No final frontier

We examine the arguments for and against building the next big particle collider and explore the technologies for different future particle accelerator projects.

In 2016, Nobel-Prize-winning particle physicist Chen-Ning Yang's criticism[1] of the proposed Circular Electron Positron Collider to be built in China sparked controversy in the media. Late last year, the publication of the proposal for the Future Circular Collider to be constructed at CERN stirred up similarly heated reactions on social media and in the popular press[2]. No big science project seems to come without controversy and the counter arguments that we hear today sound all too familiar.

It's too expensive and will rob funds from other areas. "Such a project would be gargantuan in scale, even measured against the big science ventures of high energy physics"[3] and would take away funding, putting other research areas at a disadvantage. "It endangers all of us, especially the young researchers"[3]. We do not know what we will find and there is a risk that we will not find anything useful. We are lacking the theoretical guidance and we should consider other approaches because "there are more productive ways of finding out the things we need to know about"[3]. "Clearly, the drive to embark on such a task must be motivated by something more than the lure that it is now technically feasible"[3]. Developing the necessary technology is good, but "Is it worth the cost, not in terms of dollars, but in terms of its impact on the rest of biological science"[3].

No, the mention of biological science is not a typo. You might have read these arguments recently, with almost the same wording, concerning the next big collider. But the above quotes are over 30 years old. They were used in the late 1980s in a debate around the Human Genome Project (HGP). The HGP sequenced the 3 billion DNA base pairs that make up the human genome. It was conceived in the mid-1980s, began in 1990 and the first results were published in 2001. The project cost approximately 3 billion dollars and involved 20 international groups. It was the first large-scale project to bring biology into the big science league.

When the HGP started, the high-throughput gene sequencing technology did not exist. The approach to storing and analysing large datasets was not well understood and biologists had no experience of working in big international collaborations. Despite these issues, the HGP was successful and its enormous impact on biology is undeniable.

There are striking parallels between the HGP and the biggest particle collider currently in use, the Large Hadron Collider (LHC). Knowing the genes was believed to be the end of the story and hopes were high that once the human genome was sequenced, cancer and other diseases would be swiftly cured. Unfortunately, that has not been the case as there are many regulatory layers beyond DNA. Likewise, the LHC completed the standard model (SM) of particle physics, but left many questions unanswered regarding dark matter, dark energy, quantum gravity and even some quirks of the SM itself. The HGP radically changed the way biology is done, for example, leading to the development of next-generation sequencing technologies that are now widely used in laboratories and hospitals. The LHC pioneered advances in computational science (the World Wide Web, grid computing, machine learning and data analysis) that are now broadly used in science and technology.

Lessons from the past teach us that despite difficulties, big science projects tend to work out and the outcomes, predicted or not, are well worth the investment. There is little evidence that other areas are deprived of funding, because the potential big budget is not automatically redistributed to other smaller projects. Moreover, the technological and scientific outcomes of big projects boost other areas of research.

We believe that the case for big science enterprises, such as a future particle collider, is strong. What are the options? In a series of Comments we explore different projects: the Circular Electron Positron Collider, the Compact Linear Collider, the Future Circular Collider, the High-Luminosity Large Hadron Collider, the International Linear Collider and plasma wave accelerators.

It is too early to say which of these projects will go ahead and whether they will reach their goals, but it is clear that to discover new physics beyond the SM we need to throw in everything we have: large-scale high-energy particle accelerators, small-scale low-energy experiments and astrophysical observations. In science there is no final frontier, just many frontiers to unimaginable places. One ship at a time is not enough. We need a fleet of ships to explore all those strange new worlds.

1. Xin, L. Nobel winner criticizes China collider. *Phys. World* **29**, 6 (2016).
2. Hossenfelder, S. The uncertain future of particle physics. The New York Times https://www.nytimes.com/2019/01/23/opinion/particle-physics-large-hadron-collider.html (2019).
3. Lewin, R. Proposal to sequence the human genome stirs debate. *Science* **232**, 1598–1600 (1986).