Korea-Australia Rheology Journal
Vol. 11, No. 4, December 1999 pp.265-268

Lessons from history

K. Walters
Department of Mathematics, University of Wales, Aberystwyth, UK

Abstract

The main events in the historical development of Rheology are traced and particular attention is paid to the leading players, the controversies, the priority disputes and the nomenclature disagreements. Some of the lessons to be learned from the past are then highlighted and a positive assessment is given of the prospects for rheological research in the next millennium

1. Introduction

As we rapidly approach the next millennium and contemplate where rheology is going, there must surely be a place for an objective study of the lessons that history can teach us. Of course, there is always the danger that John Ford was right when he said, History is bunk and that there is some truth in the jaundiced view of Samuel Butler that though God cannot alter the past, historians can. Hopefully, amateur scientific historians can be more objective!

In one sense, ours is a relatively recent science, but, in some respects, rheological concepts were appreciated in antiquity. Having said that, it is generally acknowledged that the great British contemporaries Robert Hooke and Isaac Newton set the early boundaries to the field in the 17 th century, although it has to be admitted that a further 150 years or so were to elapse before scientists realized that there was any need to consider anything other than the classical extremes of Newtonian viscous fluids and Hookean elastic solids. A further century was to go by before Rheology was introduced as a formal scientific discipline in 1929, and no one would question the statement that the major advances in rheology have taken place since the end of the Second World War, i.e. in the latter half of the present century. In the main, therefore, history for us is very much recent history. Nevertheless, it has its own fascination and it has much to teach us.

In this lecture, I hope to map out the main focal points in the history of our field. I shall then draw on the major events and associated personalities to make some general comments, before discussing how these can help us as we approach the next millennium.

History is very much in the present writers mind, partly because he is old enough to have lived through much of the relevant part of it, but mainly because he has recently completed a book on the subject entitled Rheology: An Historical Perspective (Elsevier 1998) written in collaboration with Professor R I Tanner of the University of Sydney, Australia. Indeed, most of the ideas propounded here are expanded in the book, which is therefore recommended as an explanatory text for those who would wish to delve deeper into the subject.

Corresponding author: kew@aber.ac.uk

2. Some Obvious Highlights

As we have already noted, the work of Hooke and Newton form the boundaries of our field. In 1678, Hooke provided a description for an elastic solid, which has formed the basis of what is commonly referred to as infinitesimal strain elasticity. Nine years later, Newtons famous hypothesis on fluid behaviour appeared in the Principia. The concept of internal friction (of thinness and thickness) was well known in antiquity, but Newtons ideas about lack of slipperiness were to have an enormous influence on the field, this despite the fact that, judged by the standards of his other scientific discoveries, the contribution was short and undeveloped. So, we readily refer to common fluids like air and water as Newtonian and *non-Newtonian* Fluid Mechanics is an important sub area of rheology, with its own ethos and journal. Interestingly, non-Newtonian does not refer to materials which do not satisfy Newtons 1687 postulate, but rather non compliance to the Navier-Stokes equations, which were based on Newtons ideas, but developed much later in the 19 th century. The well founded attempt on the part of some to redefine Newtonian fluids as Navier-Stokes fluids is therefore reasonable, even if it has proved to be futile.

Why did it take scientists so long to realize that wide classes of materials lay outside the classical extremes of Hookean elastic solids and Newtonian viscous fluids? We can only hazard guesses. What is clear is that Wilhelm Webers 1835 work on the mechanical response of silk threads is seen as the first concrete and unequivocal example of a viscoelastic solid, with a material response that is most decidedly outside the classical extremes.

In a similar manner, James Clerk Maxwells 1867 work on the dynamic theory of gases is seen as heralding the introduction of *fluid*-like materials which show some solid-like characteristics. Interestingly, the relevance of this work has had an incredibly long life-span and, at the end of the 20 th century, we still read of the Upper Convected Maxwell model, which is based on Maxwells 19 th century work, suitably embellished with 20 th century continuum mechanics.

The emergence of *viscoelastic* materials lying between the classical extremes of Hookean elastic solids and Newtonian viscous fluids highlighted the need to define carefully what one means by the terms solid and liquid. Interestingly, Maxwell provided suitable definitions, which have stood the test of time. He wrote What is required to alter the form of a (soft) solid is a sufficient force, and this, when applied, produces its effect at once. In the case of a viscous fluid, it is time which is required, and if enough time is given, the very smallest force will produce a sensible effect.

Nowadays, the time scale of the deformation process experienced by a given viscoelastic material is seen as being of crucial importance in determining whether the response is solid-like or fluid-like. Perceptively, the issue was addressed by G Fano as early as 1908. He wrote Spinnable matrials are distinguished above all by the fact that, under suitable conditions, they show elasticity in tension to such an extent that one is led to consider them, in respect of some properties at least, to be more like solids than liquids. Here, we have an early contribution which anticipated Marcus Reiners Deborah-number ideas. These were to follow nearly 50 years later.

Two other famous scientists, Lord Kelvin and Ludwig Boltzman, were to make seminal late 19^{th} century contributions to the general subject of (linear) viscoelasticity. However, it was left to later generations to make the crucial step from linear to nonlinear behaviour.

The developments in the first half of the 20th century were slow, with little cross-referencing, and it was left to scientists in the second half of the century to provide the necessary theoretical framework to describe nonlinear behaviour. This coincided with major advances in experimental rheology and the post-war years are rightly seen as the dominant period of rheological research.

3. Controversies

The overview given in the preceding section would be generally accepted and most current workers in the field of rheology are proud of the fact that great scientists of the calibre of Newton, Maxwell, Kelvin and Boltzmann have produced lasting work in their field, as well as in others. However, human nature being what it is, we should not be surprised to find that, just below the surface (and sometimes even above it!), there have been clashes of personality, priority disputes and controversy. Nowhere is this better seen than in the antipathy between Hooke and Newton. It seems that this initially arose through a priority dispute related to the inverse-square law. The dispute was never really resolved and the reputation of these two great scientists was somewhat tarnished by what now seems like a petty squabble.

An in-depth study of the historical development of rheology indicates other more damaging disputes. For those with a strong interest in linear viscoelasticity, the 19th century dispute between Oskar Meyer and Ludwig Boltzmann is relevant. In 1874, Meyer from Wroclaw in Poland had introduced what we would now recognise as the equation for a so called Kelvin/Voigt solid. In passing, we note that Meyers claim for priority of discovery is at least as strong as that of Kelvin and stronger than that of Voigt. That be as it may, the point we wish to make here is that, in the same year (1874), Boltzmann criticised Meyers work for its lack of generality and he proposed his now famous three-dimensional integral formulation of linear viscoelasticity, which has stood the test of time in a remarkable manner. Meyer in turn attacked Boltzmann, who replied to the criticisms in two further papers, before leaving the field and devoting himself to other matters.

Our third example of rheological controversy concerns two scientists who worked in the same university! At the turn of the present century, Ladislaus Natanson of the University of Cracow, Poland, introduced a *nonlinear* equation for a rheological time derivative. Stanislaus Zaremba perceptively criticised the work because the time derivative was not written in a frame rotating and translating with the medium. Zarembas work contains the classic put down: Il est donc prouvé que les equations de M Natanson sont absoluments inexactes. It seems that Natanson was Zarembas superior and the exchange ended in 1903.

What is not generally known or conceded is that Zaremba essentially introduced what is sometimes known as the Jaumann derivative, but is now more commonly referred to as the corotational derivative. Since Jaumann did not publish his work until 1905, admirers of Zarembas work can be justifiably aggrieved.

The last controversy we shall refer to concerns the vexed area of constitutive modelling in the period between 1950 and 1965. This was in many ways a time of unprecedented advance, but it was hindered somewhat by polarized attitudes.

In 1950, J G Oldroyd wrote a seminal paper in which he used convected coordinates to derive permissible processes of time differentiation and time integration in constitutive modelling. Somewhat later, other workers chose to use a rotating-frame methodology to accomplish the same objective. There was scant reference to Oldroyds work in these later developments. With the benefit of hindsight, the objective observer can now see that Oldroyd, Coleman, Noll, Rivlin, Ericksen, Truesdell and their coworkers were using *alternative* methods to arrive at essentially the *same* conclusions. There was some understandable difference of opinion concerning the status and role of simple constitutive models (like the ones designated A and B by Oldroyd), with a strong preference for generality on the part of many. At the time, this was an entirely proper discussion, but it was one of philosophy and emphasis rather than content. Not for the first time, the way forward was eventually acknowledged by most to involve a search for generality that was consistent with utility and tractability.

One can readily cite evidences of strongly held views on all sides in this productive period of constitutive activity, but these simply delayed rather than prevented real progress.

It would be wrong to paint a picture that would imply that rheology has had more than its fair share of controversy. The examples we have quoted are probably comparable in impact to those in any other developing field of science and they simply reflect the human condition. Indeed, somewhat less obvious are examples of close friendships and magnanimity. The friendship between Maxwell and Kelvin is an excellent example of this, as is that between the original founder members of the Society of Rheology in 1929, namely Eugene Bingham, George Scott Blair and Marcus Reiner. Later examples are also easily produced.

It is also appropriate to mention the growing tendency for international collaborative ventures, since these have enriched rheological research in the latter decades of the 20th century. The influential programme of research on the polyethelyne melt called IUPAC A comes readily to mind in this connection as does the equally relevant collaborative programmes on the polymer *solutions* designated M1, A1 and S1. These are prominent examples of an encouraging trend.

In some cases, the international dimension has resulted in a series of focused Workshops. A good example is the series on The numerical simulation of non-Newtonian flow. This was conceived at an IUTAM meeting in Belgium in 1978 and has spawned 11 international workshops since that time. There is a general concensus that such workshops have enriched and enhanced rheological research.

4. Priority disputes and nomenclature disagreements

In the course of its history, rheology has had more than its fair share of priority disputes and nomenclature disagreements. Some of those arose from the difficulties of publishing research work in the open literature during wartime. Specifically, work on flame-thrower gels during the second world war spawned innovative research, which was classified and remained unpublished until the war ended. One consequence of this is that questions have been raised about the appropriateness of calling the rod climbing phenomenon the Weissenberg effect, although the wartime players seem less concerned about the issues involved than some later rheologists.

Another subject which came into prominence at the same time was drag reduction. This has also generated priority disputes. The relevant phenomenon quickly became known as the Toms effect after Brian Toms, the British rheologist who first published on the effect in the Proceedings of the 1st International Rheology Congress. Not surprisingly, comparable work was carried out in the US at the same time and some believe that Karol Mysels should be credited with the discovery. There is little doubt that these questions will continue to be addressed by succeeding generations of rheologists, but the answer to such questions is far from straightforward and it is doubtful whether the issues are worth further study in any case.

On occasions, priority disputes and nomenclature disagreements have been overtaken by events. For example, whether what is now called extrudate swell should be called the Barus effect or the Merrington effect has occupied a number of idle minds at a time when most rheologists were happy to talk about die swell. More than one rheologist has made the obvious comment that the term die swell is inappropriate, because the die doesnt swell! Fortunately, there is now a general consensus that extrudate swell is the preferred terminology and, accordingly, we should be spared any further discussions on the matter in the secondary literature.

5. Hidden treasures

One cannot study the history of rheology without being impressed by some important contributions which appear to have been hidden or forgotten for decades. We have already referred to Zarembas work on constitutive modelling and its neglect by later workers. Interestingly, a similar comment can be made about the work of Jaumann, which itself had scant attention from many later workers.

The 1933 work of Hans Fromm is worthy of a mention at this point. Decades before its time, Fromm studied theoretically the rheometrical behaviour of what one would now call the Corotational Maxwell model. He obtained the correct response. More than 20 years later, numerous theoretical rheologists, including the present author, would be carrying out similar exercises for equally simple constitutive models!

Before we leave the subject of hidden treasures, mention must be made of the work of Hanswalter Giesekus. His early papers contain several important discoveries both theoretical and experimental, which, for one reason or another, have been *rediscovered* many times since!

6. Lessons from history

As fascinating as reminiscing about the past can be, it is important that we view the advances, set-backs and personality clashes as aids to the future. With this in mind, we shall now highlight the more obvious lessons.

a. Like many other fields of science, rheology has been plagued by the cult of personality. Is the present author being overoptimistic or naive in feeling that these are less obvious now than they used to be? He doesnt think so.

b. In the development of rheology, there have been several cases of reluctance to cross reference and to give credit to earlier work. Is this now an unlikely occurrence? One would like to think so! Certainly, the availability of BIDS and other cross referencing aids will make it less likely, but there remains the real danger that only work cited in these aids, i.e. post 1981, will be cross referenced, with a complete disregard for any work published before that date! At the very least, new workers in the field should be encouraged to scan the post 1945 literature to ensure that their project is novel and original.

c. We have concluded that there has sometimes been unnecessary polarization with the need to identify with one school or another. It is difficult to imagine that this unfortunate state of affairs can be eradicated completely, but we can at least make a plea for more objectivity and magnanimity.

d. It has often been said that rheology is a difficult subject. However, in the past, there have been attempts to overemphasise the difficulties and to make the subject over complicated. Nowhere is this better seen than in some of the continuum mechanics literature. At the same time, attempts to *over simplify* the subject have been less than helpful. In the future, we should heed the advice of Albert Einstein: Everything should be as simple as possible, but not simpler.

e. Surveying the field of rheology from the earliest days, it is apparent that it has, from time to time, occupied the attention of the greatest of scientists; men like Newton, Maxwell, Boltzmann and even Einstein. It is doubtful whether the future will be as kind to rheology, but there are indications that some of the young scientists entering the field are of the highest quality. One thing is certain. There are many taxing problems which still require resolution and we can remind ourselves of the philosophising of Isaac Newton as he neared the end of his life.

"I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the seashore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, while the great ocean of truth lay all undiscovered before me"

f. In the 18 th century, the British statesman Edmund Burke wrote: You can never plan the future by the past issuing a kind of guarded warning to us. However, we can be encouraged by another of his quotes: The past should be a springboard, not a hammock.