Dalitz memorial meeting 3 June 2006

Valda, members of the Dalitz family, friends, colleagues, ladies and gentleman.

My tribute to Dick Dalitz will be a bridge between this morning's celebration of Dick the scientist and this afternoon's of Dick the man. I shall describe Dick as seen by a student and (later) colleague, and try to convey something of his style as a scientist and his achievements to those here who are not scientists.

I first became really conscious of Dick's name in July 1964 when I received a letter saying that I had been accepted as a graduate student and Professor Dalitz would be my supervisor. I informed my undergraduate tutor who replied: "Dalitz is the leader – all you have to do is keep up" – an exciting if daunting injunction.

Dick's style soon became clear to his students who, within a month or two of their arrival, were set to carry out calculations of immediate relevance to experiments. We were both impressed and rather intimidated by his intellect, knowledge and reputation, and by the way he would reflect on a question in silence for what often seemed like minutes before supplying a highly concise answer.

We were also rather puzzled by him. What sort of name was Dalitz? Only later did I discover that at that time he himself did not know the answer, which will be discussed later. Why had Dick – very obviously an Australian – come to England from America at the peak of concerns about the brain drain, when the US was seen as a scientific mecca? We were reminded of this daily by the large American convertible he had brought with him from Chicago which blocked much of the entrance to 12 Parks Road, where – underneath the sign 'Theoretical Physics' – some wit had put a sticker, taken from a car radiator, reading 'Do not drain'. Why did this small and obviously modest man own such a large flamboyant American car? This paradox was removed if not resolved when it was replaced by a Mini, a worthy successor to the ancient Austin 7 that I later learned he had owned in Birmingham.

The mid 1960s was a time of turmoil in particle physics. A host of new particles had been discovered in the previous ten years, and a great deal of effort was devoted to elucidating their properties and speculating about how their existence and properties might be understood. Many of these particles had been identified using a device known as a 'Dalitz plot', which I regard as one of the two most important of Dick's many outstanding contributions to physics.

The Dalitz plot – or 'the usual phase space diagram', as Dick himself modestly called it – is a very ingenious way of representing data that describe systems of three particles. It removes purely kinematical effects and directly reveals the underlying dynamics. Dick invented the plot in 1953 when thinking about observations of a particle that had recently been discovered in cosmic rays. His analysis suggested that the way in which this particle decays might 'violate parity', or – in everyday language – distinguish left from right. Up to that time it was universally believed that nature makes no such distinction, the most obvious consequence of this belief being that it would be impossible to tell whether a film of any conceivable sequence of events had been shot directly or in a mirror.

Dick speculated privately that the laws of nature might not be left-right symmetric, but he was apparently discouraged from pursuing this extremely radical idea by his one time mentor Rudolph Peierls. Rudi, in common with almost all the world's leading theorists, thought failure of mirror symmetry impossible – or at least so improbable that other explanations should be sought, however bizarre. It was left to T D Lee and Frank Yang to show that in a whole class of phenomena mirror symmetry had never been tested, and propose a direct way of comparing certain mirror image experiments. To general astonishment they were found to yield different results: the 'principle' of mirror symmetry had been only a theoretical prejudice. Dick's work underwrote this sensational discovery, which changed basic attitudes to fundamental symmetries of nature.

When he arrived in Oxford, Dick started a series of seminars which took place at 8.30 in the evening – because according to Dick no other time was available – in the old High School on the Banbury Road, which was then an annex of Nuclear Physics. Dick attracted a series of distinguished speakers, who arrived with him – frequently a little late and out of breath – generally in a state of bafflement after the (to most) alien experience of dinner and the first few minutes of dessert here in All Souls.

As I have said, the emphasis in the mid 1960s was on understanding the properties of the many particles that had recently been and were being discovered, many with the aid of the Dalitz plot. Most of Dick's seminars were devoted to experiments on the new particles, but some were on speculative theories that might explain their existence and properties in a unified way. The idea that we might be on the verge of a breakthrough in finding a unified theory had attracted many students to the field, and was exciting considerable public attention – including, for example, a half page feature in the Sunday Times that described work by Abdus Salam, which he had reported in one of Dick's seminars, as likely to be the biggest step since Einstein.

At first I imagined that Dick would also be developing new theories. But when I asked him what ideas he was pursuing, he replied: "My job's not to make theories – it's to understand the data". Later I told him that I felt disorientated when a whole class of fashionable theories, including those advocated by Salam and other leading figures, had been shown to be mathematically inconsistent. Looking at me incomprehendingly he said "Those who jump on bandwagons must expect to fall off". In the same vein, Gabriel Karl has described reporting to Dick on a paper that considered the possible effects of a hypothetical interaction. "But there is no such thing" said Dick. "Yes" said Gabriel "but you can imagine that there is", to which Dick replied: "In that case you must stop yourself".

Dick's own approach was to immerse himself in the data, of which he had an encyclopaedic knowledge until their volume became too great. I remember him once, after quoting an experimental result, apologising for feeling that he needed to check it in one of his meticulously kept notebooks (characteristically he had got it right), adding "I used to know all the data; at one time I knew very event". I think that he saw the theorist's role as being to find a way of representing experimental data so that they directly reveal nature's secrets, as the Dalitz plot had done.

Dick's focus on understanding the data explains the apparent paradox that, despite his reluctance to speculate – which perhaps cost him the discovery that mirror symmetry is violated, his other really seminal contribution was to demonstrate the power of the quark model of sub-nuclear particles, at a time when it was dismissed by most of the theoretical particle physics 'establishment' as impossibly speculative. Their scepticism was based on the fact that the supposed behaviour of quarks appeared very hard to reconcile theoretically with then current knowledge of forces. Dick of course knew of the theoretical difficulties, but was much more impressed by the fact that the quark model provides a simple way of summarising a huge amount of data – which after all is the first prerequisite of a good model of nature.

The quark story began in 1964 when Murray Gell-Mann, who had earlier shown how to classify particles with related properties into families, pointed out that this family structure would emerge naturally if the particles were made of more elementary objects, which he called quarks. Gell-Mann equivocated over whether quarks were just a device to simplify the mathematics, or should be taken seriously as real particles. Dick, however, seized on the quark model and used it to explain many results and to predict the existence of new particles inside which quarks vibrate and rotate in different ways. Experimentalists quickly recognised the value of this work, but – as I have already indicated – most theorists looked down on it. Gell-Mann himself – then the unrivalled leader of the field – described the quark model as 'naïve', perhaps hedging his bets, and is said to have walked out of a talk on the new particles by Dick at the 1966 world particle physics conference which he based on the quark model.

By 1970, however, the data as interpreted by Dick and others, together with new types of experiments that could effectively 'see' the quarks inside protons and neutrons, had convinced many people that quarks should be taken very seriously indeed. A theoretical basis for understanding their properties, as revealed empirically by Dick and others, followed a few years later. Almost overnight the 'naïve' quark model came to be regarded as almost trivially obvious. Dick's key pioneering role was soon largely forgotten and is not mentioned in textbooks. This, and the scepticism his work attracted at first, must have been hard to take. He never publicly expressed bitterness about it, although it would have been justified.

I have emphasised Dalitz plots and the quark model, but Dick made many other extremely important contributions, for example to understanding of atomic nuclei called hypernuclei that contain unstable heavy particles – a field in which he was the undisputed theoretical leader, to say nothing of predicting (with a small hint from an experiment) the existence of 'Dalitz pair' decays of the neutral pi meson, and correctly calculating the rate at which they occur long before it was measured. From the 1950s to the early 1970s he was arguably the world's leading 'phenomenological' particle theorist. Dick remained very active in later years. Characteristically at the conferences that we organised in his honour on both his sixtieth birthday and on his retirement, he insisted on presenting papers because he had new results to report. On the latter occasion a number of people remarked that Dick could not really be retiring – 'it must all be a Dalitz plot', and they were not wrong.

By the 1980s, however, a 'standard theoretical model' of particle physics had emerged which did not appear to interest Dick much, and the style of theoretical particle physics (in both phenomenology and 'theoretical' theory) had changed. The empirical experimentally based approach which was Dick's forte no longer plays a major role because of the success of the standard model and because of the volume of the data, which can only be handled by large computers – although there are exceptions, as shown by Dick's work on the so-called top quark.

Dick worked very hard. Following a burglary in the Department one Christmas, it was thought that the large electromechanical calculator that he kept next to his desk was among the stolen items. But it turned out that he had taken it home in order to work over the Christmas and New Year break. In fact, he regularly came into the Department during holidays, and – as one of the first to arrive – often took on the chore of bringing the mailbag over from the Clarendon Laboratory where it was left when the Department was shut. Jack Paton remembers coming in while Dick was sorting the letters and putting them in pigeon holes: Jack's children, who were with him and had earlier seen Dick carrying the large bag on his very small bicycle, put two and two together and thereafter insisted on calling Dick 'the postman'. He is known to have come in on at least one Christmas day when he called Roger Elliott, just after Roger had finished his Christmas dinner, to ask for help in evicting a student of unsavoury habits who had taken to living in the Department

Dick was a perfectionist, especially regarding anything he wrote. I remember one of his secretaries, long before the advent of word processors, telling me that anything he wrote typically went through six typed drafts – even so, the final version usually contained a few improvements or clarifications inserted by hand.

He was also the ultimate professional. When, in reply to his question why I had not written up some work in a paper, I said that I did not think it very amusing, his only half joking reaction was: 'You're not paid to be amusing, you're paid to be useful'. He tackled every problem with exemplary thoroughness, and his review articles and lecture notes were wonderfully useful sources of comprehensive information, concisely presented.

Dick's contribution to the obituary notice of Dirac that he wrote for the Royal Society with Rudi Peierls is a typical example of his scholarship and concision. He traced the name Dirac back to 1100, and in under a page, managed to summarise Dirac's forebears movements and careers starting in 1721 in France, through various places in Switzerland (for which purpose he made a visit to the Valais to check what became three sentences), and finally to Bristol. His admiration for Dirac was almost as great as his respect and admiration for Rudi Peierls, and he was proud of the success of his campaigns to have a Dirac memorial plaque placed in Westminster Abbey, and to have the Theoretical Physics building here in Oxford renamed the Rudolph Peierls Centre for Theoretical Physics.

Dick created the Oxford particle theory group, attracting excellent students and post-docs, and dealing with all the paperwork connected with our rolling grant in an almost too exemplary manner, being master of all the details and finances. I don't think that many of us appreciated this sufficiently until the burden later fell on us.

He worked in the Department every day. He was always present at coffee where he would contribute to the discussion in his characteristically concise manner, with an occasional joke and wry smile, although he did not join in gossip or speculation. He would often eat lunch in the discussion room, following an unvarying routine – get out his penknife, carefully cut a celery stick, peel an apple, cut a sandwich into quarters. In retrospect, I realise that he was shy – although this never occurred to me when he was at the height of his powers, which may have led to misunderstandings of his attitudes and silences, which also reflected his modesty.

If asked what I learned from Dick, I would say above all professionalism – the importance of getting the facts and arguments right and describing them with the greatest possible clarity and precision, with the correct references. Of course I also learned a lot of physics from him, but we never actually collaborated on a problem or wrote a joint paper. If asked what I valued most, it was his kindness. I never heard him say an unkind word, and I still feel deeply grateful for the long and detailed letters he wrote me in the years I spent outside Oxford after completing my DPhil, giving me advice and analysing career options.

Dalitz pairs and Dalitz plots are, and will remain, part of the vocabulary of particle physics. I hope that Dick's name will find its proper place near the top of the roll of honour of leaders of the revolutionary developments in particle physics in the 1950s to 1970s to which he contributed so much – especially the development of the quark model. There will never be another Dick, but the spirit in which he approached the data will be needed when surprises are produced by the next generation of accelerators. His many students, colleagues and admirers will retain affectionate and grateful memories of him, and I hope that his spirit will live on in their work.

Chris Llewellyn Smith