WASC 2098

Commentary on
Combustion Propellant
R&D at ERDE
Barry H. Newman

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Dr. John Wright.

Dear John,

When we were at the AGM earlier this year you asked me if I would write "something" on composite propellant research whilst I was at Waltham in the period 1950 – 63. Owing to a number of distractions I have only recently got round to "having a go".

Inevitably the results of a personal nature and I have deliberately sub-titled it "recollections" of my work. Perhaps "reminiscences" would be a more apt description as it has brought back many happy memories of colleagues and friends – many, sadly, no longer with us. In no way is it an attempt at a reprise of the many technical reports on the subject which you told me had disappeared. Thank goodness I hear you say.

I hope it is of some use – please let me know if you have any queries – sorry I'm not on e-mail.

Kindest regards

Sincerely, Barry Newman

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# PROPELLANT HISTORY

Through a friend I received the last two issues of Touchpaper and found them most interesting. For old times sake I feel I should join the Association.

You ask for information on propellants research at Waltham Abbey. The area I worked in was concerned only with composite propellants containing ammonium perchlorate as oxidiser but a lot of original work was done when I was first on Hoppit Island (with Gordon Adams and Jack Powling) and after 1953 when I moved to South Site and was responsible for all the new plastic propellant compositions. The operating temperature limitations of plastic propellant were never fully recognised by the management and the system was preserved long after the obvious superiority of the castable composite systems should have been acknowledged. Together with John Scrivener in the late '50s we developed a range of polyester polyurethane castable propellants, one of which was adopted for the ill fated Blue Water artillery rocket system. Enormous investment and effort was put into developing both the propellant and the pilot filling plant for which I was project officer but in which Ron Campbell, John Scrivener and Bert Mather (ROF Bishopton but now deceased) and many others played key roles. Just as the installation was nearing completion in late 1962, an American system (the liquid filled Corporal) was purchased and Blue Water cancelled. Due to internal politics in ERDE all work on cast composite propellants was abandoned and, believe it or not, emphasis reverted to plastic propellant. I went to the Defence Staff in Washington DC from where I recommended (with all necessary technical details) *ad nauseum* a complete switch to an hydroxy terminated polybutadiene binder which happened many years later. A very successful use of this binder was in the LAW rocket motor. In 1966 my career took me into the RARDE sphere of armament research and into various HQ activities eventually returning to PERME in 1980 as Director.

On the question of Hans Ziebland I recall him well during my time on Hoppit Island as he 'operated' in the neighbouring bay in press house H10. There were quite a number working on the liquid motor test stand and firings were made most days. Red Fuming Nitric Acid (RFNA) was the oxidiser and most available hydrocarbons were used as fuel. I seem to recall that those in the higher octane range were particularly favoured by those with motor bikes! I never discovered what the purpose of the programme was as all the real liquid work was done at Westcott with which establishment communication was notoriously poor in those days. Dr Ziebland operated on the mini-motor scale in an highly professional manner and this must have impressed someone as he achieved an Individual Merit SPSO.

Westcott (a very separate establishment then) continued with liquids for a long time and was host to the Rolls Royce Blue Streak programme. Despite work on a packaged liquid motor taken to test flight prototype nothing ever went into service. The work for the Chevaline programme was under US direction and quite separate.

Barry Newman

*Barry has now joined the 'Friends' and we were pleased to see him and his wife Dorothy at the Reunion.*

*This is the kind of information that we are currently seeking on the propellant work at Waltham Abbey and would welcome any further insights. Ed*

**Composite Propellant R & D at ERDE Waltham Abbey: 1947 to 1963**

A brief account based entirely on the personal recollections of Barry H Newman.

**Combustion research; and how I came to Waltham. 1947 - 1950**

I think it is fair to say that basic research into the combustion of composite propellants containing ammonium perchlorate as oxidiser started late in 1947. At that time, Waltham Abbey was an out-station of the Armament Research Department (ARD) and was under the direction of a Dr Poole who was also a deputy director of the ARD. which was co-located with the Armament Design Establishment (ADE) at Fort Halstead near Sevenoaks, Kent. Later these two were joined to become the Armament Research and Development Establishment (ARDE) which achieved Royal status in 1962.

Ammonium perchlorate was the 'new' oxidiser for solid propellants and had clear advantages over compounds such as potassium perchlorate, ammonium nitrate, sodium nitrate and ammonium picrate. The last two chemicals were already in use in the earliest form of plastic propellant based on a binder of polystyrene dissolved in poly alpha methyl styrene. To enlarge the then sketchy knowledge on ammonium perchlorate it was decided that a study of the kinetics of its thermal decomposition would be a suitable topic for an extra mural contract in a university.

As was the general rule in those days the problem would have been discussed at the Physical and Inorganic Chemistry Committee (PICC) of the Scientific Advisory Council (SAC; much later the Defence SAC). The PICC was one of many subcommittees of the SAC which covered all scientific disciplines and defence interests and its membership consisted of senior academics and members of the Scientific Civil Service. Remember that during WW II the universities had been much involved in defence work.

The PICC was not only a quasi-official review body for basic defence research but also acted as an "old boy" network for distributing much valued grants for PhD students. To cut this rather long story short I was the beneficiary , to the sum of £250 pa, (well above average in those days), to work under the direction of Dr L L Bircumshaw, senior lecturer in physical chemistry in the University of Birmingham where I had graduated in June 1947.

Dr Bircumshaw had had other students working on the kinetics of thermal decomposition of various solid inorganic compounds but these had been far removed from practical application as was the case with ammonium perchlorate. Because of the Ministry of Supply funding we had regular visits from Waltham personnel who were the designated supervisory officers; thus I met Gordon Adams and Colin Lawson (both of whom I was later to work for) some years before I joined Waltham Abbey. Also, I made visits to Waltham and met the aforesaid Dr Poole, impressively knowledgeable, and also Dr Les Phillips and his extremely able assistant George Whitbread (much later to become HMCIE).

Waltham in the late '40s struck me as more akin to an overgrown nature reserve with dozens of decaying buildings and half sunken barges in the weedy canals - a fisherman's paradise rather than an active R & D establishment! Despite all that, I immediately liked the place,

and, as I had always had more than a passing interest in pyrotechnics and rocketry, felt that someday I might work there.

The work at Birmingham progressed satisfactorily and along the way I became an expert on high vacuum techniques, complex gas analysis in the 'wet' chemistry era, and adept at glass blowing. Ammonium perchlorate turned out to be an interesting and probably unique type of compound; at low temperatures up to 270 deg C only 27 - 30 % decomposed whilst some material sublimed unchanged. The residual material remained pure ammonium perchlorate though the crystals had taken on a porous 'chalky' nature. At higher temperatures over say 350 deg C it would spontaneously combust and produce somewhat different gaseous products. I did not know until years later that the surface temperature in a burning propellant was above this level and that therefore these decomposition products were the ones involved in propellant combustion. All that was to come later with work by Dr Jack Powling and several American workers.

All this work led in early 1950 to a successful Ph D thesis and two papers in the Proc. Roy. Soc. which did not appear until 1954 owing to delays with Min of Supply clearance for publication.

By 1949 I had confirmed my earlier thoughts about the desirability of working at Waltham on rocket propellants and had taken the first step by passing the Scientific Civil Service Open Competition for Scientific Officer - a lengthy interview in Grosvenor Square. I presented myself at ERDE (as it had by then become after separation from ARDE) to Dr C H Johnson, Chief Supt. He had been an academic who at an earlier stage in his career had been a predecessor of Dr Bircumshaw at Birmingham. I was assigned to Colin Lawson's plastic propellant section where the arrival of a young, post war scientist with a PhD was regarded both as a novelty and as a burden. What to do was the question and little thought seemed to have been given to the problem! My two main mentors were Colin Lawson and Phil Freeman - a more disparate pair it would be hard to imagine, though both were friendly and kindly individuals; particularly the former who was the more serious and work orientated whilst the latter was more concerned with the breeding and selling of tropical fish. In this latter regard he could be described as 'green fingered' if that is the correct term for fish; he certainly more than doubled his SSO salary for the years until he was posted to Washington in 1959.

So what to do? Being left very much to my own devices I gravitated to where something practical was happening and that was the manufacture and filling of the early plastic propellants into two varieties of 5 inch light alloy tube rocket motors about 5 ft long containing some 40 lbs or so of propellant which were used on runway sled trials. Dick Doe was in charge of this and for the next 4 months I immersed myself in the trade so to speak and got to know all about the (then primitive) quality control, all aspects of processing (then using old solvent cordite mixers) and the mystery of the pugmill and most important all the 25 industrials who operated the plant. Dick Doe became a close friend.

Small K round rocket motors were used for ballistic control in those days but the attendant instrumentation was poor and thrust measurements almost impossible. Some small scale work on new ammonium perchlorate compositions had been done by a Dr Slack but he had been transferred out of the department and his records were a complete muddle. All a bit strange to a young scientist who had had to organise his thesis work to the last minute every day and to produce detailed quarterly reports for the very people who turned out to be

anything but organised!. The only organised person appeared to me to be Dick Doe who was planning the move of all the processing plant to new buildings on the South Site.

**Composite propellant research: 1950 to 1953**

About October 1950 I was rescued. Mr Len Wiseman, Supt Propellant Research II, responsible for liquid propellant work, had decided that basic research should be done on 'model' solid composite systems in parallel with that long since underway on nitric esters (this was under Gordon Adams, Geoff Stocks and Geoff Lloyd). I therefore transferred to Hoppit Island; it was like a return to university in terms of approach to work. I was given the broadest guidelines and the free run of the looted (sorry, reparations!) German equipment to build my own test laboratory in presshouse 10.

A pressure vessel with windows on opposite sides capable of withstanding 3000 psi had to be designed in the drawing office (down by the old canteen in Powder Mill Lane) and then manufactured in the machine shop and a means of recording the regression rate of the burning surface of the 'model' propellants. This was achieved by shadow photography by projecting a powerful beam of light through the bomb windows on to a moving strip of photographic paper. I found an old German drum camera which must have been designed for the purpose and got the necessary rolls of photographic paper made specially by Ilford films. The whole equipment had to be in a protected area which was built into the bay and a dark room provided nearby. It was a hectic few months before all was ready.

Ammonium perchlorate was the only oxidiser to be used but many solid fuels of different chemical and physical properties were to be investigated. The oxidiser was recrystallised twice, hand ground, then sieved into different particle size fractions. The first fuels tested were paraformaldehyde and sucrose - vastly different materials with the same empirical formula.
Different oxidiser/fuel mixture ratios were pressed in special hardened steel moulds into sticks about ten cms long by half a cm square. These moulds were beautifully made in the workshop with which there was the greatest collaboration and interest throughout. Ignition was by an ICI electric squib via electrodes passing through the closure cap of the bomb. To discourage side burning the sticks were painted with an aqueous graphite suspension.
Flame temperatures were calculated for all mixtures which entailed lengthy calculation using the electro-mechanical machines in the computing section. These were jealously guarded by the ladies in charge, but eventually I was allowed to become an honorary member and allowed to join the club! It was years before anything like modern computers appeared and then the lead was set by the Westcott establishment.

Two very happy and productive years passed rapidly and I was joined by an excellent industrial who took over much of the manual work and film developing. We could get through at least 30 'firings' a day complete with full reduction of records. Scientifically the most important results were the effect of oxidiser particle size on pressure dependence of burning rate and also the effect of various catalysts and additives. All of this was recorded in reports which I learned only recently have all been destroyed; a pity it was not published in the open literature. Technically speaking the main advance was in the steady improvements to the design of the strand burner and its mode of operation as it was later adopted for the quality control of both plastic and extruded double base propellants. All of this was to stand

me in good stead for what was to happen next; an unexpected move from Research to Development

**Plastic Propellant R & D ; 1953 to 1959**

In late '52 there was a considerable reorganisation of the establishment and an additional superintendency was created in the propellants area so that responsibilities for double base and plastic propellants were separated. Also, increased defence expenditure followed the outbreak of the Korean war. In ERDE Dr Harold Young became SPR I (composite propellants) and Dr George Williams became SPR III (double base). I was transferred from research in SPR II to be section head of plastic propellant development (burning rates and performance) which by then had been relocated in several new buildings on the South Site adjacent to the Sewardstone Road and the boundary fence. The rheological side of the work remained on the North Site under Phil Freeman and Dr John Vernon. Charles Peers also worked in this group looking at an endless succession of alternative surfactants to replace the lecithin (a natural product) which had been used in plastic propellant since the outset.

My earlier period of learning the ropes with Dick Doe now paid off handsomely as I knew a lot about the processing side and all the staff involved and I had plenty of ideas about improving the compositions and ballistic performance. There was a small scale plant on the South Site which was soon equipped with the latest mixers and also it had the best charge hand in the business in Joe Archer who, in addition, quickly became a much trusted assistant. A strand burner was installed and my research programme from Hoppit Island was restarted with real plastic propellant compositions. Promising ones were scaled up to K round firings and thence to 5 inch diameter motors. In the next 6 years literally hundreds of compositions were made and tested, the whole mixing process was modernised and much improved binders (plasticised poly-isobutene) and surfactants introduced. At long last the new firing stand, integral with the production facility, could make meaningful measurements of thrust from K rounds so the theoretical performance figures could be verified, and also the importance of combustion instability was recognised and to some extent identified in 5 inch motor firings before compositions were released to Westcott.

There were regular meetings between Westcott rocket motor designers and Waltham propellant developers (both plastic and extruded double base) so that there was real purpose to the propellant development programme. High (above 1 inch/sec) and low (below 0.1 inch/sec) burning rates and low pressure dependence plastic compositions were shipped by the ton to Westcott and the overtime on the production plant was the envy of all other groups. Dick Doe was in charge of this aspect. The facility at ROF Bridgwater was commissioned and eventually produced acceptable propellant! The team on the South Site was unchanged for a long time but we did get one recruit and he turned out to be excellent and that was Graham Spickernell (now sadly deceased) who went on to become MD of the ROF Propellants Manufacture when they took over IMI Summerfield.

We had about six very productive years with the minimum of management interference. Bobby Brewin was acting Chief Supt for part of this time and he was a firm believer in letting well alone provided a group was successful. Dr Young remained on the North Site and there was no PSO to interfere! We never lacked money for equipment or building works as to every estimate we put in for future expenditure Dr Young applied the $\pi$ factor which in simple

terms meant he multiplied our figures by 3.14 (much more subtle than just x3!) so he was always able, though with much protestation, to cut back his demands if the financiers so decreed! In a good year we had problems to spend the money allocated!

The only real failure we had was to develop a plastic propellant capable of meeting the stringent low temperature requirement for the 2 inch air-to-air rocket despite literally thousands of tests and steadily improved compositions with better binders and surfactants. This fact alone should have sounded the death knell for plastic propellant in Service requirements though this was not to be. Despite this we had successes with the many versions of the Skylark sounding rocket, the Chevaline re -entry test vehicle, several GW boost motors and many RPE test vehicles.

**Polyurethane propellants: 1959 to 1963**

During all the time I had been involved with plastic propellant, the UK had been in the very privileged position of knowing the direction and progress made in the USA with castable composite propellants. Of course, the US involvement was enormous compared with ours with research going on in several Government laboratories as well as private companies under contract. They had never been more than politely interested in plastic propellant, recognising its temperature limitations and tendency to slump in large motors and so had concentrated on cross linkable prepolymers which could be cross linked into stable rubbers after casting into the motor. For various reasons ranging from inertia one the one hand to inter establishment politics between the two establishments ,ERDE and RPE, on the other nothing had been done in the UK to follow the US lead. (A book could have been written on this!) In addition, the UK did not have an indigenous synthetic rubber industry whereas huge US propellant companies such as Thiokol, Aerojet General and Phillips were subsidiaries of rubber companies. It is well to bear in mind that even poly-isobutene had to be imported from Germany under the trade name of Oppanol.

However, around 1957/58 a start was made on the North Site to look at polyester polyurethanes compositions using the commercially available ICI Daltorol crosslinked with toluene di-isocyanate (TDI). This material was used for making rollers in the printing trade. In a way this was too easy as the system worked, albeit with rather too high a proportion of binder to oxidiser (than was desirable from the performance viewpoint), and clones of plastic propellant compositions were made often with very poor stability because of chemical incompatibility with the TDI. New small scale mixers were developed for the much lower viscosity propellant mix and new filling techniques for K rounds. The early handling of the extremely poisonous, and noxious, TDI left a lot to be desired from the HSE viewpoint and no special precautions were taken. This work started under Phil Freeman and John Stutchbury, an attached Australian scientist.

Soon John Scrivener and I were drafted in to run the work as Phil Freeman left for DRDS Washington in early 1959 and though I say it myself the work was put on a more scientific basis. In parallel with this composition work the effect of chemical structure of the dibasic acid (chain length) on the low temperature properties of the unloaded polyurethane rubber was started by Drs Brian Hollingsworth and Frank Carver; this was beginning to show useful trends but soon gave way to work of "higher priority" on the synthesis of organic compounds of particular interest to Dr Bellamy for his studies on IR spectra! However, it was a useful

start and eventually provided the basic information for the choice of the dibasic acid for the Blue Water propellant system.(see below)

**Blue Water (an ArmyArtillery Rocket System): 1959 to 1963**

In 1959 ARDE was appointed the Design and Development Authority for the artillery rocket system code named Blue Water. RPE Westcott was the Design Authority for the rocket motor (actually fabricated by Bristol Aerojet at Banwell, Somerset) with the propellant to be developed and filled into the motors at ERDE. It was soon clear (to the embarrassment of management at ERDE) that plastic propellant could not meet the stringent land service temperature requirements of -40 deg C to +60 deg C and neither would any of the Daltorol polyurethane compositions. I hasten to add that at that time neither I nor John Scrivener had been consulted as to the choice of propellant or were in any way associated with it..

At a crisis meeting at Waltham chaired by the Project Director, Mr A N Christmas of ARDE, I was appointed Project Officer in Waltham responsible for the whole operation from propellant development to pilot filling plant design, construction and operation. At some time production was to be transferred to the ROF. On my recommendation the binder would be based on the dibasic iso-sebacic acid which had to be air freighted in from the USA!. The manufacture of the polyester was in he capable hands of Alec Pryde in the Chemical Engineering dept (SCE) and the tricky analytical problems for quality control of the hydroxyl number of the prepolymer was dealt with in the Chemical Analysis section (a building half way up the Long Walk). It is fair to say that nothing on this scale had ever been taken on before by ERDE .

We were a small team by any standards and one which would have been laughed at for such a project in the USA. John Scrivener was responsible for the propellant development on the North Site (including quality control of ballistic and mechanical properties); Dr Ron Campbell came from the double base group to take on the design and erection of the totally new filling plant on the South Site which was to occupy some old converted and some new buildings near the main gate. He was assisted by Roy Fisher, CE dept,together with BertMather ,E II, (now deceased) from ROF Bishopton on detached duty (he moved home with his wife down to Broxbourne for the duration). Oxidiser preparation was in the hands of the plastic propellant group well equipped for this task.

We worked all hours for about three years and finally the new buildings, large vertical mixer, special drying ovens for the motor liner, propellant composition, etc were all ready and a few motors actually filled. And then the anti climax; the project was cancelled in late 1962 and an American packaged liquid propellant powered missile chosen - a system which went through many teething troubles before going into service. This traumatic experience will always stay engraved in my memory, as it will with all the rest of the team who had certainly performed magnificently. In fact it turned out to be even worse for UK composite propellant development because castable propellants were abandoned for several years and the team of experienced people dispersed to other work including a new venture on high energy castable explosives under George Whitbread; a line of work which never produced anything practical and was seen as an unwarranted invasion of their responsibility by RARDE Fort Halstead!

**Life after Blue Water.**

After such an exciting and demanding job as running the major programme in ERDE anything was bound to be an anti climax. What was offered next lived up (or down!) to this expectation. The Director decided that ERDE should copy the US in a programme of research into high energy compositions (could be explosive or propellant orientated) because it was deemed important to be seen to be "doing something" to keep in with the US. This would require a new remotely controlled facility covering all aspects from ingredient preparation, mixing, filling and testing and who better to supervise this than yours truly who had managed the Blue Water project. Again I was assisted by Ron Campbell and Roy Fisher; the resultant plant design depended on moving the process between the various operations by means of a Basset Lowke electric train system. This aspect of the project the engineers entered with great gusto! I must admit to having little enthusiasm for this programme and argued that any composition which needed such careful handling was certainly not of practical value to the Services. Nitronium perchlorate was then a candidate oxidiser!! However, the work went ahead but whether it ever produced anything useful I never knew because in the Spring of 1963 I applied for and obtained the post of ERDE liaison officer in the Defence R & D Staff in Washington and the Newman family "emigrated" for three and a half years where, amongst many other things, I really learned about the large scale manufacture of cast composite propellants.

After a few years ERDE saw the light and restarted (under John Scrivener) work on castable systems using US binders such as the epoxy crosslinked carboxy terminated polybutadiene (I must say this was against my advice from Washington because of its long term chemical instability) before moving on to the far superior US hydroxy terminated polybutadiene which was isocyanate cross linked. In a way, the wheel had come full circle because this binder was as near in hydrocarbon content as could be to that of the polyisobutene in plastic propellant. After nearly 20 years of R & D on ammonium perchlorate and composite propellants in general in both UK and USA I returned to the UK in July 1966 to be Supt Explosives at RARDE Fort Halstead to find a completely different set of problems!

But that's another story, as they say!

BHN.

Barry H Newman

October 2006