Chapter 1

Is the End of Theoretical Physics Really in Sight?

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I critically analyze the claim made by Hawking in his famous 1980 lecture that the end of Theoretical Physics is in sight and that in about twenty years one would have understood the key issues in Theoretical Physics. I review the progress made in the last twenty seven years since his claim and conclude that his prophesy has turned out to be incorrect. In fact, looking at the several unsolved problems as well as exciting new discoveries, it is highly unlikely that there will be an end of Theoretical Physics, at least in the foreseeable future. At the beginning, I also address Research vs Teaching syndrome that has seriously affected Indian Science since independence.*

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1.1. Research vs Teaching instead of Research and Teaching in India

I am honored to have been asked to give the Keynote address at this prestigious conference, to mark seventy five years of Indian Statistical Institute

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RECENT DEVELOPMENTS IN THEORETICAL PHYSICS

(ISI). It is time to pay our respect to its founder Director, Prof. P.C. Mahalanobis. His monumental contribution in building this institute and starting front ranking research in statistics is well known. I would like to point out one remarkable contribution of Prof. Mahalanobis which some how has not received enough appreciation. I have no doubt that it is this crucial decision which put ISI on the world map and helped in producing many outstanding experts in Statistics.

As you all know, in India we have the great divide between teaching and research. While most institutes exclusively do only research and (almost) no teaching, in most universities most of the scientists have to spend substantial amount of time in teaching. Further, in most universities, the infrastructure and other facilities are poor and bureaucracy has caused havoc in the administration. This great divide has affected Indian Science immensely and there is a serious crisis facing the country. In this scenario, what Mahalanobis did, clearly stands out. In 1960 he started the B.Stat. and M.Stat. programs (Undergraduate and postgraduate degrees in Statistics) in ISI. In the last 47 years, this programme has produced several outstanding researchers and teachers, who are occupying key positions in many institutions all over the world. In fact, before the B.Math. programme started in Bangalore and Chennai around 2000, the B.Stat. programme at ISI Kolkata was also the best programme in India for the B.Math. students. It is worth emphasizing here that this teaching has, in no way, affected the research quality at ISI and ISI is very well known throughout the world as a premiere centre of research in statistics. As far as I know, by and large, the faculty members teach about one course in a year which I think is a very reasonable teaching load.

I would like to compare here Mahalanobis with Homi Bhabha. There is absolutely no doubt that apart from his famous work (i.e. Bhabha scattering, Bhabha wave equation etc), Bhabha also laid the foundation of the Atomic Energy programme in India and also started Tata Institute of Fundamental Research (TIFR). However, unlike Mahalanobis, Bhabha never started either B.Sc. or M.Sc programme in TIFR. Bhabha perhaps felt that research and teaching cannot go hand in hand and top researchers should be left free to do research. And once TIFR set this example, almost all other institutes in India followed Bhabha's example and exclusively concentrated on research and did almost no teaching. With the infrastructure and other facilities too dwindling, soon better scientists preferred research institutes over universities and by and large the quality of the university science departments suffered (I must make it clear that there are still some good teachers and researchers in the universities and it is only because of them that good students are still coming to science).

This surely was a big blunder on the part of Bhabha, which is one of the key reason for the decline of science in India. From my own experience of teaching at the Predoctoral level in Institute of Physics Bhubaneswar for the last 33 years (of course I must confess that this is no substitute for teaching at B.Sc. or M.Sc. level), I can definitely say that teaching one course per year is in fact extremely beneficial (rather than hindrance) to research. I have no doubt that most of (whatever) physics that I know, has been learnt because of the teaching that I did. It is sad to see that even after sixty years after independence, the country is not ready to rectify this mistake.

It is worth pointing out another contribution of Mahalanobis. In 1931 he started an international journal in Statistics named "Sankhya" and remarkably, even after seventy seven years, it continuous to be a reputed international journal. Compare that with physics. We still do not have a high quality journal from India. I think one should critically examine why we have failed while Mahalanobis has been successful.

1.2. Is End of Theoretical Physics in Sight?

Let me now come to the main theme of my talk. You may recall that 27 years ago, on April 29, 1980, when he took over as the Lucasian Professor at Cambridge, Hawking[1] gave a talk entitled "Is the End In Sight for Theoretical Physics?" As is well known, in this talk he advocated that in about twenty years (from 1980), one would have understood the basic laws of nature and what will be left can be taken care off by computers. The purpose of this address is to critically examine this assertion of Hawking. In this context, it is worth remembering that this is not the first time that some eminent scientist has made such a strong claim. At the end of the nineteenth century too, physicists (including people like Michelson) had claimed that one had understood the basic laws of physics and what was left was merely the matter of details.

In order to properly judge Hawking's claim, it might be worthwhile to first quickly look at the progress made in physics in the first eighty years of the twentieth century and judge how correct was the claim of Michelson and others. After that, I shall examine in some detail the progress made in physics in the last twenty seven years and then point out several open problems which makes it abundantly clear that the end is nowhere in sight for theoretical physics.

1.3. Remarkable Developments in Physics from 1900 to 1979

As is well known by now, the prophesy of Michelson and others was rather premature. As it turned out, there was tremendous progress in physics in the first eighty years of the last century, perhaps more progress than in the earlier 300 years. That is why many people have termed it as the golden period of physics. At conceptual level I believe that three of the most remarkable developments of the twentieth century have been Relativity, Quantum Mechanics and Chaos, since all three cut away at the basic tenets of Newtonian physics. While relativity cuts away at the idea of absolute space and time, quantum mechanics killed the idea of controllable measurement process. Finally, chaos put to end the idea of deterministic predictability. Besides, quantum mechanics and relativity have led to so many new discoveries, many of which have tremendous technological applications which have changed the very fabric of our civilization. Some of these applications are nuclear fusion and fission, superconductivity, semiconductors, lasers and space technology. In human history, normally it has taken hundreds of years just to discover one layer of matter, but in these eighty years we have uncovered two layers of matter. By mid-thirties it was first established that the basic constituents of nature are proton, neutron and electron. And by mid-seventies we had discovered the next layer of matter and established that the basic constituents of nature are in fact quarks and leptons (electron is one of the lepton) and that all hadrons (including protons and neutron) are made out of quarks. One had also established that the various interactions between quarks and leptons are well described by the so called "Standard Model" with the gauge group $SU(3)_C \otimes SU(2)_L \otimes U(1)_Y$.

1.4. Developments From 1980 to 2007

Let us now examine the claim of Hawking and see if indeed one has obtained answers to the fundamental problems in theoretical physics in the last twenty seven years.

There is no doubt that there are several areas where remarkable progress has been made in the last twenty seven years, but without doubt amongst them one area stands out. I have in mind the area of Astrophysics and

Cosmology which now has become precision science. The idea of big bang cosmology along with inflation has received strong experimental support. We now know that approximately 70 percent of all the matter in the universe is in the form of dark energy, 26 percent is in the form of cold dark matter while only 4 percent is ordinary baryonic matter. While we have almost no idea about the nature of dark energy, what is perhaps clear is that dark energy is distributed homogeneously in the universe, and that it does not cluster like ordinary matter. Further, it acts as repulsive force, i.e. as anti-gravity and seems to increase the rate of expansion of the universe. It is estimated that dark energy has been present for at least nine billion years.

In high Energy Physics, $W^{\pm}$ and Z^0 were experimentally discovered precisely at the value predicted by the standard model. Further,one also discovered the top quark. However, without doubt the most remarkable discovery has been the detection of neutrino oscillations and hence the confirmation that the neutrinos (at least some of them) are not massless but have tiny nonzero mass. On the theoretical front in high energy physics, one remarkable development has been "String Theory" which to date is the only consistent (perturbative) quantum theory of gravity and is also a consistent quantum theory unifying all four basic interactions.

In the areas of Condensed Matter and Material Science, one remarkable development is in the area of Nano science and technology which has the potential for tremendous applications. Some of the other developments have been high temperature superconductivity, integer and fractional quantum Hall effect, and soft condensed matter physics which has strong overlap with complex systems and biophysics.

There have been progress in the other areas too. For example, there has been remarkable progress in the area of quantum optics and optical communication, quantum information and nonlinear science.

Besides these, there has been remarkable development in bringing together various areas of science and showing once again the interdisciplinary nature of science in general and physics in particular. For example, in the last twenty seven years the realization has come that to find answers to deep questions in cosmology and astrophysics, which essentially deals with physics at long distance, require answers to fundamental questions in high energy physics (which addresses issues at very short distance) and vice a versa. In fact a whole new area has emerged in the last twenty seven years called "Astroparticle Physics" which tries to address questions bordering both these areas.

Another remarkable collaboration has been between the areas of theoretical physics and mathematics. For a long time, the relationship between physics and mathematics was rather one sided with mathematics providing technique and justification for physics ideas and models. However, in the last twenty seven years, physics has made significant contributions to mathematics. The best illustration of that is the work of Witten for which he was awarded the Fields Medal (which is equivalent to four Noble prizes in physics, since it is only awarded once in four years!) by mathematics community in 1990. This was awarded for his three contributions in physics (i) proof of positive mass theorem based on supersymmetry (ii) supersymmetry and Morse theory (iii) Jones polynomials from Chern-Simons theory, all of which have important implications for some of the areas of mathematics.

The other significant contribution of physics to mathematics was the work of Seiberg and Witten regarding Donaldson invariants. It might be recalled here that Donaldson, who was awarded Fields medal in 1986 for this work, had obtained these invariants while working on the differential topology of four-manifolds. Seiberg-Witten, while working on the $N = 2$ supersymmetric Yang-Mills theory, realized that it has not only Donaldson invariants but using duality they predicted the existence of duals of Donaldson invariants, now known as Seiberg-Witten invariants. What was also remarkable about their work was that whereas one requires rather nontrivial effort to compute the Donaldson invariants in mathematics, Seiberg-Witten were able to compute the same with almost 1/1000'th of the effort!

Similarly, the areas of Condensed Matter and High Energy Physics continue to enrich each others areas even more than before. In the recent years, the ideas of conformal field theory has played significant role in such diverse areas as string theory, critical phenomenon and even two-dimensional turbulence. Further, the explanation of fractional quantum Hall effect needs ideas of conformal field theory as well as abelian and nonabelian gauge theories. In fact the area of low dimensional field theory and condensed matter physics has come up in rather big way during these twenty seven years.

1.4.1. *Revolution in Information Technology*

Finally, one cannot talk about important developments in the last twenty seven years in physics without mentioning about the revolution in Information Technology. I believe that it is as big as the great industrial revolution and like the earlier revolution, this one too is going to have profound implications for all human activities including physics. I feel that most people have still not appreciated its deep significance and the profound changes that are already taking place in countries like India which are at the forefront of this revolution. I think our generation is lucky that it is able to witness this great revolution and get a feel of how society must have reacted when it went through industrial revolution.

Let me now mention few important contributions which this revolution has already made in physics and has profoundly changed the way we carry out research. Undoubtedly, the most important discovery is that of world wide web (WWW). It is worth mentioning here that this discovery was made by a high energy physicist, Tim Berners-Lee, in 1989-90 while he was addressing the problem of how to transmit data from CERN experiments to 400 experimentalists sitting over five continents. Soon came the discovery of arXiv by Paul Ginsberg as well as that of electronic mail. These three discoveries have profoundly changed the way research is being done. The younger generation cannot even imagine how research was carried out before 1990, and how one could have a long distance collaboration.

Because of the information technology revolution, tremendous progress is being made in many areas of science including physics. It has helped in providing better insight of many concepts and has given better visualization of dynamical processes. Several areas like Gnome project, bio-informatics, could not have started without this revolution. For almost three hundred years, we are categorizing physics in terms of theoretical and experimental physics. But now there is a third branch, i.e. " Computational Physics". I do not mean here problems dealing with number crunching, but I am talking about issues which can neither be addressed theoretically nor experimentally but can only be addressed by using computers. The first example of that was the famous work of Fermi, Pasta and Ulam (with significant help from Mary Tsingou) in 1954 where they wanted to answer if weak nonlinear interaction between large (though not infinite) number of particles can lead to ergodic behavior of the system or not. It is worth remembering that the results of this paper inspired the subsequent discovery of KdV solitons and the whole industry of solitons!

Further, using computers one has been able to guess several exact results and subsequently prove them analytically. I will mention here two examples where I have first hand experience. The first is concerning the new identities for the Jacobi elliptic functions[2] which Sukhatme and I discovered in 2001. While we could prove these identities analytically in few simpler cases, we had no clue how to prove these in general. So we did the next best thing. We tried to check the validity of the identities in the general case numerically using Maple and to our satisfaction we found that the identities are indeed correct to an accuracy of six-seven decimal places. We mentioned this in our paper and conjectured the validity of the identities in the general case. Only later, in collaboration with Arul Lakshminarayan, we were able to prove these identities rigorously in the general case.

The second example I have in mind concerns the problem of three noninteracting anyons. I might recall here that anyons are particles in two dimensions, which unlike three or higher space dimensions, obey any quantum statistics. In 1990 we were trying to compute the third virial coefficient of a noninteracting anyon gas (which to this day remains an unsolved problem). Unfortunately, it was not possible to separate mixed symmetry states from symmetric and anti-symmetric states and so as a first approximation, we numerically computed the third virial coefficient in the Boltzmann basis and while doing so numerically obtained a neat relation between the interacting part of the two and three anyon partition functions.[3] Subsequently, Virendra Singh proved our result analytically.

The point I would like to emphasize here is that in both cases, only because one could numerically obtain or verify certain relations that one even thought about how to prove these results analytically.

The information revolution has also raised some interesting questions like "what constitutes a proof?" Take for example, the celebrated Riemann hypothesis about the nontrivial zeros of the zeta function being only on the half-line. By now, one has numerically checked that the first 15 billion zeros of the Riemann zeta function are indeed on the half-line. The question is, does this constitute a proof of the Riemann hypothesis? While I believe that the answer is *NO*, it is not that easy to argue against those who would like to regard it to be as good as a proof.

There is one more aspect of the information technology revolution which has hardly been explored so far, and that is its use as a valuable aid in teaching. I have no doubt that the future generations will mock at us saying how backward we were in our methods of teaching. I have absolutely no doubt that in coming few decades, information technology is going to revolutionize the way we teach right from primary school to research level. Its potential for (i) visualization of very many difficult concepts and dynamical processes (ii) as a substitute for experiments (specially in underdeveloped countries) (iii) exposure to excellent lectures has hardly been realized so far.

1.5. Was Hawking Right In His Assertion?

Finally, let us ask ourselves if indeed the end of theoretical physics is in sight? The answer is unequivocally *NO*. So many basic questions have remained unanswered and one does not see any light at the end of the long, dark tunnel. For example, we still do not have any idea about what is beyond the standard model of high energy physics. In the last twenty seven years, several theoretical ideas have been discussed including string theory, but it is not at all clear if these ideas or right or wrong. Physics being an experimental science, I strongly feel that whether these ideas are right or wrong can only be decided by experiments. The Large Hadron Collider (LHC) at CERN is being built to look for the elusive Higgs particle of the Standard Model. There is hope that LHC may also discover some of the supersymmteric particles thereby indicating direction beyond the standard model. On the other hand, in case LHC does not find any new physics (apart from the Higgs particle), then there is a real danger that one may not be able to obtain large scale funding required for electron-positron linear collider and the progress of the field may be in danger. In any case, a major breakthrough in accelerator technology is badly needed if we are to explore physics at distances less than 10^{-20} meter.

As far as string theory is concerned, it is fair to say that while the ideas are attractive, it is too early to say if it is indeed TOE (theory of everything) as was widely proclaimed by several supporters of the string theory. As of now, string theory has really no predictions which can be tested in present day laboratories, and it is not even clear if in foreseeable future too this will be possible.

Coming back to big bang model of the early universe, the present picture crucially depends on the assumption that the baryon number is not an exact symmetry of nature. One important consequence of baryon number violation is proton decay. However, till today there is absolutely no evidence for proton decay. I strongly feel that unless one observes proton decay, the big bang picture of the early universe would remain suspect. Similarly, while approximately seventy percent of all matter in the universe is in the form of dark energy, one has no idea about the nature of this dark energy. Unveiling this mystery may reveal new physics and could also shed light on future direction in high energy physics. Besides, while twenty six percent of all matter in the universe is in the form of cold dark matter, we still do not have any definite candidate for the dark matter. Finally, in spite of many many attempts, it has still not been possible to have a consistent quantum theory of gravity, and this I believe is one of the major unsolved problem of modern day physics.

In nonlinear physics, one major unsolved problem is that of turbulence and in general that of spatio-temporal chaos. Similarly, the area of condensed matter physics is undergoing a conceptual change with a recent emphasis on soft condensed matter physics and complex systems which has strong interface with several issues in biology. This has opened up several new challenges and it is increasingly becoming clear that the standard courses in condensed matter physics that are being offered in graduate and undergraduate stage are inadequate to attack these challenging issues.

1.6. Conclusions

In view of what has been said above, there is no doubt that the prophesy of Hawking was rather premature and the end of theoretical physics is no where near in sight. Nature is far more subtle than what we human beings can imagine it to be. In this connection, I would like to recall a conversation that I had with t'Hooft in 1994 at Puri regarding TOE. He strongly believed that there is TOE and he felt that once we have found it, we would know. However, he was not sure, how long it would take. However, I had and still have a slightly different view point. I feel that even if we find TOE, it will raise further deeper questions. Nature is very subtle and human explorations are like opening of different onion layers. As we uncover one layer, there is next one to be explored. I feel that our attempts at understanding TOE is like trying to uncover yet another sari of Draupadi.

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