustified or unethical; in what the Swedish Academy does, while I don't exactly 100% support it, there is nothing wrong. At that, if we are talking about a field X being a strictly defined set, we need to point a *lot* more fingers. As an example, the Strings conferences used to be the flagship string theory conferences with real string theory being done. Now, there are people giving talks on results that are not even remotely related to string theory – e.g. celestial holography, arbitrary soft theorems and scattering amplitude computations, quantum information theory, etc. For that matter, most of what can arguably be considered *real* string theory is actually being talked at in Strings Math or Strings Phenomenology conferences, rather than the flagship Strings. By the same logic, one must also sternly oppose this. But we do not. Because we understand that **while** this is not in principle string theory at all, we allow interesting results to be given the light of the academia because there indeed are good results that in future could be applied to string theory itself[3]. The Nobel Prize in Physics 2024 is definitely not worse than that. Prizes and awards don't mean anything. But if you want to work within their rules and conditions, you have to play by their rules and conditions. Which is interesting, because Grisha Perelman, who is definitely one of the most important mathematicians ever, declined the Millennium prize award from Clay and declined the Fields medal. This is what happens when you value research and work more than the awards. But if you don't want to decline awards for whatever reasons (not saying you should here), you have to agree to the terms and conditions that the awarders expect you to accept.

The bottom line is that the 2024 Nobel Prize in Physics, as much as you buggers would like to say is garbage, is *at the very least partially justified*. However, if you really want your super elaborate construction for islands and non-gravitational baths attached to anti-de Sitter space and Page curve calculation to win the Nobel prize, maybe change how the Swedish Academy works. Because a lot of hep-th is also starting to stagant a little bit. Something to think about.

[3]Highly doubt celestial holography will go anywhere, though.