50 Years of Polymer Testing

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Preface

This book is both a brief history of the technology of testing rubbers and plastics and a personal account of the changes in that field over 50 years. Much of the content relies heavily on the author's experiences in the testing laboratories of the Rubber and Plastics Research Association (RAPRA) and on national and international standards committees. The structure owes much to the editorials that have appeared in the journal *Polymer Testing* over the thirty years that he has been its editor.

It is hoped that by providing some appreciation of the past it will contribute to the further development of testing rubbers and plastics in the future.

Contents

Acknowledgements

I would like to take this opportunity to thank all those people in the polymer industry who have influenced my understanding of testing rubbers and plastics, and contributed to my enjoyment of working in the field for so many years. In particular, I am most grateful for the help and friendship of colleagues at the Rubber and Plastics Research Association (RAPRA) and fellow members of standards committees.

The book would not have been possible without the input that came from being the editor of the journal *Polymer Testing* since its inception in 1980. I thank Elsevier Academic Publishers for providing me with that pleasurable duty, and for being able to draw on my editorial contributions.

1 Information

Henry Ford may have said that History is more or less bunk but he did not claim it was not fascinating. Some of it may be pretty turgid, such as learning the succession of kings and queens, as those of us of a certain age were required to do in English schools. Such knowledge is of miniscule consequence today. On the other hand, some of the intrigues and battles generate excitement, and how our societies developed over the years is educational in understanding what we now have and intriguing in its own right. The same is true of the history of science, except I would argue that the inherent interest factor is somewhat greater. Hence, the simple justification for this commentary on developments over half a century in the modest subject of polymer testing is that it has its own interesting aspects and maybe something could be learnt from it for the future.

Polymer testing can be said to have started when rubbers and plastics were first produced – if you are willing to accept the native South American Indians bouncing rubber balls as testing. The polymer came to Europe by the eighteenth century as Joseph Priestly recommended it as an eraser – and the name rubber stuck in the English-speaking world (although variations of the Indian name are used in other languages) [1]. It is not difficult to imagine Priestly and others prodding the material with their fingernail and stretching it by hand to judge basic mechanical characteristics. By 1840 Thomas Hancock had discovered mastication and Charles Goodyear had demonstrated vulcanisation – rubber was to become an important industrial material and laboratory testing methods would start to be developed.

There can be some argument as to the first plastic because natural resins were known many centuries ago. Ebonite, discovered in about 1850, was the first thermosetting plastics material and also the first plastics material which was the result of a particular chemical modification of a natural material. Parkesine and celluloid then followed, but it was not until the twentieth century that phenolics became the first commercially successful synthetic resin [2].

It is not the intention, nor is the information available, to chronicle in detail the early history of the application of physical tests to rubber and plastics, but a few facts help to put later developments into perspective. In particular, it can be surprising how far back some tests can be traced [3, 4]. Apparently, Gallieo Gallilei made quantitative measurements of material strengths around 1638, whereas Hooke's law of elasticity dates from 1678. Galileo calculated the load supported by a cantilever beam, so he could have done flexural tests. The first recorded mention of commercial methods of materials testing was by Reamur around 1725, whereas Thomas Young gave us the well-loved modulus in 1807. Poisson's ratio and elastic theory developed in 1829, whereas limits to metal fatigue were described by Wohler in 1858. Particularly surprising is that photoelastic stress analysis apparently dates from 1850, and two-dimensional stress on a plane was studied by Mohr in 1868.

A testing machine was built by Lame in France in 1824, and it is comforting to know that Telford did not rely on intuition to build bridges but instead established his own machine at about the same time. The dynamometer dates from the 1830s, whereas accurate extensometers started to be produced by about 1850. There was no problem with measuring the test-piece dimensions because the Vernier scale dates from 1630 and the screw caliper from 1640; by 1893 you could have a digital micrometer. The results could be calculated using slide rules which had also been invented in the seventeenth century after from the discovery of logarithms by Napier in 1614. It could be demonstrated that rubber was electrically insulating as the Wheatstone bridge was invented in 1833.

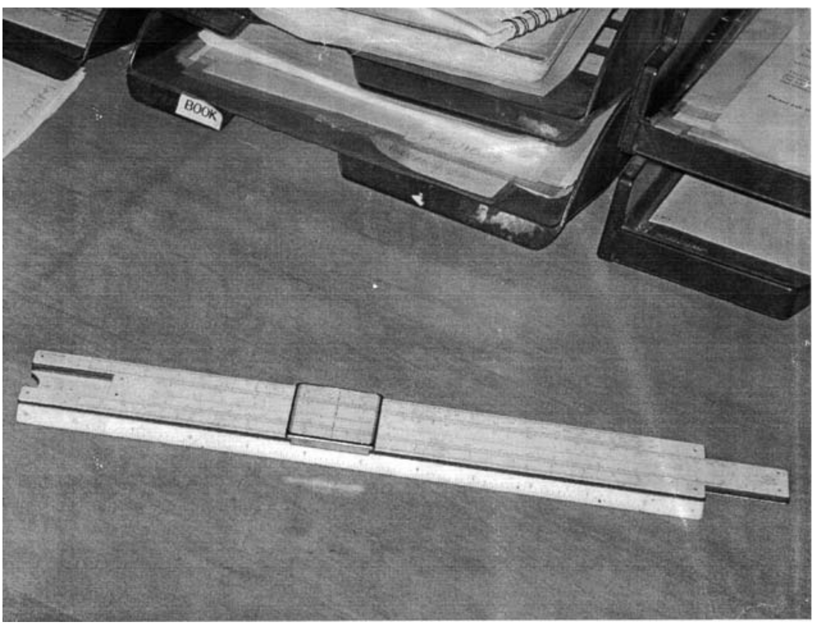

Figure 1.1 Up to less that 50 years ago slide rules would be commonplace on the desktop

The industrial revolution resulted in a rapid demand for information, so it is no surprise that commercial testing laboratories were established to support engineers, one of the earliest being David Kirkaldy in London in 1865. The Kirkaldy Testing Works were in operation until 1965, and there is now the Kirkaldy Testing Museum with several original machines from the works. This includes his hydraulically driven 350-ton machine. They also have a Hounsfield vertical rubber testing machine and a horizontal Hounsfield tensometer which was often used for plastics. Another name from that period well known in polymer circles, Tinius Olsen, is still in business today, and took over the Hounsfield Company.

Much of the early development of mechanical test methods was directed at metals, and in many cases these methods were adapted for use with polymers. This was no doubt the case with tensile machines, but it is not clear when the smaller capacity instruments needed were first introduced, presumably in the early twentieth century. Some early machines were hydraulically powered, but electric motors were in commercial use from about 1880. Right up to the 1970s the usual means of force measurement for polymer machines was the weighted pendulum.

Impact tests can be traced back to the mid-nineteenth century and the history has been chronicled [5]. It seems that early tests used a drop weight machine with usually a product forming the test piece. The notched test piece was introduced by Le Chatalier in 1892 to induce brittle failure. By 1905 the Charpy method had been introduced and the Izod method was from about the same time.

The first hardness tests were very much *ad hoc* and there were very many variations on the same theme. The Brinell test using a steel ball was proposed in 1900 and was the first of widely used and standardised methods for metals. The Vickers test with its pyramid indentor dates from the 1920s. The Rockwell tester invented by Stanley P Rockwell came a little later and introduced the application of a minor load as well as simplifying the procedure.

An account of all the early rubber hardness tests has been written [6]. This gives details of the great variety of instruments that existed and also provides a very useful insight into the development of standard methods. One of the earliest instruments was the Pusey Jones plastimeter, patented in 1912 and still found in an International Standards Organisation (ISO) standard today. The Shore A dates from about 1922 and the Research Association of British Rubber Manufacturers' (RABRM) pattern gauge (dead load) was patented in 1929.

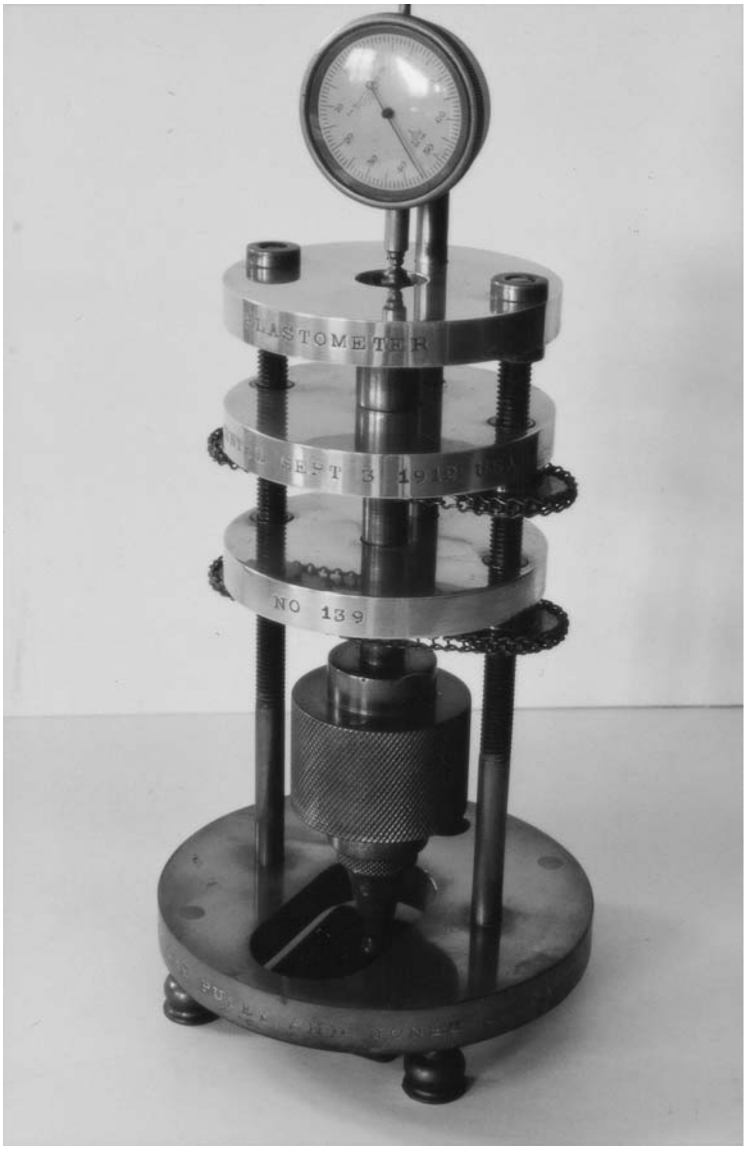

Figure 1.2 The Pusey Jones Apparatus has been in use for almost 100 years

The British Standards Institution (BSI) was founded (as the Engineering Standards Committee) in 1901, whereas the ASTM was formed from the American chapter of the International Association for Testing Materials in 1902. It was, however, a few years before any standards for polymers arrived. Lots of product standards were cited in a 1935 publication [7], mostly from commercial and government sources, but including some from BSI and ASTM. This publication also included a chapter on test methods which demonstrated that virtually all the basic tests as we know them had already been formed. This included tensile stress–strain with dumbbell and ring test pieces, cross breaking strength (flexural strength) using a simple cantilever and three-point loading, Charpy impact and the Akron abrasion tester (although it was not called that). The earliest standards mentioned were dated 1933 and covered electrical properties, gas permeability, impact, cross breaking strength and compressive strength, but most, if not all, were probably for electrical insulating materials.

The 1936 *Vanderbilt Rubber Handbook* [8] also has chapters on tests which includes photographs of several of the apparatus. Notable are the Kelly, New Jersey Zinc Company and the United States Rubber Company Abraders, which have all disappeared. We also learn that a measure of tearing energy was used as early as 1922, whereas the forerunner of the crescent tear test piece was variously known as the Goodrich test, the Winklemann tear or the peanut tear. Another interesting approach to tear was to use elongation of a strip with edge cuts as the measure of resistance. It makes one wonder why edge cut or notched test pieces went out of fashion for a while. A compression shear resistance tester appears to have been important for electric cable, but is another apparatus that has not survived. Several flex cracking testers have gone the same way. Ageing ovens, oxygen and air bombs were already in use, together with a crude accelerated ozone test in which a discharge between zinc plates was achieved with the assistance of 20,000 volts. It advises that care should be taken to keep everyone well away from the high potential side but there is no mention of shielding from the ozone. The prize line must be ‘there is no correlation between artificial ageing and natural ageing’.

The *Newnes Plastics Manual* of 1945 [9] is a superb account of the state of the industry at the time, with pictures of very mechanical-looking processing equipment. The opening chapter gives the author’s opinion on how plastics will be used in all walks of life – one lovely line is ‘it is not beyond the realms of possibility to anticipate the moulding of a bath complete’. It contains a short chapter on testing which unfortunately is very short on reference to standards. Cup flow is the only processability test, whereas state of cure was deduced by immersion in acetone. Mechanical properties were covered by tensile, compression, flexure and impact. For flexural testing (called ‘cross breaking strength’), a simple cantilever or three-point bending were considered interchangeable. In contrast to basic mechanical testing, it is surprising to see dielectric strength and dielectric constant (using a Schering bridge) being listed. They were apparently already standardised as BSS 3.

Apparently, the first polymer hardness standard (and perhaps the first polymer standard) was ASTM D314 in 1931, which was a dead load test for rubbers similar to the British method derived from the RABRM instrument and standardised as BS 2B5 in 1940. The ASTM method used a 3/32 inch-diameter indentor under a 5-lb load and had a presser foot. The BS gauge used the same size indentor but a major load of 565 g and a minor load of 30 g. The earliest date found for the Shore durometer standard is ASTM D676T in 1942. The 'T' stands for tentative and it remained so designated through several revisions. The French and the Germans had standards specifying a 10-mm ball, and a minor load of 50 g and a major load of 1 kg. The Italians, to be different, used a 2-mm flat-ended cylinder as the indentor with various loads.

These standards give a fascinating glimpse of human nature. The Shore method was by far the most popular despite it being much less precise than the dead load methods. Its attractions were its small size and simplicity compared with the heavy and cumbersome dead load instruments. Also, the scale from 0 to 100 for infinitely soft to infinitely hard, respectively, seemed much more logical than the depth of indentation used as the result by the other methods. In Germany, the Shore method was much more commonly used than their own standard, whereas in Britain there was a continuous demand for conversion tables from BS number to Shore. In the USA, the dead load method disappeared completely. It would seem that the dead load method persisted because French and British scientists were convinced of its technical superiority (with the technology of the time). When the BS method was revised in 1950, a cunning move was adopted whereby conversion of the indentation based on the known relationship with modulus and the integrated normal error curve (probit curve) produced a scale that is practically identical to Shore A. In this context, it is interesting that for flexible plastics the use of the same instrument, but with the result expressed in mm, continues as BS 2782 Method 365A.

The situation with hardness tests was broadly similar with other widely used test methods – international contact was such that there was a recognisable basic theme but with very significant national variations. The formation of the International Standards Organsiation (ISO) in 1946 (actually started operations in 1947) was to be the major force in unification and development of the methods to those that we have today. ISO TC 45 for Rubber and Rubber Products was formed in 1947 and first met in 1948 in London. TC 61 for Plastics dates from 1949. Progress has never been particularly rapid in standardisation, understandable because of the number of views to be reconciled and the problem of different languages, and it was 1957 before a batch of rubber methods became ISO Recommendations (they were not called standards at that time). The first were R33 for DuPont abrasion, R34 for tear strength, R36 for adhesion to fabrics, R37 for tensile stress–strain properties and R48 for hardness. The first plastics methods were R62 water absorption and R75 deflection under load. For those who have spotted that R33 for abrasion does not exist as ISO 33, it was discontinued sometime between 1965 and 1979. Incidentally, ISO 1 is for Standard Reference Temperature.

As a small example of the effort that went into distilling the international standards from the various national versions, extensive inter-laboratory testing was carried out in TC 45 to decide on the best form of dumbbells for rubber tensile tests. This was intended to allow optimisation of radii and width of the central portion but, despite many tests, could investigate only a limited number of options and probably missed the best geometry. There was no finite element analysis then to screen for stress concentrations. It would be very interesting to see how the geometries standardised were arrived at but, unfortunately, the results do not seem to have survived.

To complete the example of hardness tests, R48 was the dead load method and essentially a copy of the British standard. However, it was not until 1986 that Shore durometers found their way into a standard for rubbers, although methods for plastics were adopted as R868 much earlier. ISO TC 61 has subsequently standardised Rockwell methods and a ball indentation method for rigid plastics as ISO 2039.

Our period of 50 years (plus or minus a few due to uncertainty) opens approximately from when the first ISO standards appeared. We start with test methods for most physical properties standardised nationally. The exciting process of distilling international standards has begun, although only a handful have been produced. Test equipment for most properties is readily available but it is almost all mechanical; electronic instruments are still to come and the general application of computers is a long way off.

It would be only too easy for the history to become a very long procession of dates of changes and new introductions – on a par with the list of kings and queens of England. To avoid this, a novel approach has been taken. After a chapter covering the 1950s to 1970s, the rest of the book is largely based on a rough framework provided by the editorials in the *Polymer Testing* journal over the second half of the period, which are themselves contemporary comment on the testing scene.

It is one thing to scan existing records and derive a review, as has been done very briefly with the early history above. However, an additional factor with respect to this account is that it is first hand in that the author was actively involved in testing through most of the time period in question, and party to many of the changes that took place. Inevitably, this means that to some degree the account is subjective and others may see some of the developments in a different light.

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2 Consolidation and Change

The period 1950 to the 1970s was when many test methods were rationalised and consolidated into the International Standards Organisation (ISO) form. It was also a period of enormous change, first in the development of test equipment, but perhaps more fundamentally in the change of attitudes and economic climate.

The process of producing the early international standards was dominated by people who had their feet in an earlier era – they had little interest in commercial aspects but developed methods for the betterment of science and the polymer industries. Names that immediately come to mind include Ron Moakes, Alan Eagles, Bob Stiehler, Mme Lamm-Laufer, Jack Mulvey, Bill Farrell and Peter Welford. It was a relatively slower and more gentlemanly world. The first thoughts were for the scientific information they produced and a desire to share it. They believed the results were valuable but did not want to put a price on them.

My own involvement in testing polymers started in 1964 when I joined the Silent Channel Company in Huntingdon (Cambridge, UK). I came from the nuclear industry and probably thought I was going from the new to old technology, but in fact in some ways polymers were more exciting – when you have counted one neutron you have counted them all, but every bit of rubber or plastic threatens to be different.

I first actively participated in standards development in British Standards Institution (BSI) committees in the late 1960s when I moved to the Rubber and Plastics Research Association (RAPRA) in Shropshire, and became a delegate to ISO TC 45 in 1973 and to TC 61 in 1974. This was when the first wind of the industry taking a hard-nosed attitude to standards could be felt. Before then, anyone wanting to join the UK delegation to TC 61 was subject to an interview and consideration by their 'seniors' as to whether they were suitable. RAPRA gave the UK Plastics Committee an ultimatum: they either took me (who was rather younger than normal for a delegate and completely unknown to them) or nobody. In contrast, I had been 'sponsored' for entry into TC 45 by the leader of UK delegation. Nowadays, any volunteer would have their hand bitten off.

Support for the development of test methods and their standardisation continued through the 1970s, but these activities were increasingly being seen as an unnecessary drain on resources. Fortunately, the old attitudes persisted long enough for many advances to be made. Personally, I straddled two camps: my instincts were with the old guard and research for research sake, but I had sufficient experience in the commercial industry to swim with the tide, at least part of the way. At RAPRA, the development of test equipment (increasingly with a view to commercial return from manufacture) and standards were seen as legitimate and worthy activities for a research association, and had substantial budgets – today they are very definitely unwanted overheads. The research departments of large companies such as ICI and BP also devoted large amounts of time to these activities during that period and, in Britain at least, there was a major contribution from military establishments.

It seems that comprehensive accounts in textbook form of the test apparatus, procedures and philosophies were a little slow to come, although there was a review of the development of rubber testing in a history of the rubber industry [1]. The first landmark was when J.R. Scott, the father of rubber testing and Director at RAPRA, produced '*Physical Testing of Rubber*' in 1965 [2]. In plastics, the equivalent is perhaps the '*Handbook of Plastics Test Methods*' by Ives, Mead and Riley in 1971 [3]. They were from Yarsley Testing Laboratories and the book was published for the Plastics Institute. Stan Turner's much more restricted volume on '*Mechanical Testing of Plastics*' appeared in 1973 and was also published for the Plastics Institute [4].

Scott's research at RAPRA started in 1923 and he was Director from 1940 to 1958. His book could be said to be the culmination of all those years of development and evaluation of test methods, as well as encompassing the results of the standards committees. He produced literally hundreds of articles and most of his work is very relevant even today. It was Scott who personally recommended the young Roger Brown to produce a revised version of his book (he still did editing and translation work at RAPRA for many years after his retirement). As an illustration of the thoroughness in those days, before the revision was published in 1979, Scott read the proofs and among comments noted that the spelling of the name of a Russian worker was not the same on two pages a couple of hundred pages apart. That book has gone through three subsequent editions that have continued to present accounts of the methods, test equipment and standards used in the industry [5]. I was also honoured to be appointed editor when the Ives, Mead and Riley classic needed revision in 1981 [6] and again in 1988 [7]. Unfortunately there has not been a subsequent revision.

It is probably a reflection of books on polymer testing being less than plentiful that my book on rubber was translated into French, whereas the one on plastics was produced in German. I find it interesting that the English versions were 'normal' hardbound volumes but the French text was larger format in a loose-leaf binder and the German version was as small as it could be got by printing on what I think of as 'bible paper'. This could be an indication of a national characteristic, but I have not fathomed what it is. There was also a copy of the plastics book produced in Chinese on poor-quality paper and I have yet to see the royalties. If I could read the publisher's name, perhaps I could demand back payment.

The rubber and plastics industries are fairly distinct today but then they were very much more separated. The Silent Channel Company (Huntingdon Rubber Company) was unusual in that they produced polyvinyl chloride (PVC) extrusions as well as a wide range of rubber products. It was not until 1960 that the Research Association of British Rubber Manufacturers (RABRM) incorporated plastics to become RAPRA, and between the first and second editions of the plastics testing book that the Plastics Institute and the Institute of the Rubber Industry amalgamated. Very few people were involved in both industries, and at RAPRA there were separate rubber and plastics departments. However, the physical testing department covered both material sectors, giving me an unusually broad experience.

The publication by RAPRA in 1973 of the '*Guide to Rubber and Plastics Test Equipme*nt' [8] was probably seen as a very significant event in grouping the two camps together. Note the use of 'rubber and plastics' and not 'polymer'. It was not until 1999 that '*Handbook of Polymer Testing*' [9] was published. The Guide was also significant in being the first publication of its kind, the concept having arisen from several requests for advice on apparatus that I received. The novel factor was that it combined technical information (in the form of a commentary on the apparatus requirements) with commercial information (in the form of a directory of suppliers).

By the time Scott wrote his book in 1965, there were only 19 ISO recommendations or draft recommendations for rubber tests, whereas Ives, Mead and Riley [3] could list more than 70 for plastics in 1971. There is no doubt that the plastics committee moved more rapidly, but their total does include quite a lot of methods specific to particular polymer types. By 1979, the number of rubber test methods in ISO had risen to 43 and plastics had topped 100, so virtually all the most important properties were covered. The glaring omissions for rubbers were curemeters (largely because of patent difficulties) and compression stress–strain (for no apparent reason), whereas the method for abrasion was still in draft form.

So, it took about 20 years to move from the first ISO test method standards to there being international methods for virtually all important properties. Interestingly, this is a similar timescale to that taken previously for the development of national test method standards, and can be contrasted with the 100 or more years since vulcanised rubber and then plastics were introduced. In the subsequent 30 years the number of truly new subjects standardised has been relatively modest by comparison.

While the consolidation of test methods into ISO standards was taking place, there were substantial advances in instrumentation. For example, at the start of the period there were no load cells, no computers and no automation, but these had all made their presence felt by 1980. Brown and Scott reviewed developments in rubber testing in 1972 [10] and much of what they wrote is also applicable in principle to plastics. They noted that developments could be said to come from: (i) the work of standards organisations, (ii) increasing fundamental knowledge of the behaviour of the material and (iii) advances in instrumentation. For the work of standards, they gave examples of the rationalisation of the various hardness methods and the diverse types of tensile dumbbell test pieces. As regards fundamental knowledge, they pointed to several studies, from the relationship of hardness to modulus to the fracture mechanics concept of tearing energy, that had influenced test methods. They then queried if more tests should aim at measuring fundamental properties – on the basis that from these it should be possible to forecast performance, or should go in the opposite direction by making simulated service tests on actual products. This question is alive and kicking today, and most people would support the view that there is room and need for both approaches.

Before considering several particular instrumentation developments, the review emphasised that you can distinguish between those developments that: allow new techniques to be used, those that make old techniques more accurate and those make old techniques more convenient or automatic. However, all three may apply to a single instrument. It was also pointed out that more precise and/or convenient apparatus could be more complex and costly. Three important trends that they singled out for more detailed comment were dynamic testing, non-destructive testing and automation.

One of the most important developments was electronic load cells in the 1950s. They revolutionised tensile testing by eliminating the inertia and friction errors of pendulum machines. They had many other advantages –all the advantages except cost. Today, this would be reversed and it would cost more to produce a mechanical pendulum machine – a rather thought-provoking reflection of how manufacturing industry has changed. A commercial Hounsfield E-Type Tensometer ('E' for electronic) was in use at RAPRA in the late 1960s, but pendulum machines figured strongly in the 1973 '*Guide to Test Equipment*', although they had disappeared by the 1979 edition. They were of course still in use in some laboratories for rather longer than this, in fact their existence was acknowledged in some standards into this century. Twenty years seems to be a common cycle in testing because it was about that length of time from the introduction of load cells to them becoming dominant.

Eventually, electronic force-measuring elements became essential components of a variety of apparatus from dynamic analysers to stress relaxometers. In fact, anything where there was a force to be measured. Notably, introduction of direct read-out single-pan balances made the previous contraptions with chains and rider weights (which I remember with no affection) ancient history. Servo-controlled power units brought variable speed control without complicated mechanical gearing, and hydraulic systems allowed cycling up to 100 Hz with high forces. The equipment for making 'mechanical' tests was no longer largely mechanical.

When I entered the industry, it was very common to measure elongation of rubbers and flexible plastics with a ruler or a piece of string. Technicians whose fingers were being rapped by recoiling ends of dumbbells were very pleased to see mechanical clip-on extensometers that recorded automatically become the norm. Early ones were rather crude and some for plastics with small strains had to be removed before break or they broke with the plastic. Designs improved quite quickly, but it was not until the late 1970s that the first optical non-contact devices were commercialised.

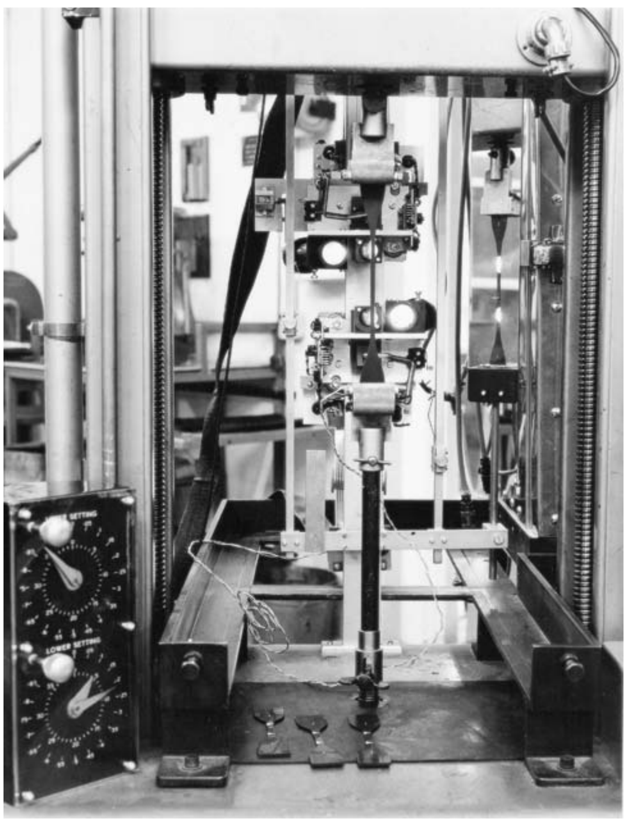


Figure 2.1 Early optical extensometer on an even earlier electronic tensile machine

These developments in stress–strain measuring equipment resulted in improvements on all fronts – several new forms of testing became possible and all the traditional methods achieved much-improved accuracy and convenience.

The early electronic tensile machines produced their output on potentiometric chart recorders (which sometimes part negated the gains in inertia). The first mention of computers in a polymer laboratory were in the late 1960s when there were estimates that a laboratory could spend half its time processing results and that introduction of a computer reduced the number of documents by 32%. The Brown and Scott review advised the advantages of data processing by computer but thought that direct links between machine and computer were justified only in a few cases! Considering the cost of computers then, they were not really wrong.

Without computers it was not simply a matter of using calculators – the only ones available in the 1960s were the size and weight of a modern laser printer and rather expensive. We had them at the atomic energy establishment but slide rules were the norm for rubbers and plastics. I vaguely remember the first portable electronic calculator being bought, probably sometime in the 1970s, by the manager of the Physics Department. The cost was equivalent to buying several PC today.

The are probably people reading this who have never seen a slide rule let alone used one, which is a bit difficult to grasp by those of us who always had one on our desk. The principle of a slide rule is very obvious to anyone who understands the mathematics on which it is based – the scales are logarithmic and you can multiply or divide by adding or subtracting distances on the scales. However, they were not very intuitive for those with little or no mathematical background. Errors were not uncommon and teaching new assistants could be hard work. Incidentally, right up to 1960 the word 'computer' referred only to someone who computes, for example, the user of a slide rule.

In the RAPRA physical test laboratory, we were very advanced and had a Mathatron, which is accredited as being the first desktop or personal computer. This did all the laboratory calculations and was used to directly control prototype completely automatic instruments as early as 1970. However, computerisation proceeded slowly and even by 1979 was not commonly incorporated into test machines. It would be many years before we all had computers on our desks. Development work on the application of personal computers in polymer technology was carried out at RAPRA through the 1970s and 1980s, which I championed in the face of opposition from certain members of the hierarchy who could not see a future in it! One local councillor was reputed to have said in the late 1990s that the Internet was a passing fad!

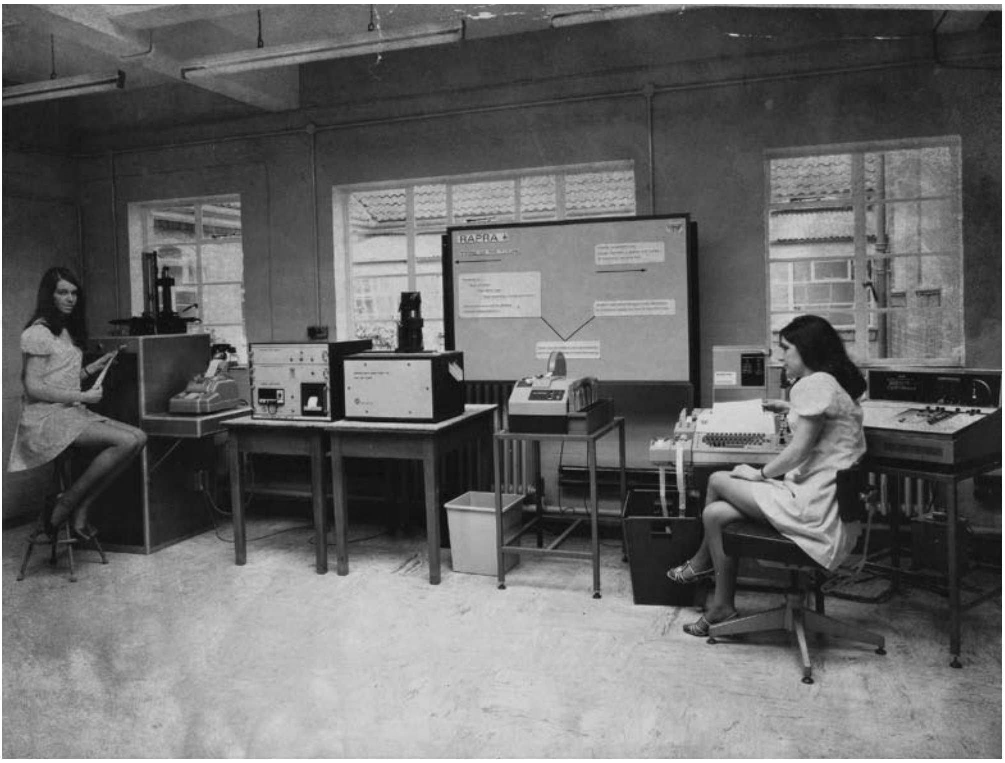


Figure 2.2 First Attempts at Full Automation of Tests being shown at a RAPRA open day (*circa* 1970)

Thermal analysis techniques are much older than one might think, with thermo balances and thermogravimetric analysis (TGA) instruments developed in the early twentieth century. However, the modern era of differential thermal analysis (DTA) instrumentation dates from 1951, and differential scanning calorimetry (DSC) was introduced in 1964. Nevertheless, they were not widely known for some time but, by the late 1970s, the variety of thermal analysis techniques had multiplied and they were being applied to polymers quite widely. As is generally the case with new technology, the speed of acceptance was greatly moderated by the cost.

The measurement of dynamic stress–strain properties was greatly restricted by the cost and complexity of apparatus. Even in the 1950s, the machines were mechanically driven. Servo hydraulic machines were a big advance, but were inevitably very large and expensive. Various instruments were developed in the 1960s, including types of torsion pendulum and electromagnetic drive instruments. However, a much more widespread application of dynamic tests came after the introduction of dynamic mechanical thermal analysis (DMTA), but this was not until the late 1970s. Essentially, DMTA is the marriage of thermal analysis with automated temperature ramp and a miniature dynamic stress–strain device with variable frequency – it takes a while for that level of instrumentation to be sorted out.

Accelerated weathering testing really started with the introduction of Xenon arc apparatus, which arrived in 1954 in the form of the Xenotest 150. One of these machines was still going strong at RAPRA decades later. The more affordable fluorescent tube type of weathering apparatus was introduced a few years later. In principle at least, these introductions allowed speedier evaluation of a material's resistance to weathering, but of course the correlation with natural exposure has eluded us to this day. In consequence, natural exposures are as important as ever they were.

Accelerated ozone testing apparatus in other than a crude uncontrolled form dates from the 1960s. It was very quickly taken up by the car industry in specifications for materials such as door seals. It was found to be more effective than tying the seals in a knot and hanging them on the outside wall. With the cabinets of that time came electrochemical determination of ozone concentration, which was a light years' advance on chemical titration. It could be coincidence, but at about the time of the introduction of ozone cabinets the quality of car door seals in terms of resistance to cracking greatly improved.

Laboratory measurement of curing characteristics of rubber was revolutionised by the introduction of so called 'curemeters', but it is easy to forget that until the 1960s a Mooney viscometer was about as good as it got. At Huntingdon, we controlled production with Mooney viscosity, Wallace plasticity, density and hardness. The first curemeters were the oscillating paddle type, but by the end of the period they had been largely superseded by the oscillating disc instrument. The Wallace–Shawbury Curometer was demonstrated in 1959 at the Salon de la Chimie in Paris. One of its selling points was the simple mechanical construction, as opposed to the Vulkameter that had a proof ring to measure force. Perhaps surprisingly, the rotorless curemeter had also been introduced in the 1970s, but its acceptance was probably retarded by the dominance of the oscillating disc.

For plastics, the melt flow index was widely used and there were several simple flow tests for thermosetting materials. However, even in 1970 more advanced capillary rheometers were not common and were not even mentioned by Ives, Mead and Riley [3] (probably because they considered only standardised methods). Only a couple of years later the '*Guide to Polymer Test Equipment*' [6] featured commercial torque rheometers as well as capillary instruments, and also discussed instrumented extruders.

Impact tests, both falling-weight and pendulum, were introduced very early in the history of plastics, but the '*Handbook of Plastics Test Methods*' in 1971 [3] made no mention of instrumented impact tests, and even in 1979 the '*Guide to Test Equipment*' [5] said that such equipment had so far been restricted to development purposes. Before the 1970s, notches for impact test pieces were cut by hand or by machining on expensive workshop equipment. Eventually, simple notch-cutting devices were developed that could be used in the laboratory, making notched impact testing considerably more convenient.

The need for creep data on plastics was recognised very early and an ASTM standard had been established by 1956 (although it was not very specific about apparatus and procedure). Even earlier than this, strain gauges were used to measure strain with circuits to compensate for fluctuations in temperature, whereas optical lever-type extensometers were in use in the 1960s. Wright introduced the digital Moire fringe extensometer that has long-term stability and records in increments of strain rather than of time in 1971. Even so, the expense of a creep facility and the timescales involved continued to restrict creep testing to larger companies.

The importance of stress relaxation of rubber for sealing applications was also recognised early on, but its measurement presented a challenge to instrument designers. Various devices were developed to allow multiple compression jigs but one measuring head. The earliest was the Lucas beam balance apparatus, and the first load cell device was described in 1969. As with creep of plastics, the cost of apparatus restricted use of the method.

The concept of using stress relaxation in tension as a measure of the ageing of rubbers dates from 1944, but the instrumentation problem was even greater for the intermittently strained procedure. The Wallace–Shawbury age testers were commercially available in 1965 but were unrealistically complicated and expensive – and not very reliable. This could be said to be an example of where the instrumentation of the time was not up to measuring the property wanted.

Ageing by stress relaxation was the first research project I worked on at RAPRA, and it was due to the frustrations of using the age testers that led to proposing a simple modulus measurement using an ordinary tensile machine – which was later included in the international standard. I was also concerned with the development of the RAPRA compression relaxation apparatus. Early examples of the jigs suffered from stretching of the retaining bolts, which caused some embarrassing erroneous results. I recall that one industrialist completely failed to appreciate that the scientific experiment does not always go according to plan.

One of the controversies of the 1970s was the discrepancy that we found between chemical titration and electrochemical methods of measuring ozone concentration. The problem was never fully elucidated, and feelings between the different camps ran higher than for any other testing subject I can remember. For many years we lived with no ISO standard and turned a blind eye to the difference in levels if different laboratories used different methods. It was not until 2000 that the relatively newly introduced ultraviolet (UV) light was standardised as the reference method. It was also around the 1970s that dynamic exposure to ozone was introduced but, even though it is logical for simulating many service circumstances, it has never challenged the traditional static exposures.

An aside that illustrates the genteel atmosphere that generally pervaded research at the time was when I borrowed an ozone cabinet from another department. I borrowed it for an agreed number of weeks when I was short of capacity but at the end of the period they did not ask for it back so I carried on using it. When this was discovered a couple of months later, it was thought that my action was most ungentlemanly – I was used to rather more opportunist behaviour in industry.

The 1970s were when considerable work was done on measurement of the thermal properties of polymers, it having been realised that diffusivity at processing temperatures was an important factor. Published results tended to show a large degree of scatter and were available only for lower temperatures. David Hands at RAPRA was prominent in improving measurements and introduced enclosed apparatus capable of measuring conductivity and diffusivity through the melt phase. It was his modelling of heat transfer that enabled tables of time to equilibrium to be generated for test piece conditioning.

Some of the developments that I was involved in were destined to obscurity. One such effort was an improved form of falling ball resilience apparatus which was intended as a very cheap and simple means of measuring damping behaviour and for following changes due to chemical exposure of rubbers and plastics. It still strikes me as a reasonable suggestion but nobody wanted it.

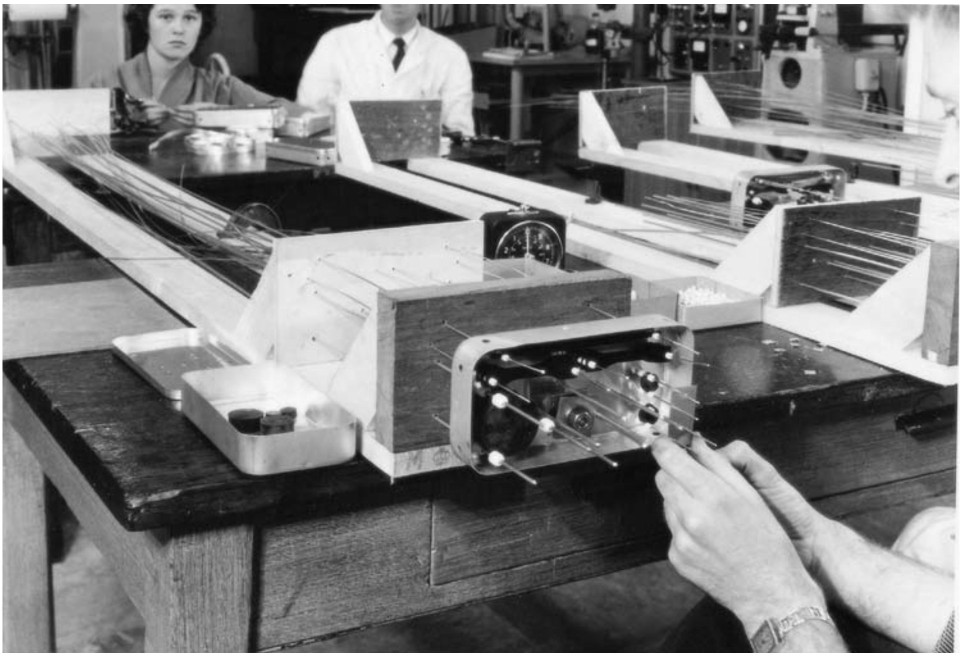
We also demonstrated pulsed ultrasonic methods of measuring dynamic modulus but these never challenged the rise of thermomechanical analysers which could generate data over a wide range of frequency and temperature. The use of ultrasonics, thermography and radiography were investigated for non-destructive testing of polymers with some success, but predictions that such techniques would become widespread in the industry proved incorrect. The expense involved meant that they were viable only in particular high-value or high-volume areas.

It was in the late 1970s that Ivan James developed a clever apparatus for measuring the friction of polymers that overcame problems of alignment of the force-measuring element and applied a normal load while utilising a vertical tensile machine so as to benefit from its drive and loads cells. I remember fighting and losing a battle with the commercial people as to which company would license it. They went for the biggest and the invention was killed off. This was an indication of the significant change in the economic climate that was taking place as previously I, as manager, would have been left to organise licensing without involvement of the powers that be.

Before the 1960s, abrasion tests were essentially confined to rubbers, and mostly to tyres, using several methods that had been specifically developed in the industry. I have forgotten the motivation, but we investigated the Taber and other instruments that had not been previously associated with polymers. Subsequently, methods such as the Taber and Schiefer were used for particular polymeric products, notably artificial sports surfaces. Interestingly, at the same time the use of most of the traditional rubber instruments declined dramatically, presumably because of their limited ability to predict the wear of real tyres.

When I took charge of the RAPRA testing laboratories, I was introduced for the first time to electrical testing. Our Guru was Ron Norman who had built most of the apparatus, but to the rest of us it was rather intimidating. I recall a failure in the power factor and permeability apparatus; when called, Ron advised that the string was probably broken. It was, – a cord drive to a variable capacitor. I am sure that nowadays there is no string and no valves. Incidentally, Ron's party piece was to generate static – by scraping his foot he could put a meter reading to thousands of volts off-scale.

By far the largest research project I was involved with was the RAPRA long-term ageing programme. This was started in 1958 when 33 ventilated boxes of test pieces of each of 19 rubbers and 9 plastics (and some factice-containing rubbers) were exposed at two locations in Australia (Cairns and Cloncurry) and one at Shawbury (Shrewbury, UK). The project was to last for 20 years with samples being tested for 12 properties at intervals. In the event, the time scale was stretched because materials lasted better than expected. At later withdrawal times, accelerated tests were also made on new batches of material, which in itself constituted a huge volume of work. The last samples were removed after 40 years to conclude what could only be called a mammoth and costly programme.



(a)

(b)

Figure 2.3 Loading the long-term ageing test pieces in 1958

Nowadays it is the norm for laboratories to be accredited to internationally agreed quality standards but these ideas were unheard of before 1980. Think of the luxury of no audits by the accreditation body and no pedantic manuals of how you must behave. In fact, we did largely followed the same basic quality procedures but in a totally informal manner. We had written test procedures and technicians had to be trained for each test. I vaguely recall that I had a list of who was trained for what – not really needed because they knew what they had and had not been instructed in, and there was no incentive to cheat. We had set formats for reporting, but they were not written down in manuals and were at best a loose collection of memos. Perhaps largely due to the less hurried pace of working, the incidence of errors was very small.

The need for calibration of equipment was understood but very little was done. It was usual to have the force scales of tensile machines calibrated by a third body (often the manufacturer). Processability instruments and hardness testers were also commonly subject to annual maintenance from their supplier. Much of the rest of the equipment was taken on trust and, to a large extent, simple mechanical construction meant that if it was OK when new it would continue to be OK until it was obviously broken. This was also true of liquid in glass thermometers which were almost the only temperature measuring instrument used. Workers were of course trained to be very observant and pay attention to detail. The benefits of modern bureaucratic accreditation may be highly debatable, but the changes in instrumentation have probably made more rigorous calibration essential because more can go wrong and errors more difficult to spot.

An illustration of technicians being observant and not working as if on a production line was the story of the failed hot water bottles. A customer had returned bottles on more than one occasion and the store company decided to investigate. The bottle for test looked as though it had a long and hard life. However, instead of just cutting the test pieces and making the measurements, the technician had a good look inside and noted that it was as if never used. It transpired that the customer had been filling it with cold water and heating it in the gas oven.

Along with a relaxed attitude to quality systems and calibration, we were not too fussed about laboratory temperature. Certainly, when I started there was no air conditioning but thick walls, few windows and not much electronics to generate heat (ovens in a different room) meant that we were usually somewhere near right. I would guess that it was around 1970 that automatic controls were introduced, although it was much later that humidity control was added in selected areas.

The basic make-up of a physical testing laboratory was the same then as now – rows of benches holding pieces of apparatus with some free-standing equipment. The general appearance would of course be very different because then much of the equipment was obviously mechanical without smart cases; there was an absence of computer monitors and printers and only the occasional chart recorder. Electrical switches, connectors and meters would clearly be from a different era, and Edison would be comfortable with them. Looking more closely, the benches were probably fixed and made of heavy timber with no cable management channels. Even the laboratory stools were made of wood. You could not judge the fashions of the day as everybody would be wearing white cotton lab coats.

Figure 2.4 RAPRA Physical Testing Laboratory in the 1970s

'Statistics' was not a word you expected to hear very often in polymer-testing circles in the 1950s. Indeed, the first '*Handbook of Plastics Test Methods*' [1] had sections on the concept of statistics and I deemed it necessary to include a chapter on the subject in the first rubber-testing book. The unpopularity of statistics was blamed on the subject being severely neglected in schools and universities, but in retrospect the lack of computers for numerical manipulation must have been a large negative factor. Also, the fact that chemists were more prevalent in the industry than physicists or mathematicians could be significant.

ASTM were way ahead of the field in generating precision data from inter-laboratory tests, but it was not until the 1980s that we became aware of the large discrepancies between laboratories that could occur even for the basic well-established tests. We very rarely had any differences with another laboratory and if there was a problem it was always found that the other party had made a mistake. I am sure I found it unthinkable that my laboratory could be wrong.

Communications changed very little over the period and were a far cry from today's facilities. At the beginning we had telephones, telex, pens and a postal service. By the end the only addition was that fax machines were also being introduced and replacing telex. All test reports and research papers were written by hand and typed by a secretary. The process of typing, checking, correcting and posting inevitably meant that it was several days before you got your results. Any diagrams would be hand drawn and, if you wanted photographs, there was the small matter of waiting for the development and printing.

There was of course no World Wide Web and the results of research were disseminated by oral presentation at a conference or by publication in a hard copy scientific journal. Most polymer journals publish some papers on testing topics, but in 1980 Applied Science Publishers launched *Polymer Testing*, a journal dedicated to testing subjects. I cannot remember whether the idea came from George Olley and Alan Singleton of Applied Science Publishers or from me but, between us, a proposed scope was developed and opinion sought from the good and the great of the testing world. The reaction was mixed, with several saying it would stand no chance, but most welcomed it as filling a gap in the market and providing testing with its own 'voice'. I was appointed as the founding editor and, indeed, have been the only editor so far.

From the first issue, *Polymer Testing* has always included editorials which comment on the current scene or changes that were perceived to be taking place. These editorials have been used as the framework for the rest of this book. The subject matter they cover has been sorted into several groups with a chapter containing discussion and comment devoted to each group.

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3 Ron Norman Remembers

Ron Norman was manager of the Physics Department at the Rubber and Plastics Research Association (RAPRA) when I arrived there as deputy head of the Physical Testing Laboratory (which perversely was not in the Physics Department). After he retired, Ron kindly wrote a guest editorial in *Polymer Testing* [1] in which he commented on some of his memories of testing. He started work as an assistant in the physical testing laboratory at the Research Association of British Rubber Manufacturers (RABRM) at the age of 16 in the mid-1930s and was involved in testing throughout a 49-year career which saw five directors and five company secretaries in office – real contact with history does not get much better than that.

The first remarkable thing is the age at which he started work and then set about obtaining a degree, starting with evening classes and later advancing to a full day release. Only someone who has studied for higher qualifications part-time would know how hard that must have been.

Spending his first six weeks doing nothing but abrasion tests (and cleaning the machines – all five of them) does not sound exciting. He told me that at the same time he suffered from all the other assistants being girls (before about 1960 we had laboratory assistants, not technicians). One wonders what all the abrasion tests were for. When I entered the industry, an assistant might spend all day on tensile testing, or hardness, or a processability test, but abrasion was carried out relatively infrequently. Perhaps there was time then to evaluate a wider range of material properties as routine, or it is likely that in those days the Association attracted rather more work from tyre companies.

Taking time to do the job properly is also reflected in Ron's achievement in buffing a sheet of rubber using a non-guided, non-guarded abrasive wheel to 0.05 mm over the length of a dumbbell – very impressive. A non-guarded abrasive wheel would nowadays give the safety officer a fit – but safety officers are a relatively modern invention.

His abilities led to him being entrusted with much of the non-standard testing work and the development of special equipment – which in the war years he carried out between roof spotting, filling sandbags and diving into the dugout shelter. He could continue with calculating results in the shelter, presumably you needed to remember to take a slide rule with you.

There is a general perception that the moral tone has declined over the years but, in the case of manufacturers, it seems that misrepresenting the samples tested in advertising matter and 'tailoring' test pieces to suit the test are not new practices. Ron remembers a customer insisting on a rather unusual and not very relevant ageing test on gloves. All but one sample failed miserably but the customer re-labelled the test pieces in the report so that the only good one appeared to be his and the bad ones were attributed to his competitors. Re-labelling failed samples as your competitor's materials is about as devious as it gets, I experienced more cases of parts of a report being judiciously deleted. Ron mentions different cures of ebonite for impact and yield tests; I remember it being standard practice to cure compression set buttons to within an inch of their life, whereas the product would have the shortest time possible.

The Research Association laboratories were probably quite unusual in that they applied statistical analysis so early on, and it would have been tedious with a mechanical calculator in which you had to wind a wheel forwards and backwards. I have never seen a Brunsviga machine which Ron used, but I imagine it was similar to the Facit produced at the same time. Both companies date from around 1920. The thought of Ron cranking out a multivariate analysis of variance on 100 test pieces seems like the definition of tedium. In the 1960s, I used Facits and (preferably) the electric-powered Marchants when working for the Atomic Energy Authority. An amusement was races between the mechanical and electric machines, and I recall that a really skilled Facit operator would win on some types of calculation.

I think it is significant that he made mention of making things – for example, turning feet for durometers and a friction machine. This was the normal order of things for laboratory assistants up to around the 1960s. On entering the Atomic Energy Authority in 1958 I underwent training that included glassblowing, building a furnace, lathe work and fabricating a tinplate chassis. Ron probably had to pick things up as he went along, but he would then not have the instructor put his beautifully soldered 48-pin Plessey plug put in a vice and yanked to test its strength.

Later, Ron tended to specialise in electrical testing, although he also did friction work and was involved in ageing studies. Continuing his part-time education, he obtained a MSc for his work on the electrical properties of purified natural rubber. He is of course noted for his classic book on conductive rubbers and plastics.

Figure 3.1 Electrical testing with apparatus built by Ron Norman

It is interesting that Ron cites essentially the same things as I considered in Chapter 2 as being the major changes to have taken place in testing, notably load cells, displacement transducers and computers. Perhaps the greatest significance of this is that it probably confirms the feeling I have that things stood still to a large extent from the 1930s through the 1940s and 1950s, before there was a period of great change. No doubt this was largely due to the Second World War and it aftermath.

He cites the large movement of the 'fixed' grip on pendulum tensile machines which, coupled with inertia, made them almost impossible to use with a fluctuating force. A friction measuring machine he built contained a load cell but it gave puzzling results. He must have been one of the earliest to discover that with a very stiff load cell it is not a lot of good if you put it in a frame that could deflect under the forces being measured.

He rightly mentions the invention of the transistor (and later integrated circuits) as being the behind-the-scenes component that revolutionised electronics and, in consequence, testing instrumentation. The first working transistors were in the late 1940s, whereas the first demonstration of a transistor radio was in 1953. However, it was several years before they were commonplace, and they were still considered to be a new introduction when I studied applied physics.

He shared my distaste for the old chemical balances – what would a modern technician make of them? However, when he was writing the editorial it seems that, although much improved and probably with increased productivity by one order of magnitude, balances were not digital. It is also probable that a technician today would be confused if presented with a vernier scale, and would want to know where the digital display had gone.

Ron's predictions for the future were the use of laser metrology, automatic test-piece handling, and improving the meaningfulness and precision of test methods. He was spot-on with the last subject, and lasers have found some applications, but programmable robots are have not ousted the technicians. Like everybody else, he did see the enormous change that has taken place in communications.

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4 The Journal

In many ways, *Polymer Testing* journal can be seen as one of the relatively few things that have changed very little in 30 years. As it said in an issue in 2009 [1]: 'The world may have changed out of all recognition, but all 179 issues of your favourite testing journal have the same familiar coloured cover and, vying for a long service medal, the original editor is still in the chair.'

The basic purpose of providing an international forum focus for polymer-testing interests has not changed. Nor would it appear that there has been noticeable change over the years in the format that is used for writing scientific papers, so the content looks much as it always did. The purpose of launching the journal was to provide an international focus for testing interests, a journal which concentrated on matters of interest to those who test rubbers or plastics, or who are concerned with the methods used to test their own (or their suppliers') products. It was thought that more general polymer technology journals did not do the subject of testing full justice and it deserved its own platform. It was also thought that the practical everyday aspects of testing were equally, if not more, important than learned theory. Whilst being broad in scope, the original concept concentrated on test methods and apparatus, and did not include articles purely reporting measured data. This *raison d'etre* for the journal and its scope was, not surprisingly, spelt out at some length in the very first editorial.

This basic purpose has never changed but some of the aspirations had to be curtailed and the scope broadened considerably. It certainly succeeded in attracting contributions on a wide range of testing subjects. The editorial at the beginning of the second volume [2] noted that there had been 30 topics covered in the first volume, but that this only scratched at the surface of the total testing spectrum. After six years, the great range of topics that had been aired was noted [3]. Some, such as odour permeability and detection of organic anions in latex, were rather unexpected. It was wondered if future increase in use of a test method could be predicted from the subjects being most frequently submitted, but it was thought that this would lead to some unlikely (or even depressing) conclusions. Except, that is, for stress relaxation of rubbers, which looked a certain bet for more widespread use.

Figure 4.1 Many topics have been covered in *Polymer Testing* but not testing golf balls for speed

The one area that was definitely under-represented was chemical analysis, and it was thought there must be some circumstantial reason. Whatever it was, it has disappeared because over the years the number of analysis papers has increased substantially. One possible notion was that chemists did not associate with the word 'testing', and this was suggested in a much later editorial [4], together with the thought that perhaps the journal title should have been *Polymer Testing and Analysis*.

What now seems astonishing is that in the first volume of four issues all the authors were from industry or research establishments – not one from a university. In contrast, the great majority of authors in recent issues are from academia. Although it is probably not the only factor, the main reason must be that the amount of research being undertaken in industry has fallen dramatically. To someone from the commercial world that is very sad.

The main aspiration that failed completely was that the journal should become a forum for discussion of matters of interest to those involved with testing [5]. Letters to the editor and brief communications of news and development failed to materialise. To some degree, announcements of conferences/exhibitions and other news items were made difficult by the practicalities of the lead time of a learned journal but, basically, the testing fraternity proved to be non-communicative. It could be that the notion that laboratory staff would have the interest and the time to interact with their peers belonged to earlier, more easy-going times. Although we used to get a sprinkling of announcements submitted, they have now dried up, presumably because the Internet is a much more effective way of advertising conferences, etc.

In the first few years it was something of a struggle to attract sufficient contributions but by volume 6 the number of issues had increased from four to six per year. It also became apparent that there were many people who thought of testing as being the generation of results rather than just the study of the test methods and apparatus that were used. This led to a rethink of the scope of the journal and the decision was taken [6] to broaden it to include contributions which were primarily reporting valuable test data.

The first reason given was that there was always a grey area before a paper was all results and no test method interest, which made selection difficult. It was also noted that the need for reliable design data had increased and that material property databases were coming to prominence. Furthermore, in many establishments, the same people are concerned with testing and the use of results. At the same time, this decision would doubtless result in a considerable increase in the number of papers submitted.

Over the years, the number of contributions which largely report test data have come to outnumber those specifically on test methods and apparatus. One suspects that there has probably not been an increase or even a decline in 'pure testing' papers, whereas the number of manuscripts in total has risen by a large margin. This may be connected to a reduction in development effort and a lack of time to produce papers by industry. In 1980, one would have expected a steady supply of papers from industry research establishments such as the Rubber and Plastics Research Association (RAPRA), but sadly that is no longer the case.

Very surprisingly, there has never been much input from test equipment manufacturers [7]. It is probable that with a few exceptions they have rather limited resources to produce formal results and generate papers, and consequently they concentrate on marketing through other means such as press releases and editorial copy. Very early in the life of the journal there was an attempt to introduce a 'What's New?' column, and we asked manufacturers to submit accounts of new equipment. There was initially some response, although the request to avoid blatant advertising was not always heeded, and the idea fizzled out quite quickly. I must confess that I do not understand why the manufacturers were not falling over themselves to get free publicity.

The trend of less input from research establishments was apparent by the beginning of 1993 [8]. At the same time it was noted that the inclusion of news items was essentially impractical. A standards news column and a section on new instruments had been tried but abandoned. Apart from the problem of lead time, it seems that failure to report new test equipment was largely due to lack of interest on the part of the instrument manufacturers in supplying appropriate copy. Perhaps it is age and disillusionment rather than acknowledging changes in attitudes, but the editor would not contemplate the possibility of including such material today. Yet, perversely, a major manufacturer responded to the editorial in 2008 to suggest reinstating the column. It would take rather more than one company to back the idea to make me have second thoughts.

It took six years after the broadening of the scope for 'flags' (which indicate the main content or basic purpose of a paper, e.g., 'Test Method or Material Properties') to be introduced [9]. I think they are called 'Doc Heads' in the 'trade'. This move was intended to be for the benefit of readers, it seems they were becoming so short of time they needed help to get to their interests more quickly. I am not sure whether that is a sad reflection on the time available for keeping up with developments or good efficiency.

The different interests of people and their attitudes to testing were discussed in a later editorial [10]. It was argued that a sharp distinction could be made between the two main groups – those interested in the testing process and those concerned about the results. This distinction is clearly very important with respect to the readers of the journal, and appealing to both camps requires a balance of articles to be maintained.

Many people test to get results – technologists, designers, those interested in material properties and behaviour, or those investigating mechanisms. They do not necessarily have an interest in testing *per se* or in the development of the test methods used. The methods and the apparatus are a means to an end. There main interest is in materials and their performance or behaviour.

There are also the people whose concern is to test. This includes those generating data for others (e.g., workers in commercial testing laboratories and in control laboratories). Then there are the test method specialists whose prime interest is in developing new and better test methods, as well as the test apparatus specialists who design and construct test equipment to satisfy the testers. Those whose interests lie in the test methods, the apparatus, and the testing process may care very little about the results obtained other than their accuracy, reproducibility and how well they satisfy the customer's needs. Their interest in the materials tested can be limited to sufficiently understanding the characteristics to apply appropriate methods and to appreciate the meaning and limitations of results. Whether a material passes or fails a specification or reacts in a particular way is not their problem.

It is easy to see many reasons why people are interested in test results but, not for the first time, it was wondered what on earth causes someone to be channelled into specialising in test methods. Put it down to one of life's little mysteries.

After 100 issues there was a rather nostalgic looking back to the first issue [11]. Several of the points discussed above were reiterated, but there were also interesting observations on some of the first topics. First, all the subjects were considered to be still relevant, so nothing had changed there. The principles given in an article on friction had only just been incorporated into an international standard, while the effect of atmospheric pressure on ozone concentration was recognised only in an annex to the International Standards Organisation (ISO) method. This indicated that it can take a long time for ideas to be accepted, and gave the opportunity to say that you saw them in *Polymer Testing* first. It was thought that as a broad generalisation the subjects that get aired in *Polymer Testing* had not changed greatly over 20 years, proving perhaps that material properties do not go out of fashion. That time had moved on was brought home by the fact that some of the authors of the first edition has since died and probably only one had not yet retired.

The topic of 'learning from your mistakes' was raised in an editorial of a 2001 issue [12]. The compilation being undertaken in the UK of a compendium of case studies of environmental failure of polymeric materials made one realise that another subject area conspicuous by its absence was accounts of failures and failure analysis. It is fairly obvious that a reluctance to make failures public would account for the lack of papers. Although commercial sensitivity and legal circumstances were often a valid excuse, that was not always the case, particularly after some period of time. However, it seems unlikely that attitudes will change and failure investigations will largely remain unpublished despite their great value to others.

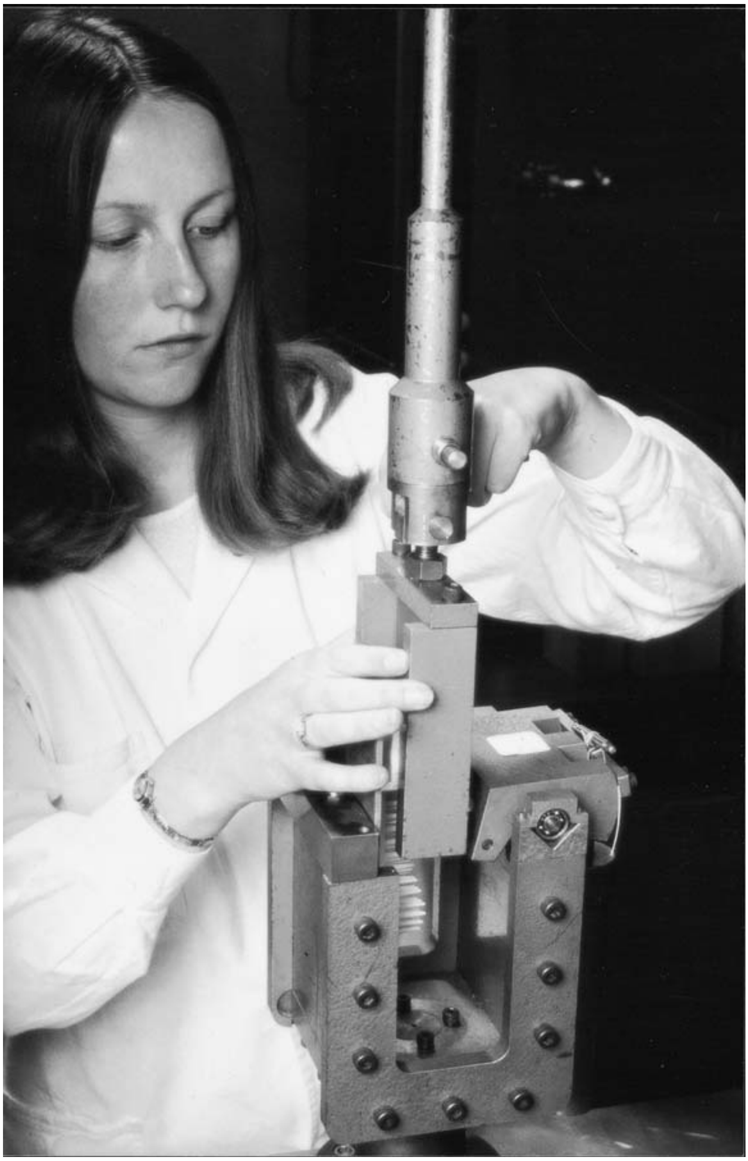

Figure 4.2 The principles given in an article on friction took almost 20 years to be incorporated into a Standard

Because of the undoubted difficulty of getting people to allow their failures to be publicised, it is a remarkable achievement that David Wright compiled his book on failure: '*Failure of Plastics and Rubber Products* which was published in 2001 [13]. Hopefully, it has made a significant contribution to reducing the number of mistakes made.

The subject of the presentation of papers and the criteria for acceptance or rejection was visited in several relatively recent editorials. This was clearly related to the need to reject more of the submissions as the number received increased. Having increased from 4 to 6 issues per year, the journal actually reduced to five at volume 10. It remained at five for several years but the number of pages increased as the effect of the scope change took effect. It then again changed to six issues and then to eight, with larger-format pages from volume 18. Now, there is space for only about 20% of papers submitted, the increase in size from the first issue having reached 725% [14].

In making the fairly obvious points that language is restrictive and that those for whom English is a second language are at a disadvantage submitting to an English-language journal [15], it was said that editing has its limits and some papers are rejected purely because of the standard of English. This has become an increasingly important factor as the editor's time is spread over more papers and there is no chance that reviewers would find time to correct language.

This editor does not like rejecting articles unless they have absolutely no merit, nor does he find correcting language an entertaining pastime. A homily was given [16] on the requirements for submissions in the form of a spoof paper, and the criteria for acceptance/rejection were spelt out on two occasions [17, 18]. Points emphasised in respect of the chances of acceptance, in addition to good English, were that the core of the scope remained test methods and that papers on methods and apparatus were well outnumbered by those reporting material properties. By 2004, only 40 out of 122 papers were flagged as test method/apparatus or equivalent. Hence, it is not exactly high polymer science to work out, given equal technical quality (and perfect English), where the best odds for acceptance lie.

All this editorial interest in presentation does not imply that standards have dropped. In fact, I would suggest that the standard of English from non-English speaking authors has improved and is still improving, even if that from native speakers is not always as good as it should be. However, it is probable that some at least of the improvement is due to greater use of help from professional language editing companies. At the same time, there is a disturbing number of workers who appear not to have been taught how a paper should be laid out. Whether the technical quality of papers has risen or fallen is a moot point. My perception is that the sheer number of papers means that inevitably there will be more poor ones and that there will be many more with relatively trivial content.

One essential aspect of judging a paper is novelty, and this was aired in an issue in 2009 [19]. You might think that novelty was an absolute property, i.e. that the contents of a paper are new or novel or they are not. In practice, things are not quite so simple. First, it is unlikely that all parts of a paper will be novel. There can be new material but an old test method, or new test method but abundant existing results for that property, and so on. Secondly, there is such a vast amount of literature being continually generated covering an enormous range of subjects that it is completely out of the question for one person to know of more than a tiny fraction. Hence, what I think is novel because it is new to me could be 'old hat' to others with closer links to the particular subject. It is even possible to make a good case for regenerating information first obtained many years ago to make it available to a new audience, or because its relevance has changed.

With some subjects, e.g., blend compatibilisation, even someone working in the field would need a photographic memory to recall what combinations of materials, methods and conditions had been reported (if indeed they had access to all the literature and the time to read it). With more uncommon subjects it may be much easier to establish if there is significant novel content, but overall clearly there are problems, with plenty of grey areas, when judging a paper on its novelty value. Perhaps the most interesting point is that many authors do not state clearly what it is about their paper that is new and why it is worthy of publication. It could be arrogance or more likely a lack of training.

With increasingly fierce competition being the order of the day, it is clear that to get your paper selected for publication it has to rise above the crowd. This means the English has to need relatively little correction (or at least be easy to understand), the construction of the paper and development of its theme has to be clear, an appropriate understanding of the subject needs to be demonstrated with no questionable technical statements, and the novelty value promoted with modest vigour. This leads to the subject of peer review which, if you pass the language examination, is how articles are sorted. I liken peer review to democracy – for all its faults it is the best system we have. Hence, it has to be used judiciously and sometimes with a pinch of the proverbial salt.

A language is always evolving and the changes, particularly with respect to new words, must be very significant over the 50 years. It has been said that Britain and the USA are separated by a common language [20].

On the other hand, put another way, they are two countries linked by having two very similar languages. They can generally communicate without much difficulty, but the differences between British English and American English are certainly significant. Leaving aside pronunciation, which is of no consequence for the written language, there are disagreements on grammar and spelling.

It is very understandable that editors and proof-readers can get rather paranoid about the niceties of language – grammar, punctuation and spelling. So, if an editor is English you might expect that he or she would carefully eliminate any Americanisms in submitted papers (and for an American editor to do the same in reverse). Observant readers of *Polymer Testing* will have noticed that we are not too fussy on that score, generally taking the line that life is too short to bother about gauge and gage and, anyway, we have readers who subscribe to both.

Britons and Americans vote along party lines but what about the others? Generally, it is more likely that Europeans, especially older ones, will use British English, as would those from former British colonies, whereas South Americans, Japanese and Chinese, for example, have probably learnt from American books. As we receive papers from most countries in the world, we get a good mixture of English spelling. We even have submissions with a mixture of English and American in the same paper. However, it is notable that differences in grammar and the meaning of words, as opposed to differences in spelling, are rarely an issue in submitted papers.

Differences are not restricted to English and American. For example, in recent years Europeans have had more widespread need to use English and sometimes introduce new words and use existing words differently (for example, they are very keen on the verb ‘to elaborate’). With time perhaps some of these will stick, and serve to illustrate that a language continues to develop.

Where we are less easy-going is in the use of SI units. I admit to ordering wood at the builders merchant with the section in inches and the length in metres (we do not like meters), we do not do pounds and fahrenheit if we can possibly help it.

The bottom line is that the technical content of a paper is more important than whether you write colour or color. However, the best technical paper is of little use if the English is incomprehensible, so we have to be fussy about how words are strung together.

Whatever the future brings, it is almost guaranteed that there will always be a need to exchange information, so journals in some shape or form will continue. Whether they would still need an editor is not likely to bother me in another 30 years.

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5 Computers and Communication

Computers have had an enormous impact on the automation of test equipment and, consequently, on test procedures. However, the other areas where they have caused little short of a revolution, together with the need for new training and new thinking, are in data handling and communications.

When *Polymer Testing* journal was launched, we wrote all our reports and articles longhand which were then typed up by a secretary, who by then probably did have an electric rather than a manual machine. For anyone too young to remember, it must be difficult to appreciate the painful process of producing a document when the only copies were carbons and typing a wrong letter could only be dealt with by painting on correction fluid and retyping.

Going back to the 1960s and before, the exchange of test reports and data was generally by the good old-fashioned postal service. Messages could be sent by telex (which was initiated as early as 1935 in Europe and lingers on until the present day) but I do not remember it being used much for testing matters. Fax became viable in the 1970s and its use became widespread quite quickly (incidentally, it was first popular in Japan because with the technology of the time it was faster to write the language characters than to type them). For many years, we used fax for all quotations and much of the correspondence about testing jobs, as well as using it to send test reports as quickly as possible after completion. It may appear to be a somewhat clumsy process, but it was remarkably effective. Although now largely superseded by the Internet, fax still has its uses, for example, if electronic signatures are not accepted, and it can take less time that scanning and emailing.

Very early in testing history there would not even have been the telephone for communication, although senior people would expect to have one on their desk by the time I started work. It was not a direct line of course and outside calls went through an exchange. In many establishments you had two phones, one internal and one external, or if of insufficient rank only the internal one. Also, we have now largely forgotten that before the 1960s you did not press buttons ('touch tone') but dialled ('pulse dialling') to make a call.

A phone conversation is sometimes essential to get understanding between people on details of test requirements or the meaning of results but, as a generality, the phone is a great device for disturbing people and wasting time. The early mobile phones were coveted but the only real use was for people doing on-site testing. Now they are almost an addiction but I remain sceptical of their contribution to efficiency. Telephone conferencing has great promise as an alternative to physical meetings but has been a long time becoming commonplace. At least 15 years ago it was used effectively by some solicitors for discussing the input of expert witnesses and test results, and one can imagine it would be very useful to discuss details of test methods or the appearance of samples. Perhaps conferencing will grow as Internet Protocol (IP) telephony gains more ground. I have recently experienced the convenience and economy of using Skype to discuss test equipment with people on another continent.

During the 1980s, the use of personal computers (PC) for business tasks increased at an incredible rate. This followed the introduction of the IBM PC in 1981, and WordStar word-processing software. My memory is that the secretaries had WordStar but the first software in the laboratory was a DOS programme called Open Access. We migrated to Microsoft Works when Windows 3.1 arrived (Word was too expensive). Steve Hawley tells me that the Open Access company did not think it worth producing a version for Windows!

In 1989, an editorial [1] probably exaggerated a little in saying that technicians were now so used to a keyboard that they rivalled traditional typists. However, PC were then commonplace in the laboratory and the point had been reached where most people could produce their own reports, even if they could not all have their own individual computer. The time saving in the speed of writing and not needing to interpret handwritten scrawl was considerable, and would lead to the demise of the typing pool.

A point made in the editorial was that this great change had happened in almost all cases without formal training. It was also a time when the education system was being accused of falling literacy standards, and when language skills in new technicians and graduates could not be taken for granted. Spell-checking software was a saviour for many. For those used to the modern trend of being sent on a training course to learn how to switch on a computer, what happened then must be difficult to believe. People simply learnt as they went along and shared the information.

At this time, my wife acquired an Amstrad PCW for word processing at home (IBM PC were a bit expensive). It may seem odd that, although championing the use of computers in the laboratory, I did not have my own PC until late 1993. The explanation is simply that I had the luxury of a truly professional secretary, to type my reports, and technical staff to run the statistics software. The realisation that being computer-literate would become essential was eventually prompted by no longer being able to have my abstracts profile on cards for manual filing. I was able to progress quickly because of having expert help to hand and the advantage of having learnt to type properly in my youth.

Do-it-yourself (DIY) publishing was in its very early days and was something I very quickly latched onto for newsletters and adverts. I still have desktop publishing files to do with the Rubber and Plastics Research Association (RAPRA) Testing Club from January 1994, although the first editorial to be typed by me was for an issue in 1994 [2], a few months later. We graduated to a PC at home at the same time and since have become a computer-centric household. I installed a wired network in 1998 and at the last count there were twelve PC scattered about the place.

Inevitably, there were problems with the adoption of word-processing other than the literacy of the operators. It was only too easy for mistakes to be made from the use of templates and shell documents to automate report generation. Consequently, the laboratory accreditation authority quickly brought in stringent systems to help avoid such errors [3].

Like most such rules, they appear for most situations to have taken bureaucracy rather too far, so that one could have a whole set of written procedures for what is a fairly simple, commonsense process. Nevertheless the essential reasoning is valid in that it is only too easy to make mistakes if new reports are produced by editing an old one. This could be crucial in a laboratory mass-producing test certificates by an almost automatic process, and one assumes that it is this sort of situation which prompted the regulations.

Once computers had taken a hold, the rate of change was faster than anything seen before, which inevitably had considerable financial implications [4] and also an ongoing need for training. In the field of computers, it is a matter of 'tomorrow went yesterday'. The rate of development is such that what was state-of-the-art two years ago is now definitely dated. Between 1993 and 1997, the hard-disk capacity of my computer went from 200 Mb to 2 Gb while the processor speed increased from 33 MHz to 160 MHz. Sometime in that the period the CD-ROM drive became affordable so that large numbers of floppy discs were not needed for the latest software. There was a sort of 'chicken and egg' situation of improvements in hardware, allowing more demanding software or advances in software necessitating increased hardware performance. In the RAPRA Physical Testing laboratories, I adopted a policy of buying a new computer each year and cycling the machines down a ladder of less demanding jobs, with monitoring oven temperatures at the bottom.

A couple of years later [5] the self inflicted change of getting a new computer prompted more comment on the costs of upgrading. They seemed to be a larger drain on the laboratory finances than test equipment because of the frequency of upgrading and the numbers involved – and that was before the cost in lost opportunity time because of the hassle involved was added.

The hassle of changing computer proved on that occasion to be very real. The new operating system needed getting used to, the peripherals had to be swopped over, and there was the small matter of transferring all the data. This was a bit tedious and time-consuming but perfectly tolerable until the problems started. Par for the course, some software would not work on the new system, one machine was not appropriately set up, and re-configuring the Internet connections did considerable damage to the nervous system and needed several calls to a friendly technician. Quite apart from the hassle, the episode was estimated to be worth more than the price of the machines in lost opportunity time. Computers are meant to save money and increase efficiency.

The cost of computers to a laboratory was illustrated by a quick calculation. If you have to upgrade 20 machines, which is about the headcount of a medium-sized laboratory, add on the printer, modem and the latest software upgrades, the cost then was ~£40,000 plus the time. That would buy a very nice tensile machine and extensometer with plenty of change and time to generate a stack of data. What did we do with this capital before computers? This was enough to tentatively query if the cost/benefit ratio of computing was favourable.

Those times were recalled in 2009 [6] to note that computing was now less newsworthy and to reflect on how the cost of computing had changed. An estimate was that the cost of a PC had dropped by a factor of about four since the early 1990s, whereas the cost of laptops had fallen even more. The introduction of netbooks has made the cost of being equipped to work while travelling approach a trivial level, not to mention being a very welcome drop in weight for ageing technologists.

In general, there has been a drop in the cost of communications. Access to the Internet for the individual has been a matter of more and more (or faster and faster) for less and less – although there may be some debate as to whether the costs are sustainable. All laboratory computers being connected to a network with routine access to the outside world is normal. The main topic worthy of comment is how to maintain the same level of connection when you are stuck in a tunnel with nothing but a phone.

In 1980 you might have heard of a cellphone but you certainly did not have one. The rise in popularity of the mobile phone was parallel in time to that of the PC, except the phone achieved almost universal penetration. Like Internet connections, you now get more for less money, although many would say the costs are still too high. The cost of making landline telephone calls has also progressively reduced over the last decade and with voice-over-Internet techniques they can be made for no added cost. The means of exchanging data has never been better or cheaper.

The Internet Protocol Suite (commonly termed TCP/IP) for interchange of information was in use from 1983, but it was not until the early 1990s that the Internet became available to the public at large. The growth in popularity was to a considerable extent due to the implementation of the World Wide Web (www) and the Mosaic browser. It then developed very rapidly. In 1994 *Internet* magazine claimed there were 400 websites worldwide and they had looked at them all. In 1996, the estimate was 100,000, whereas a couple of years later it was in the millions.

Although I was late in having my own computer, I accessed the Internet relatively early, first connecting in mid-1995 and having a website very soon afterwards. This was ahead of the millions and well before having access at work. At the beginning of 1996 [7] it was commented that friends, the journal publishers, universities and anyone to do with computers could be contacted by email but not most of those that I dealt with in the polymer industry. Clearly, industry was slow jumping on the bandwagon of the moment and adopting the new technology.

Efforts to find anything relevant to polymers on the internet met with failure. From the beginning, the information available was largely entertainment in various guises and for commercial purposes. Science is but one small sector of interest and polymers a tiny fraction of that. At that time there were not the sophisticated search engines we have today, so it was commented 'There are vast numbers of web sites and multitudes of discussion groups and no directory. A lifetime could be spent surfing around great oceans of trivia with only a slim chance of happening upon anything of any substance. You could lurk around news groups for years without having a decent discussion on gripping foam test pieces. It can all take a lot of time, be most unrewarding and runs up a large telephone bill.'

One of few things of value I found was the website of the academic publishers Elsevier. You could find all about *Polymer Testing* journal and all their other publications. It is interesting that the address was *www.elsevier.nl* rather than *www.elsevier.com*. I included my first email address in the editorial, *ruyton@cityscape.co.uk*, in the hope of being sent information but my memory says that none materialised.

Despite this, it was confidently forecast that the Internet bandwagon would not stop rolling and, like the developments in computers, was moving at an exponential rate. It was expected that the Internet would become a way of life just like the telephone, and that testing polymers would not be left out. I predicted that before too long you would be able put out a call for who knows where to buy a closed cell content apparatus, scan equipment catalogues on line, download material selection software and distribute reports of meetings with one press of a button: nice to be proved right occasionally.

The management of computers and communications received attention on several occasions. The paperless office never did materialise although by 1999 the electronic copy of documents was often considered as the prime record [8]. A telling snippet from that editorial was that articles to *Polymer Testing* could be submitted on a floppy disc or by email, but in the latter case large pictures could not be sent because of the download time. I have never been in a paperless office and an excuse made then is probably still true – I file by project on paper and by subject on computer.

One downside of all the technological innovation is that it can go wrong. In particular, computer networks were singled out for giving trouble [9]. Very advantageous (if not indispensable) they might be but they are also very easy to upset. The example was given of when my simple home network took offence at a new printer and became selective as to which machines could talk to each other. Insult was added by messages directing one to consult the network administrator, a role nobody in the family wanted. Networks can still have bouts of acting mysteriously today.

It is the same with sophisticated modern test equipment. Once upon a time if a spring broke, as they inevitably will, you fixed it. Now, when a piece of electronics inside a black box gives up, as they inevitably will, you have to call a black-box doctor whose favourite cure is to replace the entire box.

It was noted that, although relevant polymer information had increased on the internet, networking among polymer people had not really got going. The internet as something you can interact with was still a little way off. It was thought that perhaps there is a parallel between the human aspects of networking and the technological aspects in that it is a great idea when it works, but the problems can get in the way. In the case of human aspects, there were probably a lot of systems and protocols to be worked out before we learnt to use the technology most effectively. Probably true, as was the prophesy that even then, it will not work all of the time.

An editor is particularly exposed to viruses because of getting mail from all corners of the earth, and even some that seems like it came from a different world. A few years back infected files from contributors were received most months, and this prompted an editorial homily [10] telling people to get up-to-date antivirus software. Universities and industry have since put their house in better order in this respect.

The concept of the home office was not really viable until computers and Internet links were commonplace, but by 2000 there was a substantial and growing body of people who worked from home [11]. I had just become one of them having retired from full-time working, and waxed lyrical about the pleasures of not travelling every day to an office or laboratory. A whole new market for office products had opened up and the suppliers were not slow to exploit it.

Home working for desk-based staff is one thing but if anything practical is involved it looked decidedly unworkable. The possibility of remote testing of polymers was mooted and considered to be not so far-fetched and technically feasible – but not likely to happen soon.

I now find it difficult to remember how I edited articles before they were submitted electronically. Before 1994 they would all have come as hard copy which was marked up with changes required/suggested and posted back for correction. The corrected copy with any further marking up was posted to the publisher where it was typed up again: sounds unbelievable.

Electronic copy on a floppy disc would have been encouraged as soon as I had a computer but it was not until 1999 that we insisted on text (but not figures) in electronic form [12]. With only dial-up Internet connections that rose eventually to 56 kbps (having started at 14.4 kbps), attaching figures to email was not very practical and a floppy disc was soon filled. It is interesting that hard copy as well as electronic text was wanted because detailed editing was considered easier on paper. I would still agree with that but we have had to learn to edit on screen. There was another reason in that occasionally there could be problems with fonts or layout not being reproduced properly on the recipient's computer. Fonts from the Far East continued to give problems for many years and it was only quite recently that SimSun was installed on a new computer.

A couple of years later many homes still did not have a broadband connection [13] whereas much of industry and universities would have fast lines. Hence, authors were again reminded not to send large files. We were starting to solicit for figures to be sent electronically and suggested that the obvious way was to use CD. Curiously, not many people seemed to have that facility so, presumably, industry lagged in getting CD writers. An alternative suggested was a Zip disk – believe it or not I still have a Zip drive that works.

Finally, in 2004, broadband reached my backwoods home office which prompted a resumé of the changes in the submission and processing of articles [14]. Thereafter, hard copy was sent less and less often, although we still liked to have it in case of glitches in the electronic version. You have to stop and think that this was only five years ago. The next editorial [15] returned to the problem of when technology goes wrong, recognising the clear trend that as technology got more complicated so it became more difficult for the amateur to understand what was wrong and how to correct it. However, by then we had learnt the universal correction method for electronics – turn it off and start again.

After about three years of receiving most articles as email attachments over a broadband link, *Polymer Testing* dragged itself right up-to-date and went over to online submission of articles [16]. In one sense this is the same as using an email attachment because the transfer is entirely electronic, but the difference is that you upload the files to a website instead of sending an email. This is a good example of how the internet has developed to be an interactive medium. The editor and reviewers can access the articles, revised files can be uploaded, and the authors advised of progress. When the process is complete the article is available for publishing.

I had been wary of this online submission system being too complex for a small editorial office. In the event, the Elsevier Editorial System (EES) has been very successful and resulted in considerable improvements in efficiency for the editor. The whole process is light years in advance of the procedures when the journal first started – although it has failed to convert me to a totally paperless office. The change must have had a big effect on me because in an issue in 2007 [17], I recounted a nightmare of how a worst-case scenario of article submission might have been.

In this dream, all articles were submitted by telephone. Now, some people love the telephone but my usage of this most intrusive of technologies is near zero; I work almost exclusively in email and get perhaps three *Polymer Testing* related telephone calls per year. Anyway, in the dream it was like a call centre, I sat with headset and typed in all the details such as authors, title, key words, etc. The text was recorded on tape and the figures and tables appeared and were somehow pasted into the tape. Perhaps a computer-based fax could have been involved, but the details were a bit vague. Anyway, dreams do not have to obey boring mechanistically realistic rules. This process was getting me increasingly agitated, so I woke up.

Perhaps the point of the dream, like all good nightmares, was to show me one version of a personal hell. It did show that the telephone, although I concede that it has its uses, can be the least effective communication method. Certainly, even allowing for some magical manipulation of data, it was a far less efficient method of article submission than the good old snail-mail process of years ago because I was chained to the phone waiting for authors to call. It also gave the reminder that most telephone calls could be done by text or email without interrupting anyone.

After waking up the first thing I did was to check that the EES was still there and, would you believe, it was down for maintenance.

Polymer Testing is still produced as a traditional hard-copy publication. However, if you have a subscription to Science Direct it is also available in electronic form at *www.sciencedirect.com*. Articles in pdf format can be downloaded or the article viewed complete with direct links to some of the references. Also, articles in press can be previewed. None of that would have been contemplated in 1980.

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6 Conferences

Traditionally, journals and conferences were how technical information was disseminated, plus of course sponsored, private work in technical reports. Very frequently, the communications given at conferences also appeared in hard copy as preprints and/or were subsequently published in Proceedings or in journals. Nowadays, publication on the Internet gives a third means of dissemination.

It is obvious that the essential difference between conferences and journals is that in the former you meet and can interact with the authors of communications and all the other delegates. This potentially offers considerable added value to the information presented. If the conference is held in conjunction with an exhibition there is additionally the opportunity to learn of new developments in instrumentation and have demonstrations of equipment.

On the face of it, conferences have clear advantages over the simple printed pages of a journal. However, there is one large snag – expense. Not only do you have to pay to attend a conference, but the cost in time is enormous and the efficiency level is likely to be very poor if only a few of the communications are really important to you. Nevertheless, when I entered the industry, major conferences were considered important because of the opportunities for human interaction, and time was always found to attend the big ones, particularly if associated with an exhibition. These were not of course specifically for testing so usually only a few communications were really relevant.

In 1972 the Institute of Physics and the Materials Science Club in cooperation with the Plastics Institute and the Institution of the Rubber Industry decided to hold a conference on the '*Testing of Polymers for Service*'. I was dragged along to the initial organising meeting by my boss, Jim Berry, and to my horror was designated chief cook and bottle-washer. I no longer remember much of the details but, despite having absolutely no previous experience, put together a two-day programme and organised for it to be held at the Palace Hotel in Buxton (Derbyshire, UK). I have no idea why Buxton was chosen but can remember that as organiser I was given a superior room, which did not amuse Jim. There were some prestigious speakers and the subject matter was varied, but it demonstrated the problem with conferences in that they can only cover a small sector of a broad subject or, as in this case, have papers spread thinly over the field. It was well attended, probably because the great and the good of the sponsoring organisations turned out.

Figure 6.1 Conferences are for meeting people – the one and only Madame Lamm-Laufer

I do not think there was another testing conference in the UK until 1984 when the National Physical Laboratory (NPL) took it upon themselves to organise one on 'Measurement Techniques for Polymeric Solids'. This time I got the honour of giving the introductory paper and, with Brian Read, edited the book of papers, which were also published in *Polymer Testing* journal. With such a wide scope this was again just scratching a few lines in the testing spectrum. I tried to be too clever with my presentation but what went wrong does serve to illustrate the visual aids we had then. There were no overhead projectors and no projectors attached to computers. Each message or picture had to be rendered onto a slide by the Photographic Department. I decided to intersperse some 16-mm film clips with slides (no such thing as video). At such a large conference, the slides and film were manipulated by a projectionist in a soundproof booth – who failed to get the required coordination between stills and movie and there were embarrassing blank periods.

When overhead projectors first appeared they were hand-drawn and the 'poor relation' used only at small seminars attracting just a few enthusiasts for a given subject. It was not until they could be produced on a computer that they superseded slides completely. They were of course far easier and cheaper to produce than slides, which resulted in the amount of visual aids used in a typical talk to increase enormously. It also became viable to quickly include new test results into a paper. Another advantage of the computer software was the production of notes pages which I think had a big effect in reducing the number of speakers who read their paper verbatim from a typed script. All in all, they were instrumental in greatly improving the standard of technical presentations. This gives rise to asking why was reading a paper the original normal procedure? Perhaps the written text was considered the definitive version not to be tampered with in the name of making the presentation more interesting. Certainly, if you had been issued with a preprint then a straight reading became especially tedious.

Relatively speaking, overhead projectors were in vogue only for a short time because projectors taking input from a computer were introduced. They were initially very expensive and not available at all venues, but are now universal. The effects that can be realised have become very sophisticated, but the basic process for generating the display material has not changed since the first computer overhead projectors, which were themselves copies of slides. The effects include having video clips so you do not get problems of less-than-bright projectionists.

One purpose of an editorial after the programme for the NPL event had been outlined in *Polymer Testing* [1] was to ask if readers would like there to be more similar conferences. I do not recall any replies. Leading up to this question, it was noted that attendances at conferences had dropped off in recent years and some had to be cancelled. It was argued that if a journal specific to testing was desirable so were dedicated conferences because that would eliminate the problem of only one or two presentations being relevant. There was also a plea for more general conferences to have a testing session to avoid relevant presentations being scattered over several days – which seems obvious enough.

Nothing much was heard about testing and conferences for more than ten years until in 1995 when '*Polymer Testing 95*' organised by European Plastics News and the Rubber and Plastics Research Association (RAPRA) was announced. It was suggested [2], rather pessimistically, that the lack of conferences devoted to testing was because the subject was not thought sufficiently glamorous and testing staff were expected to stay 'chained to the bench'. Perhaps the pessimism was justified because the conference was switched from Switzerland to the UK because of lack of support, and in the event only 64 people attended [3]. This was despite there being a well-organised programme with five sessions on important and topical subjects.

The editorial argued that any laboratory chief inclined to restrict attendance of staff at such events should read the keynote paper. This stressed the importance of testing and the many people who rely heavily on the millions of results produced. The answers testing produces have to be the right answers and to continue getting the answers right necessitates reading and hearing the latest developments.

The organisers were not deterred because it was followed by '*Polymer Testing 96*' and '*Polymer Testing 97*', with the latter having the format of five part-day seminars. This was rather like buses: none for 11 years and then three in a row. However, it would seem that three was sufficient because there do not appear to have been any repeats in subsequent years. Presumably, there was simply not enough support. I suppose I am biased and too close to the subject, but I still cannot understand why there is such a poor turnout for a testing event.

One notable exhibition with a general testing interest held in the 1990s was '*MTQ Testing for Quality*'. RAPRA exhibited there for several years and later at '*Materials Testing 2000*'. On one occasion we got a lot of publicity from featuring a mannequin that was stolen at the end of the show, but generally it was not the most exciting of events. I should say that a glamorous mannequin was not gratuitous because we had tested several of them for strength of arm attachment. '*MTQ 98*' was mentioned in an editorial [4] partly because it was the venue for the first public airing of the '*RAPRA Testing Knowledge Base*' but also to comment again on how few exhibitions and conferences there were on testing.

The Materials Testing exhibitions still exist, with the 2009 event taking place at a hotel in Blackpool (UK) – not quite the size of the National Exhibition Centre. The general testing interest, and particularly attractions for polymer people, seems to have dwindled and the show has gone back to its non-destructive testing roots. Despite the obvious advantage of being able to meet with people in the same field and see the latest equipment, testing conferences and exhibitions for polymers just do not seem to work. The editorial saw one positive point – not least of the alternatives is the printed word which, despite having been about for a long time, is still an extremely convenient and cost-effective way of presenting accounts of new developments. Unlike the exhibitions, *Polymer Testing* appears several times a year and you do not have to travel to see it.

The big polymer exhibitions and conferences do of course feature testing but it is not the main attraction. Even something as big as the '*International Rubber Conference*' *(IRC)*, does not necessarily attract the very large numbers of delegates that could have been expected a few years ago. At IRC 2001 in the UK there were estimated to be only about 200 people and the same the previous year when it was in Finland [5]. In comparison, it was reported months before the 2001 '*International Conference on*

Composites Engineering (ICCE)' that more than 500 presentations had already been submitted. Doubtless, the difference reflects the relative numbers of people active in these fields, the relative prosperity of the industries and, possibly, the fact that *ICCE/8* was held in Tenerife (Spain).

It was hinted in that editorial that perhaps when *Polymer Testing* on the World Wide Web (www) becomes interactive we can have a mini virtual conference every few weeks. With the way the Internet and telephony have been developing, this is increasingly not a daft idea.

I gave presentations at and/or manned an exhibition stand at several of the large rubber and plastics conferences over the years, including American Chemical Society (ACS) meetings in the USA. Both activities bring a rush of adrenalin that you do not get from ill-treating test pieces in the laboratory. Rather less exciting was traipsing round the country giving talks at regional polymer institute evening meetings, although the response was usually rewarding. Regional meetings still take place but I do not think testing appears on the bill very often – whether this is because nobody is interested or there is nobody to give the talks I am not too sure.

The most impressive conference at which I gave a presentation was not on polymers *per se* but on sport – '*Sport and People*' in 1982 at Wembley Stadium (London, UK). My subject was testing artificial sports surfaces and it was quite an experience, both from the size of the stage and audience, and from being mixed in with some of the household names from sport and television. The conference was opened by Dick Jeeps, chairman of the Sports Council, and the inaugural address given by Neil Macfarlane, the parliamentary Under Secretary of State. Chairmen of sessions included Sir Stanley Rous and Ron Pickering. Fellow speakers were Emlyn Jones and Jimmy Hill, while the exhibition was opened by Sebastian Coe. Testing plastic bottles or car door seals never got that treatment.

A facet of many conferences was being given some sort of memento, delegates usually got bags and speakers might be presented with a framed certificate. A certificate is a nice gesture but in all truth it is not a lot of use and soon gets consigned to the attic. There could be a study into the variety of shapes and sizes and construction of bags dished out at conferences. A few of the ones I collected have proved very useful and continued to see active service for many years. However, if you go to a lot of conferences you will collect a heap that have limited application and have to be cleared out at regular intervals.

The international standards meetings have been very strong on giving bags and little presents to delegates, although this is a custom which is probably now dying out. It cannot be very easy to come up with something original, relatively cheap and universally useful and, judging from some of the things I have collected, ending the practice is not a bad thing. A log of rubber wood was original and a talking point for some time, although not too convenient to carry home. More difficult was a concrete wall plaque (depicting a couple of saints I think), made worse by my wife also receiving one. I confess, we dumped one in a bin at the airport but the other is still in our garden. A metal bowl and wooden nesting boxes have retained a place on a windowsill but most of the other gifts have been forgotten. However, in this respect also, '*Sport and People*' proved a cut above the rest with a very nice inscribed decanter.

References

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7 Standards

The purpose of standard test methods is to enable everybody to do things in the same way, which avoids confusion and allows results to be strictly compared and for experiments to be reproduced. Members of standards committees have spent many hours developing standard procedures and negotiating to compromise on differences until we have many widely accepted international standards published.

An early editorial [1] was something of a plea for people to use standardised methods and, preferably, to use internationally agreed methods. The most irritating circumstance was thought to be in manufacturers' technical literature because the use of different methods makes comparison of products virtually impossible. Even with use of the same standard, attempts to compare data fails if there is no mention of the test details if the standard allows options. However, it was accepted that there was still too much variation between national standards and insufficient methods totally accepted internationally. This situation has steadily improved, with the great majority of countries having aligned their methods with the International Standards organization (ISO), but even today we have not reached the ideal state of everybody using the same set of international methods.

This theme surfaced again after the **American Society for Testing and Materials** (ASTM) changed its name to ASTM International [2]. The generally understood structure for 'official' standards is that each country has a body that produces national standards and then there are ISO and the International Electrotechnical Commission (IEC) that produce international standards. In the great majority of countries there is one national standards body that is a member of ISO and/or IEC, where the aim was to seek rationalisation of the differing national requirements to end up with mutually agreed international standards – the gain being to reduce barriers to trade and in many cases the cost of testing.

Things are a little different in the USA in that the most widely known standards organisation is ASTM, but that the society is not the national standards body. ASTM has developed a complete range of standards for rubbers and plastics, but they are generally not aligned with international methods. Hence, the USA constitutes a large hole in the utopia of universal agreement for polymer testing. The reasons appear to have a commercial rather than a technical basis.

ASTM has always been international in that you do not have to be American to become a member and their standards are used very widely, but clearly it is not international in the same sense as ISO. The name was changed to ASTM International to reflect its position better and, as it said, 'To compete (and win) in the standards development arena the changing needs of the global marketplace needed to be recognised and embraced.' In the UK, the British Standards Institution (BSI) calls its website 'BSI-global' and has expanded many of its services to offices throughout the world. These moves indicate aggressive commercial practices being introduced to the standards field.

Traditionally, the development of international standards has been undertaken in a cooperative and non-commercial manner. A mainstay of this was the tradition that test methods developed in a commercial laboratory were made freely available for standardisation at the national level and, in turn, methods standardised by national bodies were offered to ISO for conversion to international status. An editorial [3] took up the theme that many national and international test methods could in fact be called second-hand (or even third-hand) standards because they had been passed on from their initial life in commercial laboratories.

The basic premise was that there were no restrictions as regards copyright of the submitted material. However, this premise was shattered in 2004 [4] when ASTM objected on the grounds of copyright violation to ISO basing standards on its methods – even though they had been proposed by American delegates! The older among you may remember the mathematician Tom Lehrer's extremely amusing song about how to get on in science – plagiarise. Plagiarising the work of others is generally condemned, and if results were taken and passed off as one's own the ASTM position would generally be supported, but the situation with standards has been different. A national standard is offered to ISO, it is not taken. Almost all countries in the world support the idea of a unified international standards with which national standards can hopefully concur. Also, in the problem cases arising in TC 45, the wording and structure of an ISO document are very different from those of an ASTM document. The last thing we need in standardisation is competition; it is difficult enough to obtain consensus without standards bodies vying with each other. Sadly, it seems to be a sign of how the times have changed over the last 50 years.

The ISO is a non-governmental organisation and the members are national standards bodies, not delegations of national governments. To take the UK as a typical example, the national body, BSI, receives help from government but is independent. The members of its technical committees are drawn from all interested parties, and should a government department decide to join it is just one voice among many. This is a very important point because in some quarters the notion is held that European standards bodies are puppets of government.

You would have also thought that that the last thing we needed would be more standards bodies. One national body per country feeding into ISO is a very sensible model. However, the arguments of most people practically involved with standards that another layer between national and truly international served no useful purpose and promised duplication and yet another workload was ignored in Europe [5]. The motivation for European standards was essentially political and economic with no intrinsic technical value. The European Committee for Standardization (CEN) was formed in 1961 by the national bodies of the European Economic Community (EEC) and European Free Trade Association (EFTA) countries, but there was little or no impact on polymers until 1990.

CEN TC 249 for plastics was formed then but there has never been a specific committee for rubbers, for which we should be grateful. CEN 249 adopted the sensible policy of using ISO test method standards, which it could be argued proved that it need not have been created in the first place. It also meant that going to their meetings was not exactly an uplifting experience. Converting the ISO methods to CEN standards was not exactly a quick and simple job and revisions get out of sync. Nothing was gained and more effort from the volunteer committee members was needed. Presumably, it created a few jobs. When rubber test methods are needed in CEN it seems that the ISO methods can be used without there being any conversion: so much for bureaucracy!

I was also involved in CEN 217 for sports surfaces during the 1990s which proved to be very different from previous standards experience. There were no ISO standards, it was a new subject, and most of the delegates had no standards experience. Perhaps the biggest lesson was the value of regular contact and the exchange of ideas which has been getting less and less prevalent in testing because of economic considerations. There was pressure to produce CEN standards relatively quickly, which meant a concentrated and expensive effort. However, it also meant that some product tests were rushed through without really being properly verified, which could lead to problems later.

An interesting aspect of a new CEN committee was the need to decide on the language used. It amused me to vote for French and lose, knowing that there was a majority for English without the need for the UK. There was cooperation between the sports interests represented, but there was also quite fierce competition between countries – to the extent of who got the convenorship of a working group being decided on one occasion by the chairman's casting vote. What with the element of competition, the lack of expertise on how the standards system worked and the inevitable commercial considerations prevalent in anything to do with sport, it was all rather hard work.

ISO standards are published in English and French but a newcomer to TC 45 today would see no sign of the dual language in the proceedings unless there were comments on a draft international standard (DIS) applying to the French text only. When I was first a delegate to TC 45 and TC 61, expensive professional interpreters were employed and we had translation to and from French and English in the plenary sessions and the subcommittees. This slowed things down but occasionally provided amusement. The interpreters were very good and became very well versed in the subject so that one was able to say 'Professor X said *so and so* but he meant to say *modified so and so*'. At one meeting in Canada, we had simultaneous translation with headphones for the delegates. The French turned to the English translation because the Canadian version of French was a bit beyond them. An experience I will not forget was spending an evening with the interpreters when they changed language according to the subject.

Figure 7.1 The more formal ISO TC 61 plenary in Ottawa, Canada in 1976

All resolutions were also produced in both languages but even that has been discontinued in the testing committees of TC 45 in recent times. The changes reflect not only the need for economy and efficiency but also the fact that the use of English has become more universal. One thing that some people would say was a good thing about CEN was that it greatly encouraged otherwise reluctant Europeans to learn or improve their command of English.

It was also recognised in 1980 that standard test methods are not always suitable for research purposes and certainly are often inadequate for generating design data [1]. However, that is no excuse for not using them for quality control purposes, determination of the basic properties of materials and in performance specifications. It was suggested that one reason was ignorance and perhaps the standards bodies needed to pay more attention to publicity. There are still far too many cases of standard methods being ignored; we see it frequently in articles submitted to *Polymer Testing* journal. I am not convinced that it can be blamed on lack of publicity by the standards bodies. It is more likely due to a lack of education in academia and an attitude of some research workers that anything standard does not apply to them. In manufacturing industry, specifications for the products are a way of life and standard test methods are expected.

Polymer Testing has always championed the use of standard test methods, but in practice has been very laid back as to whether they have been used in reported work. After 27 years, the editorial [6] was again encouraging the use of standard methods and in particular international methods. This must demonstrate that not a lot has changed over the years with respect to all research workers recognising the value of standards.

In 1980, manufacturers' data sheets frequently ignored standards and, worse, gave no indication of how data were obtained. This was sometimes to deliberately gloss over the fact that test conditions had been chosen to show the material in the best light. Even when a standard was cited, attempts to compare data could fail because the reader was confronted with results and a reference to a standard but no mention of the test details, although the standard allows options. The test report sections of standards make it quite clear that all relevant details should accompany the results, including the particular issue of the standard, test speed, test-piece size and so on. Without these it is often totally impossible to make meaningful comparisons with other data; for example, in a Charpy impact test it is essential to know which test-piece size was used and the notch geometry. Here, there has been improvement because of the discipline that was needed for material property databases, as discussed in Chapter 14.

When standard test methods have been produced for all the generally recognised physical properties, you might ask what standards committees do to fill their time. In fact, they are kept busy in three directions – revisions, new ways of measuring properties, and additional properties. Revisions reflect advances in instrumentation by changing or broadening the specification of the apparatus to be used and attempt to satisfy the increasing need for reduced uncertainty by tightening tolerances and procedures. This has been a more or less continual process over the last 25 years, encouraged to a considerable extent by the depressing results of inter-laboratory comparison testing. When a standard is reviewed after five years there is a high chance that some change will be proposed.

A prominent theme for revision in recent years has been improvements aimed at reducing uncertainty of results. This is very laudable and it makes sense to tighten procedures to eliminate sources of variability. As well as tightening procedures, test equipment manufacturers are fond of promoting reduced tolerances on all the parameters. One snag that arises with this is the increase in complexity and apparatus cost that may go with it. This was raised in respect of the needs of 'developing' countries at a TC 45 meeting [7]. It was suggested that the improved standards are of no avail if people cannot afford to change their apparatus. There is certainly a limit to the effectiveness of tightening apparatus parameters, which we are now reaching in some cases because the uncertainty is dominated by material variability, with variation in applying the procedures in second place.

It is not surprising that alternative ways of measuring properties are occasionally suggested, generally as a result of new instrumentation, and there is also a trickle of more obscure properties to emerge from the woodwork at intervals. All of this is fine in principle but there is a danger of standardising for its own sake and adopting methods used only by very few people, not to mention unnecessary complication [8]. In practice, there is some difficulty in deciding if a method warrants the standardising process. Economic restraint has meant that standardising authorities have introduced formal routines for the adoption of new work with the aim of only accepting projects that meet with almost universal approval. However, internationally at least, almost all suggestions will get through because a number of countries will vote 'yes' to anything.

Individual organisations or individual companies will push strongly for the adoption of their own method, and this is probably a strong reason why for some properties two or more alternative standard methods exist. Everybody is entitled to their own choice but, when it comes to standardising, the wishes of individual factions must be tempered by the need to avoid proliferation. Indeed, it is the aim of a standards committee to reach consensus agreement, not several agreements. However, despite the objectives, alternatives may have to be accepted for good technical reasons as well as because different procedures have long been accepted in different quarters.

To what extent unnecessary standards have been produced in recent years and which are the guilty ones is inevitably subjective – feel free to disagree. At the time of writing, TC 61 has reached the amazing total of 12 proposed methods for dynamic properties of plastics. Whether any of these are really used in commercial specifications is questionable, and it is probable that a general guidance document as published for rubbers is all that is needed. TC 45 has been suffering an increase in abrasion methods, despite having published a general guide. It can be almost guaranteed that the new methods will not be cited in specifications because they are development tools. It is essentially a matter of wanting the status of an international standard for particular pieces of apparatus. Because there are always enough people who will say 'yes', it is highly likely that we will have more methods proposed in the future that do not justify the time and expense of the standardisation process.

There have of course been a few new methods standardised in the last 20 years that are definitely justified. A few have simply been more extreme cases of the slow speed at which standard production can grind. The outstanding example is the procedure for measuring ozone concentration [9] which took nearly 40 years to complete. Others are genuinely new, such as transient methods for thermal transport properties, although the variety of these could become too numerous for convenience.

The standard for ozone resistance may be an extreme case of time delay in bringing standards in line with progress in technology, and ozone testing enthusiasts will quickly point out the rather special problems that this particular subject presented to the standards committees. However, it was not the only case of standard test methods being somewhat long in the tooth, although the situation has improved more recently because of the review procedures being more effectively applied. Standards are hardly credible if they advocate the use of outdated technology. In this respect, although the problems causing delay may sometimes be technical (or rather technical disputes), efficient updating in terms of time scale and technical relevance, needs resources. There were no signs in 1985 that the industry was prepared to give the support that is really needed and the situation is even less encouraging today.

There are of course two sides to every coin. If standard methods are revised too frequently they may be most unwelcome. Apart from the general nuisance of frequent change in procedure, there can be an unreasonable cost in replacing apparatus. More generally, it would mean that material property data would be quickly outmoded if the change in test procedure affected the result. Chopping and changing the standard methods would not encourage enthusiasm for standard presentation of data.

One way in which standard test methods become complicated is due to the need to cater for the differing needs of different groups of people [10]. Originally, the methods were largely aimed at routine control and characterisation of material properties, and the only need for a variety of test conditions was to cater for the amount of material available and as a compromise to different national practices. Over the years there has been standardisation of methods more relevant to generation of design data, extending the range of test conditions to enable multipoint data and the production of what are really guides to measuring a property.

Quite recently, there has been some recognition that a standard could become something of a hybrid that did not really suit anybody. Hence, there was a proposal in TC 45 that for all properties there should be two or more standards or parts of a standard. The first part in each case would be a guide to the measurement of that property which outlined the principles, the significance of the result, the effect of test procedure variables and the techniques available. The second part would revert to be being a procedure for control purposes which had closely specified test parameters and procedure. Conceivably, there may be need in some cases for more than a single method or single set of conditions. An example would be low-temperature tests on rubbers where tradition and technical need dictates that several quite different methods are needed. The needs for design data would be discussed in the guide document for most properties, but the model could allow for additional parts which gave particular protocols or particular test methods for the generation of such data. Examples might be fracture mechanics based mechanical tests and the estimation of lifetime from ageing tests.

There could be several advantages to this approach. Confusion or incompatibility arising from choice of conditions in existing methods could be eliminated, whereas the total range of conditions specified could be increased. It would not be of insignificant value to bring the numbering of related standards into some order, and it could even attract a greater input into standardisation from the academic community. Whether enthusiasm will be sustained for this revision of all methods to be achieved has yet to be seen.

The problem of manufacturers' data sheets ignoring standards has been largely overcome. The movement for comparable single and multipoint standards to generate input for databases started with a group of large plastics manufacturers in the 1980s, progressed to TC 61 and eventually to TC 45. This is discussed in Chapter 14.

In tandem with the tightening of standards to try and improve uncertainty of results, there has been the inclusion of precision data and the development of standards for calibration of test equipment. Indeed, it was the results from inter-laboratory precision trials that prompted steps to improve the test methods. ASTM pioneered the generation of precision statements around 1980 and a few years later the same process started in TC 45 and TC 61 (see Chapter 11). There has been a strong will to generate the data, but progress at ISO has suffered from the old complaint of lack of time and money to organise and take part in the testing. Quite recently, Goran Spetz from Elastocon rejuvenated the process in T 45 with input from people who do not actually attend ISO meetings.

Except for a few isolated cases, standards specifically for calibration did not start to appear until quite recently, and only for rubbers is there a general system for introducing a calibration clause to all methods. The rationale is that a calibration schedule makes it clear to a calibration laboratory exactly what parameters need calibrating and what the requirements are. This came from an initiative in the UK and was a good example of a national standard (which had taken a lot of time and money to develop) being freely given to the international community. The work originally was for rubbers and plastics, but it has not been taken up in TC 61.

Test methods for rubbers and plastics have always been developed in separate committees, and largely there have been no problems with areas of overlap – hose, cellular materials and coated fabrics having been assigned amicably. The introduction of thermoplastic elastomers in the 1970s threw up a bit a challenge for the testing world because of their hybrid nature. Whilst materials processed in a plastics environment would probably be tested with plastics methods, the rubber sector saw them as competition for conventional rubbers and assumed that rubber methods would apply. Indeed, TC 45 started using the term rubber, vulcanised or thermoplastic in the titles of their standards, probably sometime in the 1980s. In 2004, TC 61 raised a new work item on presentation (NWIP) of single-point data for thermoplastic elastomers, which caused a demarcation dispute [11]. The outcome was that the NWIP was dropped and it was agreed that there would be cooperation as regards standards for disputed materials. This included the application of the great majority of rubber test method standards for general physical properties.

Generally, the rubber and plastics industries do not have a history of cooperation on test methods – to the extent that they cannot even agree on the details for the application of the same test instrument [12]. The tolerances on the Shore durometer are now specified differently for the two groups of materials. They must have started from essentially the same base but then developed independently.

Whilst it is to be expected that, as a generality, there has to be differences in test methods because typically plastics are a great deal stiffer than rubbers, it would be reasonable that where there is common ground it would be shared. However, the Shore durometer instance is not unique in polymer testing. Tear tests doubtless had the same starting point but the overlap of methods has lessened to there being only one common test piece internationally (more in national methods). They share a method for interpretation of the tear (and adhesion) force traces, but it was developed in the rubber camp and ignored by plastics.

Both industries realised there was a need to investigate environmental effects but the different priorities for material types ensured that procedures would diverge. For rubbers, heat ageing is the number-one environmental test and the ovens are very carefully specified. If plastics are heat-aged, then a common perception is that almost any oven will do. For plastics, ultraviolet (UV) resistance is the prime concern but is rarely considered for rubbers. However, there is a weathering method for rubbers and, would you believe, it refers to the plastics methods for details.

There has simply never been any enthusiasm to get together and explore what common ground could be established. For several years I attended both committees' meetings but I do not think I achieved much cross-fertilisation of ideas. For example, differences in the low-temperature brittleness and durometer hardness tests have been just accepted for 40 years or so, although recent divergence in the durometer apparatus specification has caused real troubles for calibration laboratories. It is difficult to say how much the lack of cooperation is due to the time and cost restraints of physically getting together and how much is bloody-minded arrogance.

There is an interesting difference in terminology [13]. Have you noticed that ISO test method standards for physical properties of rubbers always talk of test pieces for the particular shaped object that you pull, squash, or indent, whereas the equivalent ISO plastics standards always talk of specimens or test specimens. Going back to the days when British standards were not copies of ISO, the same distinction was evident – rubber used test pieces and plastics used specimens. When I first encountered this I put it down to plastics committees having been dominated by chemists, whereas more physicists and engineers had a say in rubber circles – purely founded on the observation that chemists seem to like the word specimen for a small sample of a substance.

The ASTM rubber committees, not for the first time, do not agree with the ISO because they use specimen. Indeed, the amendment to an ASTM standard which prompted the editorial was to eliminate the use of test piece because this is apparently deprecated in ASTM circles, whereas unbelievably they come up with product piece for a test piece taken from a product! Hence, there can be another slant on the subject in that the ISO plastics committee has an American secretariat, whilst in its formative days the rubber committee had a British secretariat. A look at what wording the ISO rubber committee for chemical analysis methods uses was not conclusive – in three drafts checked they used test portion, sample and test piece. This defeats the chemist/physicist argument, but there is also a lack of specimens considering the working group has an American convenor.

One thing is certain, sample has a statistical meaning and refers to the material or objects selected from the total population. Having taken your sample, for the sake of argument a sheet of polyvinyl chloride (PVC), it could form the test portion for a chemical test, but for physical tests it would usually be necessary to prepare specific shaped bits from it. A British English dictionary is quite clear: specimen is (a) an individual animal, plant or piece of mineral used as an example for scientific study or display, (b) a sample for medical tests. This would indicate that test piece is the more apt word for physical tests but, despite the Google search engine having 64 million entries for test piece, the dictionary refused to list it. We therefore fail to trace the origins of the differences but there is little doubt they started way back in history and time has had no effect whatsoever – just like in cooperation on methods.

TC 61 is much larger that TC 45 but one respect in which they are similar is in the number of countries that are represented [14]. In 2006, both had 27 participating members (plus about the same number of observer countries). This is not very impressive out of the Google count of 193 countries in the world. The great majority of the rest must use polymers but, for whatever reason, do not contribute to the effort. I have not kept records, but from memory the level of participation has not changed a great deal in over 25 years, which is a little depressing.

There are up to 30 working groups in TC 61 and eight in TC 45 that are specifically concerned with testing. The number of experts attending a working group varies but, using recent TC 45 meetings as an example, the attendance averaged about 25 delegates. Most of these people will have attended more than one working group so, at a guess, there might be no more than 40 experts active in formulating the rubber test methods in the world. Forty rubber testing enthusiasts in one place is quite formidable, but is a rather petty figure when put against the thousands involved in testing rubber materials worldwide. The figures for plastics will not be significantly different relative to industry size.

The primary medium for the dissemination of standards has always been the printed page. ISO standards, and most national documents, have usually been distributed as individual methods of a few pages' each. Some national standards were initially grouped together into books, for example, the UK test methods for plastics [15], and earlier for rubber. However, revision became a problem and the idea was dropped just about everywhere except by ASTM.

The UK plastics book was initially very convenient, having all the methods in one place and one purchase yielding a complete set. Very rapidly, however, it became a revision problem with lots of amendment slips stuck in everywhere and ultimately a sort of albatross around the neck of the committee concerned. When I became chairman of the committee in 1978, the decision had long been reversed and revision into separate documents for each method was well underway. Indeed, I think the previous chairman expected to see the conversion completed. It became my aim to see every standard revised in a year or two but as the years rolled by my ambition was limited to seeing the 1970 book completely withdrawn within my lifetime. Well, in 1992, 22 years after it was published, BS 2782 1970 was finally withdrawn as being superseded, mostly, I must make clear, due to the determined efforts of the committee secretary. Anybody who has seen through a standards project will appreciate my immense pleasure and relief when that milestone reached. It was definitely a celebratory occasion. Ironically, I later had nostalgic feelings for the old book which for years was carried around the laboratory like a bible.

Figure 7.2 A very young Roger Brown pontificating in a working group meeting in Budapest – note the Smoking

The complete set of ISO standards for rubber was published as books in three volumes in 1984, although this was in addition to, rather than instead of, individual documents. These volumes also quickly became dated and were not even kept going by the issue of amendment sheets. Hence, they were not exactly a success and I rather doubt that anybody found them useful.

In stark contrast, the ASTM books have been a roaring success for many years. Perhaps this can be attributed to the scale on which they are produced being sufficient to support annual updates, thus eliminating the problems of galloping amendments. As long as you are not too bothered about a few months' delay in working to a revised method then the annual book purchase system certainly has many convenience factors. However, buying a new set of books each 12 months with only a modest number of changed pages does not look an attractive deal financially, but it has been a happily accepted procedure. Perhaps an American could tell us exactly why the system works.

Certainly, the contrast of the BS 2782 saga and the ASTM experience teaches us that a basic idea that is made to work in one place may not so do in another, especially if the formula is not copied exactly.

In the last few years, more and more people access standards electronically, a process that few would have even contemplated 20 years ago. Now, it is the norm to buy a standard as a computer file but it would be interesting to see how many people still print the computer file to get a paper copy. You will have to count me in. The Swedish standards body produces the ISO testing standards on a CD that is updated annually, which is the electronic version of annual books, and presumably many people find this convenient.

The process of transmitting data electronically is a fine example of standardisation. We would not have the magic of the Internet if it were not for the standards on which the protocols and system that transit our data heavily rely [16]. There are also standards for the way in which information is put together and how it is presented at the other end, but this is not so critical and we all know that, for example, there are variations on standard vanilla hypertext mark-up language (HTML) and on how web browsers work. Believe it or not, this has been seen on the websites of national standards bodies where not all features work in all browsers.

Strict standards may make communications work but it is another matter if the software in which the communications are written is considered. Sure, the commonly used file formats for drawings and photographs are universally understood but, for word-processing software, we essentially have a commercial situation and the only reason we can understand each other much of the time is because of the near universal acceptance of one product. At one time there were several software packages that we might have used when we wrote our reports, but Microsoft Word became a *de facto* standard. Not that it was a perfect standard because there could still be problems on occasions with articles for *Polymer Testing*.

It could be another case of the more complicated a thing is the more trouble it will cause. If a document is relatively simple the odds are it can be successfully read in more than one flavour of software, but use clever formatting and it may go pear-shaped. For example, if you embed diagrams in a Word document using an older version the pictures may neither be visible nor print in a recent version. Odds are they will appear in Open Office but the legend will be distorted. Hence, for years I kept an old computer with old software in a spare bedroom (actually, being pessimistic, I still do).

LaTeX is said to be a superior format for scientific papers but it is used by very few people. About once a year the editor has to remember where the software is and how to use it when a .tex file lands on his screen. The pdf format is treated almost like a standard and is a superb way to exchange documents without needing to consider what software the recipient has (although an old version of the reader will not cope with the latest version). However, pdf is not suitable for submitting papers because it is a most inconvenient format to edit, and the files rarely convert successfully back to a word-processing format.

As I write, all the signs are that open-source standards, just like our international test methods, will replace proprietary software. Pity it didn't happen 20 years ago.

I have been involved in standards development since the late 1960s, have been delegate and committee convenor in ISO TC 45 and TC 61 and in CEN committees, and am still leader of the UK delegation to TC 45 and chairman of several national committees. As mentioned in Chapter 2, initially there was almost competition to be accepted as a national delegate, whereas nowadays most countries have to press-gang people into going. TC 61 was always concentrated into just over one week with many working groups with limited scopes, several meeting at the same time, hence necessitating several delegates. TC 45 was rather more laid back when I started, with the meeting extending over two weeks. That could not of course last, and now it has been reduced to five days.

The size of the UK delegations (and those of many other countries) has also shrunk. It was the practice to have a Business Travel Agent, block-book aeroplane seats and hotel rooms for 20–30 people. We now find our own way with less than half the number of people, and only some of those staying the whole time. I remember there being 29 UK delegates to TC 45 in India but, one morning, due to stomach upsets, only two of us made it to the meetings. For the 1975 meeting, Malaysia organised an aeroplane for the European delegates, which was probably one of the earliest flights to go non-stop. I sat next to Charles Thompson's wife who told me of the much more elegant and leisurely times when she first went to Malaysia accompanying her husband who worked on one of the rubber estates. They went first-class by flying boat, landing each evening before dinner and taking off again after breakfast. Nowadays, travel aspirations are dominated by cost.

A recent editorial [17] asked 'What is it that motivates people to work on standards?' I suggest we need to find the answer to this rather quickly because the average age of members of polymer standards committees is dangerously high and an unreasonable proportion are officially retired or operate their own consultancy part-time.

The core reason is probably the logical one that the company you worked for considered participation in a committee to be in their interests and you were selected to be their delegate. In the case of testing, the company got to know in advance and could influence changes that might affect their capability or would need investment in new equipment. Unfortunately, for many years the trend has been for any activity not considered essential to be cut – payback on standards participation is not immediate and to a large extent not easily quantifiable – and so it is an easy target. The net result is a lack of new, younger people joining the standards making process.

Figure 7.3 The ISO fosters friendships – the Anglo-Japanese dinner

Once introduced to standards work, many people are hooked. It is a challenge to produce better standards and to fight your corner against conflicting interests, and also fosters great satisfaction to see something you helped to create be published and widely used. With few exceptions, the work is conducted in an atmosphere of cooperation and you can reasonably expect the best technical solution to be adopted rather than something that owes more to politics or sales hype. On top of this, the committees are cemented by the standards community tending to be a close-knit family where you make many long-lasting friendships. Standards have taken me to at least 23 countries and provided more memorable occasions that I can count. This is not the place to relate the experiences, but whenever ISO friends meet up in a social situation the stories are remembered and re-told. Overall, it is a pastime that comes highly recommended, the only problem seems to be how to create the industrial climate to allow people to get started.

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8 Why are We Testing?

The reasons why we test are one thing that has not changed over 50 years. It may be that the relative importance or frequency of the various reasons has changed, but that is not immediately evident and probably would be impossible to prove. Nevertheless, different aspects of how we test, including appreciating the other person's view, have attracted editorial comment over the years.

My contribution on the '*Requirements for Physical Testing of Rubbers and Plastics*' was published in 1984 [1], and the theme has been repeated in the introductory chapter of textbooks. The basic reasons are:

- Quality control
- Predicting service performance
- Design data
- Investigating failures.

It is necessary to identify the purpose of testing before considering which properties to measure and which methods to use because the requirements for each of the purposes are different. This may be an obvious point, but failure to appreciate what purpose the results must satisfy easily leads to unfortunate choice of method and conditions. Also, poor appreciation of the merits and limitations of a particular test can arise from lack of consideration of why another person is testing and what they need to get from their tests.

Cooperation with others is a prerequisite for successful progress, and an important aspect of cooperation is understanding the aims, motivations and needs of others [2]. In the field of testing of polymers we clearly have several divisions of interest – different areas of testing, different materials and different reasons for testing. The physicist is not usually very concerned with chemical analysis methods, the plastics producer probably does not have to test rubbers, and the quality controller may not be very excited by the latest procedure used in academic research. Nevertheless, it can be advantageous to sometimes pause and attempt to appreciate the other person's interests.

I would suggest that, as regards testing, the most widespread lack of appreciation is of the different needs of those concerned with quality assurance and those investigating materials from a design data interest. If you want design data it is only too easy to belittle the routine standard tests – but your much more involved multipoint data are not viable for control. This difference was probably much less if you go back 50 years simply because the need for more relevant design data was not so widely appreciated.

Understanding the other person's position is a highly important aspect of standards development, as we saw in Chapter 7. In fact, standards committees must be one of best places to learn the views of others because you are forced to debate the alternatives and arrive at a consensus. A great pity is that the representation on standards committees is probably now only 25% of what it was when the period started.

One aspect of standards cooperation discussed was that between the rubber and plastics committees – which was seen to be rather less than ideal. An interesting point in a 1984 editorial [2] was that in the UK we had one Rubber and Plastics Research Association (RAPRA), one Plastics and Rubber Institute and one Polymer Engineering Directorate. It could be argued that this represents some advance because originally RAPRA was the Research Association of British Rubber Manufacturers (RABRM) whereas the Plastics Institute and the Institute of the Rubber Industry were separate. Now of course they are both swallowed up in the Institute of Materials, Minerals and Mining – largely for cooperation of a financial nature.

You need to visit other laboratories to appreciate the differences [3]. We can all be insular, for example, it is very easy to lapse into thinking that all laboratories are roughly similar to our own. However, sometimes the thought can suddenly strike you that they can be very different. The purpose of some laboratories is to provide a quality-assurance function and they will have a vastly different philosophy, and probably markedly different apparatus and staff make-up, compared with a corporate research laboratory or a university laboratory.

It is obvious that laboratories vary greatly in size, and that some conduct mostly physical tests and others chemical analysis. However, even these clear differences can be forgotten in conversation and debate, such that two people can be discussing a subject without being conscious of the other's very different standpoint. The laboratory of a manufacturing company, whether for quality control or research, will be concerned with that company's products, whereas a commercial test house will always be dealing with someone else's production as well as probably dealing with a greater range of items.

Whenever you visited someone, a tour of the laboratory was almost always given. Now, it is highly likely that for safety or security (or some other trumped-up) reasons this would not be allowed. Goodness knows how you are supposed to cooperate and learn from each other in that sort of circumstance.

You cannot please all of the people all of the time but by discussion (and looking at what they are doing) you can get a little nearer [4]. An example of quite extreme difference in attitude was the ASTM and International Standards Organisation (ISO) standards. Apparently, there were people who objected to ASTM International publishing ISO standards because it would be seen as endorsing ISO! What a contrast this is with Europe where most countries have discovered the simple answer which is to adopt the ISO as the national standard. This may not be acceptable on all occasions but it should be the aim. In the case of test methods this is essential if we are to be able to freely exchange and compare data.

This adoption of ISO is a well-established pattern for most Europeans and, hence, has perhaps become second nature. There is no difference in principle for the USA; they would have to put up with the same compromises that Europeans accepted, having done their best to influence the ISO method to their views and preferences. One factor must be that people can approach standard test methods from different positions or starting points and, hence have markedly different attitudes. Doubtless, the Americans consider the European attitude to be strange and *vice versa*.

A parallel could be drawn with the different ways a researcher and a factory quality-control person will see a test method. What they need to get from a test is different so they judge by different attributes. What is good to one could be most unsuitable to the other. The fact is they do live in different worlds. The same is true of many things from food to clothing – and, for example, how sophisticated a test apparatus needs to be to appeal to different laboratories. A truly geographic difference of viewpoint can be seen in laboratory temperatures. Probably 23 °C is the most widely accepted but in the tropics 27 °C is rather more economic, and at one time 20 °C was the most popular norm. If you talk to sports equipment people they would like something lower in their standards because that would be more practical for sports halls. Mind you, I still come across people who are not sympathetic to the higher temperature being standardised for tropical countries.

Faith in getting understanding suffers a setback if you consider where we do not agree on terminology. The use of the words 'test piece' and 'specimen' was discussed in Chapter 7. Other examples are 'testing', 'analysis' and 'characterisation' [5]. Chemical analysis and physical testing have always been with us and presumably reflect the usage of different disciplines. I do not recall 'characterisation' being used before the late 1970s, and I think it came from the academic world. I still do not understand exactly what it is supposed to mean. The rather hybrid term 'thermal mechanical analysis' appeared when the technique was introduced, and puts a different slant on the word 'analysis', but we have got used to it.

A difference in the understanding of 'environmental testing' arises more and more frequently, the conversation can go on for minutes before it is realised that one is talking of the effect of the environment and the other the effect on the environment. In at least one country's standards there are 'properties' and 'determinations' both prefixed by 'test method', all of which others might call 'test procedures'. A diffusivity buff will get a little confused if you talk of 'fire tests' as 'thermal properties', but 'fire' and 'melting point' have been lumped together on occasions together under a 'thermal' label. There are many more questionable uses of terms in the wide scope of testing.

It probably serves little purpose, but I looked in my most unpretentious dictionary. 'Analysis' is 'detailed examination', 'testing' is 'critical examination or trial' and to 'characterise' is to describe 'character'. I still wonder why we use all three.

There has always been a need to test products to predict or prove service performance, but independent product evaluation increased considerably after the formation of organisations such as the Consumers Association in the UK about 50 years ago [6]. One of their most important services is the testing of comparable products and reporting on the attributes and faults of each, often ending up with a recommended 'best buy'. To do this they obviously need to test the products in a scientific, but practically useful, manner. This sounds like the ultimate job which any tester would love to have – putting lots of products, which as a consumer one inherently has an interest in, through their paces. Certainly there must be plenty of variety, many of the 'test pieces' could be very interesting and, presumably, most of the test methods are non-standard. Devising a fatigue test for toasters seems fun and it could be argued beats working on ten grades of polypropylene at n hundred batches per year.

In 'ordinary' laboratories we do comparisons – sometimes you or your client wants to compare the competition, or the different offerings at tender are to be evaluated. We then inevitably face the problem of what is most meaningful or relevant, e.g. a higher tensile strength against less wear resistance, and it doubtless depends on the application. Then there is the problem of proving statistically, or rather to one's complete satisfaction, that one product is better or worse than another.

It must of course be exactly the same at *Which?* What one person thinks is an important attribute another considers to be trivial, which probably explains why I frequently disagree with their conclusions. It must be very difficult to be confident of the significance of results with small samples and sometimes quite complicated products. Not only do they have the usual problems, but a million consumers hanging on your every word.

Figure 8.1 Practical test of performance

Nevertheless, I am sure it is a very rewarding job and it leads to thinking which aspect of testing provides the greatest challenge and which process any given individual finds gives him or her the most satisfaction. For some, it is planning a programme of tests to get all the information required in an efficient manner, the more complicated the better, but they do not actually relish doing the experiments. Many people could happily spend their lives designing test rigs to measure the most unlikely properties on unbelievable products but be almost uninterested in the results. Some consider that the enjoyment comes from the practical experimentation and would quickly pass the numbers on to others. Yet another group find the challenge in the analysis of results; it does not matter who produced them, the interest is in what secrets can be extracted by manipulation. Of course there are those for whom the results are everything, the actual testing being a necessary evil on route.

Results of tests by the Consumer Association are publicly reported and are regularly picked up by the newspapers, but most laboratories very rarely get that sort of exposure for their work. It was suggested [7] that perhaps testing had a negative image from being associated with failed products, with that image being intensified by the quality control demands, legislation and accreditation schemes we have been increasingly subjected to. Testing is not generally a headline subject, except perhaps when some piece of forensic evidence is being challenged or when used by publicity people to hype the dubious claims of a consumer product. It is not even given great priority by industry, often being considered as a necessary evil. The necessity arises from meeting certification requirements and clients' quality-control schedules, whereas the evil arises from the costs involved.

This lack of 'celebrity' status exists despite the fact that without test results engineers could not efficiently design new products, those products could not be proven before being put on the market, and the quality during production could not be guaranteed. People who work in testing know that it is important but question whether we give it enough publicity. International Testing Week might be taking things a bit far but it does no harm to remind people how valuable the test laboratory is to a company's operations.

However, if you want publicity then you need to test consumer products, and probably top of the list is anything to do with sport. The editorial gave the example of the press conference when the British Equestrian Trade Federation (BETA) launched their new standard for protective jackets, the important content of which is a test to measure shock absorption during impact. The real interest was of course in the fact that a specification was being introduced, but the test procedure is such a central part of the requirements that it was the topic which received attention.

Figure 8.2 Impact resistance of equestrian jackets

Apart from the example of equestrian jackets, I made several television appearances in the 1980s on programmes such as '*Grandstand*' and '*Sports Night*' to pontificate on the merits and failings of artificial sports surfaces. We tested much more important things but they did not interest the media.

One of the reasons for testing surfaces was to establish the performance for football pitches to equate to natural turf. I sat on a committee chaired by Sir Walter Winterbottom which established the criteria, but the Football Association 'moved the goalposts' so to speak and artificial surfaces were banned in the first-class game. It is very frustrating when science is outvoted by 'politics'. Incidentally, one of my prophesies on television was that England would not again win the World Cup until they regularly used synthetic surfaces, on the basis that they need to develop skills under hard, fast conditions. There is no sign of me being proved wrong.

The other reason for testing that can have a glamorous side is investigating failures when the courts are involved. I have done my share of litigation work and can vouch for it being the most exciting, if also the most nerve-wracking, side of testing. An editorial [8] discussed the loyalties and possible bias of expert witnesses in the context of a change in the legal system in England which was interesting in illustrating the different worlds of the court and the laboratory.

Traditionally, when acting as expert witnesses, scientists were hired by one party to the dispute. Although as a matter of conscience one would expect scientists to be completely impartial in their investigations, there is inevitably a loyalty to the side paying the fee and effort tends to be concentrated on demonstrating factors which are to the client's advantage. Each side hired an expert and if things went that far they fought it out (through the medium of the barristers) in the court. My experience was that when there were no cut-and-dried test results, the outcome depended on the skill of the barristers and the perceived credibility of the experts. However, when there were valid test results, they always convinced the judge.

As a result of the change, expert witnesses have a duty to the court, rather than to client and lawyers, and in some cases there will be appointment of a single joint expert to cover both sides. If you are working for the court it follows that you would have no particular reason to be biased to one side and this should be a formula for the most objective and balanced expert view being presented. One possible problem comes to mind in that if you know yours will be the only expert opinion there will be nothing like the driving force of fear of being made to look stupid to ensure that you carry out the task with utmost diligence. Scientists by definition should be searching for the truth but if everything was that pure the two-expert system would not have become established. There is also the small matter that many cases are far from clear-cut and the expert opinion is just that rather than fact.

The other advantage of having a single expert is that you have halved the costs. However, what baffled many scientists involved in expert witness work was how answering to the court squares with responsibility to whoever pays. There was no mention of the court footing the bill. Lawyers I spoke to at the time suggested that the system would not cut the number of experts but increase it: one joint expert witness and two 'expert advisors', one for each party. I have not had the opportunity to find out what has happened in practice.

A slightly sad aspect to court work [9] is that it is relatively lucrative, so that you could earn more helping to sort out trivial disputes than solving real technical problems. If the perception that litigation is increasing is true then there will be more misplaced scientific effort, but at least there will be more income for test laboratories. This prompts the thought that all test results have a value, not simply in terms of what they cost to obtain, but in terms of their importance [10]. Some tests are quick and cheap to carry out, others very time-consuming and expensive. However, a single quick test can sometimes turn out to have importance out of all proportion to its cost. If the importance is very high, getting it wrong could be disastrous. So, another side to increasing litigation could be the increased danger of a laboratory being sued. Fortunately, I never found myself in that position, although discrepancies in results from rather hastily standardised sports surface tests were on occasion embarrassing.

The greatest test consequence is likely to be where a safety product is involved and, generally, where formal verification to a contractual specification is called for. As more and more products and transactions become subject to performance and safety specifications, it is clear that more test results will assume greater importance. Considering that there seems to be a tendency for people to be looking for someone to blame, laboratories will have to be extremely vigilant.

Figure 8.3 Failed cricketer's box – filming for a television programme

If you work in a contract-testing laboratory covering the whole spectrum of polymers then you experience just about every conceivable reason for testing and a huge variety of materials and circumstances. It is my perception that there has been no change in that variety over all the years I have been involved. If it is manufactured, at some point in time it will need to be independently tested. Some of the more obscure things I remember are adhesion of price labels to the soles of shoes, strength of narrow-gauge railway fishplates, proving that urine caused degradation of foam carpet backing, proving claims of how long a microwavable 'hot water' bottle retained heat and checking if decorative candles burned dangerously.

The examination of the shoe price labels was interesting because it was not simply a matter of how strong the adhesive was but that it was strong enough for the label to stay on whilst in the shop exposed to sunlight but peel off with no trace remaining immediately after the sale. The fishplate work arose because it was proposed to replace traditional materials with a plastic and is a good example of the need to devise mechanical product tests that simulate well enough the stresses in service. The carpet job was an investigation of why returned goods had failed. The store involved was not convinced by our diagnosis of urine (we could smell it) so we had to go through relatively costly chemical resistance testing to prove it. The culprits were of course children. My wife, who was running a bed and breakfast establishment, was useful for the bottles because we could do the trials in a bed that represented typical hotel standards. The microwaved bottles were not a patch on the traditional product. We trialled the candles in a fume cupboard, which was just as well because the wick came adrift in bad candles, touched the glass and – bang!

Because tests cost money they are generally seen as a necessary evil [11], on a par with going to the dentist. The similarity goes further in that prevention being better than cure it is advisable to test before there is a failure, which might be equated with the logic of regular check-ups on the condition of your teeth. Hence, it is paradoxical that as commercial attitudes hardened from the 1970s so the call for more quality control and certification of products became greater. The testing personnel can classify the various reasons for testing but for many of those paying there is only one reason, and this applies to all the examples detailed previously – because they have to.

The attitude of only testing when forced to is most apparent if you are a commercial laboratory trying to make an honest living, when a very high percentage of your enquiries and work comes because someone has a problem. In many cases they have bought something that failed or something they have made has failed and they have been forced to find out why. There is a direct relationship between the seriousness of problem and how much money they consider reasonable to solve it. Any dentist's fees seem reasonable if you have not slept for two days but it takes a little discipline to make the appointment before the pain starts.

The other situation is if somebody has made testing mandatory through a specification or even legislation when the question is 'We have to provide test results, how can we minimise costs?' Whatever the pressure to test, these people are looking for a way out, and doing the test is a last resort. Even when somebody has come to the conclusion that test results would be a good selling point the chances are high that they will back-off from their planned programme when they hear the cost. The level of test avoidance can be petty – less than the cost of a night in a hotel. Apparently, for many people, almost anything rates higher priority than testing. Minimising costs is of course a laudable thing but why is testing always at the bottom of the list?

There are of course the more enlightened; those dedicated to innovation, which generally necessitates testing, and those genuinely convinced that quality assurance, which must include regular verification, pays (many only pay lip service to it). Maybe my view that the avoiders far outnumber those giving priority to testing is coloured by being a test provider, but there is certainly still a very significant section of industry for whom testing is the exception rather than the norm.

Even within the testing world there is a similar syndrome with calibration. There are laboratories where the routine, formal calibration of all equipment to traceable standards is simply not carried out. Here the last resort is when they are in dispute with another laboratory or they are found out by a quality scheme inspector.

I suppose that we are inclined to advise, quite correctly in most cases, that one test is not enough, a whole programme is needed to get the answer and we cannot provide it instantly. We develop new methods and apparatus which may be better but are also more expensive. We introduce calibration routines which are more sophisticated and more certain but which add to the overheads. Then we say that the results are not exact and may not completely prove the point in question.

None of this makes the testing fraternity the most loved, but it is remarkable how dentists thrive.

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9 Knowledge and Management

There are some people lurking in the corners of laboratories who would say that knowledge and management should not be spoken of together. Scientists generally do not like being managed [1], particularly by someone who they perceive to be technically inferior – 'He (the administrator) knows nothing about the subject and therefore cannot organise it'. One section of people considers that technical management is a non-entity – you cannot 'manage' technical work and in any case there is no need to. Another section of people think that managing technical work is just like managing anything else – that there is almost an advantage in having a manager who knows little of the technical detail. The first sort of person does not really believe in what he says himself and the second sort, if gaining a place in technical management, does much to foster the complaints of the first.

When I started work, and for many years afterwards, all the management positions in the laboratory (but nowhere else in the company) were filled by technical people. They were promoted largely on technical merit, which did not necessarily mean that they had the first idea of how to manage. The position now is very different with management skills being essential for any supervisory position.

The first I saw of the change in attitude was in 1976 when the structure at the Rubber and Plastics Research Association (RAPRA) was turned upside down. Previous managers were sidelined to become Principal Consultants and the new heads of department were chosen as those perceived to be good at managing. I was one of those promoted and, seeing that a scientist I highly respected did not look happy, told him that he was a better physicist than I would ever be but I was in the position because I was a better manager. We have always got on very well.

Over the next 20 years the role of management of technical activities became increasingly commercial and, by the time my sell-by date arrived, technical ability was seen to be almost irrelevant at the higher levels. Over the last few years I suspect that the trend has continued so that if I was forced to go back I could well be one of those lurking in the corner.

Generally, a prime requirement of a successful manager is to have the respect of his subordinates and nowhere is this more true than in science. It would seem that a technical manager needs to be a paragon of virtue who is professionally skilled in management, has intellectual capacity, scientific ability and experience to at least match that of his staff and, furthermore, is appreciative of the special requirements of technical management. Doubtless a great many do not fully live up to this ideal and, if what I am told is true, many nowadays do not come even near it. However, perhaps it can be put forward as a definition of the standard to be aspired to.

One of the particular requirements of technical management is to appreciate the speculative or experimental nature of much research where, despite careful planning, good will and hard work, no useful product results, i.e., there is a gamble in trying the unknown. A production manager finds this difficult to understand. On the other hand, if scientists were allowed to experiment to their heart's content, many of them would never produce anything and, today, that is just not viable.

There must be several differences between a laboratory providing a testing service and other types of scientific work. Are there particular requirements for the person managing or controlling a test laboratory? I do not suppose it has ever been researched.

Administration is not the same thing as management but they are obviously related. Administration in its various departments could often show an uncanny knack for not having the faintest idea of the needs of technical work, or in some cases would perversely not provide it. In particular, it needs to be understood that accounts are there for the benefit of the accounts department and not the rest of the company. There will always be potshots at administration, as in the piece on 'administratium' [2] lampooning continual reorganisation:

'The heaviest element known to man is administratium. It has no protons or electrons and, hence, an atomic number of 0. It does, however, have one neutron, 125 assistant neutrons, 75 vice neutrons and 111 assistant vice neutrons totally a mass of 312. These particles are held together by force involving the exchange of meson like particles called morons. Since it has no electrons, administratium is inert but it can be detected chemically as it impedes every reaction it comes into contact with. The discoverers claim that a very small amount of the element causes a reaction normally occurring in less than a second to take over four days. Administratium has a half life of about three years at which time it does not decay but instead undergoes a reorganisation in which assistant neutrons, vice neutrons and assistant vice neutrons exchange places'.

That piece reminds me of a little trouble I got into when working for the Atomic Energy Authority and wrote a poem on paper-fueled reactors in protest at delayed payment of travel expenses.

It is probably a myth that one change over the years has been change itself – that there has increasingly been more of it. There were lots of changes in equipment (and attitudes) in the early years of our period and I cannot remember a time when people were not complaining about change. The fact is that change is a good thing if you agree with it, and absolutely diabolical if you do not. It is also a fact that, often, change is very inefficient and causes more effort than it is worth – that of course is the change that I do not agree with.

The editorial reciting the piece on administratium was brought on by suffering yet another reorganisation at the place where I laboured as a lower form of neutron when not editing *Polymer Testing* journal. In observing another round of change I noted that one thing stood out as constant. People and their titles swapped places, but the laboratory stayed exactly where it was. In fact it stayed exactly as it was: no equipment was promoted and none was made redundant.

Now, if continual change is essential for a thriving business how come that test equipment only needs to be changed when in a state of total collapse or its sell-by date has been long forgotten? It is of course an exaggeration to claim that new test apparatus is never bought, but most laboratories would agree that management show a certain reluctance to spend in this direction and require long-winded justifications which have to be bomb-proof. If the energy and expense that goes into organisational change were lavished on laboratories what a happy lot the scientists would be.

Well, perhaps not entirely so. Too-frequent change is confusing and highly inefficient. It takes time to install, calibrate and commission new apparatus and there is a considerable penalty in retraining staff. Equipment suppliers would like us to buy a new model every year but, even if our company applied its structure principles to apparatus, we would argue that the small steps in instrumental advance were outweighed by the disruption.

So, if it is inefficient to change apparatus frequently, why is it a good thing to change the organisation as often as possible? This seems to bring one back to a certain element.

I don't care what the educationalists say, there has been change in the level of qualifications that people in laboratories have and are perceived to need. I do not suppose that we can any more have the likes of a certain senior scientist who at interview always looked at the candidate's hands because he did not believe in a physicist with clean fingernails. Nor could you get a banded engineer in a government laboratory without a paper qualification, or one at that grade who stapled his shirt together when it tore whilst mucking in with practical work.

The decline started as more and more people went to university and the government made it easier for children to get more school certificates of one sort or another. At one time you could be confident that someone with a Higher National Certificate (HNC) would have good technical ability and practical experience, and be no slouch having achieved this on one day of study per week. By 1980, we would be looking at a minimum of a 2.2 university degree to be sure of basic technical ability only to find at interview that half of them were like the joke: 'Six munce ago I cudn't even spell injuneer and now I are one.' For commercial laboratories, the great majority of PhDs on offer were simply not suitable, the good ones having been snapped up by large companies.

It is only fair to say that I have a certain degree of bias because, for economic reasons, I left school early and took a HNC via the part-time route. After HNC, I did a further course in reactor physics but, having got my first job in the polymer industry, found no reason to obtain further bits of paper. One I had a foot on the ladder, nobody was interested in qualifications other that the ability to do the job, which is what really matters. It gave me much pleasure to be elected a fellow of the Plastics and Rubber Institute in 1983 as I claimed (probably wrongly) that apart from a certain Royal I was the only fellow without a degree. The institute, now called the Institute of Materials, Minerals and Mining (10M^3), gave me another unexpected pleasure when I was awarded the Hancock medal in 2006 for services to the rubber industry. I also find it amusing, but is probably a sad reflection on something in society, that a lot of people think you cannot be editor of a learned journal if you are not a professor, so I am addressed in that way more often than as mister.

For years, the standard starting point for laboratory assistants (later transformed into technicians) was five ordinary ('O') levels. Then we got General Certificate of Secondary Education (GCSE) and it reached the point where the bits of paper proved very little. My colleague had to introduce little tests or, if you like to do the schools' work for them. I particularly remember one girl making the rest of the candidates look most inferior but she had the fewest school qualifications. The point of formal qualifications is that they should allow you to concentrate on character and other attributes appropriate to a particular job.

I should not leave the subject of interviewing without mentioning two instances, one I think in my favour and one not. I always used the job application form to structure an interview. Reaching the section on hobbies and interests one undergraduate had a perfect set of all the right things – sport, music, and practical skills – so I asked whether he got pissed on a Saturday night like the rest us. He answered 'Sorry, I thought you took that for granted.' I said 'Hire him', and of course he turned out to be brilliant. In another case the selected candidate turned us down so we looked at the applications again. One stood out as better than anything so I asked why we had not selected him. My colleague said that I had remarked that we were not going to hire 'A poncey thing like him'. He turned out to be excellent and it transpired that he had been feeding cows, remembered the interview date, and hastily bathed using his sister's toiletries and put on the new suit reserved for her wedding.

In all this selection of personnel, there was never anyone claiming to have a qualification specifically in testing polymers, or for that matter in testing anything [3]. This means that people come to testing by what might be said to be an indirect route, and at whatever level there has to be a training process. When I entered the industry, most (if not all) the senior laboratory staff came via rubber or plastics technology courses with a chemistry orientation. In a research environment like RAPRA, physical testing had some technologists but most were physicists, including one new graduate who did not believe in electricity. At some point, the discipline of materials technology was introduced, which seemed to signal the watering down of degree content such that it became broader but did not really befit anyone for anything.

Figure 9.1 When you are developing new equipment you have to train yourself

New assistants had, of course, to be given practical training on the job. In the polymer industry, this was usually fairly specific to the tests carried out and did not include the broader training I received in the United Kingdom Atomic Energy Authority (UKAEA;

see Chapter 3). It was also normal in the 1970s for most of them to have day release for further education. Even study for the Institute of the Rubber Industry (IRI) exam was available locally when I came to RAPRA, but now there is nothing polymer within commuting distance. Later, management became less generous in granting time off. I cannot remember when the change from laboratory assistant to the term 'technician' came in, but it recognised that after training and further qualifications they were very skilled people.

In contrast to day release being less available and deficiencies in the skills of graduates, training has become very much in vogue in the last 10–15 years [4]. The roots of this trend seem to be in the quality assurance and accreditation movements, which are answerable for a number of things. It seems that now you have to get a certificate to prove you can wipe your nose, but the fact that basic training in laboratory skills has been missed out is overlooked. More important than having learnt first-hand from a real expert is that you have a signature on a training record. All my training in polymer testing came from practical instruction from people who were highly experienced, supplemented by reading the standards and textbooks. I have kept my white coat but, sadly, I do not have the souvenir of a training record.

Rather curiously, we are told there is a shortage of skilled people in many areas and you need not look far to find someone bemoaning the perceived decline in practical skills of new graduates. It then occurs to you that much of the training being touted around has nothing to do with the basic knowledge needed to do the job. It is not about learning the 'trade' but about supposedly essential add-ons that relieve stress and/or look good on the *curriculum vitae*.

The microscope is pretty much taken for granted and considered by many to be a 'simple' instrument, but a professional microscopist was quoted as saying that if a group of regular microscope users was asked to set up an instrument for optimum image almost none of them would be able to achieve a faultless image. Part at least of the problem was said to be lack of teaching. People think that the microscope is 'simple', so they assume it does not need to be taught. They therefore concentrate on more exciting things with bells and whistles or where the knowledge may have a better pay-off.

Is this the same for many other subjects? So many apparatus are automated and virtually run themselves that they can be operated with relatively little knowledge or understanding. Until of course they go wrong. With ever-greater pressures on time in the name of efficiency, which is a term for cutting expenditure, and expectations of people being higher in terms of wanting rapid progress, it is probably not surprising if insufficient time is spent on learning the trade thoroughly. We could end up training people to cope with the problems and errors which occur because of lack of in-depth training to carry out the job in the first place. In fact, some quality procedures look a bit like that already.

Not unrelated is the chicken-and-egg situation of who trains who. The manual says that staff shall be trained and examined and the records entered before they are let loose on the apparatus. The senior person therefore trains the junior person and everyone is happy. The senior has of course been trained by the more senior. There has to be someone who starts this chain. In practice, particularly in experimental situations, the assumption has to be made at some level that the person is competent to make the measurement by virtue of his or her educational background and ability to understand and work out the right procedures using whatever information is accessible. It is assumed that this includes thorough grounding in the basic skills. Something tells me that if the basic training is lacking there could be a lot of wrongs perpetrated and quality could be on a downward spiral.

At times over the years, RAPRA provided a training service. The expensive, but very thorough, way was on a more or less one-to-one basis whereby a trainee came for several months and learnt all relevant tests, including carrying out a small research project. This relied on very generous funding from the likes of the British Council. Later, we ran two day concentrated training courses based on lectures rather than practical involvement. These courses were quite popular for a few years but they faded out as companies became less enthusiastic about paying real money.

Interestingly, however, laboratory staff do not have to be accredited to do their job, whereas tradesmen such as plumbers must [5]. No doubt it will come. This editorial arose from my brain managing to make an obscure connection between two very different news stories. In our local paper, there was a report of a plumber who was convicted for testing for a gas leak with a lighted match. Apparently, he was experienced but not formally accredited or registered as required by UK law. Strictly, it was the lack of accreditation that he was guilty of, although the court took a dim view of the match experiment and the newspaper found it the more interesting aspect. Actually, in a well-ventilated room, using a naked flame to find a leak is probably no different to turning on a cooker and lighting it in the normal way – but one cannot really expect a magistrate or a newspaper to see it in that perspective.

The second story was in a technical newsletter reporting that it was the tenth anniversary of the United Kingdom Accreditation Service (UKAS), the body responsible for laboratory accreditation, and saying how successful it had been over this period (accreditation is rather more than 10 years' old but UKAS succeeded previous organisations).

The slightly twisted connection came from the thought that a gas appliance may use a polymeric hose or seal, the quality of which will be proved by testing. The technician carrying out the tests may well have more qualifications than the plumber, but there is no regulation that requires he or she to be accredited as a *bone fide* responsible tester. Considering the potential financial value and the safety issues hanging on incorrect product evaluation, this seems a little like double standards, with the plumber being treated more severely. Not that a license to operate a tensile machine would carry too much kudos, although it did remind me of the notice saying 'Before operating this machine a manager should seek advice from a technician.'

As regards the plumber, we had a new gas cooker fitted recently and the delivery man said he was not allowed by law to disconnect the old one – I have applied for a degree in undoing a gas bayonet fitting with an endorsement in attaching a blowtorch to a propane bottle.

Appropriately trained and experienced laboratory staff are very valuable assets at all levels [6] and when one leaves the work is inevitably disrupted. Predictions of an increasing shortage of skilled staff have been raised for almost as long as I can remember, but it is amazing how replacements have kept coming. Nevertheless, a lot of knowledge and experience has been lost from the industry over the years, sometimes actively prompted by management. In fact, one gets the impression that as managements become less technical and more commercial, they show less concern over the effects of lost expertise – those that remain will cope and a new person will soon learn, and they often get away with it.

One inevitable loss of expertise is through retirement [7], but it seems strange why so many people have been forced or allowed to go early. It seems like wanton waste. It makes you think that at one time knowledge was considered intrinsically valuable but now it is valuable only if it can be used to make money. In fact, early retirement from our industry is not particularly new. Somewhere in the region of 20–30 years ago, very large companies such as ICI had massive downsizing of technical departments, although the severance terms were very good (including subsidising the salary of people going to smaller companies). In the last few years, early retirement seems to have diminished greatly because of problems with pension schemes making it unviable.

In fact, much of the early-retirement expertise has not been immediately lost as the people have turned to providing consultancy services due to economic necessity or because they had no desire to stop working. Companies may still be managing to replace lost expertise, but precious little of the new talent is going into standards work. Most of the polymer committees would fold up if it were not for the input from retired people This supply of labour will only last a few more years, but no doubt the process of evolution will in some way or another accommodate the inevitable change.

One of the obvious very fast areas of change in recent years has been computers – they should be loved because they are the only thing ageing faster than you [8]. Computers are relatively young and moving quickly, but when things are mature they slow down, as we have seen with the introduction of new and revised standards and, unfortunately, with the renewal of apparatus. Just like people really.

In fact, the bottom line is probably that an overall process of continual change has always been with us and always will be. In detail, some things may speed up and others slow down, and how you view it will depend on where you are on the evolutionary cycle. An Editorial in 1992 [7], discusses how old men will claim that nothing matches their knowledge and wisdom, whilst the young will say that times have moved on, it is different now and we know better. This will apply to technical matters, training and management, and in that sense nothing changes. The trick is to understand that change is a good thing if it is called continuous improvement.

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10 Economics and Automation

In the early 1980s, when the *Polymer Testing* journal was very new, the UK went through a period of economic recession. Quite simply, money being in short supply means that economy measures need to be taken, and development and research are often the first areas to be curtailed [1]. Enforced early retirement, discussed in Chapter 9, is one of the measures that is highly likely to feature in management thinking at such times. As *Polymer Testing* approaches the 30-year mark, things have gone round at least one circle and we are again in a period of great economic problems [2]. Interestingly, the perception this time is that in industry generally a disproportionate number of casualties have been in supervisory and management grades. Maybe it is just possible that the logic of investing more in technology to improve competitiveness in times of hardship will not be forgotten.

Properly applied, automation is a powerful tool for making processes more efficient and cost-effective, and it has the bonus that it can eliminate human error at the same time. A lot of the automation we have today, particularly computers controlling almost everything, is taken for granted but it was not always like that. About the only thing automatic before the 1950s was relatively crude temperature control of ovens. From then until 1980 saw the introduction of many examples of automation applied to test methods, including thermal analysis, servohydraulic test machines and various forms of extensometer. Although early-prototype computer-control of test machines was seen as early as 1970, it was not until the 1980s that they really became commonplace.

At the 1982 Rubberex exhibition [1] it was noted that test-equipment suppliers were there in considerable numbers and almost all were displaying automation. Pets and Apples were everywhere, and it was clear that computerisation was a trend that would not be reversed. Indeed, this could be said to be the start of the electronic revolution. Just as in office activities and communication (see Chapter 5), computers rapidly became incorporated into many forms of test apparatus. It is probable that this trend did much to aid the health of the test instrument companies during difficult economic times because in 1982 they appeared to be in better health than the economy might suggest. Also, no sooner did this trend get under way than there were queries as to the costs of maintenance and possible calibration problems.

It has been mentioned in Chapter 2 that at the Rubber and Plastics Research Association (RAPRA) we had a Mathatron computer in the late 1960s which was used with prototype automated test equipment, notably a hardness and density apparatus. However, its main use was in the calculation of results. Even by 1979 computers were not that common in test apparatus, and it has to be remembered that they were far less user-friendly than now and a great deal more expensive. In the 1970s, we used Apple computers to study the application of computers in polymer technology, but memory is very hazy as to which models first started to appear in such apparatus as tensile machines. Neither can I remember what we used for processing results after the Mathatron, other than a rather strange portable, programmable calculator which printed on a roll of paper.

Figure 10.1 Data manipulation circa 1969 – Mathatron computer

It is a measure of how forward-thinking some people were at RAPRA that considerable expenditure was made at that time to develop computer applications in the industry. The work was rather loosely contained within the testing department, although most of the effort was directed at non-testing applications. My memory is that it took a long time to produce very little, although I also remember championing the expenditure against reluctant management.

In the 1980s, computers controlling test equipment increased, with the IBM and its clones becoming the norm. However, in the dynamic testing laboratory we used first a Sirius and then Apricots, which did not become IBM compatible until later. Steve Hawley remembers that initially we also had Apples for monitoring ovens, but that we went straight to personal computers (PC) for office work. He also recalls an original IBM with a green screen that suffered badly from static controlling a servohydraulic tensile machine. The static was so bad that certain staff simply could not use it because it crashed in their presence. IBM compatible machines also became more common for office applications and took over the handling of test results. It is interesting that when the Mathatron was used for calculations, one lady did all the processing, but with the advent of PCs all technicians dealt with their own results. The mindset is probably that in the days of mainframes you had a computer whereas the term 'PC' speaks for itself. Another thing Steve Hawley remembers is that keyboards at that time did not have a separate numeric keypad, which made number crunching a painful job.

By the early 1990s, all new major test equipment was computer controlled and it was becoming the norm for most people in the laboratory to have their own PC. There was also a step change in usability when the DOS operating system finally gave way to Windows; the first version we used in the RAPRA laboratories was 3.1 in 1992. All the computer controlled apparatus were stand-alone but, when we had a completely new physical testing laboratory in 1995, there were commercial network systems available with centralised facilities for storage and processing of results. The costs were too high for us to contemplate and such systems are better suited to a high volume repeat test environment rather than our great diversity. Not long after, the explosion in the use of email and the World Wide Web (www) meant that we had a network installed which connected workers but not machines.

A few years earlier we had pioneered transferring results between two geographic locations by use of telephones and modems, but if I remember correctly it was none too successful. Just 15 years later, everyone and their grandmother could routinely exchange data at speeds that were previously unbelievable.

The greatest claim for automation is that it saves time and money. However, proof of this is not always very clear [3]. Although there are many very clear-cut cases, there are others where the gains are rather less obvious, particularly if extra maintenance and calibration are factored in. What is perhaps surprising are the countless claims of greater efficiency but the apparent complete lack of documented proof. Perhaps 'spin' started in the laboratory. However, automation in so many forms is now taken for granted so there can be no doubt that overall it has greatly improved efficiency. From the technical point of view, the second gain of reducing human error and variability is even more important and has enabled considerable improvements in many test methods.

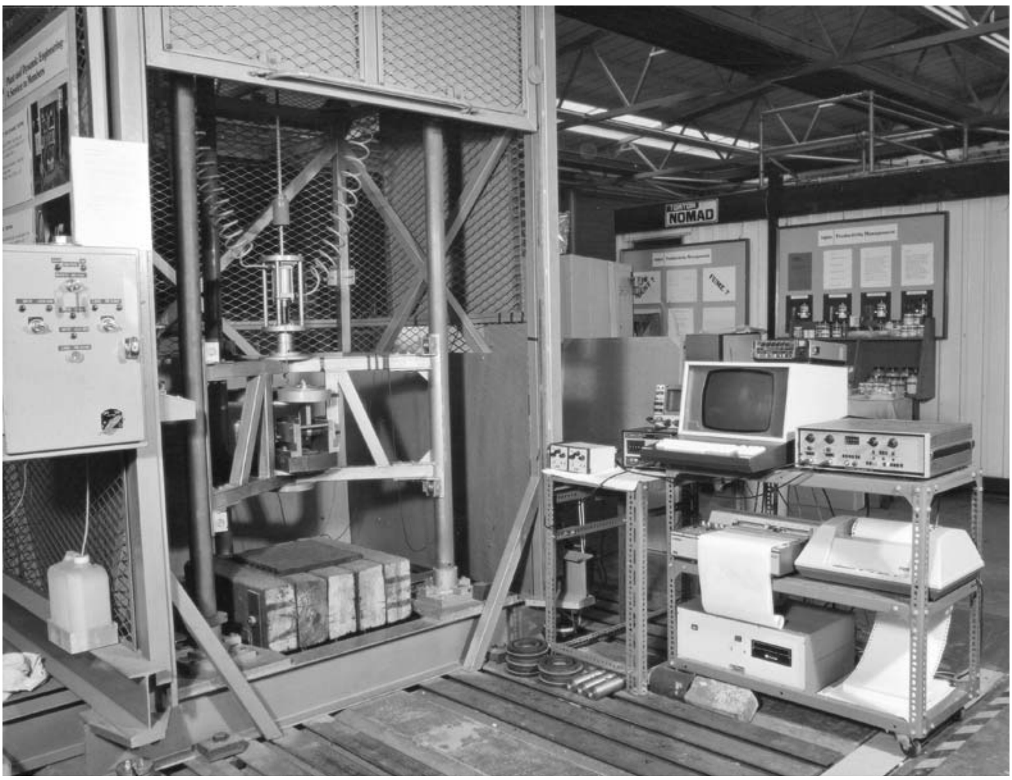

Figure 10.2 Early automation with computer and chart recorders

One aspect of automation that has not developed as many people expected is in sample handling. As far back as 1970, prototypes of robotic hardness and density testing, as well as tensile machines, were made but have never caught on widely. It is probable that the simple reason is that for such a degree of automation to be worthwhile you need to test an awful lot of samples. As regards hardness and density, a rubber company would then have been testing every batch of material and generating a lot of results but it could be that the introduction of curemeters lessened this need.

A rather more futuristic concept related to automation aired in a rather light-hearted manner [4] is remote working. The logic of examples from the pub trade and use of dumb terminal computers was applied to a testing laboratory. If we consider that a laboratory plus technicians equals test results, we also have to appreciate that it intrinsically means high costs. Applying the logic of the blade computer, we could remove the laboratory with its equipment and place it centrally. Our technicians can then use the equipment by remote control and produce the same results as before. The great beauty is that other groups of technicians can also use the equipment and, by a process of time-sharing, the cost might be much reduced.

The objection that equipment can be used only by one person at a time is easily countered by the fact that the real gain is for equipment that is used relatively infrequently; popular tests would need multiple units. The other objection is of course that the technology, including robot test-piece loaders, to operate a remote laboratory is not available nor is likely in the near future. Never mind, this is not as stupid a notion as some arguments for cutting testing costs that are inflicted on laboratories.

The other novel aspect of the testing equation is that you could alternatively remove the technicians and, instead of the logical end that you are left with a laboratory, the results still appear due to the 'magic' of automation. This is not known to work for long but is a very attractive idea to management.

More important than playing games with what happens when you subtract an element from the testing equation is that it draws attention to the fact that test results are produced by a partnership of skilled people and clever apparatus. You might be able to juggle their relative numbers but you cannot afford for either to be weak if you want to maintain high standards of output.

The idea of remote working was barely imaginable 40 years ago and not taken too seriously in 2000 [4] when I was still waiting for the chance to drive a remote tensile machine from my home computer. Nevertheless, if you had equipment that could be shared by more than one set of people, operating it by remote links would be extremely attractive from a cost point of view. By 2007 [6] it seems that this concept had become a reality because there was an article in *Materials World* journal [7] that workers at Imperial College, London (London, UK) and the Oak Ridge National Laboratory (Oak Ridge, TN, USA) could use equipment in each other's laboratory via a high-speed link. The cost of this set-up was said to be rather less than the cost of the two pieces of equipment involved.

If testing becomes more efficient through automation, one would expect it to be reflected in the charges made by commercial laboratories. By 1990 the feeling was that costs and, hence, prices had increased above inflation [8] rather than decreased. The argument was that what had been gained in efficiency was more than negated by the increased costs of labour, more sophisticated apparatus and greater quality assurance.

It was claimed that you could not replace even the most mundane of things without spending more than would have bought a laboratory a few years ago. There was no cheap apparatus anymore, it had to be automatic, have a computer; throw on a further kilo of money for the software and a bit more for the smart presentation. This would not be so bad if the apparatus never broke down or took longer to become obsolete. Being more sophisticated, their state-of-the-art disappears faster than teenage fashion. There is more to break down, so it does so more often and you can no longer make repairs with string. (We had string in our electrical permeability apparatus until the last decade). The man who comes to mend it costs more than your testing scientists and, before long, he will tell you the parts are no longer available. There is a certain feeling of being 'had over a barrel'.

This trend has continued so we have the rather bizarre situation of a relentless drive for greater efficiency continually being undermined by increased salaries, ever-more complicated equipment and the imposition of more and more control measures. Going back to simple mechanical equipment may be out of the question but there does seem something wrong with the system.

The suggestion that modern test equipment does not last as long as the old equipment is not to say that the standard of engineering has declined, but that the pace of development demands continual change and the economics of repair favour replacement. Continual change hopefully means continual improvement, but a downside is that it is not a great help to laboratory efficiency, including the effort of seeking funding for replacements [9]. It takes a lot of effort and considerable skill at convincing people they will enjoy a payback to raise money for equipment or, for that matter, anything else. However, the annual round of begging for the capital budget was probably invented by Noah – and he had to build his own boat.

The rise of the quality movement, resulting in greatly increased control measures and the accreditation of laboratories, has been significant factors in increasing the cost of testing. Some of the factors needing control are directly due to computerisation [10], which adds credence to the 'swings and roundabouts' argument. The example considered in the editorial was the introduction of stringent regulations regarding the use of word processing for reports in accredited laboratories. Again, it is rather difficult to find quantification of the costs or measures of the effectiveness of the regimented procedures. It was also noted that a far worse situation for potential error is probably in the processing of raw data by a computer to yield the quoted test results, and the hope was expressed that inevitable control measures would not be too bureaucratic. In fact, there has been some evidence that accreditation bodies have matured and improved in the commonsense department.

An aspect of quality that has proved particularly expensive is calibration [11]. Costs rocketed from a nearly zero baseline prior to 1970 to a sizable proportion of the salary bill by 1990. An estimate was that it added at least 10% to the cost of tests in a commercial laboratory, whereas one example claimed that the annual cost of calibrating an oven was more that the cost of the oven. One galling aspect of calibration costs is that you can go on getting more and more finicky about the detail, but there must be a point where the cost effectiveness cannot be justified. Many would argue that this point was reached some time ago.

Also around 1990, it was felt that the costs of standardisation had been an increased burden on industry [12]. In retrospect, this was largely a biased view from a UK Research Association because previously RAPRA was heavily supported by government funding to do standards work on behalf of industry. As in so many things, government support consistently decreased, which seems particularly unreasonable for standards because only a minority can fully take part and, hence, bear the costs, but the whole country benefits. The fact that effort put into standardisation has dwindled in many countries can in large part be blamed on the lack of central funding. Perhaps the model of trade or research bodies doing much of the work on behalf of industry was just too fair and efficient to appeal to governments.

After a period when editorials concentrated on costs, the subject changed to what was free [13]. This meant what the polymer community gave for free in the way of advice, help, the sharing of knowledge and contribution to such things as standards. In fact, it turned out to be more depressing than the consideration of costs because it concluded that commercial pressures meant that testing personnel had less time to contribute to a whole range of activities. An example of lack of funding restricting a worthwhile initiative was the Versailles Project on Advanced Materials and Standards (VAMAS) project [14]. VAMAS is a memorandum of understanding initially signed by Canada, France, Germany, Italy, Japan, UK and USA to undertake international collaborative projects aimed at providing the technical basis for drafting codes of practice and specifications for advanced materials. As such it is a great idea for cooperation in prenormative research which would be expected to give great support and impetus to international standardisation.

VAMAS work is carried out by a series of Technical Working Areas (TWA) which cover a wide span of subjects from superconducting and cryogenic structural materials to materials databanks. Three TWA were specifically concerned with polymers.

TWA 12, of which I was chairman – a position doubtless gained through some lapse in concentration – was concerned with Efficient Test Procedures for Polymer Properties. A number of very knowledgeable people joined this group and I dare say more would have liked to contribute. Generally, they received no funding to take part and no research monies to carry out a share of the projects which were set up. Consequently, they found it extremely difficult to find time for significant practical contribution. As a result, the initial quite modest work programme proceeded slowly and enthusiasm for new projects was dampened. It is not difficult to imagine that useful work in this area is easy to identify.

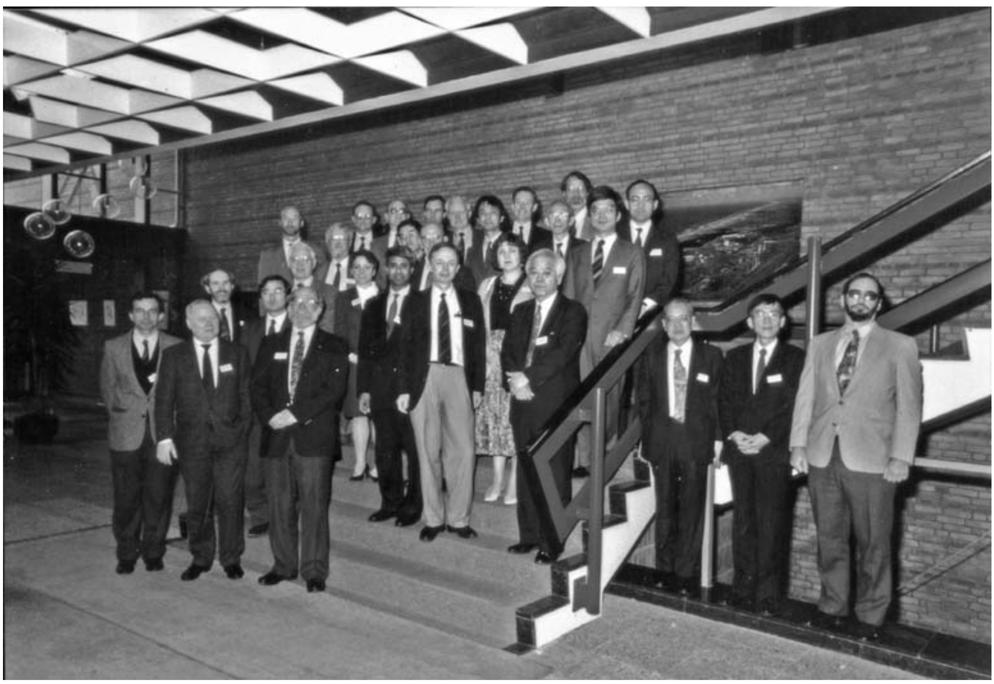

Figure 10.3 Delegates to the VAMAS meeting in Petten (the Netherlands) – Governments forgot to fund the practical work

This is another example of where scientists would be only too pleased to contribute to the improvement of knowledge but it was a sign of the times that not many are given the time to work for love. It is also an example of a very good idea for cooperative research which has probably achieved far less than it might have done through lack of adequate funding. International standards had been struggling for support and the VAMAS initiative was just the sort of input it needed. It was a nice example of politicians setting up a grand cooperative exercise but forgetting to give it sufficient funds.

The cost of testing has been a perennial topic of interest and continued to surface in editorials in various guises. This included a light-hearted look at the relative expense of different approaches to producing a new product [15] and the human tendency to cut corners to save money. Most people will have heard the story about the chemist, physicist and engineer asked to add up 2 plus 3. The chemist says about 5, the physicist reckons 5±0.1 and the engineer answers 5 but we had better make it 9 for safety. This may be a grossly exaggerated tale of the different reaction of the three disciplines but it does illustrate the point that ask different people the same question and you can expect to get different answers depending on the person's experience, training and how they approach the problem.

The point can be made, perhaps with less exaggeration, by considering how technical people might respond to a question such as 'How thick should I make this plastic sachet for packaging fish?' The technologist could very likely say that it should be in the same material but just a bit thicker that the one they have been using successfully for vegetables. The testing person will naturally tend to an answer which involves making several sachets of different materials and thickness and testing them to simulate fish loading. We can have confidence that the design engineer will want complete details of the expected stresses together with comprehensive data on the properties of the candidate material so that he can go away and calculate the necessary thickness – plus of course a safety margin.

Assuming we can find a design engineer with the necessary skills, he has the problem that to do his calculations he needs the correct material property data. As we know, a common complaint is that there is a lack of design data and a need for better tests which yield data more suitable for design purposes. Any lack of such data and test methods is often attributed to the technical difficulties and effort needed to develop adequate tests and the cost of data generation. Perhaps the problem is not so much the technical difficulties being large and the cost high, but that there is a lack of incentive to make this investment when the lack of data can often be compensated for by the technologist's experience and the tester's ability to get answers from prototype products.

Quite clearly, the technologist will be cheapest – although there was a substantial cost to get his experience – but the approach will not work when the new product breaks completely fresh ground. Neither does it optimise the thickness in cost/performance terms. The tester's approach could be called a bit hit-and-miss and will not be viable for complex or large products. However, in many cases it is less expensive than proper design and can be used to optimise the solution. The engineer's approach may be 'correct' but is likely to be expensive and if the expense can be avoided the commercial manager will be attracted to a short cut.

From a testing point of view, it can be observed that we may be needed whichever approach is taken. They may not believe the technologist without a trial. The engineer needs data and will probably want to have his design proved before going into production, particularly as the other element often said to be lacking is accurate design methodology. Unfortunately, any testing costs money and everybody looks for a short cut around that. What happens when they stop investing in training technologists?

There was a suggestion that testing is not seen as an activity that adds value, and testing staff can feel undervalued [16]. In universities this is perhaps to do with academic status – doers not thinkers. In industry it can be linked to perceived added value. The process of inspection is widely regarded as an activity that does not add value. At best it finds defective goods that should never have been produced in the first place and at worst it costs money but achieves nothing. The quite logical argument is that inspection becomes unnecessary if the manufacturing process is properly controlled.

Routine quality control testing is essentially an instrumented form of inspection and, hence, shares its status as a commercially undesirable activity. It is but a small mental jump to go from inspectors to quality control technicians to all forms of testing staff and categorise them as not adding value.

Such thinking does of course ignore the fact that to successfully eliminate inspection you need a great deal more measurement to produce data that enables a better quality product to be consistently produced. Data are needed about the process, the quality of the product, and material properties for design. Data need to be higher in quality and reliability and often have to be produced more quickly, which means investment in apparatus and expertise. In research, the thinking remains ‘hypothesis until proven by measurement’.

The trick to being more highly valued must be to produce higher-value results. Value has nothing to do with cost or quantity but the added value that the results give to production, profits or the aims of research. Perhaps more basically, the trick is to ensure that your results are perceived to be of higher value. As a technician I achieved a measure of usefulness by being the only one who could make boron trifluoride counters with a 100% success rate. The trick to avoiding spurons being counted took a few seconds but I had no problem assigning several hours college work to each counter.

Perhaps testers need to follow the modern trend and get a publicity agent to shout loudly and often about their great importance in the community.

Because of the cost, there is considerable incentive to cut the amount of testing done, and that has been common in practice. It can be argued that unexpected failures could in many cases be traced to a lack of testing [17]. It is a fact of life that sometimes products suffer from unexpected failure – and there is a well known law that says the failure will occur at the least convenient moment and under the most unfortunate circumstances. There is also an argument that if sufficient testing of the right sort had been carried out at the design, fitness for purpose and quality control stages then products would not fail, or at least fail much less frequently. Certainly, many, if not most, failures could have been prevented by adequate testing. In fact, I can recall several failure analysis investigations where that was indeed the case, but it is open to debate as to whether such instances have increased over the years.

A problem is that it may be difficult to see any short-term added value in testing. Sadly, there have been clear signs that many companies have taken every opportunity to cut back on their testing facilities and the amount of testing they undertake as a simple and apparently easy way to save money when economic pressures are high. Quite a few have found that this does not pay in the longer term.

There are several reasons or purposes for testing and one of the lesser ones (in terms of how often it should be needed) is to investigate failures. Actually, it has been joked in commercial testing laboratories that what they lose through companies cutting back on design and fitness for purpose test programmes they regain from failure analysis.

The speed of producing results is directly related to cost, so it is not surprising that companies continually seek ways (including automation) to increase productivity. There is no doubt that considerably more results are now produced per technician hour than 20 years ago, and more again than 50 years ago. In fact, the world was generally a much slower place back then – and I am in a position to personally vouch for that. We generally worked at a gentle pace, investigated interesting results, added measurements to increase the technical value and discussed our findings. One aspect of the increased pace is seen in impatience in waiting for data [18] which can result in some laughable demands. Everyone in commercial laboratories (and probably many in quality-control and research laboratories) have stories of clients who, having ordered a three-week ageing test, telephone after two weeks to ask where the results are, and they are not necessarily pleasant about it. It is not so many years ago that a turnaround time of three weeks for a very modest piece of testing would be thought of as rather good. Waiting two weeks for a quote was not uncommon.

Times changed and laboratories, like everyone else supplying a product or service, were expected to meet shorter and shorter delivery schedules. First fax and then email encouraged a climate where virtually instant response is the norm. If people can reply instantly, why can't they produce test results instantly? We are back to explaining why you cannot have three weeks ageing data any more quickly.

There is nothing intrinsically wrong in striving, or being forced, to improve efficiency so that results are delivered faster, and perhaps simultaneously improving other aspects of the quality of service. There is a problem in that it produces more stress and makes life for the laboratory staff more difficult. That, it can be argued, is the way of world and why should testing staff be any different?

Perhaps there is a more important problem relating to technical quality. If pressures for speed in scientific work are met by improving the efficiency of the operation it is likely to include the cutting out of frills and anything which is less than absolutely essential to the brief. There can be no time to investigate interesting results, to modify the programme in the light of the information found or to add measurements or interpretation which might increase the technical value.

In manufacturing industry, all manner of organisational tools have been developed to aid the quest for efficiency. There have been things like Manufacturing Resource Planning (MRP) and then Enterprise Resource Planning (ERP), not to mention Finite Capacity Scheduling (FCS) and several other systems, each claimed to be more powerful than the last. All these are related to faster turnaround and on-time delivery becoming even more important that price. Perhaps someone will develop systems in this vein for testing laboratories to pursue the notion that speed is more important than knowledge.

One editorial [19] waxed lyrical about how in retirement you might be able to turn the clock back and have time to follow up interesting sidelines or try out a novel test method. A snag would then be lack of facilities, but in practice retired people have been conspicuous for the absence of articles they contribute, which indicates that the interest evaporates or retirement brings its own treadmill. As Ron Norman told me when I chased a book chapter, 'You must understand that retirement is a full-time occupation'.

With the lower pace years ago it was probable that work often expanded to fill the available time, whereas by the turn of the century it was more likely that work had to contract to fit the time allowed [20]. In many cases it would perhaps be more appropriate to say that work had to contract to fit the available budget, resulting in test programmes being curtailed or even cut completely. Hence, a more general version of Parkinson's Law is that work changes size to fit the time available. It is very difficult to judge exactly how much testing is really necessary in a given circumstance, but 30 years ago the tendency would have been to err on the side of too much, whereas now it would be to err on the side of too little – which brings us back to the prevention of failures.

A good illustration of the changes in the economic climate can be seen in the '*RAPRA Long-Term Ageing Programme*' mentioned in Chapter 2. In 1958, far-seeing technologists put 19 rubbers and 9 plastics into controlled storage at sites in Australia and the UK representing hot/wet, hot/dry and temperate climates [21]. Sets of test pieces were withdrawn at intervals to get measures of the long-term durability of the materials. Up to and including the 20-year report, the quite considerable testing costs were met from RAPRA general research funds with the exposure provided courtesy of the Ministry of Defence (MOD). In contrast, the 30- and 40-year testing required a major fundraising effort, and results were initially restricted to sponsors. Economic factors dictated that the substantial reports at the end of the programme were available only to subscribers and the valuable information gained has not appeared in the open literature [22].

In 2006, within a few months of an editorial commenting on the immeasurable contribution to testing by Dr J.R. Scott, RAPRA Technology was bought by the American Smithers Group [23]. Dr Scott worked at RAPRA for many years and was its Director of Research over a period when very significant contributions to advances in rubber technology were made.

This event prompted thoughts of the origins of the Research Association back in 1919 and the valuable contributions made by its research in the earlier decades. Although a particularly British institution, RAPRA became an internationally known polymer research organisation with members from many countries. It started as the Research Association of British Rubber and Tyre Manufacturers (RABRTM) in 1919 with rented accommodation at University College, London. It got its own premises in Croydon in 1921 and the name was shortened to RABRM. Many years later, the scope expanded to include plastics and the association moved to a much larger site at Shawbury in 1953. With the inclusion of plastics, the name changed to the Rubber and Plastics Research Association (RAPRA) which was later simplified to RAPRA Technology.

The idea of Research Associations arose from the need for reconstruction of industry after the First World War. The model was for cooperative technical centres for industry sectors and the funding was based on a government grant which equalled the contributions from industry. Although the funding arrangements underwent changes, the basic concept of cooperative research with government support continued until the last decades of the twentieth century. It was in this atmosphere of research essentially free from commercial pressures that Dr Scott and others made their great contributions to rubber, and later plastics, test methods.

Sadly, the days of such luxurious funding of research are now long gone. Even university departments have had to develop fundraising skills. No one would deny that such things as the investigation of the effect of test variables or the development of standard test methods are highly desirable activities that benefit the industry as a whole. However, it is a totally different matter to persuade a company that the expense was justified by the gain to them alone, and it is probable that much of Dr Scott's work would not be undertaken in today's economic climate.

Looking on the bright side, a happy ending to RAPRA being bought by the Smithers Group is that even if generous funding for research has long been a matter of history, polymer work will continue there.

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11 Quality

When I entered the polymer industry, the term 'quality control' was used for the tests on batches of material and the inspection of finished products. There was not a lot of testing of the product, nor was there any quality control of the laboratory. There was certainly an element of 'them and us' between the inspection personnel and the production workers, but where I worked it was all very friendly. However, while undertaking Kitemark inspections for the British Standards Institution (BSI) I did once visit a rubber boot factory where operatives were not averse to spitting in the direction of anyone dressed in a suit.

At some point in the 1970s the terminology changed to 'quality assurance'. This was to some degree hype but there was a marked change in attitude. The production staff became much more involved in the quality of what they produced, and ultimately doing their own inspection. The laboratory continued to carry out control tests on the materials used. There was increasing importance of being able to deliver assured quality and, as the 'quality movement' gained momentum, it resulted in quality in the laboratory coming under scrutiny and the introduction of formal accreditation schemes.

One of the first standards was BS 4891, '*A Guide to Quality*', in 1972, with BS 5750 and ISO 9000 appearing in 1987. For laboratories in particular, ISO Guide 25, '*Requirements for the Competence of Test Laboratories*', was published in 1982, and BS 6460, '*Accreditation of Test Laboratories*' in 1983. The British standard was replaced by BS 7501, and EN 4500, '*General Criteria for Operation of Test Laboratories*', was introduced in 1989. It was not until 2000 that the International Standards Organisation (ISO) guide was replaced with ISO/IEC 17025.

The pioneer of laboratory accreditation was Australia. It set up the national scheme, the National Association of Testing Authorities (NATA), in 1947. In the mid-1950s they had 200 laboratories accredited, whereas in the UK I doubt there were many people who had heard of accreditation. It was not until 1972 that New Zealand became the second country to have a scheme, followed by Denmark in 1973. There were only a handful by 1980 and the UK was still among the earlier countries to have a national scheme when the National Test Laboratory Accreditation Scheme (NATLAS) was set up in 1981.

At the Rubber and Plastics Research Association (RAPRA), we were accredited by the Ministry of Defence (MOD) scheme a few years before this. We probably had little option but to join if we wanted to continue getting MOD development projects. When NATLAS arrived we were automatically transferred. NATLAS changed to the National Measurement Accreditation Service (NAMAS), which sensibly included calibration laboratories, in 1994. This changed to the United Kingdom Accreditation Service (UKAS) in 1995.

National accreditation schemes are all very well, but for international trade you need test results to be accepted in other countries. Movements to enable this started fairly early but it has taken a long time for mutual recognition to be anything like comprehensive. The International Laboratory Accreditation Cooperation (ILAC) started as a conference in 1977 and it was not until 1996 that it became a formal cooperation to establish mutual recognition agreements between accreditation bodies. The first agreement was, not surprisingly, between Australia and New Zealand in 1981, with a few more added during the 1980s. The European Cooperation for Assessment was set up in 1977 but apparently it was not until 1992 that we had any hope of a pan-European agreement [1]. In 2000, the ILAC Agreement included 36 accreditation bodies from 28 economies.

Comment in 1981 [2] indicates that in the early days of accreditation some scientists found it difficult to accept the formality of schemes, finding it a form of management and an insult to their integrity. Junior staff seemed very aware of the need for quality control. This was a very healthy sign because many, if not most, of the details of ensuring a high standard of quality are entrusted to technicians. They are not only relied upon to carry out the measurements carefully and correctly, but usually have the responsibility of entering details of apparatus used and its state of calibration. As much as anyone they need to be aware of the need for quality and to be conversant with procedures for updating standards and checking that instruments have been correctly calibrated.

It is surprising how mistakes arise from seemingly trivial sources and how such occurrences manage to beat the best quality control systems. One of the greatest safeguards is to have *all* the staff very aware of, and enthusiastic about, the need for quality control. It is very easy with imposed regimented systems for there to be a lot of paying of lip service. I suspect that this became the case in more than a few companies as accreditation got more and more complicated and restrictive.

There was a hint in this direction some 10 years later [3] with the suggestion that rules and regulations could become a justification in themselves. There is inevitably a measure of bureaucracy in an accreditation scheme which, unfortunately, appeared to be increasing at that time. The illustration was that we had a debate with our assessors as to exactly who was defined as the Head of Laboratory. We had to bow to the written regulations but remained unconvinced that this had any affect on how we carried out our work.

The editorial noted that the trend in standards was for the performance of products to be specified and not to regulate how you achieve it. The message to accreditation bodies was that they could perhaps do well to take serious note of this philosophy lest the rules and regulations become a justification in themselves. Despite all this, I surprise myself in retrospect at how positive the tone generally was about the benefits of the dreaded external accreditation audit. We never failed to make at least one improvement to our system as a result of an assessment visit. An outsider sees things to which you have been oblivious, simply through being too familiar with your own procedures.

There was wry comment on tightening of procedures for getting rid of old samples. At one time we simply burnt everything in the corner of a field. Today, such an action would give people apoplexy, although it would probably cause more trouble from the environmental authorities than the accreditation assessors.

People had become somewhat preoccupied with quality by the 1990s. There was something the matter if a company was not certified to ISO 9000, and all commercial test laboratories were expected to be accredited. One factor that helped the growth in certification was that it made life a lot easier for your certification/accreditation if you dealt only with certified companies and all your calibrations were done by an accredited laboratory.

There was a period when the UK government gave grants to help companies get ISO 9000 certification, and I became a consultant under the scheme. One of the achievements that I rather liked was in a tyre retreading factory where I developed a system with absolutely no paper to accompany the product as everything was written on the tyre itself. I have no idea whether they kept the system. The biggest thing the activity taught me was that it is hopeless trying to become a quality organisation if you do not have support from the top – and several did not. If the top was not committed, the consultant would struggle to get cooperation at the bottom.

A slightly more bizarre case of government funding was when the European Economic Community (EEC) sponsored a small group to go to Indonesia and give lectures plus counselling to the Association of South-Eastern Asian (ASEAN) countries on how to sell into Europe. I was billed as the 'quality expert' with the task of preaching the importance of being able to demonstrate a quality system. However, I have my doubts as to whether anybody benefited greatly from the exercise.

Rules and regulations have continued to increase so that accreditation is now just one necessary evil in a morass of things you have to conform to [4]. There are unintelligible rules for food labelling, long-winded procedures for removing an incompetent worker and seemingly impossible requirements for ensuring that people can work and play safely. Then we have data protection acts, managing risk potential of computer-based systems and even rules for how much of your product must be recycled. The list is virtually endless.

The objectives of all this regulation are well meaning and no doubt convincing arguments can be made that it is essential or that things would be much worse without it. Nevertheless, however good the benefits may be, there is always a downside in that regulations introduce the penalty of time and effort, and hence cost.

It seems that the problem with any form of regulation is that it starts out as a very sensible idea but then people with nothing better to do make it more complicated and compliance more difficult. It should then be no surprise if people become more concerned with compliance than with the underlying need. Just tick all the boxes and it will be fine.

This became evident whilst I was maintaining the RAPRA quality system in that you were encouraged to have your manuals conform to a model, which I flatly refused to do. Very recently I saw how bad this has become when a health and safety inspector just would not accept my individually done risk assessment for a Bed and Breakfast business. She insisted on transferring all the procedures to a readymade box-ticking questionnaire –which was apparently all she could understand.

Way back in time, scientists were totally responsible for the accuracy of their own apparatus and may well have built it themselves [5]. By the middle of the twentieth century, one expected the manufacturer to be responsible for calibration in most cases, although there was still some equipment that you had to devise yourself. In fact, there was not a lot of apparatus that had much calibration after (one assumed) the manufacturer proved it initially. With purely mechanical apparatus, you do not expect the calibration to change unless there has been damage. In my first laboratory, the tensile machine was calibrated annually (although, being a pendulum device, the only problem could be friction) whereas most of the other apparatus was on a maintenance contract with the manufacturer, and I do not remember any calibration certificates. Enormous faith was placed in the manufacturers of weights, thermometers and dial gauges to have supplied accurate goods, and an even greater faith placed in the goods not changing with time.

Figure 11.1 Early attempts at calibrating our load scales

It must have been in the 1970s that we adopted more systematic calibration in conjunction with accreditation schemes, with the requirements becoming more and more onerous over the years. An editorial in 1980 [5] gives the general climate of 1980 when accreditation and calibration needed promoting as they were relatively in their infancy.

Calibration has since then had editorial mention on several occasions. By 1986 [6] it was clear that the requirements had already matured very considerably. Comments were made that any laboratory that had become registered under an accreditation scheme, such as NATLAS, has been forced to take calibration of its instruments very seriously. The realisation that to be sure of accurate results there is a need to prove the calibration of every instrument by a traceable chain back to nationally recognised standards had in quite a short time caused an enormous increase in the effort which was applied to calibration. Accreditation schemes were the main force which changed these attitudes and practices, not only laboratory accreditation schemes, but also schemes which accredit the whole quality control operation of manufacturing companies because these also demand calibration of test apparatus.

There was already some reservation that the schemes could become too onerous and needed to be kept in proportion. Calibration is very expensive and in one sense a lot of it is 'wasted' because most instruments do not require adjustment when they come to their annual or monthly check. The object of calibration schedules is to be as sure as is reasonably possible that all the parameters of the apparatus which affect the result are always correct. It cannot provide an absolute guarantee because a fault or drift could occur at any time, but the calibration intervals need to be chosen in light of the best knowledge available and then adjusted in the light of experience according to generally recognised rules.

The more you get into calibration the more you think of what needs checking, and there is a definite danger of becoming paranoid. The calibration of ageing ovens started with a single temperature measurement in the centre; then it was realised that you need to scan and define a 'safe' volume. For rubbers at least, a calibration of the airflow was added and now the ovens are continuously monitored with several probes in case the airflow changes during the night. This needs computer control and recording, with time spent every day to check that there have been no blips. It is telling that the annual cost of calibrating an ageing oven was thought to be more than the capital cost of the oven.

Comprehensive control is probably justified for some ageing work but there has to be a limit, and commonsense has to prevail in the conflict between perfection and economy. It is a complete waste to over-calibrate; if you are measuring in hours you do not check the clock to microseconds. The levels have to be suited to the measurement and you cannot transfer attitudes from one area to another. Length and temperature in a physical testing laboratory are very different from a full-scale fire test rig; ±0.01 mm on dumbbell thickness and ±0.5 °C on ageing temperature is a different world from ±50 cm from the flame and between 800 °C and 850 °C.

The sentiment was repeated in an Editorial in 2002 [4] in which it was suggested that a general problem with regulations was that they increase in complexity and onerousness until it is uncertain which is most important – complying with the regulations or doing the job. It also noted that with relatively simple test equipment a calibration certificate was charged at more than the instrument.

The first standard for calibration of specifically polymer apparatus was probably BS 5214 for tensile machines in 1975. This was the basis of ISO 5893 published in 1993. A general standard for calibration of the apparatus in all rubber and plastics standards was a mammoth piece of work but was completed as BS 7825 in 1995 [7]. In describing the standard, the tone of the editorial indicates that there were still people who would not think such standards to be necessary, and likely to encourage the 'accreditation police' to demand more. However, the most important thing was that it recognised that a polymer laboratory needed guidance on to carry out calibrations whereas a calibration laboratory needed help as to what needed attention in polymer equipment.

A couple of years later [8] it was noted that ISO TC 45 had decided to produce a standard based on the British work. This was finally completed in 2004 as ISO 18899 and, hence, it is not much more than yesterday that we had a general calibration standard for rubber test methods. Whilst there have been a small number of calibration routines standardised for plastics, sadly, TC 61 have shown no interest in a similar general work.

The rationale for ISO 18899 is that it gives basic guidance on how to calibrate and that each test method standard will have a calibration schedule included that tells a calibration laboratory what is needed. The schedules were part of the calibration standard in the British standard but this is not very convenient for keeping up-to-date. It was expected that in some cases, e.g., tensile machines, there would be additionally more detailed calibration procedures in separate standards. One such standard has been completed for hardness of rubbers by TC 45, whereas TC 61 have produced one on the same subject for plastics. It says not a lot for cooperation and standardisation that TC 61 ignored the rubber work and the two standards do not agree!

The basic approach to calibrating force scales has not changed for many years and is based on making comparative measurements with a standard, essentially statically. However, many tests are carried out dynamically but the validity of static calibration has had to be brushed under the carpet simply because there was no suitable facility. In 2006 it was reported [9] that a project was under way at the National Physical Laboratory (NPL) in the UK to develop dynamic calibration procedures – better late than never.

There are many factors that cannot be calibrated in the normal sense but which could affect results. In consideration of this, it is always prudent to consider very carefully apparently small or even trivial aspects of a test method because they sometimes have a large effect on results [10]. Indeed, it is often argued that differences between operators and equipment unaccounted for in the test method standard and calibration schedule are the source of a significant amount of the variability that our tests methods suffer from.

There are many examples that are well known, even if the effects have not always been fully elucidated. For example, how well the debris generated in an abrasion test is or is not removed can have a profound effect on the abrasion mechanism and, hence, the amount of wear. It is abundantly clear that the accuracy of alignment of the test piece in most destructive mechanical tests is important, but it is generally not precisely measured. There are claims that the level of clamping pressure in brittleness tests affects the material behaviour, although this could well be masked by other effects. Probably the most worrying are factors that we have not yet become conscious of.

The subject of the effect of secondary factors on results was highlighted by seeing a small article devoted to the cleaning of weathering test pieces at intervals during exposure. It is extremely obvious that natural exposure will subject the test pieces to contamination, which it would be reasonable to assume could change the deterioration measured. In fact, one can envisage it having an influence in several ways. Equally, the exposure location will determine whether the level of contamination is large or small, although even in relatively clean areas the length of weathering trials gives plenty of time for significant accumulation of foreign material.

OK, you can correct for contamination by washing but almost inevitably you modify the surface. So, you can formulate a washing schedule which attempts to specify how much water, what detergent and how hard the technician rubs with what type of sponge. The effect of this standard procedure will of course be highly variable depending on the nature and level of contamination you are trying to remove. Before long you are into a circular debate of what is natural, and what represents real-life service conditions – and we have not yet got to how it affects any correlation between natural and accelerated weathering. Perhaps weathering exposure sites should always be in areas where it rains distilled water for an hour every day.

Standard reference materials could be thought of as a rather special case of calibration. The best-known reference materials in the polymer world are probably the ASTM oils – and they are not themselves polymers. Standard abrasives are in the same category. It is very difficult to produce rubbers or plastics that are reliably consistent between batches and/or over time, which is one reason why not many have found widespread use [11]. This problem has been circumvented to a large extent for hardness standard rubbers by each standard block being individually calibrated. A lot of work was put into standard reference plastics many years back but it came to nothing, mainly because of the costs. There are currently suggestions to produce standard rubbers for a number of properties but my experience is that it would be more fun to lose your money on the horses.

There was some pressure to make standard friction rubbers succeed because of the use of polymers in tyres and footwear. Using the standard material wears it out, so the problem of change with time can usually be much reduced. The Transport and Road Research Laboratory (TRRL) standard friction rubber is very old and with passage of time there was some confusion with the specification [12]. The cause appears to be, surprise, surprise, something to do with the rubber formulation refusing to yield the same results as it once did, so the specification was changed, but not everybody were made aware of the action. Hence, it was also a glowing advert for why you should record what happened for posterity.

For as long as I can remember, standards committees have, on occasions, undertaken inter-laboratory tests to investigate aspects of a method, such as different test-piece geometries. Back in the 1970s and before, these trials were often not reported [13] and did not generate precision statements. As many methods were developed as much as 60 years ago, it is clear that the reasons for particular requirements or tolerances have been lost in the mists of time. Again, a lesson in the need to publish the results, and perhaps *Polymer Testing* journal should have been started 30 years earlier!

ASTM was the pioneer of producing precision statements in standards and it was not until the mid-1980s that ISO started out on this path [14]. I am not too sure what pressures started ASTM on the precision trail, but the fact that they were successful in getting data generated is no doubt connected with the size of the USA industry. The broadening of markets had made the discrepancies that could occur between laboratories more obvious and the attitudes could be polarised as, on the one hand, those that were alarmed and, on the other, those who put it down to being the other laboratories' fault. Both attitudes were right in that there was every reason to be alarmed at the magnitude of the uncertainties but at the same time most of the worst cases could be readily traced to lack of calibration or not following the standard in detail. Whatever your view, the bottom line was that the poor levels of reproducibility of most standard methods began to be exposed and the ASTM results could not be ignored.

Alan Veith, who has been a major force in analysing inter-laboratory comparison over several decades, saw improved standards, less operator-dependent apparatus and international accreditation schemes as the ways of improving reproducibility. He felt that the greatest source of improvement was accreditation programmes. He also acknowledged that improvements would be expensive. I would add to that the limitations of the intrinsic variability of polymer materials, which are almost always very significant.

It was clear that in the late 1980s reproducibility was seen as a very serious problem. [15]. Inter-laboratory comparisons had shown reproducibility of quite common tests to be much worse than had been hitherto believed. This was worrying not only from the point of view of pure testing, but also for the validity of specifications and their limits. Some limits are probably tighter than the test will apparently reproduce.

The possible causes of differences could be listed as: material, preparation, adequacy of standard, calibration of machine and operator error. It is perhaps useless to speculate on the relative contributions of these factors as doubtless they will vary from test to test and circumstance to circumstance. There is also the likelihood of interaction between them. It is fairly obvious to see that one approach to improving the situation could involve a vast number of interactive comparison exercises which, quite apart from bulk, would involve horrendous costs. A suggestion was that many of the necessary trials could be relatively cheaply conducted in one laboratory.

The problem figured among a raft of initiatives that surfaced in standards circles. Initiatives concerned with reproducibility showed interesting diversity of philosophy [16]. Prominent among the initiatives to address problems in testing were those concerned with reproducibility, and there is an interesting contrast in philosophy between the programmes suggested in this area [17]. The classical approach is via rounds of inter-laboratory testing and an iterative process to gaining improvements. Essentially, the 'round robin' tests show laboratories how they rate against the norm and the more extreme are encouraged to investigate possible reasons, and/or the test procedure is examined to close loopholes. In ideal situations, each laboratory is physically investigated and there are (rather isolated) instances when great improvements have been achieved through this process. The great disadvantage is the long time scale and very high cost.

A very different approach suggests that round robins are not the best first step. Rather, the test method parameters should be analysed to measure the effect that reasonable (or unreasonable) variations will have on the final result. This will lead to a knowledge of what disagreement is likely and, if necessary, to modification of written standards. This approach has the advantage of being relatively inexpensive in total labour and provides a formal analysis of a method which is useful beyond any participating group. Probably, these approaches are complementary and both could be used to good effect.

An interesting variation on inter-laboratory testing was proposed around this time which likens the comparison of results between laboratories to the comparison with a standard reference material. One laboratory, or one per county, is designated as an 'anchor' laboratory. The anchor laboratory organises a round robin in the usual way but its own results are then taken as the reference. By normalising any laboratory's results to that of the anchor laboratory, even gross differences between a pair of laboratories can be eliminated.

Investigations of reproducibility can be simply to find out by how much and why laboratories differ but when they are run as proficiency testing then they take on more regulatory overtones. One advantage of the anchor laboratory concept is that, in principle, it allows the commercial need for agreement to be achieved, although it may not find out why the original differences occurred.

However, in the event there were only initiatives and no resulting action, due entirely to there being no funding. Several years later it was still being pointed out that the best aid to improving quality of results was to be subjected to an accreditation scheme [18].

The results of inter-laboratory trials to measure reproducibility are in fact estimates of uncertainty. They are of course estimates made in a very general scenario, and probably most people regard them as simply giving an idea of the sort of differences that can be expected. The practice of calculating estimates of uncertainty was well established in the 1980s for calibrations but not widely applied to test methods [19]. However, as the accreditation bodies tightened the regulations, the spotlight moved from more onerous requirements for calibration to uncertainty [18].

The estimation of uncertainty is, with no pun intended, not an exact science. The principle of the classic approach as given in the *Guide to the Expression of Uncertainty in Measurement* (GUM) [20] is simple enough – get the uncertainties of all the possible contributing factors and combine them. In practice this is very involved because of the number of parameters to be considered. Getting figures for some parameters means costly experiments and for others an educated guess is about all you have. Not surprisingly, laboratories were very nervous of the implications of having the requirement to make estimates thrust upon them.

The story of the chemist, physicist and engineer was repeated [21] to illustrate different approaches to making estimates. Perhaps the most important thing is that by then technologists generally had become much more aware of uncertainty – in contrast to 30 or 40 years earlier when uncertainty did not arise. The chemist and the engineer are both recognising that there will be variation even if they are bit vague as to how big it is. Engineers have always had to make an estimate on the side of safety so they come up with a figure to allow for uncertainties, but how they arrived at it may not be too clear. The physicist's figure could be seen as the estimate of the measurement uncertainty, ignoring the variability of the material – the material is the chemist's problem.

This brings one to the sometimes annoying fact that for most polymer measurements in accredited laboratories the uncertainty due to material variation is much greater than that arising from the actual measurement. The only way a test laboratory can reduce the uncertainty of their measurements is to reduce the uncertainty of contributing parameters, such as tolerances on force measurements or dimensions [22]. For example, this can involve cutting the accuracy of a load cell from ±1% to ±0.5% and to use a better thickness gauge that gives ±0.005 mm instead of ±0.01 mm. In light of the uncertainty due to the material, this becomes a case of diminishing returns. Currently, we have reached the point where the only one to gain from more accurate instruments is in most cases the instrument manufacturer.

An alternative to the classic compilation of an uncertainty budget is to use the figures from inter-laboratory trials. This generally produces a conundrum in that the estimates so obtained are rather larger than those from the classic approach of an uncertainty budget. This is much worse if you follow perceived wisdom and use the reproducibility figures rather than the (to me) more logical choice of repeatability. The reproducibility may be contributed to by laboratories having poor calibration but much of it would have to be assigned to unspecified and somewhat mysterious variations in how the tests were carried out – which is a polite way of saying operator variability. However, the relatively low cost and simplicity of this approach makes it attractive if the data are available. It is going to be very interesting to see how the estimation of uncertainty unfolds in the future.

Estimating the uncertainty of parameters such as the force or length is straightforward and is based on the results of a formal calibration of the instruments. Such parameters are functional parameters in that the influence of any variation on the result can be directly calculated through a known relationship, for example that strength is directly proportional to force and inversely to the square of length. More problems are provided by what I have called non-functional parameters – i.e., they are not included in the relationship for calculating the test result and are, hence, not part of the calibration. This includes parameters such as the effect of variation in test temperature on flexural modulus, the alignment of a tensile test piece in the grips or the contribution of the suspension thread when measuring density.

In many cases an intelligent guess says that the factor is negligible but to prove it would take rather a lot of time and effort, or in some cases how to measure/estimate the effect is none too clear. Accreditation auditors are not keen on intelligent guesses and this aspect of uncertainty estimation has increasingly become a problem for accredited test laboratories. To address the problem it has been proposed in ISO TC

45, the standards committee for rubber, that a guide to uncertainty estimates for non-functional parameters should be developed using the combined expertise and experience of the members. If this was achieved there could be some very interesting conclusions – and an auditor could hardly say that the designated experts from about 20 countries are wrong. Unfortunately, lack of input data will probably limit what is achieved. In the absence of hard data, we will have made no advance on the educated guesses of the engineers of old.

The level of uncertainty for the determination of the kilogram is on an altogether different plane to the levels encountered in polymer testing [23]. The kilogram is the only base unit still defined in terms of an artefact, a lump of metal stored at the Bureau International des Poids et Mesures in France. There are several disadvantages to this and a number of national measurement institutes are working towards getting a fundamental realisation. It is also a rather odd situation when you remember that everyday scales and balances no longer have real weights.

One approach is to define the kilogram in terms of the volt and ampere by balancing electrical and mechanical power in a so called Watt balance apparatus. At the NPL in the UK, they had achieved an uncertainty of 70 parts per billion (ppb) by this route, whilst at the National Institute of Standards and Technology (NIST) in the USA had got down to 36 ppb. For us in the polymer industry, a few parts per billion one way or another are really not worth thinking about but for the kilogram they need to reach 20 ppb. However, rather more to the point is that the British and American results differed by 300 ppb, which is rather more than the combined uncertainty of the two experiments.

It does not stop there because another approach is to define the kilogram in terms of a fixed number of atoms in pure silicon, which on then face of it seems a more direct route. Work on this method by the International Avogadro Project (superb title) had yielded a rather larger uncertainty of 300 ppb but was some 1100 ppb distance from the Watt balance.

Although this may be light years away from our levels of uncertainty, the same points are illustrated. Two different methods, for example, for measuring impact strength, are likely to give different answers. Also, it is all very well getting a very precise set of results but if they are not accurate then the usefulness is somewhat limited. Mind you, where we have an advantage is that we have no idea what is the true answer for impact strength or any other polymer property. As for the kilogram, no doubt they will eventually reach their goal.

It is now taken for granted that the test laboratory will be subjected to the same sort of quality system as the factory. There are of course differences and one editorial

[24] pondered on whether or when statistical process control could be applied to testing. That line of thought may not be very productive but it could not have been contemplated before 1980. The changes over time in appreciation of statistics were raised in a general way some years earlier in 1992 [25].

In the 1979 edition of *Physical Testing of Rubbers* [26], I wrote of statistics, 'that a great many technologists lack even a basic understanding of the subject.' The remark was prompted to a considerable degree by my experience in industry where the chemists, as technologists were then called, had almost exclusively come via the chemistry route, and would be perfectly happy to place great significance on a tiny difference. If the tensile strength of one batch of a new material was an odd 0.5 kPa higher (actually a few psi in those days) than the standard compound, this was considered an improvement. The fact that the next batch had decreased in strength was put down to it being just another day and another story. The problem was not that the reasons for differences were not investigated, but that statistical techniques were generally not applied and there was little appreciation of the random variation.

I also wrote that the lack of appreciation of statistics was not due to the lack of published information, but possibly a result of the subject having been neglected in schools and universities. There were plenty of good textbooks and all the basic concepts were covered by ISO, British and probably other national standards. In fact, in 1976 a British standard entitled '*Guide to Application of Statistics in Rubber Testing*' was published (although there was not one for plastics). This guide languished for several years but was eventually revised and enlarged.

By the time the 1986 version of *Physical Testing of Rubber* [27]was published it was acknowledged that 'the situation has been improving enormously in recent times.' By the 1990s the comment would have to be rather different; statistical techniques became much more widely appreciated and used. Probably several factors contributed to the improved awareness and greater application, but the most interesting is the notion that much was due to the market forces of the quality movement.

A theme through all the discussion of quality matters has been the continued introduction of more and more systems and regulations. Another subject to have suffered the same sort of treatment is safety. I was subjected to more safety measures than most when working in the nuclear industry but they all addressed serious dangers that were easy to appreciate and did so in a sensible manner. In a 1960s polymer factory we had fire-alarm practices and laboratory mills had guards (although they did not guarantee an injury-free existence) but there was not a lot else heard about safety. By 2005, a physical testing laboratory was so dangerous that you could not walk through it without safety spectacles and steel toecaps [28].

These things come by degrees. One of the earliest was ‘do not wire your own electrical plug’ (more to safeguard electricians’ interests than your safety). Then safety shoes were encouraged in case you took to juggling heavy weights. The tiniest drop of mercury suddenly had the potential to turn you into a mad hatter, which put pay to traditional thermometers, whilst even the mention of asbestos was lethal. One by one, use of solvents became less acceptable, and then all chemicals had to have formal assessment as to their dangers. By the later 1990s you needed a risk assessment before you did anything. There was at one point a lovely notice that declared that if water was invented now it would be banned as only a few centimetres could be lethal and, worse, the same water could go on to drown several people. None of this does much for the quality of the test results and I cannot see that it does much for the quality of life.

There are some bizarre dangers lurking in the polymer laboratories of the world. A lubricated test piece expelled from the platens during a compression stress strain test could be painful. I suppose one could get a hand trapped in a compression tester. I did witness a tin of beans put to heat for lunch in a moulding press that had a faulty valve. The press crept shut and the result was quite spectacular. There was no mention that hard hats have now become compulsory, but a rather petite young lady did once manage to knock herself out by pulling the handle of a test piece stamping press onto her head.

In fact there is no need to look for obscure misfortunes: there are plenty of potential sources of serious injury to be found in polymer test methods that are quite obvious. How many people do you know who have mislaid fingers after operating a two-roll mill? Heating oxygen at 2 MPa to 70 °C for a bomb ageing test would strike some people as pretty silly, and generating up to 50,000 V for electrical breakdown strength measurement is no better. In the normal world there are penalties for setting fire to heaps of foam but fire test laboratories think nothing of it. Full-scale product tests can be very impressive, but also disconcerting – crushing a glass reinforced plastic car body in a stressing frame, deliberately inducing explosive decompression in a large hose or simply driving a tyre against a large steel wheel at 150 km/h. Ozone is one of the less pleasant materials we deliberately generate, and do not forget the falling weights in impact test machines.

A conclusion I have come to is wear boots and glasses but do not tell the safety people about all of this or we might find that testing is banned.

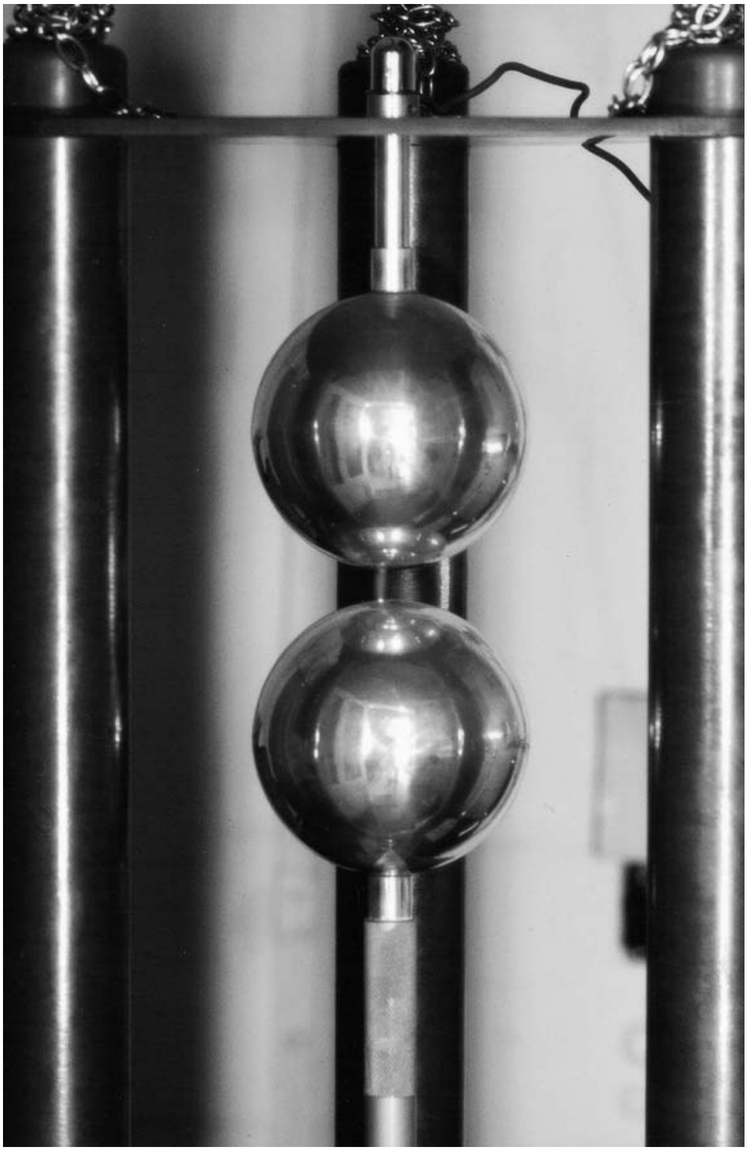

Figure 11.2 Sphere gap for calibrating up to 50 kV – perfectly safe

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12 Test Methods

Here we are concerned with test methods for polymers, but first one could reflect on the fact that if all materials could be tested in the same way there would be no need for specifically rubber or plastics methods [1]. In fact, there would also be rather fewer standards and textbooks on testing, together with no need for *Polymer Testing* journal. As it is we do not just have polymer-specific methods but different procedures for the various classes of polymers. In my experience, the cooperation between the different polymer industries as regards methods has not been particularly good, and interaction with different groups of materials is at a very low level. Certainly, there is sufficient common ground for there to be far more interaction between the different industries, leading to more cooperation and coordination of test methods than we have seen in practice. This is particularly true of the relatively very similar material categories of plastic and rubbers, but even here the interaction has left much to be desired. In some cases it would be very helpful if procedures could be better aligned but the practicalities of getting the necessary cooperation are not trivial.

One area where coordination has been sought is in the specification of tensile test machines and calibration of test equipment. However, initiatives started in the rubber field never penetrated the hard plastics protective cover. Recently, a somewhat unexpected move from the international committee for the mechanical testing of metals has suggested that their standard for tensile machines should be extended to cover other materials. The differences in the mechanical properties of rubbers and metals are about as big as you get but the rubber standard for tensile machines already references that for metals, so a common standard would seem highly feasible. If metals could bring rubbers and plastics together that would be quite something.

Your view of test methods is doubtless coloured by which procedures you regularly use, which in turn is a consequence of the laboratory's function. My first polymer work was in a production environment, so testing was dominated by the routine rheological and mechanical properties used for quality control, plus the basic requirements of material specifications. This added up to plasticity, density, hardness, tensiles, ageing, set and swelling, plus the rather more exotic resistance to ozone. Because the company produced sponge products, flocked window channel and composite rubber/polyvinyl chloride (PVC)/metal extrusions, there were also product-specific compression, adhesion and abrasion tests. However, for most products everything was based on material properties with no performance testing. Hence, there was no fatigue testing of bellows nor compression and shear tests on engine mountings. One test we lacked was something to monitor the expansion of foam, and my first foray into test development was a laboratory salt bath for testing materials used in extruded seals. My memory is that results were more confusing than helpful because I was not reproducing the stresses on the extrusion in production conditions.

When I moved to the Rubber and Plastics Research Association (RAPRA), the range of tests widened dramatically, with measurements for just about every property possible on rubbers, plastics, composites, foams and coated fabrics. This sort of variety is probably encountered only in a laboratory covering rubbers and plastics that is operating a commercial testing service. Additionally, there were research programmes to develop new test methods and apparatus.

Most, if not all, of the major advances in test methods and instrumentation up to 1980 have been mentioned in the opening chapters, together with some of the test method development projects at RAPRA. In particular, automation and computer control of test equipment were covered in Chapter 10. To recap, by 1980 test methods for almost all properties had been standardised at the International Standards Organisation (ISO), although there were one or two obvious gaps. Test equipment had gone past the purely mechanical era with electronic load cells and servohydraulic drives being well established, but computer control was only just starting to become widespread.

As a generality, the established test methods were firstly aimed at quality control testing or basic data generation. However, it was appreciated that there was a lack of methods for design data. An editorial in 1982 [2] gave a short essay on the reasons for testing – the content in expanded form has since been repeated in several publications. The basic reasons were for: quality control in the course of production or purchasing; provision of design data; investigating failures; and proving or predicting if a product will be satisfactory. The piece ended with questioning if there is common ground between the four reasons for testing and if there could be, for example, a knock-on advantage for quality control from development of design tests. This could be said to be akin to developments in grand prix racing ultimately benefiting everyday cars. However, unlike the car situation, I cannot say I can think of an example in testing; but then rather less money is invested in design data test methods than in Formula 1 racing.

Non-destructive testing (NDT) had been tipped to be one of the big growth areas. Only a few articles on the subject have been published in *Polymer Testing* (they would mostly be aimed at a more specialist journal) but the widespread interest in NDT was considered [3]. It would seem that NDT is a subject that has potentially enormous value in the polymer industry but has consistently failed to develop past being used for a few (but very significant) products. However, when NDT is mentioned it is the major techniques such as ultrasonics and radiography that come to mind. These methods generally require sophisticated equipment and it is recognised that expense is one of the main barriers to more widespread adoption of such techniques. This comment is of course ignoring the traditional tests that are essentially non-destructive, such as hardness and electrical properties. Electrical tests have probably been undervalued but there has been an appreciable increase in their application in more recent years.

The particular case of non-destructive monitoring of a destructive process using the same test piece was also raised [4]. NDT are particularly efficient in that you obtain your information without destroying anything. If a NDT method is not possible, as in ageing studies, then a procedure which allows multipoint data from the same test piece is far more attractive than one which requires a new test piece for each point in time. This might be said to be non-destructive monitoring of a destructive process. The efficiency here is not just in saving material but also the improvement of reproducibility gained. The discussion continued on the theme of efficiency of test methods in terms of the value of data and cost. The value or usefulness of a result is not easy to judge or to put a number to and, indeed, a result may have a very different usefulness to different people or in different circumstances. Nevertheless, it is a most important consideration because, however good the test, it is not much use if you do not find the results valuable. This reminded me that some years later a project management committee rejected the very efficient stress relaxation for monitoring ageing in favour of simple tensiles on cost rather than value grounds.

There was also reflection on the trends and developments of the last few years [5]. It was said that predicting the growth and growth of automation was easy 25 years previously. In retrospect, this seems something of an exaggeration because even in the Brown and Scott review of 1972 predictions for its increasing importance were relatively muted. However, in 1987 there was little doubt that if it was not already automated it would be soon.

One aspect of the adoption of computer-controlled test equipment was how much smoother and smarter it looked [6]. It is rather difficult to beautify a pendulum tensile machine whereas a load cell can be housed in a moulded casing made as elegant as you like. However, the point made in the editorial was that this hides the fact that the tests being made had not changed one bit in decades and we still lacked methods relevant to design and service performance.

The other interesting point made in that editorial was the interaction of time available to do the test and the improvements in equipment as regards quality of the results. It will doubtless never be proved, but it could well be that reproducibility worsened (or got no better), despite the better equipment, due to lack of care in operation. Certainly, I do not remember any greater problems of disagreement between laboratories back in the 1960s using pendulum tensile machines, mine being one of the pair saved by Noah.

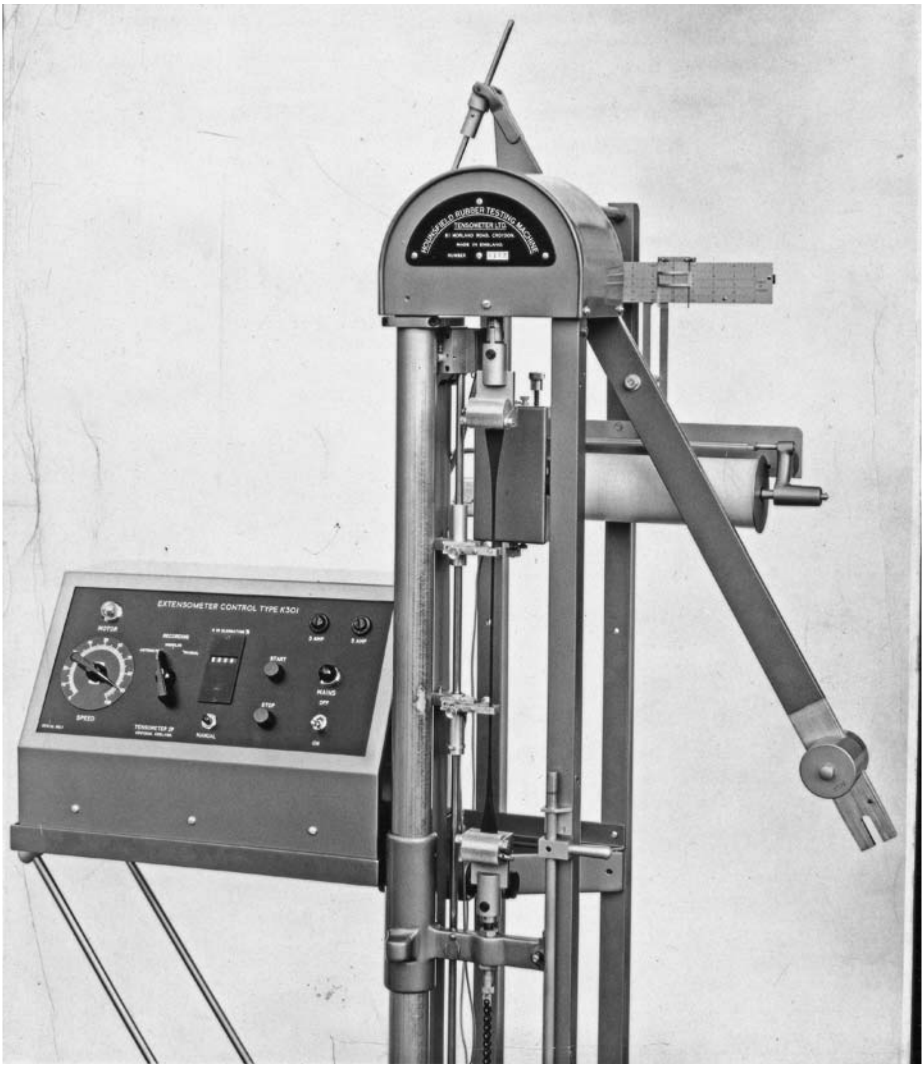

Figure 12.1 My first pendulum tensile machine was rather older than the well-known Hounsfield

Along with automation and NDT, the third predicted growth area was dynamic testing. This also had not fulfilled expectations, which I found baffling when considering the number of important applications where it was relevant. It was a similar situation with compression stress relaxation of rubbers. There could be only two basic reasons for the methods not being used – people were stupid or it was too expensive or complicated – the latter was rather more likely. In retrospect, my surprise at the lack of use probably had much to do with being in the privileged position of having a very well-equipped laboratory with a development mentality. More recently, dynamic testing in particular has become much more commonplace, due in large part to the development of relatively modest cost and easy-to-operate analysers. Everything seems to ultimately cycle round to money.

Another prediction, probably made in the 1980s, was that concerns for safety and quality would create new demands for testing. This seems logical and a safe bet, but in 2000 [7] it was far from clear how much of any increase had been negated by other factors. Also, although there had been considerable effort put into modelling material behaviour, this had not yet had any appreciable effect on reducing the amount of testing needed. I have a sneaking feeling that this could be like a number of great cost and effort cutting initiatives and have the reverse effect. Overall, it would seem that predictions for testing are like predictions everywhere – there is a good chance that something unforeseen will upset the apple cart, or at least slow it down.

The significance and value of test methods is a perennial subject. All the testing gurus and all the textbooks will tell you that almost all the common physical tests yield results that have no absolute meaning and are dependent on the particular test methods and test conditions that were used. In consequence, such data are of limited use for design data or for predicting service performance. This implies that they are less valuable than data produced by the relatively few 'superior' methods that give absolute information applicable to various geometries and service conditions.

The commonly used physical tests might be of relatively little value for design purposes but it was noted that they form the great bulk of the tests reported in *Polymer Testing* [8]. This is hardly surprising and, because of cost factors and the need to characterise new materials, nor is it something likely to change. It becomes apparent that a great many materials are characterised but, for understandable reasons, relatively few results that are more significant for particular products and application are thereafter produced and reported. For example, almost every material gets run through a thermal analyzer, but not so many have a comprehensive durability programme carried out on them or their dynamic performance evaluated under service stress–strain conditions. I find this just a little disquieting.

The thought of what was the most important development in polymer testing was prompted by a poll that placed the beer can widget (the little plastic device in a beer can that produces the creamy head) at the top of inventions of the last 40 years [9].

That a device of such minor importance should be placed ahead of heart transplant surgery, the Internet, contact lenses and genetic manipulation, to name just a few, is at first extremely surprising. It becomes a little more plausible when you learn that *T3* magazine covers gadgets and, judging by it cover, is intended to appeal to young men.

As regards developments in polymer testing, going well back in time, there must be technicians who would vote for the introduction of extensometers that halted the practice of holding a length of string next to the rubber dumbbell and receiving a sharp rap on the knuckles when it broke. Perhaps the development at about the same time of electrical-force transducers could be deemed to have the greatest overall effect on the measurements we make and to have been essential to the introduction of a whole raft of test instruments.

Generally, an argument can be made for the labour saving brought by computers in the manipulation of data and control of machines. The same principle applies to the case to be made for automatic control systems that enabled efficient long-term simulated environmental exposures and the much more precise control of parameters in a spectrum of tests.

It is essentially the developments in measurement and control instrumentation that made the invention of the thermal analysis techniques possible and revolutionised how the effect of temperature on many properties can be studied. Perhaps differential scanning calorimetry (DSC), thermogravimetric analysis (TGA) and others would get a good quota of votes.

In chemical analysis, a case could be made for any one of a number of modern techniques, gel permeation chromatography, Fourier transform infrared (FTIR) spectroscopy, gas chromatography/mass spectrometry (GC/MS), nuclear magnetic resonance (NMR) and so on that make classic wet chemistry seem like pre-history. Also, there might be devotees of electron microscopy or the more recent microscopy developments.

What has had the most effect depends on where you stand. If fracture is your thing perhaps instrumented impact is the best thing since the proverbial sliced bread. If you are a rubber processor then the developments leading to the modern curemeters could be seen as a fundamental advance. I spent a quiet half-hour trying to think of some apparently small development that would be the choice of quite a significant number of people, but without much success. Except, of course, that for services to after work relaxation, perhaps it had better be the widget.

A rather more serious poll was taken by the TMS on the greatest materials moments in history [10]. This largely demonstrated the same thing – that what you would vote for depended on where you were sitting. I was rather irritated that rubber was not listed as a prime material category but Goodyear's invention of vulcanisation came in at number 15 (no mention of Hancock and mastication). Standinger's conclusion that polymers are long chains of short units linked by covalent bonds was the next polymer event at 34. Bakelite marking the beginning of the plastics age made number 43 and Nylon followed on at number 46. The top 50 was completed by Griffiths and fracture mechanics. Nominated but not making the top 50 were Brinell hardness, the first fibre-reinforced laminates, plasticised PVC, Kevlar and electrically conducting organic polymers. Number 1 was the periodic table of elements, which I can go along with, but there followed the rather mixed bag of iron smelting, the transistor, glass, and optical microscopy to complete the first five. Sadly, there was not a single polymer testing moment. Perhaps a lesson we could draw is that there is little point in trying to put tests in order of importance, and every test has its value.

Since 1980, relatively few new test methods have been introduced. Where new test method standards have been published, it has mostly been a case of standards playing catch-up. For example, the delay in adopting Shore hardness for rubbers and the oscillating disc rheometer could be said to be due to political reasons, whereas one can think of absolutely no good reason why methods for compression stress–strain and creep of rubbers did not appear until the late 1980s. Subjects such as dispersion of fillers and test piece dimensions were delayed simply because there were no compelling reasons to have a standard.

There does of course have to be a gestation period before a technique is sufficiently widely used to reach international standardisation, and in some cases this has been a remarkably long time. Tensile stress relaxation for ageing tests was suggested in 1944, and practical work and commercial apparatus reported in early 1970s, but the standard did not arrive until 1985. The work underlying tension fatigue of rubber was done in the 1960s and we got the international standard in 1984.

On the other hand, standards can retain old apparatus for many years. The fatigue testing of rubbers was taken as an example [11]. There are two distinct types of rubber fatigue test, those intended to induce growth of cracks without heating and those in which the prime aim is to induce temperature rise. Mechanical reciprocating apparatus for both forms of test were developed many decades ago and became standardised internationally.

All the cut growth tests initially involved some form of bending of a test piece, and the most successful, the DeMattia test, is still on the standards books. Flexing may simulate tyre sidewalls but it is a rather ill-defined mode and it was not too long before a test in tension was introduced – although it was still a simple constant-strain mechanical device. That is also still with us, although it is obvious that the same action can be achieved with a servohydraulic machine, allowing a choice of strain, force or energy loading.

The heat build-up devices owe their origins to trying to simulate tyres, with the most amazing combinations of compression and shear having been used. The relatively straightforward compression (Goodrich) flexometer and the so-called rotary fleometer still feature in international standards and involve ponderous and complicated mechanics. Once again, a far better approach would be to use a servohydraulic machine where a range of conditions could be tested.

Of course, in practice, more modern equipment has been used for fatigue testing by many workers, which makes it more surprising that the old standards have persisted. However, although there has not yet been a move withdraw the existing methods, about a year ago a standard for a constant-stress flexometer based on a servohydraulic machine was published, whereas a cut growth test in tension with optical recording of the cut length was even more recent.

There are quite a few plastics methods that were standardised internationally surprisingly late. It is probable that tear, falling-weight impact, tensile impact, interlaminar shear and ball indentation hardness were simply delayed due to the slow machinations of the standards process. However, the instrumented versions of impact tests were a later introduction that had to wait for development of the technology. It is likely that lack of interest was responsible for forced vibration dynamic tests, abrasion and optical properties of plastics not being standardised at ISO earlier. This would also be true for friction of rubbers and plastics, which did not get standards until the 1990s.

Fracture mechanics methods for plastics must take the prize for the longest gestation when you consider that the concept goes back to Griffiths in 1920. The European Structural Integrity Society committee on polymers and composites dates only from 1984 and it took them 15 years to develop test methods so that an international standard first appeared in 2000. However, despite the concept being so old, fracture mechanics was little known in the plastics world until after 1980 so it is can be considered as a very important post-1980 introduction. This time scale must surely be attributed to the difficulties of the experimentation and its interpretation rather than economic factors.

Thermal analysis is another subject that, despite dating from much earlier, did not reach the status of international standardisation until the late 1990s and into the present century. Although adoption was more rapid, there is always a delay before commercial considerations create the pressure for rationalisation. Measurement of thermal transport properties can be traced back at least to Mr Lee and his disc, but standards for conductivity and diffusivity of plastics had to wait until the last few years. This must also be a case of the commercial need for standardisation taking time, as the need for transport data for polymer processing was well recognised in the 1970s, and standards for thermal insulation were produced much earlier.

A very simple correlation can be seen between the introduction of standard methods for evaluating the biodegradation of plastics and the steep rise in worry over the amount of waste we generate. It is, however, interesting that in this case there was a very short gestation period, which perhaps illustrates the power of political pressures.

There are some interesting contrasts between rubbers and plastics with respect to when test methods were standardised. An accelerated ageing method for rubbers was introduced in 1961 but there is still no general method for plastics. In contrast, a procedure using the Arrhenius relationship to predict lifetime from accelerated tests was published for plastics in 1974 but a procedure for rubber had to wait until 1997. This seems lacking in logic so most probably is due to pressures other than technical. More understandable is that plastics had a weathering standard way before rubber because this reflects the perceived relative importance for the two categories of material.

Also understandable is that the importance of tyres resulted in a variety of tests being introduced for both abrasion and fatigue of rubbers, but even now these are lacking for plastics. Tyres and applications involving damping may also explain the early introduction of forced vibration dynamic tests for rubbers, but commercial rather than technical reasons seem more likely for the more recent absurd proliferation of procedures for plastics. The relative volumes of material used no doubt accounts for why there has been no interest in developing biodegradation and fire tests for rubbers, whilst these subjects are very important for plastics. I find it particularly curious that a standard for determining dimensions of test pieces was published for rubbers in about 1980 but there was no equivalent for plastics until 2004. Put it down to differences in philosophy.

The publication of a new edition of *Physical Testing of Rubber* [12] was used to illustrate how test methods had continually changed in detail, necessitating revision of the standards, but the basic tests could be traced back in many cases to the first testing book written by Scott in 1965 [13]. Furthermore, most of the research work carried out by Scott is still considered relevant today. The same could no doubt be said in principle for plastics methods.

Whilst there seems to have been relatively few really new test methods introduced in more recent years, there has been a continuous stream of new apparatus from the test-equipment suppliers. This is just another way of saying that instrumentation to measure the same things has continually evolved and been improved/become more sophisticated. Tongue-in-cheek comment on how much we have progressed was made by relating test developments to the wizardry of Harry Potter [14]. A bunch of scientific wizards have conjured up an apparatus that drives itself, and spells that cause great volumes of data to be processed in a flash. Sometimes you cannot use them unless you know magic passwords. More powerful charms can produce results without testing although, as for Harry Potter, they sometimes go wrong. It is quite surprising how much magic we have mastered.

The companies producing polymer test equipment have been remarkably stable considering the turmoils that industry in general has suffered. Many of the well-known names started before 1970 and are still going, albeit in some cases having been absorbed into other companies.

According to my cursory investigation, the prize for the oldest goes to Tinius Olsen who, at the time of writing, have clocked up 128 years, having taken in another stalwart, Hounsfield Test Equipment. Second appears to be Suga Instruments with 88 years, very closely followed by Brabender at 85 years. With Zwick following at 70 years, H W Wallace at 65, Instron at 62 and Ceast at 55, the roll call is remarkably international. Hampden Test Equipment, now Satra Hampden, make 45 years, whilst Alpha Technologies (previously Monsanto) and Davenport (now Lloyd Instruments) have reached at least 40. Even that relative newcomer Elastocon has come of age at 21 years. Please do not all email at once to tell me what other notables I have left out.

Although they are good at introducing new models of equipment, instrument manufacturers are not renowned for producing articles. Very early on we asked manufactures to submit accounts of new equipment [15] with the idea of having a 'What's New?' column. This was short-lived and I must confess I still do not understand why the suppliers were not falling over themselves to get free publicity.

This did raise the question of how much commercially available test equipment was developed by the manufacturers as opposed to in other industrial laboratories or in universities. During the 1950s to the 1970s I can vouch for many developments coming from 'other industry'. Whilst having no hard proof, I would suggest that contribution from this source has now decreased to a very low level, whilst the majority of instrument development (as opposed to new methods) arises from the manufacturers' own work. With notable exceptions, they still don't produce many articles.

The continual introduction of new models of apparatus implies that the test laboratories are expected to buy them. They do not necessarily do this as often as the manufacturer would like [16]. In conversation, a test equipment manufacturer bemoaned the fact that a laboratory we knew had examples of his instruments that dated back many years. It was something of an irritation to him that they would not buy new versions and did not seem upset at the image having old equipment might give. In his position that is perfectly reasonable.

In the laboratory he was referring to there are state-of-the-art, shiny new instruments which are automated, computerised and almost drive themselves. These happily sit alongside mechanical test devices which I know are 40 years' old. These old instruments still work and, as they say 'If it ain't broke don't fix it'. Furthermore, they are still convenient enough to use and the advantages offered by new models might be attractive but are not that compelling when viewed against the inevitable budget restrictions. It is quite possible that in some cases they have definite advantages in that they are likely to be simple, robust, easy to maintain and need relatively little training to use. Looking old-fashioned should not detract from usefulness and some of us could take exception to the idea that being old gives a bad image.

There are some types of test equipment where anything over two or three years' old is definitely dated and cannot produce the results that some people now expect or want. We all have examples at home called computers. They might still work and be perfectly good for the output they were designed to give, but they cannot do things that are now thought essential. It is equally true that other instruments can go on delivering the data needed for decades. I have the prototype International Rubber Hardness Degrees (IRHD) pocket durometer from 1949 – it works as well as today's equivalent, unless I wanted to carry a miniature computer in my other pocket to record the results. Mind you, my comments on this subject should be tempered by the fact that I also like classic cars.

However, I also like a new toy as much as the next child [17]. In this day and age many children can get the new toy on demand but others still have to pester parents for months, and even then success is not guaranteed. If the parents have little money any new toy involves a sacrifice. Very few workers have been able to get new test equipment on demand and the trend has been more the other way, with funding becoming more and more difficult to obtain.

Pestering the boss does not work as well after a certain age and the process of getting permission to purchase equipment can be drawn out, with multiple justifications of the time that will be saved, the quality that will be improved and the positive effect on the bottom line. To the scientist, the advantages of the new equipment are obvious but to the accountant it is a toy – unless it can be proved that it will make a return on money invested. Not surprisingly, many justifications for funding are just a little fanciful, or even a children's fairy tale. If children are more successful at getting their toys, perhaps we should employ them to make purchase requests.

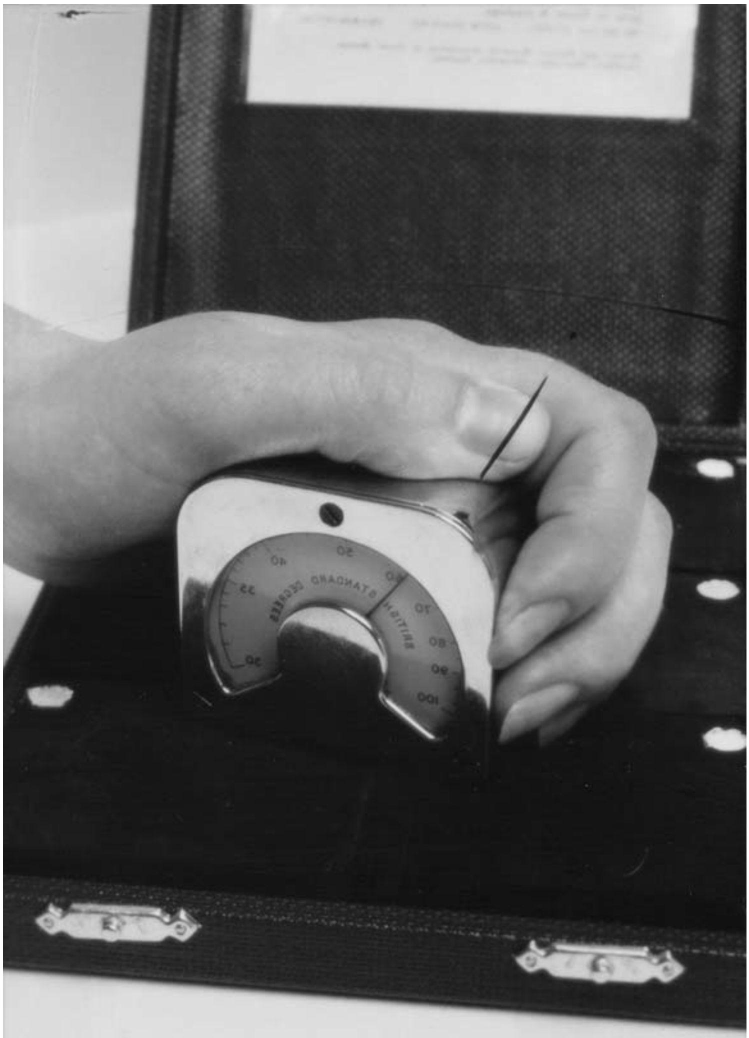

Figure 12.2 My 1949 prototype international rubber hardness degrees (IRHD) durometer is still in good working order but the calibration certificate is out of date

When the justification process is successful, eventually the shining new equipment is installed. With the complications of modern designs it probably will not work straight out of the box and there will be a learning period to master all its new features. This had better not take too long because the accountant is already looking for signs of his return. Also, one should not look too pleased or he will be more convinced that he has sponsored a new toy.

In all the fairy stories with a happy ending, the promised improved capabilities materialise and before long you are wondering how it was ever possible to exist without the new machine and even the accountant is no longer complaining. Of course, it is not always like that and new instruments can develop faults or not perform in all respects as well as one had hoped. Rarely though does new equipment go the way of many toys and the pleasure of owning it disappear rapidly. After all, it will be a very long time before you get another one.

If you need new equipment and the powers that be will not come up with the money, then a possible alternative is to make it yourself. This was relatively prevalent many years ago [18] when there was a culture in many laboratories that you were developing new things and it was natural to make apparatus yourself. In my first few years of work there was plenty of opportunity to use the skills gained during practical training with the United Kingdom Atomic Energy Agency (UKAEA).

It is not surprising that in-house apparatus building now seems to have diminished. The need for laboratories to produce their own instruments has decreased as much more is readily available and, as test apparatus has become more and more sophisticated, so it has become much more difficult for the non-specialist to design and build to the required performance. Whilst self-building has probably diminished, there is clear evidence in articles to *Polymer Testing* that the tradition is still alive in many universities – and hopefully some of those developments find their way to commercial production.

The great majority of testing laboratories would consider it an unobtainable luxury to have their own workshop and, hence, be able to repair equipment, design and build new apparatus as well as laboratory furniture without having to rely on a facility whose priorities lay elsewhere and/or would incur exorbitant costs. I had that luxury for a number of years when RAPRA had a very active test equipment development programme. There is no doubt that we produced some significant prototype new apparatus, but what I remember most is the way it enabled us to circumnavigate the capital expenditure budget. As head of the laboratory, I was not greatly restricted in buying materials but capital was subject to central scrutiny and authorisation. Hence, a lot of equipment, and furniture, was obtained by buying components on a maintenance pretext and constructing whatever was wanted.

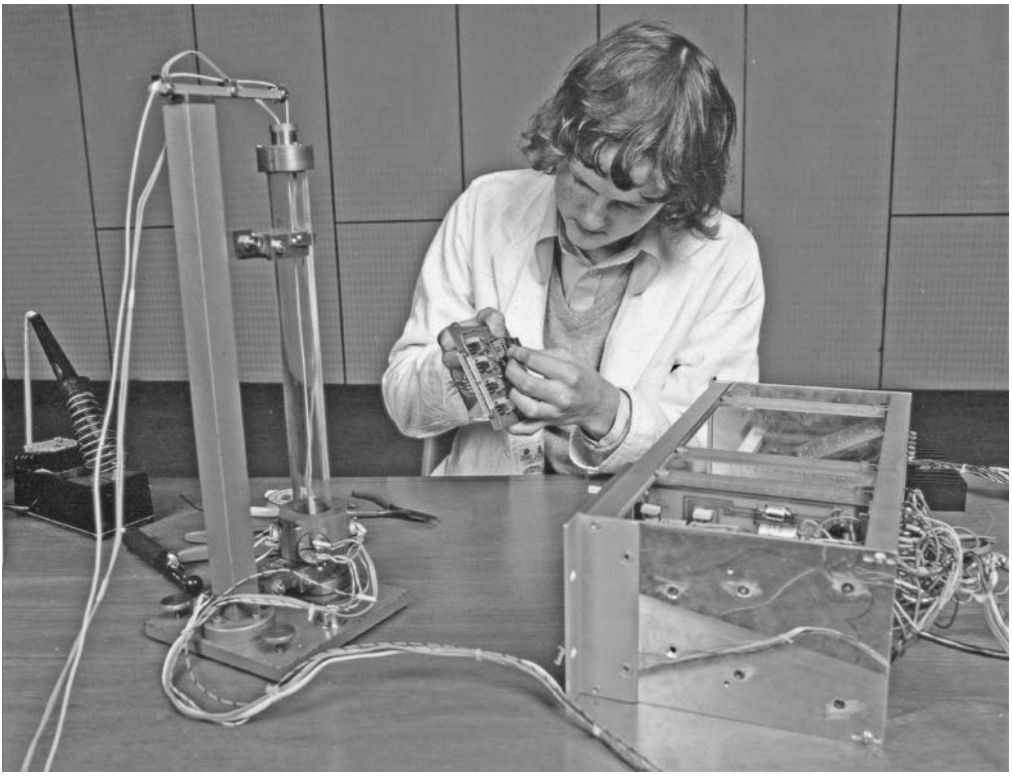

Figure 12.3 Making your own test equipment

A similar luxury was the company maintaining a fully equipped photographic department which meant that recording the state of products or test pieces, including microscopy and on-site work, was professionally supported and involved no hassle. Plus, we could freely produce illustrations for publicity purposes. A very young person today might wonder what was the big deal about this because now almost all photography is digital and, unlike the film cameras then, requires no processing facilities or skill. This is perhaps as striking an example of how technology has changed that one could think of.

It may seem curious that with money for new equipment in short supply a laboratory can become overcrowded. One answer suggested [19] was that modern equipment takes up more room that old equipment simply because of the space needed for the monitor and keyboard. It is also a fact that there has been an increase in specifications calling for tests on whole products, and product test rigs are almost inevitably larger than apparatus for laboratory test pieces. A knock-on effect from this is the increase in sample storage space required, which has probably been compounded by quality systems demanding that samples are kept for longer. All in all, this could add up to a significant increase in space needed, and hence cost, which at first sight may not be very obvious. And this is all before we consider the human trait of not wanting to get rid of the old when a replacement has been bought. However, now that computers have shrunk, cathode ray tube monitors are almost history and many consumer products have been miniaturised, perhaps laboratories will follow the trend in houses and become smaller and be put closer together.

Laboratories generally evolve over time with new equipment added and old apparatus replaced at intervals, but the ideal solution to an overcrowded laboratory is to build a new one. I suspect that I am almost unique in that this has happened to me three times [20]. The first was very modest but did mean that all my equipment and staff were brought together in one room, and was separated from the noise and mess of compound development. It was probably significant in that the company were accepting that the need for testing was likely to grow in the future rather than go away. My experience was extremely limited, but I kept the ovens from interfering with temperature control of the room and nothing delicate was upset by vibrations.

The second occasion was an amazing experience because it included being involved in the negotiations to buy a new building as a satellite to our existing operation, designing the interior layout and the facilities as well as equipping it with apparatus and hiring new staff. It was a large undertaking. From the historical point of view, the interest is that it was a viable proposition because it was at the time that very large chemical companies were restructuring, shedding much of their technical facility and outsourcing testing. There are no lessons in laying out a laboratory but experience in operating one must have good effect because it seems my colleagues and I did not make serious mistakes The facility was still looking good and functioning well many years later except for the sample storage. I underestimated the growth in traffic about as badly as our road planners but, fortunately, the store area was easily extended.

The third new laboratory was due to physical restructuring of part of the main building at RAPRA, where I suspect we were the fortunate beneficiaries of some grandiose scheme that were prevalent with managements in the 1990s, rather than it being done simply for the benefit of physical testing. The editorial raised the question of which is the best part of a new venture – is it the new room and infrastructure or the shiny new apparatus? I would conclude that the chance to have a layout just to your liking wins every time. You can fight again for new apparatus tomorrow and the day after but you will be lucky to get one chance of a new room.

The general testing picture is of gradual successful development and advance over the years in instrumentation and test methods but there have been things that did not quite work out. Some examples of developments that were 'before their time' were remembered [21]. Notable amongst instrumentation was completely automatic apparatus for density, hardness and tensile stress–strain which, although developed as far back as the 1960s, has failed to catch on. This is doubtless due to a misjudgement of the economics, in particular overestimating the volume of tests that were going to be needed. The lack of application of non-destructive methods such as ultrasonics was mentioned earlier and simply indicates that such measurements are viable only for particular types of product. There was slower uptake of stress relaxation, dynamic tests and fracture mechanics methods than might have been anticipated, but these have now become widely accepted. The first searchable knowledge-based systems did not sell well simply because they came before the potential customers had understood and accepted the concept. No doubt there have been cases where the incorporation of advanced features into an apparatus failed to realise the anticipated sales. Success is not only getting the bright idea but getting it at the right time.

It is probable that people who in the past developed biaxial and multiaxial tests were somewhat miffed that their product did not take off because this is another area of testing that has not yet fulfilled its potential [22]. Multiaxial stressing is of course what most products are subjected to in service and, consequently, how they should be evaluated in the laboratory. However, testing in this manner is much more complicated or expensive than normal uniaxial methods. This provides the simple explanation of why multiaxial testing may be desirable but has been relatively rarely used in practice. This is the same scenario as stress relaxation of rubber seals being more relevant than compression set, but the latter, simpler test is most often used.

It could be imagination, but in the last 2–3 years there seems to have been increased interest in multiaxial tests, judging from a few articles that have been published. Also, the National Physical Laboratory in the UK publicised a very sophisticated large multiaxial test facility and the development of a method for polymer composites, which must indicate that demand is there. If people are developing new methods, then the technology is not standing still.

One factor has been the development of techniques for full-field, non-contact strain measurements, and there have been several articles in *Polymer Testing* describing the application of such methods to polymers. When these techniques are combined with multiaxial stressing, it can greatly increase the value of the information gained over that from the basic mechanical tests, and also allows comparison with the output of finite element analysis. OK, the full-field strain measurements add to the already complex multiaxial methods but perhaps the enhanced rewards just tip the scales in favour of multiaxial being attractive to a much wider audience. Should that be the case, perhaps automated multiaxial analysers will soon appear on the scene in great numbers and we can look forward eventually to the costs dropping.

If you tried to equate the frequency of usage of tests to the importance of the results you would no doubt find some contradictions and a great deal of argument. One area singled out for comment was lifetime prediction [23]. This is of course a very difficult subject but no one would doubt its importance. The perception was that the number of articles submitted on the subject was small in relation to the size and importance of the problem. This situation could be related to the difficulties but also, and perhaps more significantly, to the costs involved. It is also apparent that the situation varies enormously depending on the product, with materials such as tyres and pipe having received a great deal of attention, whilst less critical components are ignored. David Wright, one-time Technical Director at RAPRA Technology, tells in his book '*Failure of Plastics and Rubber Products*' [24] of setting-up a large plastic creep facility which was never gainfully employed to comprehensively characterise a commercial material – apparently the incentive for suppliers to test did not overcome the aversion to the expense. The comments could no doubt be generalised to many other test areas.

For many polymer properties there is more than one test method [25]. Whilst in some cases there is very good reasons for having different approaches, in many others the duplication is technically unnecessary. For example, different types of abrasion test measure different aspects of the property but we could do without a variety of tensile test pieces. However, whatever the irritation or confusion that multiple polymer testing procedures may cause, we do not have the scale of problem that exists for surface roughness where, apparently, when filter types, filter cut-off levels and the 80 odd parameters are taken into account there are over 1000 different options. No one parameter satisfies all requirements but with that total number it would take a long time to find which one suited your purpose best. Despite the choices available, it has been reported that 10 parameters are enough for 60% of companies, about 20% use only one parameter and 81% of parameters are used by less than 5% of companies.

One would think that there was a good argument for some rationalisation but there has been very little sign of that taking place. In fact, in some areas we seem to be getting more rather than less alternatives becoming standardised. There are now an unwieldy number of dynamic methods for plastics, and methods for thermal transport properties are multiplying. For rubbers, alternative abrasion apparatus have been introduced, and whether we need five tear test pieces could be questioned.

A measure of low-temperature performance is very important for some applications, and to expedite this for flexible plastics and rubbers some rather clever devices were invented which were relatively cheap and simple. However, we have different apparatus for low-temperature modulus of rubbers and plastics [26] - Clash and Berg apparatus for plastics and Gehman apparatus for rubbers. Both operate in torsion, the force being applied by a system of weights and pulleys in the Clash and

Berg instrument and by a torsion wire in the Gehman. Whilst being quite ingenious designs, these instruments could hardly be called sophisticated. Nevertheless, they served their purpose very well for many years. The question here is why both have not been consigned to the bin in favour of more modern thermal analysers. It would seem that old habits and old apparatus die hard.

A different slant on multiple choice was aired in an Editorial in 2009 [27]. It was pointed out that giving the people choice, whether it be of school for their children or of hospital when they are sick, was generally considered a good thing, so perhaps we should be grateful for any choice of test method and not look at the drawbacks.

Modelling of polymer properties has been deemed to come within the scope of *Polymer Testing* although the number of articles has been relatively modest. There are those that talk of modelling as an alternative to testing whereas I see it more as an extension or a complementary activity. You have to test to establish and prove a model and the hope is then that the model can be used to predict for other circumstances or materials. You have only to look at the prediction of ageing via Arrhenius or other relationships to see that there is a very long way to go before the goal will be reached, despite much effort being expended in recent times. Modelling has been only briefly touched on in an editorial [28] when the extremely ambitious aim of a model for drape of fabrics was reported. The general conclusion is that testing will be needed for the foreseeable future and beyond.

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13 Product Testing

Looked at very simplistically, it would seem that there are equal needs to test materials and complete products, both for quality control and proving fitness for purpose. However, in most polymer laboratories there are far more tests on materials carried out than on products, sometimes to the extent that product tests are virtually non-existent. In trying to see the reasons why material testing became so dominant, the only conclusion can be that it is purely a matter of our old friends: cost and time.

It is generally fairly easy to see when testing the product would be desirable, and this would be relatively often, but the judgment then has to be made as to whether the costs involved can be justified or the risks of not testing being acceptable. For a new product, some form of evaluation of its fitness for purpose is generally essential, but in many cases this will be achieved by the 'proof of the pudding' approach of trying it out in service, or simulated service. Considering the uncertainties of devising satisfactory laboratory product test methods, this can be considered to be a reasonable decision.

It then follows from costs being the dominant factor that there are likely to be two circumstances when the most probable decision would be in favour of on-going product testing. Firstly, cases where the value of the products is low and a suitable test can be devised that is no more complicated or expensive than control testing the material. Examples would be impact testing a plastic bucket or the stiffness in compression of an engine mounting. Secondly, products in which the implications of failure are very serious, such as safety-critical items. In these cases it is particularly advantageous if a non-destructive method can be devised, and a good example is the proof voltage testing of electricians' gloves.

My first encounter with product tests back in the 1960s involved examples of low cost items – peel adhesion of the flock from a flocked car window channel and compression set of sponge door seals. We could not carry out compression stress–strain tests on seals or mounts because we had only a pendulum tensile machine that was not adaptable to compression mode! A little later at the Rubber and Plastics Research Association (RAPRA) we routinely did electrical tests on gloves, burst tests on condoms and drum testing of tyres, whereas pipes for falling-weight impact tests turned up every so often.

There were lots of other things that appeared on occasions during my early years at Shawbury, but they did not occur frequently enough to make a big impression.

Making tests on pieces cut from products is a sort of 'halfway house'. It has obvious and substantial advantages, and could be usefully applied to a large percentage of products. Specifications were long ago written with this in mind for such items as rubber safety boots, hot-water bottles and polyvinyl chloride (PVC) floor tiles, while tanks and other containers are obvious candidates. The question of when it is desirable to test specimens cut from products was raised [1] having been prompted by a paper in which moulded and cut test pieces were compared. It was thought that most specifications made no effort to include performance tests or to make provision for properties to be determined on test pieces from the product. A paper that I wrote in 1980 [2] comparing results from test pieces cut from products and from the material used for their manufacture revealed very significant and sometimes alarming differences. Whilst this demonstrated conclusively the value of cutting from products, the extra costs involved are usually more persuasive.

Over the last 30–40 years, there has been a considerable increase in product testing with a trend of more product tests being included in product specifications. This would seem to be due to people wanting to see more evidence of fitness for purpose and a policy in some quarters for specifications to be performance rather than material based. One consequence of increased requirements for product testing is the cost burden – not only on the manufacturer paying for the tests but on the laboratory that has to fund all the new equipment [3]. The other consequence is that there may be only a few laboratories having any particular test rig, and the specification can leave scope for variations in interpretation. The result can then cause potentially horrendous reproducibility problems. This was evident during the development of European standards for synthetic sports surfaces during the 1980s and 1990s.

On the positive side, artificial surfaces also illustrate some of the advantages of product performance tests. Because much effort was put into devising methods that evaluated the sports performance, they were effective tools for the development of improved products, and could be considered at least an indication of how a given surface would play. Because many of the tests were essentially non-destructive, it became unusually easy to demonstrate whether or not a given installation was performing to standard some time after its construction.

Sports surfaces became what must have been one of the most tested products as on-site evaluation became part of all installation contracts. There are several reasons for this, but one is that it received a lot of funding from the likes of the UK Sports Council to make it technically feasible, and this happened because of the perceived importance of sport. Throughout history, it would seem that testing is always subject to economic and social pressures.

Figure 13.1 Measuring ball-rolling resistance on site

On-site testing brings its own particular problems, not least the transport of equipment and maintaining calibration. The sports work developed when accreditation authorities were getting increasingly stringent and they found the strange apparatus difficult to cope with, but never did come with us to see it in real-life service. It also has to be said that on-site testing can provide some very interesting or amusing experiences. I have been scared stiff going into the bowels of a large ship to take insulation samples, locked in the sports ground of a prison when it started to snow, and happily stood by the apparatus while a television reporter hammered the supplier of a notorious, bouncy football pitch. Probably the most unforgettable was a week spent with Gerry Lamming making measurements on the single-ply roofing of a fire station, a school, a restaurant and several other buildings in the southern states of the USA. It was extremely hot and I don't like heights but the job had to be done. I doubt testing staff would be allowed to simply climb up ladders carrying measuring gear today without special training and a safety line.

The same subject of increased use of product tests, the cost and the requirements of European standardisation came up again in an editorial five years later [4]. Inevitably, when a body such as the European Committee for Standardisation (CEN) starts developing a raft of new product specifications with an emphasis on product performance and safety the committees charged with the work start 'inventing' new product tests. Given that product tests tend to be expensive, that they have relatively narrow applicability and many of the committee members are new to international standardization, there is a very high chance of test methods being poorly specified and inadequately proven before they become 'law'. Calibration and laboratory quality control will not compensate for a lax or ambiguous standard.

Test methods were traditionally developed by test method committees which are almost exclusively composed of experts in testing. Product committees are more densely populated by production people, users, and salesman with hardly a tester to be seen. We should not be surprised that the test methods developed in product committees usually leave something to be desired. This is just what you do not want with the particularly challenging subject of developing product tests. Perhaps it illustrates again that the main barrier to greater use of product tests always has been, and always will be, the time and effort needed to overcome the intrinsic difficulties of simulating service in the laboratory.

There are doubtless some sectors where product testing is particularly prevalent. An obvious case is in tyre manufacture – a high-performance, safety-critical component. A general polymer laboratory with mostly product tests merited comment [5] because it was unusual. A further slant on product tests was noted as there being a need to build rigs to solve one-off problems, hence a particular product test rig may be transient, and a laboratory specialising in solving one-off testing problems will become very expert in the construction of *ad hoc* devices that would rival the designs of Heath Robinson. Instead of the tidy benches of well-known equipment from well-known manufacturers, the image is more of sprawling mechanical and electronic units of unknown parentage which sometimes connect together. Here, the question of reproducibility does not arise because the rig is used in isolation to compare products.

Some rigs can be multipurpose, such as large loading frames and falling-weight impact towers. One of the less likely products we evaluated using such rigs was fences for horseracing made from artificial materials. This naturally involved an evening at the races for research. One of the more dodgy tests involved subjecting a large armoured hose to very high pressure with an inflammable gas which was then rapidly released to induce explosive decompression in the rubber. This rig was rather carefully constructed and operated but the gas went to atmosphere, although I think we backed off from flaming it like they do on oil rigs. This is another case of attitudes to safety being on a different planet.

Some of the arrangements for fire testing of products can look especially *ad hoc* but are generally to carefully standardised protocols. In this context, the story is told of Keith Paul, head of the fire laboratory at RAPRA, visiting a local furniture shop and saying he would have three of a particular design of settee. The new assistant asked him what colour to which he replied he did not care as he was going to set fire to them. The assistant ran to the manager yelling that he had a madman in the shop. The manager peered from his office and advised there was no problem as it was only Dr Paul who often did that. I have never asked Keith if this was true.

Devising tests to evaluate a range of products cycles back to the Consumer Association work mentioned in Chapter 8 [6] which suggested that for technician satisfaction it beat boring material tests. Consumer Association testing also tells us that, when it comes to presenting results to the public, product tests that measure practical attributes are essential, even if sometimes the scientific merits look questionable. Significantly, consumer publications such as *Which?* magazine have gone from strength to strength over the last 50 years.

The service life of tyres has received some not so desirable publicity in recent times because of dramatic failures causing injuries and even deaths [7]. If the cause was perceived to be a fault in the design or manufacture of the tyre, the conclusion (and the blame) is fairly straightforward. However, when there was no such obvious cause the lawyers were very easily confused and had to try harder to find a way of blaming the manufacturer.

The problem is of course not new, although in earlier days it was generally very rare because tyres wore out long before their material properties had deteriorated. Alternatively, if they were left as the spare, the ravages of ozone usually became very evident. Remembering some of the tyres I had in my impecunious youth, the performance of cars and the skill of drivers might also have something to do with it.

One of the earlier instances that highlighted the problem was when military Green Goddess fire engines in the UK were brought out after many years of retirement because of a firemen's strike. The tyres looked perfect and tests showed that all the properties were up to specification, but once run and heated up they deteriorated very quickly and accidents ensued. We demonstrated that the antioxidants had simply been used up over the years so there was no longer any resistance to thermal ageing. Recently, another accident reported involved a classic sports car with apparently very little-used tyres. In fact, they were 25 years' old and it is reasonable to assume that the cause was the same as in the case of the fire engines. There have been other instances in the intervening years and it is now more generally appreciated that a tyre can look to be in perfect condition but simply because of age has lost antioxidants and could fail very quickly when put into service. Clearly, this was not something that was built into the requirements for proving the safety of tyres. Testing the old tyre would probably find the problem but that is a trifle too late and it is a case of where predictions from an ageing test programme would be rather more useful.

With ever-increasing sophistication of laboratory test equipment it is good to be reminded that some very effective product testing can be achieved with pretty crude apparatus [8], or indeed by simply giving people the product to use. A reunion with an old associate reminded me of the *ad hoc* approach to do-it-yourself product testing. He would quietly take samples of newly developed artificial sports surfaces and, quite separately from any laboratory programme, subject them to his own brand of testing. Resistance to loading was covered by laying the surface in front of his garage and driving over it for six months, whilst resistance to wear from foot traffic was effectively evaluated by placing samples at the entrance to a busy sports centre, and probably had a significant degree of acceleration over intermittent use on a football pitch. He took a similar practical approach to devising do-it-yourself methods for resilience and ball spin, and it is likely that these methods were as good if not better than the 'scientific' methods devised in the laboratory. In fact, his experiments on the spin of a cricket ball led to the laboratory getting a much better idea of how players judged a surface for its ability to take spin.

A gentleman with a suspected faulty hot-water bottle tied the stopper to a beam of his garage and swung on the bottle to prove the inadequacy of the accepted laboratory test for stopper integrity. The claims of manufacturers for the heat retention of gel-filled bottles was best evaluated by putting the bottle in a bed, whilst there is no better way of proving the safety of candles than by burning them.

A safety footwear factory had the habit of giving golf shoes to the visiting quality inspector with the request that he wore them and reported on performance every few months – this practice was probably rooted in bribery but, nevertheless, was a neat way of getting other people to do your testing for free. One sports company always donated areas of new surface materials to the staff social club and, hence, achieved the manager's dream of getting staff to work without pay. I am sure this idea could be extended with advantage to all concerned – at the moment I have testing space for a few square metres of horticultural glazing. One of the most spectacular examples

I remember was tying a full-size oil retention boom to a bollard on the quayside, attaching a load cell to the other end and towing it with a Land Rover. One can only hope that in these days of legislation, accreditation and certification such enterprise is not killed off.

Figure 13.2 The real way to test a plastics bath – product featured in a Rapra film on testing

The success or otherwise of a product test depends essentially on how well it represents the performance in service. It could be said that the better the correlation with service the more useful the test [9]. The editorial was prompted by a dispute involving an accident in a children's playground, where a small child fell from a height and sustained leg injuries. A point at issue was whether a polymeric safety surface should have been provided and whether it would have prevented or reduced the injury.

The relevant performance test for a playground safety surface is well established in standards and involves dropping a metal sphere containing an accelerometer from a series of heights. The deceleration is monitored and the signal integrated in a prescribed way over the time of impact to give a measure of the level of severity. A surface is judged on the height at which the specified index of severity is reached. The same basic principle is used in tests on sports surfaces, helmets and packaging.

The results of the test can tell you whether or not a surface passes the specification limits that have been set, and will give the decelerations and forces realised in an impact under the particular conditions of test. However, in particular instances the usefulness of the results can become debatable because the specification criteria were derived from consideration of car accidents and the evidence relates to head injuries only – hence the logic of using a rigid, spherical impacting body. The stresses of service are extremely complex and very variable so that mimicking them is a huge challenge. If it is a leg that gets broken, or for that matter any other part of the body, there is nothing to relate the test results to likely severity, and it is easy to see that the forces involved in leg impact are likely to be different to those in a head impact. This is not a million miles away from trying to relate the output of a simple tensile test with the failure of a complex product, and demonstrates that all tests have their limitations.

When you take on an abstract property such as 'soft touch' of plastics [10] you must be feeling very brave. 'Softness' in this context is a very complex concept that goes far beyond the usual understanding of softness as a measure of stiffness. Hence, to characterise 'softness' a whole list of parameters were being measured: surface roughness, friction, compressive properties, bending and thermal conductivity, which contribute to the 'feel' of a material. These tests were carried out on a range of materials. The work included development of a rather ingenious but simple instrument for friction between the human finger and the material, which also measured the normal force applied. The possibility of defining a standard finger is an intriguing thought.

The same materials were ranked for a range of parameters which included warmth, roughness, compressibility, stickiness and friction by a sensory panel. One interesting detail is that such trials are carried out in the dark so that such things as the colour of the material do not subconsciously influence the result. It can be expected that there will be some difficulty in getting correlation between the measured properties and rather more abstract sensory reactions of a panel of people. However, it would appear that things are worse than this because people do not always agree on the sensory perception. This includes opinion on the suitability of a material for a particular application, for example, lack of consensus on what made a good soft-touch screwdriver handle. Also, it was expected that the judgments could vary with age, ethnic group and so on of people on the panel, which would seem to make any attempt at correlation with the laboratory results a case of chasing rapidly changing goalposts.

Somewhere behind this there must be an implied assumption that people are correct and the laboratory measurements are manfully striving to replicate human judgments. Now, if the people cannot agree they have very poor uncertainty performance and by the normal standards of rating test methods the procedure should be rejected. Hence, I conclude that a nice simple combination of two or three instrumental measurements should be taken as the true value and the people told to get themselves recalibrated.

As a final word on product tests, many years ago I was told to produce a promotional film on testing, despite having absolutely no experience of scriptwriting, producing or directing. I decided to feature laboratory tests *versus* the products in service. By choosing amongst other things a pipeline in a brewery, a luxury glass reinforced plastic yacht and a high-performance car I had a most enjoyable time. They wouldn't let laboratory staff do that sort of thing now.

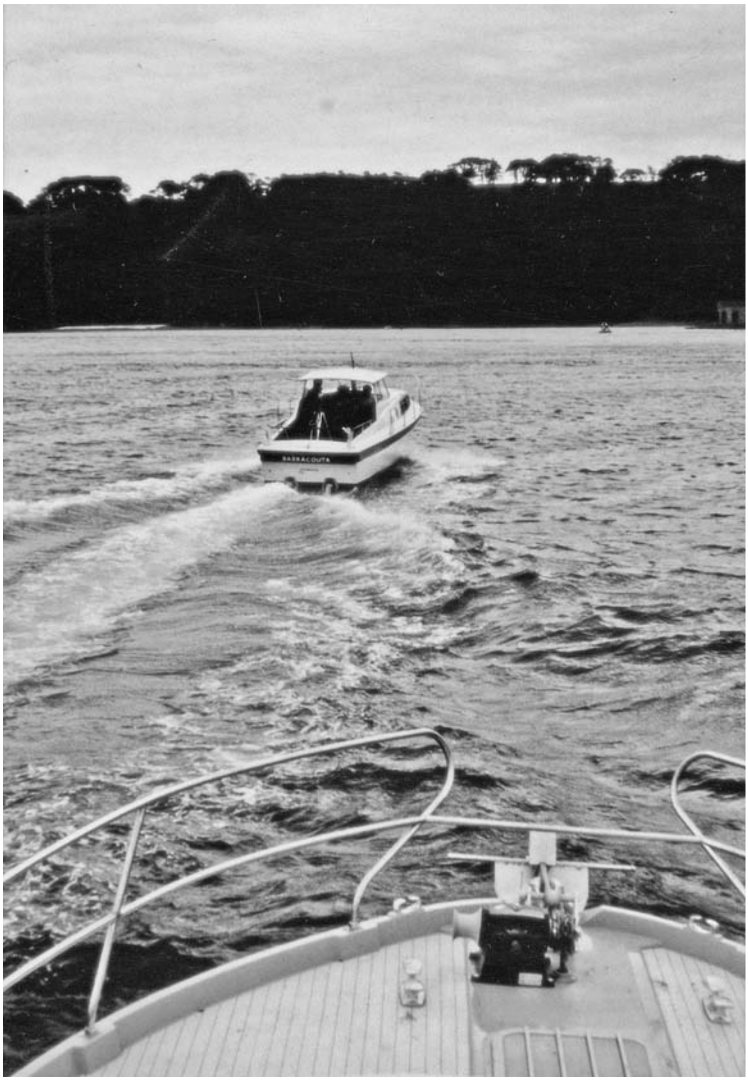

Figure 13.3 This is actually product testing

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14 Material Data

In the first year of the journal *Polymer Testing* [1], the inconvenience of not being able to make valid comparisons of manufacturers' literature was raised with no expectation that anything would be done about it. At that time, this was accepted as a fact of life and the manufacturers were thought unlikely to mend their ways. Only five years later [2] computers had become so prevalent that initiatives were being taken to produce material databases for polymers, which could create pressure for data to be generated strictly according to chosen standard procedures. Doubt was expressed as to whether it was realisation of the power of the computer to readily allow selective access to large quantities of data or the existence of large numbers of computers needing something to go on them, but everyone then seemed to want to put information on to disc and call it a database.

Computer databases were not a new concept because collections of the abstracts of published articles were well-established [3]. They were kept on mainframe computers which users generally had no access to. Output was obtained according to a profile of the user's interests on cards which could then be filed. The Rubber and Plastics Research Association (RAPRA) had what was probably the most extensive database at that time which had been in existence for some years. Certainly, I remember collecting the cards in the late 1960s.

That database is still going today, but of course you receive the output at intervals as a computer file of updates. Also, you can now build up your own bespoke database from the files you receive on your computer, which might have more power than the one on which abstracts were first collected. All journals now require an abstract to be submitted by the author, which can be put straight into a database, but at one time there was a job of abstracting published papers to produce the input for the early databases [4]. That little cottage industry for retired scientists has gone. Getting the author to do the work undoubtedly saves money but whether the result is as good is highly unlikely.

Papers nowadays also have keywords included which are intended to be useful for categorising the work and to enable it to be found from the ever-increasing mountain of scientific work. It is only too obvious that if the keywords are to be used for searching then they need to be selected extremely carefully. The aim is to deduce exactly which terms somebody wanting the subject of your work would use whilst not making it so vague that it turns up in almost every search.

Purely as a simple illustration of the subtlety of search terms, I investigated 'polymer testing' in an Internet search engine. It was very impressive that the journal came up on the first page, although admittedly it was a sponsored link. Amongst polymer testing laboratories and test equipment companies (mostly from the USA) I was gratified to find a conference I was involved with coming in at number 12, with my '*Handbook of Polymer Testing*' [5] at number 16 closely followed by another of my books. The order of results might be questionable but at least the relevant international journal and a standard text came well up the order.

When instead I put in 'rubber testing' results were rather less good. There was no sign of the journal, nor of my books, although predictably there was a Super Sexy rubber website. In a simple way, polymer testing can be said to equal rubber testing plus plastics testing. Hence, one would expect to find a polymer testing book that includes rubber when searching for rubber testing. No chance, robots don't know what rubber or polymer are so they don't think that way. However, it does make one a bit more confident that we will not be totally replaced for some time to come. While writing this, just five years later, I tried the search again and the journal was up front without any sponsoring but the increase in all things polymer meant that my books had been relegated. The robot still could not equate rubber with polymer.

A point being made with respect to the literature databases was that if you had access to them there was no excuse for omitting references to relevant previous work. This came up again several years later [6] when it seemed that, despite databases, relevant work was overlooked. With the vast amount of existing literature, it is no doubt a tedious process to check everything that has been published on a subject, and even more onerous to expect reviewers to check that there is not repetition. Perhaps an extreme reaction, but a friend told me that having written one book he had no wish to do another because he found deciding which references deserved inclusion to be a form of torture. Nevertheless, authors have a duty to check previous work, but whether they all bother to do so diligently, or indeed whether all authors have subscriptions to comprehensive databases, is questionable judging from submissions to *Polymer Testing*.

Going back even further, material property databases existed in non-computerised form. We entered the results of physical tests on the company's rubber compounds onto punched cards. I cannot remember the exact details but there were spaces for the type of polymer, the tensile strength and other properties. For example, you could punch slots for 2000 psi (yes, psi in those days) and 500% elongation. If you then put a needle through the stack of cards at the 2000 psi point only the ones complying would be selected. This could be repeated with 500% elongation. It worked remarkably well and saved a lot of reinventing of the wheel. The main problem as I remember it was that a compound might refuse to yield the same results on remixing a year later.

One of the initiatives for polymer property databases attracted support from major suppliers and resulted eventually, after much effort, in international standards for plastics as ISO 10350 for the acquisition and presentation of comparable single-point data and ISO 11403 for multipoint data [7]. There will inevitably be disagreement when a subset of particular test procedures are selected for use when data sheets for materials are to be produced, but the standards are now well established. These standards and their use by manufacturers does not of course mean that all published plastics data is presented in the specified format, but it must have made significant improvement to the uniformity of data and benefited the process of material selection.

A similar initiative for rubber had to wait about 20 years, largely because the situation there is very different with a great number of unique compounds rather than a series of grades. However, a format that was more applicable to the rubber situation was developed and at the time of writing the standards have eventually been published by ISO [8]. As an illustration of how we do not all think alike, one country (not the USA) consistently opposed the rubber documents on the grounds that they only allowed International test methods!

The initiative for single and multipoint data for plastics spawned work on design data, with the priority subject being durability [9]. Apart from various durability initiatives, a guide to acquisition and presentation of design data for plastics was developed, and some 16 years later an international standard appeared. Some things you have to wait for.

Quite soon after the initiative for material property databases, the subject of storage and retrieval of knowledge was raised [10], and the term ‘knowledge base’ appeared. This was at the time when the so-called information explosion was gathering force as the power of computers was being used to amass increasingly huge amounts of data. Test data are one aspect of knowledge but they attain much greater value if they are accompanied by interpretation. The concept that knowledge from many sources could be collected, stored and made intelligently retrievable was revolutionary, and was possible only as a result of computerisation.

RAPRA saw the potential for polymer knowledge bases [11] and in about 1990 started to put considerable resources into developing commercial products, with one for rubbers and one for plastics being produced. The advantages of a computer environment for collecting together information are that a vast amount can be condensed onto a single CD, the data can be searched and linked in a sophisticated manner, it can be interactive and the content is relatively easily updated. In the RAPRA model the knowledge base consists of three fully integrated sections, a bibliographic database, a topic base and activity modules plus a section which has the elements for users to create their own customised knowledge base system.

Figure 14.1 Testing for RAPRA chemical resistance data sheets

A knowledge base specifically for testing rubbers and plastics was developed relatively cheaply as a cooperative project within the RAPRA Testing and Quality Group, but it was 1998 before it was generally available. A bibliographic section is a collection of abstracts, in the case of the RAPRA product coming from the world's largest polymer abstracts database with a special selection being made for testing (including of course from *Polymer Testing*). A 'topic base' is a sort of multimedia encyclopaedia with the complete content of testing textbooks, reviews and articles on a wide range of subjects. The testing knowledge base system (KBS) had topic sections on Text books and Guides', 'Standards Information', 'Laboratory Management', 'Testing Related Information', 'Material Properties' and 'Data'. Activity modules are interactive software such as programmes to calculate test results, convert units and predict lifetimes. With about 60 or 70 items from textbooks and a database of test-equipment suppliers to a programme which calculates Mooney Rivlin constants, it is apparent that the amount of material was vast, and the objective of a knowledge base to have all there is to know on the subject was not an idle boast

There is little doubt that a comprehensive collection of polymer information, including textbooks, expert authored articles, literature abstracts, calculation software, etc in electronic form which is readily searchable is very desirable, but unfortunately is expensive to produce and to maintain. The editorial in 1997 was very upbeat about it, but the economics plus the development of the Internet meant that the model was not successful.

Another side to the storage of information is the durability of the records, which was given an airing in an Editorial in 2009 [12]. As it pointed out, barring flood, fire and termites, paper records can last much longer than a lifetime. Digital records take up far less space than hard copy and, in principle at least, are even easier to access. They are also cheaper to produce and distribute and, being much less of a physical burden, are perhaps less likely to be discarded when they are no longer making a good return. However, a big question with digital records is how long will they last. Certainly, that potential problem makes me nervous of putting trust in having only the digital versions of important things.

The more important polymer records are all available in libraries, or rather they should be available in a library. For almost all my working days I had the luxury of an extensive traditional library, with real paper books and journals dating back to the beginnings of the rubber industry, being immediately available for reference. That library was also available to the industry at large at a reasonable cost. I was given to understand recently that the facility I enjoyed is no longer available, and I have no idea what happened to the books. There was no problem with the longevity of the records, just a problem with human lack of care or interest, or one suspects a problem with making a traditional technical library sufficiently profitable. There have almost certainly been several other collections of polymer-related literature around the world that have been lost when companies closed, moved or were taken over.

The oldest of my files of technical data are less than 20 years' old but there are already some that I cannot read on the newer software, which necessitates at best the time and hassle of making conversions or otherwise having to keep old software on old computers. In this respect, there was very recently a report in a computer magazine that the world's first general-purpose emulator was being developed. This is part of a project known as Keeping Emulation Environments Portable (KEEP) an aim of which is to future-proof the technology so that every bit of software created can be coded to be read by newer and faster computers in the future. Clearly, this would be of immense value to future historians but I do not suppose the likes of you and I will be able to make use of it. There is also the small matter that the storage media of hard disc or CD will not last for ever, so back ups are essential. There may be no inconvenience in storing digital records for posterity, but if ongoing effort is needed to update files and storage media then I would hold no more hope for digital information being retained than for the collections of hard copy. I will be preserving my entire printed collection of *Polymer Testing*.

When the knowledge bases were first being developed, personal computers were fairly commonplace in laboratories, although by no means universal. However, the Internet was in its infancy as far as the world at large was concerned and I doubt anyone thought of it as fulfilling the role of multiple knowledge bases. I had access at home in 1995, which was before it reached most companies. The first editorial mention of the Internet was at the beginning of 1996 [13] when it was seen as pretty non-productive from a polymer information point of view. However, I must have been fairly astute because I saw that it was going to become a way of life. What seems not to have been apparent is that hyperlinks would allow connection between separate discrete pockets of information.

The next editorial [14] confirmed that no polymer information had been found on the Internet but went on to extol the virtues of a testing knowledge base. It must have been a bit later that the penny dropped that a knowledge base could be equally as well hosted on a website as on a disc, with hyper text markup language (HTML) being far less complicated than the specialist software that RAPRA had used. As an aside, we had a small lesson in markup languages [15] to draw attention to the fact that a language, MatML, was being developed specifically for web based material property data. It is apparently still being developed but it is not clear how widely it is used, and the development seems to be largely an American activity.

The Internet was the up and coming thing of the time and it featured again that year [16]. The news was that RAPRA Technology in the UK was launching a website specifically for polymer information and this was viewed as marking the beginning of there being a great range of polymer information available. RAPRA had invested in its own web server which would not only carry RAPRA information but space was on offer to the polymer industry to have their own pages and, hence, create an Internet focus for polymers. One of the first items to be uploaded was the *European Plastics Directory*, and companies listed in that could have a direct link to their own pages.

Clearly, the creation of a major Internet presence for the polymer industry with the added value of links bringing many companies' products and services together was exciting. Potentially, there could be a huge collection of technical and commercial information which could include knowledge bases. The editorial commented that maybe it was still a little early to be able to find online where to buy a closed cell content apparatus, and indeed place an order, or download software to make uncertainty calculations, but had no doubt that it was coming. However, I suspect that there were a large number of people who did not appreciate the force the Internet would soon become. In the next few years the number of websites exploded exponentially with just about every company clamouring to use the web to promote its products.

One year later [17], what was going to be the real-life situation was becoming more apparent. By then there was a lot more technical information to be found on the web but it was not there in the depth and quality which some people had hoped for. The bottom line with the web, as with everything else, is economics, and inevitably there will be tight limits on how much valuable data is made available for free. Roughly speaking, the situation is the same today, the amount of web information is now almost unbelievably vast, and the technology of search engines to help find it has become very sophisticated, but it is still in largely un-joined up discrete bits, and the best stuff you have to pay for. It makes a really good comprehensive knowledge base sound perfect.

Following from the Internet was the Intranet, which is basically the adoption of Internet technology to a private club. This can be a magnificent way to make data available to the chosen few, and might be termed an 'intra-knowledge base'. One early use we found was to host our ISO 9000 and United Kingdom Accreditation Service (UKAS) manuals and procedures – no fear of copies being out of date, there was only one copy. Nowadays, an appreciable number of people have a form of Intranet at home but, whilst it would probably distribute a music collection, you will not find the results of tests for the biological degradation of plastics. But then, your only chance of finding such data on the Internet would be to pay for it.

A particularly restricted form of local network is one that links computers in a laboratory [18], which in its basic form allows the free exchange of results between staff. It is difficult now to imagine not having this facility but it was quite *avant garde* in the 1990s. The more sophisticated set-up with the test equipment all linked is rather more challenging and has only been considered worthwhile in relatively few cases.

One sign of a mature industry is said to be when what is known is perceived to be worth more that what has yet to be discovered [19]. This means that further research suffers from diminishing returns with increased costs to investigate more obscure topics. How near we are to this with rubbers and plastics is debatable. However, a consequence of the deficiencies in the system for making data freely available is that much of what is known is in fact known only by relatively few people. This can be the only explanation of why there are still cases of the wrong choice of material, and incorrect processing leading to failures. It was concluded that, quite apart from any need for more information, we are still desperately lacking in the effective distribution of what we have got. I suspect the advances in technology have not done a great deal to change that over the last ten years.

This could bring us back to the abstracts databases [20] because papers in journals are a very important source of data. The increase in size of *Polymer Testing* was used to illustrate the growth in information available – growth which is not likely to slow down. The only hope of efficiently sorting and searching that vast mass of data is by the use of more and more sophisticated computer systems. Several such depositories of published information that can be searched online exist, but to solve the problem of not everyone having access is a financial matter.

Books, reviews and reports of major projects are the other essential stores of knowledge, but they are rarely available electronically so there is no computer search facility. Another downside is that they are usually expensive so many people are limited in what they can access. Research reports can be particularly pricey. The results of the RAPRA Long term Ageing Programme [21], together with the complementary accelerated ageing project, is a case in point. This was probably the largest ageing trial on polymers ever attempted, and it is most unlikely that anything similar will ever be done again. However, the objective of providing extensive and unique data on material lifetime was to some degree restricted by the cost of the reports [22] which only a relatively few people were willing or able to pay.

While you were reading this chapter people all over the world were beavering away to produce more data. I had a look at the iSmithers website where there were figures that give some idea of the magnitude involved. The Polymer Library virtual library contains over 1,000,000 records dating back to 1972, which equates to about 2.5 million pages of original published literature, with 22,000 new records added every year.

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15 What's Next?

The simple answer [1] is: 'I don't know!' Predictions made 50 years ago that we would have more automation, more non-destructive testing (NDT) and more dynamic testing were not wrong, even if not totally right as regards magnitude. They were projections on things that already existed so did not involve much lateral thinking. On the same lines now, we can expect automation and communications, including remote working, to become more and more sophisticated. However, 50 years ago we had no idea of the extent that computers would rule the world, nor of the Internet revolution. Fifty years of working in science and technology have not equipped me with the ability to foresee the next dramatic development to change our lives.

Applying simple logic, it would be reasonable to suppose that there will continue to be a need for tests on materials and products for quality control, design data and proof of fitness for purpose. However, I would expect that computer modelling will slowly improve such that quality testing can be simplified and performance of new materials estimated more accurately. One would also expect reduction in testing due to this to be balanced worldwide by increased production and no let-up in demands for quality and safety. NDT will always be highly desirable, albeit so often uneconomic. I can see NDT for quality control merging to some degree with computer modelling.

Product testing can only get better because it has suffered in many cases from rather *ad hoc* development and does not have the best record for reproducibility. It is difficult to see the call for proof of fitness for purpose going away, so it would follow that more effort will need to be put into devising test procedures for an increasing range of products, and the general standard should then rise. The level of sophistication of tyre testing might be the target to aim for.

I am lacking in vision to see fundamental changes in test equipment, only continued evolution. The properties needing to be measured are presumably not going to change, but it would be expected that the control of the equipment will become even more sophisticated (and complicated), as seems to be the ongoing situation with motor cars. It will also be very interesting to see how long it takes for the remaining ancient designs of mechanical equipment to be replaced (if indeed it actually ever happens). Maybe simple mechanical things will make a comeback.

The computer buffs have been prophesying various changes that will take place in the computers we use, from being able to wear them to speech input replacing the keyboard. Whatever; underneath they will still be computers doing even more, even faster. There is clearly still mileage in the development of communications. The logical next phase is always-on video/audio links between workers as the norm (and probably domestically as well). That would logically mean that more remote working would be on the cards, but I find it difficult to estimate how quickly that will happen. I'll stick my neck out and say not as quickly as many will forecast.

The scenario of remote working must mean some serious social changes if technicians are going to be at home with the children (who only have virtual schools) happily knocking out tensile stress–strain curves in a tensile machine farm some 50 km away. It will need the next generation twice removed to get their heads round that. A really unusual job would be having to travel to the farm to service the machines. But then, farm labourers always lived in tied cottages on site.

Changes in attitude, behaviour and the way of doing things seem to evolve quite slowly, and in some respects follow cycles of fashion. When it comes to business practices, management and education practices I can only hope that there will be a measure of rediscovering some of the old standards, but I do not really have much faith that it will happen. There are no signs that the world is about to become an easier place and the best bet is that things will get worse before they get better. In the case of education, the politicians in the UK have mucked about with the system so much that it just could be that they rediscover the best of the older models.

Logically, you would expect quality systems to go on being honed until getting a bad product was almost unthinkable – in most areas we do pretty well now. This forgets human nature and the equation between price and quality. Together, these factors will surely guarantee that there will always be a measure of shoddy goods that slip through and there will always be demand for relatively low quality goods at a giveaway price.

I am somewhat pessimistic about the future of standards, seeing a danger of the great advances made internationally being eroded away by national rivalries, apathy and lack of funding. It is only a few years before most of the present committee members will have gone to a testing Valhalla and young replacements are keeping themselves well hidden. One possible saviour would be for the work to be conducted by video conferencing, hence saving dramatically on time to attend meetings and making the cost less abhorrent to the bosses.

In the same way that I see a continued need for testing, there must be continued demand for material property data. To say that it must increasingly be such that it can be used for design (and for modelling) is to repeat what has been said for decades. Nevertheless, it must be true and it is simply that the time scale is very protracted. Whatever happens, the prophesy is not wrong. The other restriction is the cost of data generation, which is not about to get cheaper, meaning that access to the best data is likely to remain in the domain of the privileged.

We have seen the communication of information become more and more electronic with all the primary sources being stored on computers that can be accessed remotely. Reverting to pre-electronic times is unthinkable, but it leaves the question of how much communication in person and on paper will survive. My view is that conferences in whatever form will continue because of a basic human instinct to want to interact with others. The demise of printed books and newspapers has been forecast for ages and refuses to happen. I suspect there is another human instinct at work. Certainly for reference works, it is simply more convenient to pick up the printed volume than to go searching online.

When it comes to journals, the case for retaining paper copies seems very tenuous. You do not use journals as a reference work, but rather search for some topic across many journals when a particular need arises. Then, any relevant papers can be individually printed for further study. However, this is to a large degree irrelevant. What really matters is that there will always be a need for the results of research to be published and, to give some structure and order to the mass of output, we will publish them in something we all a journal, whatever form that might take.

One of the problems of conjecturing about the future is the difficulty of separating hype and wishful thinking from what is really likely to be feasible within a reasonable time scale [2]. My theory is that, whatever you predict, the odds are heavily stacked on the side of you getting it wrong because, however reasonable it may be at the time, there is a good chance that something new will come along to alter the course of things.

Apparently, it was suggested at a ministerial conference in Europe a few years back that e-Science could be the next thing after the rapid expansion of e-Commerce and that testing should be part of this evolution. It was not too clear what e-Science is, but it would lead to the setting up of virtual institutes by linking complementary research and industrial capabilities from scattered geographical locations. I leave you to decide whether or not the editorial was anywhere near the mark when it suggested the possibility of virtual institutes conducting virtual testing with virtual people which leads to virtual solutions.

References

1. R.P. Brown, *Polymer Testing*, 2010, **29**, 1, 1.

2. R.P. Brown, *Polymer Testing*, 2001, **20**, 4, 355.

Abbreviations and Acronyms

ACS	American Chemical Society
ASEAN	Association of South-Eastern Asian countries
ASTM	American Society for Testing and Materials
BETA	British Equestrian Trade Federation
BS	British standard
BSI	British Standards Institution
CD	Compact disk
CD-ROM	Compact disk – read only memory
CEN	European Committee for Standardisation
DIS	Draft International Standard
DIY	Do-it-yourself
DMTA	Dynamic mechanical thermal analysis
DOS	Disk operating system
DSC	Differential scanning calorimetry
DTA	Differential thermal analysis
EEC	European Economic Community
EES	Elsevier Editorial system
EFTA	European Free Trade Association
EN	European norm (standards)
ERP	Enterprise Resource Planning

FCS	Finite Capacity Scheduling
FTIR	Fourier-transform infrared spectroscopy
GC/MS	Gas chromatography/mass spectrometry
GCSE	General Certificate of Secondary Education
GUM	Guide to the Expression of Uncertainty in Measurement
HNC	Higher National Certificate
HTML	Hypertext mark-up language
ICCE	International Conference on Composites Engineering
IEC	International Electrotechnical Commission
IHRD	International Rubber Hardness Degrees
ILAC	International Laboratory Accreditation Cooperation
IOM^3	Institute of Materials, Minerals and Mining
IP	Internet protocol
IRC	International Rubber Conference
IRI	Institute of the Rubber Industry
ISO	International Standards Organisation
ISO TC	International Standards Organisation – Technical Committee
KBS	Knowledge base system
KEEP	Keeping emulation environments portable
MOD	Ministry of Defence
MRP	Manufacturing Resource Planning
NAMAS	National Measurement Accreditation Service
NATA	National Association of Testing Authorities
NATLAS	National Test Laboratory Accreditation Scheme
NDT	Non-destructive testing
NIST	National Institute of Standards and Technology

NMR	Nuclear magnetic resonance
NPL	National Physical Laboratory
NWIP	New work item proposal
PC	Personal computer(s)
ppb	Parts per billion
PVC	Polyvinyl chloride
RABRM	Research Association of British Rubber Manufacturers'
RABRTM	British Rubber and Tyre Manufacturers
RAPRA	Rubber and Plastics Research Association
SI	Internatinal System of Units (in English)
TCP/IP	Transmission Control Protocol/internet protocol
TGA	Thermogravimetric analysis
TGA	Thermogravimetric analysis
TMS	The Materials Society
TRRL	Transport and Road Research Laboratory
TWA	Technical Working Areas
UKAEA	United Kingdom Atomic Energy Authority
UKAS	United Kingdom Accreditation Service
UV	Ultraviolet
VAMAS	Versailles Project on Advanced Materials and Standards
www	World wide web

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Published by Smithers Rapra, 2009

Since the beginning of the rubbers and plastics industries, an essential requirement has been that the materials and the products are tested for quality control, to establish their fitness for purpose and to provide design data. This book gives a unique personal account of the developments in the technology of physical testing of polymers and of the changes in the working environment in which testing was conducted over the last fifty years.

The authoritative account reflects the author's position heading the testing laboratories at RAPRA and his involvement in international standardisation for many years. Much of the structure of the book is based on the editorials that have appeared over the thirty years that the author has been the editor of Polymer Testing Journal. They cover observations on the evolving changes in the conditions and attitudes in laboratories, the interaction of commercial and social pressures and the revolutions in instrument and information technology that have impacted on testing.

In providing an appreciation of the past history of testing, it is intended that this volume will contribute to the further development of testing rubbers and plastics in the future.

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