ANNUAL TRANSACTIONS OF THE NORDIC RHEOLOGY SOCIETY, VOL. 15, 2007

Fibre Flow Research History: Part II. Continuation

Ulf Björkman

Dept. of Fibre and Polymer Technology,
Royal Institute of Technology (KTH), S-100 44 Stockholm, Sweden.

ABSTRACT

The history of fibre flow research is continued. Own experiences are related to principal questions about theory formation for technical fibre flow systems. Suggestions for future modelling are presented.

INTRODUCTION

In the first part of this research history[74] was shown that the theoretical development was founded rather on mid-19th century microhydrodynamics, early 20th century physical and colloid sciences than on fibre flow experiments carried out in the industry.

First some terms should be defined. A *floc* here means a gathering of objects (birds, fibres, globulins, etc.), *flocky* something consisting of flocs *or* giving the impression of being that. *Flockiness* is the character that gives the flocky appearance *or* the degree of agglomeration. *Flocculation* is the classical process (e.g. *colloidal*) of forming flocs by bringing objects together through directed motion (*orthokinetic*) or undirected (*perikinetic*), the latter being what remains when the first has ceased (e.g. Brownian motion). It should, however, be kept in mind that, as for e.g. technical fibre suspensions, flocs may also form through the break-up of networks (*splitulation*).

EXPERIMENTAL, CONTINUED

The advancements in colloid science in the first quarter of the 20th century, see e.g. Ostwald[75] (1916), were followed in the pulp and paper industry. Over the centuries, practical knowledge of the influence of various parameters had accumulated. Paper uniformity was then discussed in terms of fibre type (hardwood, softwood, rags, straw, etc.), stock composition (i.e. fibre mixture), processing (sulphite, Kraft, etc.), consistency (i.e. fibre concentration), degree of grinding, mixing intensity, temperature, contents of alum, bentonite clay, gums (e.g. Locust bean, Caraya) etc. The traditional papermaking tool for visually judging grinding results was the *blue glass*; i.e. the fibre suspension was spread on a cobalt-stained glass plate.

The scientific basis for this papermaker knowledge of the effects of pH and alum began to be discussed in scientific terms around 1930 by Campbell and Yorsten[76] at Canadian Forest Laboratories (later Paprican). They studied the *flocculation* in very dilute fibre suspensions passed through a screen, and reported electrolytic effects on e.g. the retention of fine material. Strachan[77] in 1935 discussed the degree of beating, the degree of agitation and the fibre dimensions, but also mention "colloidially active material present in stock solution" as influential. In 1939 Wollwage[78] reported a larger investigation at Kimberly-Clark. In a *flocculation* tester consisting of a mixing chamber followed by an 8-foot long 3-inch diameter glass tube, the gradual appearance of *flocs* was studied bye eye and camera. "Laminar" flow was maintained by keeping the water-based Reynolds' numbers well below 2100. To allow observation of the formation of the whitish flocs against the black background, *cf.* the blue glass, the standard volumetric fibre concentration c_v was kept as low as 0.01%. For fibres with technically realistic lengths l_f and widths d_f corresponding to aspect ratios $r = l_f / d_f$ of about 100 this gives the *fibre centre span* $N_{cs} \approx 0.966(c_v \cdot r^2)^{1/3} \approx 1$, Björkman.[79, 80] This *linear* measure is useful in imagining the inner geometry of a fibre suspension, Fig. 1.

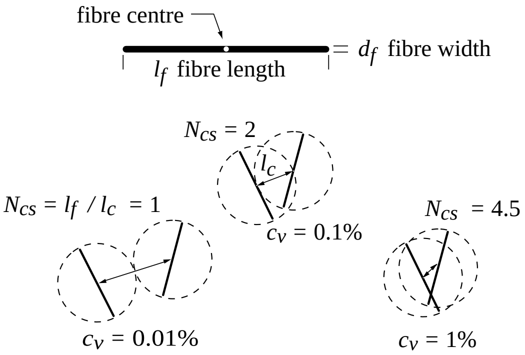

Figure 1. The fibre centre span N_{cs} is the ratio between the fibre length l_f and the distance between the fibre centres l_c when the fibre centres are distributed evenly in space, i.e. rhombohedrally. Three examples with aspect ratio $r = 100$ are shown.

Wollwage (*ibid.)* studied the influence of fibre content between 0.005 and 0.02%, corresponding to $N_{cs} \approx 0.8$–1.3, and a marked effect on flocculation was observed. This was, however, interpreted in terms of *statistical* non-uniformity, rather than due to fibre entanglement.

At late 1930's the main actor in fibre flow after WW2 made his appearance, *viz.* Stanley Mason, later professor in chemistry at McGill University. He started his career under Otto Maass, also chemistry professor at McGill. In one of Mason's[81] first works from 1940 he already displayed all his characteristics. He designed an oscillating disc viscometer for measuring the viscosity of ethylene in the critical region, possibly a part of Maass' research program. Mason's instruments were always masterly designed and built, although often not entirely new in conception. A good supply of students and skilled co-operators, e.g. Alan Robertson, carried out the experiments. Mason was also fortunate in having economic support directly from Maass' influential student and his own friend Edgar Steacie, president of the National Research Council of Canada.[82]

In his first central fibre suspension work from 1948 Mason[83] summarised what he had achieved hitherto and also pointed forward to his work in the 1950's. In the introduction he declares it necessary to clear out 'doubtless erroneous ideas by the term "flocculation" when applied to pulp suspensions'. He continues by describing coagulation in colloidal systems, and concludes that the difference to his systems is that Brownian motion does not play a role for fibre motion. The importance of liquid motion for bringing the fibres together is emphasised, followed by a discussion of the motion of individual fibres. Jeffery[40] (1922) is not cited directly but Burgers[48] (1938) and Goodeve[60] (1939) are included in the reference list where these early works can be found. Mason discusses the effective fibre volume in flow fields by taking the length of the straight fibre as the diameter of a sphere, and finds that for a normal fibre this is about 250 times the fibre volume. "This rotation therefore provides a means whereby the chance of a collision of two fibres is greatly increased" and "if the consistency exceeds 0.13 per cent, the fibres will interfere with one another, and collisions will become inevitable." To what extent he was aware of the pre-war tradition is not known. The *critical concentration* and *effective volume fraction* concepts, however, corresponds to similar concepts used by Staudinger[64] (1932) in his attempts to explain the linear dependence of the specific viscosity for homologous series of polymers with the help of the Einstein's viscosity formula. Eisenschitz[65] in 1931 discussed the relation between Staudinger's view and Einstein's formula. The result of Mason's as yet unpublished flocculation experiments in a large transparent Couette instrument was reported in 1950 in Hubley *et al.*[84] This work is basically an extension of Wollwage's (*ibid.*), including automated measurement of the flocculation with a photocell and a somewhat strange flocculation measure. In a slightly later work, Mason[85] adds "chemical flocculation" to "mechanical flocculation".

According to Steenberg,[82] Mason's efficient radius calculations played an important industrial role in the late 1940's and in the 1950's by inspiring the eventual decrease in headbox fibre concentration from about 1% to about 0.1%. This led to better paper formation at higher machine speeds if the wire section was lengthened.

Mason's research in the following years focused on studies of the motion of individual or a few particles in well-defined flow fields, often comparing with Jeffery's[40] (1920) theoretical solution. Cinematographic recordings were here central, e.g. Mason,[86,87] *cf.* Henri[27] (1908) and Eirich[57] (1936). Mason's entire fibre flow *oeuvre* was summarised in 1967 in Goldsmith and Mason,[88] in the series *Rheology. Theory and Applications* edited by Eirich. Here is repeated that fibre flocs are formed through collisions with direct reference to Smoluchowski.[33,34] Mason's crowding idea[88] was in the 1980's developed to the *crowding factor* by Kerekes *et al.*[89]

Although, somewhat in the outskirts of this rheology history, Mason also continued the pre-war tube flow tradition, by e.g. Brecht and Heller (1935).[7,8] Here he, however, used rather dilute suspensions to be of general technical interest. One original tube flow experiment, should, however, be commented since it may distract from the basically non-coherent nature of technical fibre suspension. Thus, in Forgacs *et al.*[90] from 1958 the tear strength of a fibre network plug is determined through a modified tear length measurement in a vertical tube. Basically such networks have, however, no inner strength. It is the tube wall that keeps the suspended network together that gives this.

Later Mason broadened his approach to include also colloidal effects. This may have been influenced by Steenberg's fibre flocculation studies in the 1950's, also with dilute systems, e.g. Andersson and Brunsvik[91] (1961), *cf.* Wollwage (*ibid.*). In 1953 namely Mason and Steenberg arranged a one-year switch between Olle Andersson and Alan Robertson to exchange knowledge [they even switched apartments]. During this year Robertson just made a copy of his instrument in Montreal. Steenberg's own fibre flow tradition did not, as did Mason's, rely on microhydrodynamics but more directly on the colloidal tradition. Steenberg's professor Arne Westgren at Stockholm's University in the beginning of the 20th century studied gold colloids and was proud of being cited by Smoluchowski.[33] Mason's interest later included blood rheology, i.e. where Eirich had started 1929 under Wolfgang Pauli Sr.[74] Also here Mason made important contributions.

Mason died in 1987, Mark in 1992 and Eirich in the 1990's, but Steenberg steams on, at 95 still upsetting his surroundings and office wall-in-wall with the author. Mason's suspension research was continued by his student Theo van de Ven[92,93] (1989, 2006), now also chemistry professor at McGill.

THEORY, CONTINUED

In the first part of this research history[74] the microhydrodynamic development was followed from its start by Stokes[42] in the 1850's to Jeffery[40] around 1920. A number of other development lines also exist that should be outlined before continuing.

One line went towards higher velocities with initially a "paradox" presented by Alfred North Whitehead[94] in 1889, of later philosophical fame together with Bertrand Russel. This was solved by the original Carl Wilhelm Oseen[95,96] (1910, 1927), physics professor in Uppsala. The development was continued by his student Hilding Faxén[97,98] (1921, 1922), later professor in Mechanics at KTH, and was also elaborated in the many editions of Horace Lamb's Hydrodynamics[99] (1924–32), see e.g. Lindgren.[100]

Another line, partly covered in Part I,[74] went towards more general flow fields like Einstein,[12,17] and another toward non-spherical particles, e.g. ellipsoids by Oberbeck[101] in 1876. The two paths coalesced in 1921 in the work of Jeffery,[40] and apropos coalesce another path with deformable particles (drops and elastic spheres) was opened by Taylor.[102, 103]

Various interactions, e.g. with walls had been treated before by e.g. by Smoluchowski[32](1912) and Faxén[97,98](1921,1922), interaction with other particles by Smoluchowski[32] in 1912, Vand[104] in 1948 and with a cage model 1952 by Simha,[105] etc. At the high concentration end, attempts with thin layer theory were later tried, e.g. for rigid spheres by Frankel and Acrivos[106] and for stiff straight fibres by Batchelor,[107] both from 1967. With percolation theory de Gennes[108] [Nobel Prize in 1991] in 1979 modelled the transition from individual rigid sphere flow to plug flow. The state of art in 1965 was summarised by Happel and Brenner.[109]

With the development of rheology in the 1950's corresponding calculations were repeated for more complex suspending fluids. Since this development less directly concerns fibre flow, we refer to the references in e.g. Schowalter.[110]

Not much later, the modern computer development started and numerical methods (computational fluid dynamics, CFD) could manage more complicated cases than before. The arrival of personal computers brought these simulations to the researcher's desktops. In e.g. Kim and Karrilia[111] from 1991 results like in Happel and Brenner (*ibid.*) were formulated more directly for computers. Numerical methods for non-Newtonian liquids were treated in e.g. Crochet *et al.*[112] The basic idea is, however, principally the same as used already by Stokes, *viz.* to solve the flow equation for the continuous phase with the particle surfaces as boundaries.

The first to apply CFD for flexible fibres (linked rods) was Klingenberg[113,114] (2000, 2003). The resulting animations *look* real, but here one is faced with the formidable experimental task of verifying that this really is the case. For natural fibres the CFD approach is in addition principally impossible with all fibres individual and with the individual properties unattainable. CFD can, therefore, not be the general solution for fibre flow.

This reminds of similar problems with earlier statistical theories for fibre networks, e.g. Meyer and Wahren.[115] Also here simulated results can be brought in reasonably good agreement with experimental in spite the statistical distribution of the fibres in space has *nothing* to do with the actual formation mechanism (in addition, the role of the liquid was forgotten). The same critic applies also the paper formation and derived paper mechanical literature, Deng and Dodson.[116] Nothing is wrong with statistics *per se.* The principal doubts begin when statistical formation methods start to be regarded as physical forming mechanisms without experimental evidence that these actually occur in reality, as has e.g. been made for paper formation.

RHEOMEMBRANCES

My own engagement in rheology started in Biochemical Engineering with fungal fermentations, e.g. the Penicillin process. The porridge-like consistency of these non-Newtonian fermentation fluids is caused by the fibre-like hyphae, and results in mixing problems in industrial fermenters.

As not uncommon, I suppose, rheology had to be learnt through self-teaching, and in my case practised with a borrowed Haake Rotovisco. This was funny, with its gated gearshift lever almost like driving a Ferrari. The literature was less funny. Skelland[117] was passable but Coleman, Markowitch and Noll[118] required prolonged chewing.

Actually, a small research tradition in fungal fermentation rheology existed, with about six original works, Björkman.[119] The earliest, from 1954, also concerned the Penicillin process and was by one of its fathers, Ernest Chain[120] [German/English biochemist, Nobel Prize in 1945 for the penicillin process with Alexander Fleming and Howard Florey].

In such studies, typically, the rheology is followed during the fermentation, the rheograms adapted to some standard model (Bingham, Ostwald-deWaele, etc.), the model parameters plotted against fermentation time together with other fermentation parameters like cell volume, fibre morphology, etc. to help identifying parallel developments and eventual simple relations.

Anyone who has made this for fungal fermentations knows that the microhydrodynamic models are useless. Stress may go down whilst cell volume goes up or *vice versa.* It is very evident from the visual appearance of the mycelium (wetness, fluffiness, etc.), filtration properties, etc. that surface chemistry also plays a role. The rheology of such living suspensions is also very labile. A few minutes outside the fermenter may be sufficient for the consistency to decrease markedly due to oxygen starvation. Some researchers adopt a dilution technique for one and the same sample and manage to get correlations with cell volume, but this is doubtful since the same correlation cannot be assumed to be valid for an entire fermentation, and therefore being of small practical value. Such problems and other led over to more general questions about flow mechanisms.

Somewhat earlier, in 1973, a first microrheological attempt appeared, Roels *et al.*[121] Here Casson's model developed for printing pigments in oil ink was adapted to mycelial mashes. Dilution technique was used and also so-called turbine rheometry based on Metzner's *efficient viscosity* concept, which then caused myself[119] and later Duffy[122] confusion, and also played a role in the arrangement of NRC2006 at KTH.

The 8th International Rheology Conference in Naples, 1980 was memorable for a number of reasons, i) Clifford Ambrose Truesdell III, continuum mechanics' poseur par préférence, opened with his favourite subject constitutive equations, ii) R.B. Bird graced us at distance by delivering his invited lecture through a herald, and iii) Stan Mason presented a film containing fibre motion, my only meeting with him. I could then not see the connection between his work and my own. I still cannot.

It took years before it was realised that there was special branch called fibre flow, and that was perhaps fortunate. The word fibre then just gave associations to textiles. Possibly, the smallness of the mycelial fibres (barely visible under flow) made a floc approach natural, like in e.g. the active sludge process that also belongs to Biochemical Engineering (although a comparison with such chemically flocked systems was later understood to be wrong).

When finally the long fibre flow research tradition was found it came as a surprise that it had to not a small extent been formed at the pulp and paper research institute STFI about 200 metres north of my work, and about 100 metres south where the micro-hydrodynamic tradition of Oseen and Faxén continued at Mechanics.

One day a work was found that contained something I had not thought about, *viz.* bulk compression. With the paper branch's long tradition in wet pressing this had worked its way into fibre flow,[123] and a *mechanistic* model for plug flow been developed at STFI by Moller, Duffy and Titchener[124](1973). I had before only thought in terms of floc surface alterations. I rang up and found that Klaus Møller was still there (he is now at Norske Skog), ran over and complimented him for such a good idea and was met by the answer that it was not good at all. Asking why, he said that they had shown it for the Mechanics people and they said it was "unphysical".

I had no reason to doubt this, but to understand why embarked on a prolonged struggle with Truesdell and Toupin[125] and similar that in the end gave not the least better understanding of fibre flow. Therefore, Rivlin's opening lecture[126] at the 9th Int. Rheology Conference in Acapulco, my last, was heard with satisfaction. Although his own books on finite deformation were more readable, they were equally useless for the non-coherent fibre systems that could withstand just small strains before rupturing. Still today I think the model of Møller *et al.* is good. Perhaps not perfect, but what model is that, it in a simple way catches the relevant physics.

To what extent Truesdell's *rational mechanics* is rational may thus be discussed, and in the end he had himself to admit that no fundamental restrictions on e.g. the strain-energy function, besides those arising from symmetry and frame-indifference, can be obtained through tensor juggling.[127] What he, however, understood well and that I benefited from (e.g. in N_{cs}) was that basic definitions should be kept free from mechanistic assumptions (not to speak about superficial analogies). For a person who started with BSL[128] it took years to straighten this out. Last time I passed Madison, Bird was busy preparing its 2nd edition and I asked if they this time would change sign in $\tau = -\mu\dot{\gamma}$. But no, and meanwhile BSL is getting heavier for each new printing due to even broader margins and even more fillers (clay, cheaper than fibres) until finally no one manages to lift it. Then, time is hopefully ripe to say goodbye to that piece of *non-rational mechanics* that has caused myself and other investigating materials that contain both viscous and elastic phases *unnecessary* extra confusion. This is a pedagogic problem.

A practical problem thus concerned the smallness of the mycelial fibres that prevented direct observation of flow details. One effect that e.g. was difficult to understand was how network ruptures could continue to widen after initial rupture, when they were filled just with an inelastic water solution. When later changing to the about ten times larger and less labile wood fibres everything became easier. By arranging the experiments properly one could just sit down and see what happened. This resulted in a 90°-turn from the traditional primarily stretch-out view of continuum mechanics to a primarily compressive view, which is natural for these non-coherent floc systems. After this, earlier poorly understood phenomena got simple explanations but much certainly remains to be explained, Björkman[5].

Not a frequent seminar visitor, one, however, changed my view of the fluid dynamic flow theory and that was attended by mistake. In the middle of the 1990's I went to Mechanics to hear about Frederick Lanchester's scientifically conceived cars[129] but the lecturer, the late Martin Ingelman-Sundberg, talked about his flight theory from the late 19th century.[130,131] This was, however, just an excuse for discussing modern views about the origin of the lift. Sundberg[132] like Lanchester counted with air's inertia (like also Lillienthal and already the keen observer Leonardo wrote, "The sail drags the boat because it deflects the wind" and similar about birds[133]). Not especially controversial I thought, but to my surprise the attending professor in flow physics heckled the lecturer through stage whisperings so that the speaker's few supporters who had met up shrunk and did not dare to open their mouths among the professors and their research students. When the flow physicist finally, I could hardly believe my ears, denied that air's density played a role I, who never open my mouth in seminars, was forced to support the speaker. The professor then exploded and stormed out of the room. Afterwards, I realised that I had tripped into an absurd dispute about matters I thought had been settled at least 100 years ago.

The easiest way of understanding this comic scene would be that this professor was not too clever, but this cannot be the whole truth since, as I later learnt, even one of the most cleverever professors once fell into the same trap.

In 1916 Einstein wrote a popular article about waves and flight.[134] In 1917 he was consultant at the German aircraft company LVG to design a more efficient wing and came up almost triangular profile that was patented. After tests in the Göttingen windtunnel it proved so spectacularly ineffective that he was never again asked to design aircraft components.

Now Einstein had no problem in admitting his many mistakes, learn from then and move on. His great achievements, I suppose, must be ascribed partly to this flexibility in mind. In this case he first attributed the failure to "a man who thinks a lot but reads little". In 1954 a former LVG employee, Paul Ehrhardt, wrote him about his "cat's back airfoil" and got the answer that he had just applied Bernoulli's equation, but not developed the idea further to include torque balance etc. up to downwash.[135] His letter ends "I have to admit that I have often been ashamed of my folly in those days …" and in his Fig. 5 it is easy to see where his intuition went wrong by applying traditional continuum reasoning in wind tunnels and transferring to free flight. The text reveals that he thought his theory was new. I don't know if it was, but his arguments remind of Rayleigh's for the Magnus effect from 1870, and that ought to have been known.

That even a profound thinker of this calibre here went wrong points at principally problematic sides of the fluid dynamic flow theory (besides its non-eloquent and cumbersome mathematics that makes mistakes easy and to remain undetected[12,17]). Namely, being so abstract and detached from ordinary mechanical and physical thinking, that the connection is easily lost, and also to keep in mind that it is just applied mechanics that cannot violate or override it.

It is not always easy to judge people's way of thinking from their writings, but recalling what I had been told by the flow professors at KTH through the years a fairly strange picture evolved. Many viewed Navier-Stokes' equation as a natural law. One believed that the molecules sat in place and moved along the flow elements. Another was expert in both mechanics (gyros) and hydromechanics but mentally treated them as separate boxes (like Einstein). Another said that the applicability of continuum theory started with about three particles across the rheometer gap. One was glad he did not have to do with my fibre flow systems since "it is impossible to do any theory with them". For another this was no problem at all because "We have research students that are good in counting". CFD-professors often lacked knowledge or interest in the underlying physics. It was more a numerical or programming challenge.

Steenberg recently invited the flow physicist for lunch (always Godthem at Djurgården) to explain that water is perhaps not world's best choice of model substance in their studies of the flow of porridge-like fibre suspensions. He thought he had succeeded until back at KTH, the last words the flow physicist says when leaving the taxi is that, is it not anyhow fantastic what Navier-Stokes' equation may predict.

Personally, I find it difficult to see it as more fantastic than any other balance or transport equation. But in some quarters the main satisfaction seems to come from showing that it is applicable everywhere. And universities continue to teach e.g. flight with help of Bernoulli's equation (*and* paper machine behaviour) that cannot even in a direct way explain how a plane can fly upside down (for paper machines *this* is at least true). This is a pedagogic problem but the story (besides leading to interesting insights into flight) was a help to definitely set aside the fluid dynamic flow theory for technical fibre flows. Hopefully, it may help other to do the same if it is less well adapted. It is not forbidden to make an own theory.

An intriguing question, not without relevance for the development of fibre flow theory, is why Mason did not continue to technically more interesting systems or raised to generalities. Even Faxén within his rigid sphere sphere tried to generalise a bit. That a mathematician like George Batchelor[107] could produce fantasies could be understood, but Mason, surrounded by pulps at Paprican?

Steenberg's[82] answer was that he was not at all surrounded by pulps ("he barely knew how they looked like") and not too interested in technology. This seemed so strange that another emeritus had to be asked, *viz.* Ants Teder[136] who stayed there in 1963. His version is that McGill, being a relatively young university, aimed at fast scientific reputation (and succeeded). The method was to be *very* scientific, which also meant non-technical. Mason's fibre flow research therefore officially was modelling of molecular motion, *cf.* Eirich[73]. This *could* also have been his deeper motivation. Einstein developed his viscosity formula[12,17] to help proving the existence of atoms.

And going to myself who without asking for it [through a series of fantastic events that will be described elsewhere, including the theft of Eirich's film that for political reasons has not yet been retrieved.] temporarily got Paper Technology on my desk, this subject does not seem to contain much of science (as Mason possibly thought), mainly a lot of machines. And it is not the fibres *per se* that make fibre flow scientifically interesting, but that they generate a crowded floc flow that may be used a model for flow more in general, *viz.* if the flocs are substituted by atoms and the liquid by space itself. Or even more generally, if philosophically void attractions at distance may be substituted by shadowing effects. The Mason story may seem strange, but one may sympathise with it and feel that it may contain some truth.

CONCLUSIONS

1. Fibre flow research begun experimentally at the beginning of the 20th century with technical systems. At about the same time a physical/colloidal experimental tradition developed and together with an older microhydrodynamic theoretical tradition formed a theoretical flow tradition for dilute suspensions (with appending own experimental tradition). The two traditions continued to develop independently up to about the 1940's, when the theoreticians had extended their models to higher concentration and started applying them on technical systems.

2. As a result a number of skewnesses were introduced. The first and most obvious was that they were treated as *fibre* flow systems instead of fibre *floc* flows. The second and less obvious was that the fibre flocs were viewed as the result of a flocculation process and therefore regarded as coherent. This led to the adoption of a traditional primarily stretch-out continuum view instead of a more realistic primarily compressive particulate system view.

Around the millennium shift Duffy[122] tried to start a debate about the modelling in this field, without response. When, at

NRC2003 in Tórshavn in Faroe Islands, I was asked to host NRC2006 the chance appeared to do the same by bringing people from the different traditions physically together. The result can be found in last year's transactions. As host I unfortunately did not get time to attend enough lectures to judge if it succeeded. In one debate Duffy and I, however, seemed to be agreed that the theoretical modelling of these *complex systems* needs, i) a more creative element than just solving flow equations for fibres in liquid, ii) to start on a higher level and iii) to be founded on observable mechanisms.

To conclude, a lack of genuine interest in the technical fibre flow systems *per se* by theoreticians and a hydrodogmatic approach have belated a reasonable theoretical development and a more profound understanding of these systems for about a century, since nothing had prevented the development to continue from where e.g. Sigurd Smith[9] started in 1919 (his work was found around 2000).

REFERENCES

Ref. 1 to 73 can be found in Part I, i.e. ref. 74

74. *Björkman, U.* (2006): "Fibre flow research history: Part I. Background and beginning", Ann. Trans. Nord. Rheol. Soc., vol. 14, 41.

75. *Ostwald, W.* (1916): "Die Welt der vernachlässigten Dimensionen", Steinkopff, Dresden.

76. *Campbell, W.B. and Yorsten, F.H.* (1932): "The properties of pulp suspensions", Forest Prod. Lab. Rep., Quart. Rev., no.10, 38.

77. *Strachan, J.* (1935): "Flocculation in papermaking", Papermaker and Br. Pap. Trade J., Jan. 1, TS, 1 and 17.

78. *Wollwage, J.C.* (1939): "The flocculation of papermaking fibres", TAPPI Papers, series 22, 578.

79. *Björkman, U.* (1999): "Flow of flocculated fibres", ISBN 91-7170-178-8, KTH Högskoletryckeriet, Stockholm.

80. *Björkman, U.* (2005): "Floc dynamics in flowing fibre suspensions", Nord. Pulp. Pap. Res. J., 18, 247.

81. *Mason, S.G.* and *Maass, O.* (1940): "Measurement of viscosity in the critical region. Ethylene", Can. J. Res., 18B, 128.

82. *Steenberg, B.* (2006): Prof. Paper Technol.1949-79, KTH, Stockholm. *Personal communication.*

83. *Mason, S.G.* (1948): "The flocculation of cellulose fibre suspensions", Pulp. Paper. Mag. Can., 49, 99.

84. *Hubley, C.E., Robertson, A.A. and Mason, S.G.* (1950): "Flocculation of suspensions of large particles", Can. J. Res., 28, Sect. B, 770.

85. *Mason, S.G.* (1950a): "The flocculation of pulp suspensions and the formation of paper", TAPPI, 33, 440.

86. *Mason, S.G.* (1950b): "The motion of fibres in flowing liquids", Pulp Pap. Mag. Can., 51, 93.

87. *Mason, S.G.* (1980): "Rheology of suspensions", in "Rheology," vol. 2. Proc. 8th Int, Congr. Rheol., eds. G. Astarita *et al.,* Naples, p. 631, Plenum, New York.

88. *Goldsmith, H.L. and Mason, S.G.* (1967): "The microrheology of dispersions", in "Rheology. Theory and Application," vol. 4, ed. F.R. Eirich, Academic Press, New York.

89. *Kerekes, R.J., Soszynski, R.M. and Tam Doo, P.A.* (1985): "The flocculation of pulp fibres", in "Paper making raw materials", 8th Fundam. Res. Symp., 265, Mech. Eng. Publ., Oxford.

90. *Forgacs, O.L., Robertson, A.A. and Mason, S.G* (1958): "The hydrodynamic behaviour of paper-making fibres", Pulp Pap. Mag. Can., 57, 117.

91. *Andersson, O. and Brunsvik, J.-J.* (1961): "Flocculation of streaming fibre suspensions", Svensk Papperstidn, 64, 493.

92. *van de Ven, T.G.M* (1989): "Colloid hydrodynamics", Academic Press, London.

93. *van de Ven, T.G.M* (2006): "Interactions between fibres and colloidal particles subjected to flow", Ann. Trans. Nord. Rheol. Vol. 14, 14.

94. Whitehead, A.N. (1889): "A second approximation to viscous fluid motion", Quart. J. Math., 23, 143.

95. *Oseen, C.W.* (1910): "Über der Stokessche Formel und über eine verwandte Aufgabe in der Hydromekanik", Arkiv mat. astr fys., 6,

96. *Oseen, C.W.* (1927): "Neuer Methoden und Ergebnisse in der Hydrodynamik", Akad. Verlagsgesellschaft GmbH, Leipzig.

97. *Faxén, H.* (1921): "Einwirkung der Gefäßwände auf den Wiederstand gegen die Bewegung einer kleinen Kugel in einer zähen Flüssigkeit", Diss. Upsala Univ., Uppsala.

98. *Faxén, H.* (1922): "Der Wiederstand gegen die Bewegung einer starren Kugel in einer zähen Flüssigkeit, die zwischen zwei parallelen ebenen Wänden eingeschlossen ist", Ann. Physik, 68, 89.

99. *Lamb, H.* (1927 – 32): "Hydrodynamics", 4–6th eds., Cambridge Univ. Press.

100. *Lindgren, R.E.* (1999): "The motion of a sphere in an incompressible viscous fluid at Reynolds numbers considerably less than one", Phys. Scripta, 60, 97.

101. *Oberbeck, A.* (1876): "Ueber stationäre Flüssigkeitsbewegungen mit Berücksichtigung der inneren Reibung", J. reine angew. Math., 81, 62.

102. *Taylor, G.I.* (1932): “The viscosity of a fluid containing small drops of another fluid”, Proc. Roy. Soc. A, 138, 41.

103. *Taylor, G.I.* (1934): “The formation of emulsions in definable fields of flow”, Proc. Roy. Soc. A, 144, 501.

104. *Vand, V.* (1948): “Viscosity of solutions and suspensions”, J. Phys. Colloid Chem., 52, 277.

105. *Simha, R.* (1952): “A treatment of the viscosity of concentrated suspension”, J. Appl. Phys., 233, 1020.

106. *Frankel, N.A. and Acrivos, A.* (1967): “On the viscosity of concentrated suspension of solid spheres”, Chem. Eng. Sci, 22, 847.

107. *Batchelor, G.K.* (1967): “The stress generated in a non-dilute suspension of elongated particles by pure straining motion”, J. Fluid. Mech, 46, 813.

108. *de Gennes, P.G.* (1979): “Conjecture on the transition from Poiseuille to plug flow in suspensions”, J. de Phys., 40, 783.

109. *Happel, J. and Brenner, H.* (1965): “Low Reynolds number hydrodynamics; With special applications to particulate media”, Prentice Hall, (1973): 2nd ed. Nordhoff Int. Publ., Leyden.

110. *Schowalter, W.R.* (1978): ”Mechanics of non-Newtonian fluids”, Pergamon, Oxford.

111. *Kim, S. and Karrila, S.J.* (1991): “Micro-hydrodynamics: Principles and selected applications”, Butterworth, Boston.

112. *Crochet, J.*, Davis, *A.R.* and *Walters, K.* (1984): “Numerical simulation of non-Newtonian flow”, Elsevier, Oxford.

113. *Schmid, C.F., Switzer, L.H. and Klingenberg, D.J.* (2000): “Simulations of fiber flocculation: Effects of fiber properties and interfiber friction”, J. Rheol., 44, 781.

114. *Switzer, L.H. and Klingenberg, D.J.* (2003): “Simulation of fiber floc dispersion in linear flow fields”, Nord. Pulp Pap. Res. J. 18, 141.

115. *Meyer, R. and Wahren, D.* (1964): “On the elastic properties of three-dimensional fibre networks”, Sv. Papperstidn. 67, 432.

116. *Deng, M. and Dodson, C.T.J.* (1994): “Paper: An engineered stochastic structure,” Tappi Press, Atlanta.

117. *Skelland, A.H.P.* (1967): “Non-newtonian flow and heat transfer”, Wiley, New York.

118. *Coleman, B.D., Markowitch, H. and Noll, W.* (1966): “Viscometric flows of non-Newtonian fluids”, Springer, Berlin.

119. *Björkman, U.* (1991): “Mycelial flow”, ISBN 91-7170-085-4, KTH Högskoletryckeriet, Stockholm.

120. *Chain, G. and Gualandi, G.* (1954): “Aeration studies”, Ist. sup. Sanit. (English Edn.), 17, 1109.

121. *Roels, J.A. van den Berg, J. and Voncken, R.M.* (1974): “The rheology of fermentation broths”, Biotechnol. Bioeng.”, 16, 181.

122. *Duffy, G.G.* (2000): “The importance of mechanistic-based models in fibre suspension flow”, Appita, 53, 337.

123. *Meyer, H.* (1964): “An analytical treatment of the laminar flow of annulus forming fibrous suspensions in vertical pipes”, Tappi, 47, 78.

124. *Moller K., Duffy, G.G. and Titchener, A.L.* (1973): “The prediction of pipe friction losses from rotational viscometry”, Sv. Papperstidn., 76, 493.

125. *Truesdell, C. and Toupin, R.* (1960): “The classical field theories” in ”Hanbuch der Physik”, III/1 ed. S. Flügge, Springer, Berlin.

126. *Rivlin, R.S.* (1984): “Forty years of non-linear continuum mechanics”, in “Advances in rheology”, Proc. 9th Int, Congr. Rheol., Mexico City, vol.1, B. Mena *et al.,* Univ. Nac. Aut. Mex., Mexico City.

127. *Ball, J.M. and James, P.D.* (2002): “The scientific life and influence of Clifford Ambrose Truesdell III”, Arch. Rational Mech. Anal., 161, 1.

128. *Bird, R.B., Stewart, W.E. and Lightfoot, N.E.* (1960): “Transport phenomena”, Wiley, New York.

129. *Kingsford, F.W.* (1960): “F. W. Lanchester. A life of an engineer”, Edward Arnold, London.

130. *Lanchester, F.W.* (1907): “Aerial flight, Vol. I: Aerodynamics”, Constable, London.

131. *Lanchester, F.W.* (1908): ”Aerial flight, Vol. II: Aerodonetics”, Constable, London.

132. *Ingelman-Sundberg, M.* (2001): “Describing aerodynamic lift fundamentally wrong for eight decades– Bernouilli reversed”, Mekanisten, 3, 27.

133. *Hart, K.* (1985): “The prehistory of flight”, Univ. California Press, Berkeley.

134. *Einstein, A.* (1916): “Elementare Theorie der Wasserwellen und des Fluges”, Naturwissenschaften, 25 aug., 400.

135. *Grosz, P.M.* (1988): “Albert Einstein and his role in German aviation in World War I”, WW1 aero, No. 118, Feb., p. 42.

136. *Teder, A.* (2007): Prof. Cellulose Technol. 1992-2001, KTH, Stockholm. *Personal comm.*