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WHEN A PHYSICIST COMES KNOCKING

By John Blyler

Will neutrinos from a supernova in the Tarantula Nebula save Einstein endangered postulate? Does all research have to be immediately profitable? Will collisions at Fermilab's Tevatron slow the speed of declining scientific prominence in the United States?

These questions evolved from a short interview conducted by LPE with Lisa Randall, renowned physics professor at Harvard University. Her insightful responses gave clarity to some of the complex issues facing science and engineering. Next Tuesday, Randall will launch a new season of speakers for the [Institute for Science, Engineering and Public Policy \(ISEPP\)](#) lecture series in Portland, Ore. Her presentation will draw heavily from her latest book, "[Knocking on Heaven's Door](#)."



LPE: Recent experiments at CERN suggest that neutrinos may travel slightly above the speed of light. Does this really contradict Einstein's postulate that no matter can travel faster than the speed of light.

Randall: First of all, the result is likely to be wrong. Even the experimenters who presented the result are aware of this, and presented it because they measured to the best of their ability, found this result, and want others to search for possible flaws or to do similar experiments to see whether they confirm or contradict their conclusions. In fact, this result seems to be inconsistent not only with known physical laws but with other results, such as the speed of neutrinos measured from SN1987a, a supernova that emitted neutrinos that were detected on Earth in 1987. There are caveats—energy dependence or neutrino-type dependence, but they don't seem likely. On top of that researchers have argued that neutrinos traveling at these speeds would lose so much energy that the result is inconsistent.

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"Ed, Power isn't the problem, it's the specification. Frequency (or gate count) is the problem. Microprocessor companies..." - Gary Smith

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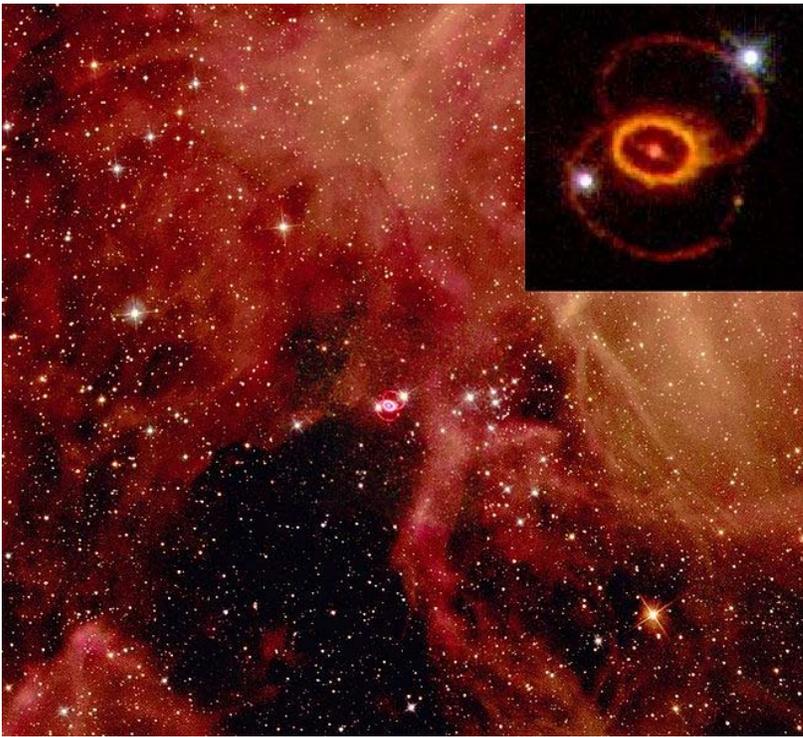


Fig. 1: Circumstellar rings around SN 1987A, with the ejecta from the supernova explosion at the center of the inner ring.

But what if the measurement were indeed correct? What would it mean? Clearly it would say that Einstein's postulate is not absolutely valid. As I discuss in my book, that wouldn't mean the hypothesis was useless or even wrong. It would be a very good approximation that works very well over a large range of parameters. But that doesn't mean the assumptions must be true with arbitrary precision for all ranges of possible measurements. It could be that when experiments have greater precision or a greater range of regimes of distance or energy, for example, that chinks can appear that are hints to new underlying theories. After all, special relativity doesn't mean Newton's Laws are wrong. But it does show they are approximations valid at speeds less than the speed of light. In a similar manner we might ultimately find a more fundamental theory than Einstein's Theory. But Einstein's Theory would still be useful.

LPE: In your book, you explore how we decide which scientific questions to study. How does the current state of engineering technology affect the capability to conduct certain scientific experiments?

Randall: Of course technology is relevant, as is expense. Big scientific experiments involve tradeoffs. The question is how to accomplish the desired goals using achievable technology.

LPE: Without engineering expertise, scientific advancement would be at a standstill. What is the right balance between science and engineering, in terms of education, funding and other support?

Randall: That's a tricky question. In any case, we shouldn't have to make that decision at the K-12 level. Science, math, computer and technical knowledge are all worth teaching in those grades. In terms of funding research, it's tricky to choose. Both are important. I suppose the basic division is between the type of research a company can immediately profit from, in which case the private sector should and will contribute, and basic or long-term research, which just won't happen without funding. It is too tempting to get caught up in funding research that purports to have immediate impact. But that might be the least essential for public funds—unless it really is basic research in disguise.

So maybe we should just understand the value of basic research, acknowledge it, and stop attempting to second-guess everything that will prove useful when making funding decisions. Smart people doing innovative research often make advances, even if they aren't completely spelled out in a research proposal.

LPE: Looming budget cutbacks may close U.S. particle physics labs leaving only Europe's Large Hadron Collider (LHC) to find the 'keys to universe.' Is this true? Does this suggest the United States is slipping in scientific research and, if so, why?

Randall: Fermilab still has a plan, but the Tevatron collider located there, which was for the last couple of decades the world's premier facility, just closed down. Fermilab does plan a so-called "intensity frontier." Rather than go to the highest energy—you're right that Europe now dominates—the idea is to go to the highest collision rate and measure processes that occur only rarely but that can still hold clues to underlying physical theories.

Certainly in terms of experimental research the U.S. is slipping, although it should be pointed out that the LHC has more Americans on each of the major experiments than any single other country. Theory (which is what I do) is still extremely strong here. But we can't be sanguine about the future of fundamental research even at the theoretical level at this point, even though it is clear that having great universities with the best

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scientists has been essential to American dominance in the last half century. It is critical to continue this tradition.



Fig. 2: The Tevatron (background) and Main Injector rings form a circular particle accelerator located in the United States.

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