Kenneth G Wilson (1936–2013)

The Classics article in this issue is a short autobiographical sketch by Kenneth Wilson, the sole winner of the 1982 Nobel Prize in Physics. The piece brings out the bare facts of Wilson's life in his own words – very few of them, in fact. No one reading it would realize that he was one of the most celebrated among twentieth-century theoretical physicists for changing the perceptions of strongly interacting systems – either in particle physics or in condensed matter. The Nobel citation mentions the cryptic term 'renormalization group'. An article elsewhere in this issue (pp.15–36) brings out some aspects of this remarkable idea. This article fills in the background against which the Classics article should be read – the problems faced by theoretical physicists in the 1960s and how Wilson was able to make an enormous impact on them.

The first half of the twentieth-century saw the establishment of a very successful theory of photons and electrons called the 'Quantum Electrodynamics' or QED, within a framework called the 'Quantum Field Theory' or QFT. QED is now verified with amazing precision in its domain of applicability. All atomic and condensed matter phenomena could be investigated in this framework, at least in principle. This triumph was recognized by the award of the 1962 Nobel Prize to Tomonoga, Schwinger, and Feynman. But their work on QED had its limitations as well. Their methods were not applicable when extrapolated to very high energies. QED also left out the weak interactions responsible for beta decay. It took another twenty years to bring the weak forces under the QFT framework (*Resonance*, Vol.19, No.1, pp.18–44). However, the forces holding protons and neutrons together in the nucleus, which also govern the processes involving pi mesons and other particles, seemed hopeless to tackle from first principles. This led some people to abandon the QFT framework altogether.

One physicist who had made some progress on the difficult problem of strong interactions in this period was Murray Gell-Mann at the California Institute of Technology. He took on a young research student who had graduated from Harvard University in 1956 – Kenneth Wilson. Gell-Mann suggested that Wilson work on a particular, simplified model of strong interactions. This is called the 'Fixed Source Model', and is meant to imitate the more realistic problem of pi mesons interacting with nucleons, including the crucial feature that the number of pions was not fixed since they could be created and destroyed in a relativistic theory. In spite of the simplification, only some approximate approaches were available. Interestingly, it appears that Wilson's own strongest interactions at Caltech were not with the famous figures like Feynman and Gell-Mann but with a young faculty member – Jon Mathews, who introduced him to computers. Wilson's fascination with computers, over the years, turned into the firm conviction that many of the hitherto intractable problems of physics needed a more computation oriented approach. This belief was not for want of mathematical ability! In fact, Wilson taught himself calculus in his early teens and majored in mathematics at Harvard, even proving a conjecture by Dyson. Perhaps it was precisely his ability which enabled him to see the limitations of purely analytic approaches to contemporary physics problems.

Harvard University has a prestigious postdoctoral group called the Society of Fellows. Wilson was admitted to this even before he had written his PhD thesis. He later spent time in Europe, and then took up an Assistant Professorship offered by Cornell University in 1963. In the US system, people in this position are reviewed for 'tenure' (permanent appointment), after about five years, based on their body of published work. However, Wilson was given tenure in two years, without a published paper – a sure sign of the respect he was held in by his colleagues. As a PhD student, he had spent a summer working on plasma physics. He was asked by his guide, the famous Marshall Rosenbluth, to write up the work he had completed in just three months. That experience convinced him that he should only work on really hard problems where progress would be so slow that he did not have to write a paper that often! The problem he chose to focus on was the problem of strong interactions, in particular, the fixed source model.

The best reference for anyone interested in the development of Wilson's thinking in this period is a long interview that he gave in 2002, available at: http://authors.library.caltech.edu/5456/1/ hrst.mit.edu/hrs/renormalization/Wilson/index.htm. A briefer and less personal account is in his Nobel Lecture (https://www.nobelprize.org/nobel_prizes/physics/laureates/1982/wilson- lecture.html).

From these sources, it becomes clear that while pursuing his main goal single-mindedly, he maintained wide exposure to other kinds of physics. The trigger for his Nobel Prize winning work on the critical point was a seminar in the Chemistry Department by his colleague Ben Widom. Wilson was intrigued by the strong experimental evidence for universal, mathematically singular behavior, very near the critical point. Physical quantities like specific heat, magnetic susceptibility, etc., behave as fractional powers of the deviation of the temperature from the critical temperature. A partial explanation for this behavior had been given by Kadanoff (*Resonance*, Vol.21, No.10, pp.875–898). Wilson saw – as Kadanoff himself did – that this 'scaling picture' was not a quantitative calculation and was based on plausible rather than provable assumptions. In the years from 1965 to 1970, Wilson developed parallels between the problems faced in understanding critical phenomena and the problems he was tackling in strongly interacting field theory. By 1970, he had come up with a method for calculating critical behavior, based on fairly elaborate computation (for that time). He was happy enough with this scheme to publish it but did not see it as the breakthrough he was seeking.

The trigger for further progress came from discussions with another Cornell colleague – Michael Fisher. Fisher was an expert on critical phenomena and had done extensive work on another approach, based on calculating a large number of terms in a series in the reciprocal of the temperature. Progress in theoretical physics often comes from recognizing something which is small and using a power series in that variable – and the reciprocal of temperature did not work. The discussions between Wilson and Fisher revealed that the small quantity was a surprising one. Experimental systems are in three space dimensions, where d=3. The particle physics problems that were Wilson's primary interest were in d=4 since time was also involved. Wilson and Fisher introduced 'epsilon' which was 4−d (in d dimensions). Wilson's earlier elaborate numerical calculation was simplified when epsilon was small. More importantly, Wilson was able to bring his insights into the field theory to create a systematic procedure for working in powers of epsilon. This was the breakthrough. The Kadanoff scaling picture now had an analytical and systematic basis.

After 1972, the rest of the world thrived on the epsilon expansion for several years – hundreds of papers were written on the critical properties of various models, of which there is no shortage in condensed matter physics. To Wilson, the epsilon expansion was just one more tractable case of his general renormalization group framework. In the general case, one has to keep track of a large number (in principle infinite) of interactions. The earlier work in particle physics, and his own epsilon expansion were particular cases where one could confine to just a few terms. He gave a set of lectures in Princeton which became a standard review article – the famous *Wilson and Kogut*, which physicists all over the world tried to decipher. By 1973, a breakthrough came in strong interaction physics, from Gross and Wilczek in Princeton and Politzer at MIT. This theoretical discovery is called 'asymptotic freedom' and states that the forces between quarks and gluons (which in turn make up neutrons and protons) become smaller at high energies/short distances, unlike the case of QED mentioned earlier. This discovery was recognized by the 2004 Nobel Prize in Physics. The basic theoretical tool used was the renormalization group. Wilson records wryly that he had earlier written a paper on the renormalization group and strong interactions, which listed every possibility except the correct one!

He continued to work on problems in statistical physics which required the full renormalization group framework – keeping track of dozens of terms on a computer. Inspired by another colleague, John Wilkins, he took up a twenty-year-old puzzle – the interaction of a single magnetic impurity (say a manganese atom), with the conduction electrons in a metal like copper. Like the fixed source theory, it had proved to be a hard nut to crack for more than a decade. The qualitative behavior had been conjectured earlier, but Wilson was able to give a quantitative solution based on his NRG (Numerical Renormalization Group) method. In this one case, it turns out that there is an analytic solution, which was found later by Wiegmann in Russia, and

Andrei in the US. Wilson also pioneered lattice gauge theory which is today the most quantitatively successful method of calculating – on a computer (preferably a large one), the properties of strongly interacting particles.

Wilson had very few research students. One of them, H R Krishnamurthy, now at the Indian Institute of Science, has written a first-hand account of how it was like to work with Wilson in this period. This appears in the *Ken Wilson Memorial Volume* (World Scientific 2015). The following paragraph brings out Wilson's informal yet focused style.

"My periodic interactions with Ken regarding my thesis research were invariably rather brief, but pleasant and rewarding. Ken was very informal – I never had to make an appointment to see him, and would walk into his office whenever I wanted to, which was typically when I had some progress to report, or to seek help when I faced some obstacles in my work. There I would generally find him, mostly in his signature grey pants and white shirt, often with his feet up on the table, and deep in thought. But he never seemed to be perturbed by the interruption, and would turn to me with the twinkle in his eye that used to be a ubiquitous feature of his demeanor, as can be seen in so many of his photographs."

One more remarkable fact deserves mention. Computers are basically a young man's game, and the technology evolves rapidly. Most senior scientists have to rely on their research students or younger colleagues for writing elaborate computer programmes. Not Wilson – here is Krishnamurthy, again on this topic.

"Ken suggested initially that I modify the NRG program he had written for the Kondo problem and use that for the AIM [Anderson Impurity Model] project, and gave me a copy of his program. I was flabbergasted when I saw it – it had well over a thousand lines of code, pretty much as one single program (except for calls to a matrix diagonalization subroutine), and there was not a single comment statement in it! Many important variable names were chosen in ways I could not fathom; there was an XXXX and a YYYY! I had to go through the code line by line, annotating it along the way, which took me a while; then I understood and appreciated how tightly and intricately knit it was. All available symmetries of the Hamiltonian had been used to reduce the sizes of matrices to be diagonalized to the minimum possible, and storage of arrays had been maximally optimized to reduce memory requirements. I have always wondered how Ken kept track of what was what in the program, and how he debugged it. Knowing how awesome he was as a programmer (he was one of the very few physicists I have come across who knew how to write machine code, and would use it to optimize the innermost computations inside 'do loops'), I am inclined to believe that he had such algorithmic clarity that he wrote code that needed very little iteration and debugging; and that he had prodigious memory which helped him keep track of obscure variable names."

At the height of his fame and achievements, in 1988, Wilson moved from Cornell to Ohio State University, with a focus on physics education, though he kept his interests in fundamental physics and computation alive. In 1995, when just 59 years old, he moved to the small town of Gray in Maine, which has a population of only a few thousand and is located among lakes and forests. He retired formally in 2008. These moves are hard to understand in conventional terms but speak to us of a man who lived by his own lights. The obituary which appeared in the New York Times ends by quoting his wife, Alyson Brown, a computer scientist.

"Ken was the most lacking in small talk of anyone I ever met," Ms. Brown said. When he died, she sent an e-mail to friends, saying: "Ken died last evening. He always liked to do things quietly and without fuss, and that's how he left us."

Suggested Reading

[1] H R Krishnamurthy, Ken Wilson – A Tribute: Some Recollections and a Few Thoughts on Education, Ken Wilson Memorial Volume: Renormalization, Lattice Gauge Theory, the Operator Product Expansion and Quantum Fields, World Scientific, May 2015, Read Full Text at: https://arxiv.org/ftp/arxiv/papers/1701/1701.00093.pdf

Rajaram Nityananda
School of Liberal Studies, Azim Premji University
PES South Campus, Electronic City, Bengaluru 560 100, India.
Email: rajaram.nityananda@gmail.com