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Bernard Schriever and Early US Military Spaceflight

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DISCLAIMER

Research for this thesis was conducted under the auspices of a Royal Air Force Chief of Air Staff's 'Portal' Fellowship while the author was a serving Royal Air Force officer. The view expressed within are, however, the author's own, and should not be taken as representing the opinion or position of the Royal Air Force, the UK Ministry of Defence or HM Government.

DECLARATION

I confirm that this is my own work and the use of all material from other sources has been properly and fully acknowledged.

24 October 2016 G DOYLE

CONTENTS

ABSTRACT

This thesis explores the life and work of General Bernard Schriever, principally with regard to his work introducing intercontinental ballistic missiles (ICBMs) into United States Air Force (USAF) service during the 1950s, and his related efforts to develop systems for manned and unmanned military spaceflight. It situates his life and work in the early Cold War of the 20th Century, and through exploitation of the Schriever Archive at the Library of Congress in Washington DC, and recently declassified US Government documents, principally those released by the National Reconnaissance Office, it seeks to explore his understanding of the strategic context of his work.

It posits that to introduce a technically advanced system into military service, it is necessary to evaluate both technical and non-technical risks to progress, and to possess or achieve sufficient administrative control to reduce those risks and improve overall programme maturity in both domains. By considering Schriever's successful introduction of ICBMs into USAF service and his management of the early USAF reconnaissance satellite programmes, it demonstrates his understanding of all three facets of progress. It then examines two areas where Schriever failed to achieve his stated goals, firstly in his attempts to develop a military manned spaceflight programme in parallel with the national programme being run by NASA and secondly in respect of his advocacy for space weaponization. In each case, it shows that Schriever's failures can be explained by insufficient progress along one or more of the identified lines of activity.

Recently declassified US Government sources, and the Schriever archive in the Library of Congress, which includes unpublished book-length manuscripts by Schriever and co-authors, are used to corroborate his views. The thesis concludes by critiquing the analytic model used and suggesting alternative sources that might shed further light on Schriever's work.

BIBLIOGRAPHIC NOTES

The Schriever Archive at the Library of Congress. This thesis draws on extensive exploration of the Schriever Archive at the Library of Congress in Washington DC. The provenance of the Archive is described in Chapter 3. References follow normal scholarly convention with two variations specific to this thesis:

> When the Archive was curated, all content, regardless of classification, was allocated to a Container according to subject and/or date. Classified content was then withdrawn and stored separately. The author submitted numerous 'Mandatory Declassification Review' requests to the Library staff in an attempt to gain access to the classified content, with some success. When content was declassified, however, it was released to the author without indication of its original Box and Folder location. Eventually, this content will be relocated to its original containers, but within this thesis, most such documents are, of necessity, noted as being in the '*Schriever Archive, Recently Declassified Documents'* folder.
>
> The full citation to an item in the Archive requested by the Library would be: "*Container number*, Bernard A. Schriever Papers, Manuscript Division, Library of Congress, Washington, D.C.'. To aid future scholars, all references in this thesis also indicate folders within the individual containers. In the interests of brevity, after the first instance of an archival source, references are abbreviated to '*Schriever Archive, Box 'x', Folder 'y'.'*

The Sheehan Archive at the Library of Congress. Reference is also made in this thesis to the content of the 'Sheehan' Archive. Mr Neil Sheehan wrote a biography of Schriever, published in 2009.[1] He is also the source and donor of a large archive at the Library of Congress relating to his previous publications in the 1970s and 1980s. On publication of his biography of Schriever, he donated his source material for that publication to the Library too. Eventually, it will be incorporated into his existing collection at the Library, but it is currently (2015) awaiting processing. Mr Sheehan and the Library granted the author access to the unprocessed acquisition relating to Schriever. Currently, it comprises 21 containers, as sorted by Mr Sheehan during his writing. There is no finding guide for the acquisition, but the container names are descriptive. There is some duplication in the collection – Sheehan would copy his interview transcripts, for example, and file them under multiple themes in various boxes if the interview had covered more than one relevant topic. In this thesis, material from the Sheehan collection is referenced simply as being from the '*Sheehan Archive, Box 'x'.',* noting the container used by the author. There can be no guarantee that these references will translate into any new storage scheme when the acquisition is processed, but they should then be locatable via the Sheehan Archive Finding Guide in the normal way.

Republished Primary Material. Two series of republished primary documentation have been used in this thesis. The NASA publication 'Exploring the Unknown' (currently in 7 volumes) comprises a large amount of primary material from NASA

[1] Neil Sheehan, *A fiery peace in a cold war : Bernard Schriever and the ultimate weapon* (New York: Random House, 2009). See also Chapter 3, p70 *et seq* within this thesis.

archival sources, reprinted verbatim with linking explanatory essays.[2] The US Air Force Space Command publication 'Orbital Futures' (2 volumes) does similar, drawing on USAF sources.[3] In each case, reference is made to the archival document as reprinted, with a note that it can be found in one or other of those publications. References to the explanatory essays are treated as content of an edited volume and follow normal referencing conventions.

Web-published Historical Volumes. The author has been given and referred to copies of various source documents during the course of his research. Some appear to be downloaded reprints, but the web-source of the downloads is not always clear. References to these publications are made in the normal way for printed sources by title, author, publisher/place of publication and date. The reader seeking to access titles referred to, but not otherwise locatable, is recommended to try searches at the US Defense Technical Information Center (www.dtic.mil), the NASA History web-page (www.history.nasa.gov) and the National Reconnaissance Office (NRO) Freedom of Information Act (FOIA) 'reading room' (http://www.nro.gov/foia/index.html).

[2] John M Logsdon (ed.), (1995-2008), *(SP-4407) Exploring the Unknown: Selected Documents in the History of the US Civil Space Program* 7 vols.; (Washington DC: NASA History Office).
[3] David N Spires (ed.), (2004), *Orbital Futures: Selected Documents in Air Force Space History (Essays and commentaries by David N Spires)* 2 vols.; (Peterson AFB, CO: USAF Space Command).

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I would firstly like to thank my supervisors, Professor Colin Gray and Dr Geoff Sloan; without their encouragement and support, I fear I would have made little progress. Other staff and my fellow students within the Graduate Institute of Political and International Studies at Reading also encouraged me to keep going; I would particularly like to acknowledge the support of the various Directors of Graduate Studies during my time in the Department, including: Professor Beatrice Heuser, Dr Alan Renwick, Dr Patrick Tomlin and Dr Robert Jubb.

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I was also helped by the staff at the archives of the Smithsonian Institution in Washington DC (both at the Institution Headquarters and at the National Air and Space Museum), the archives at NASA Headquarters in Washington DC and the archives of the Rand Corporation in Santa Monica, California,

I am equally indebted to staff at the following university libraries: the American University in Washington DC, Boston University, Boston, MA (for records relating to Boston University Optical Research Center), the University of California at Berkley and Stanford University, Stanford, (both in CA), George Washington University (including the National Security Archive hosted at GWU) and the National Defense University at Fort Lesley J McNair (both in Washington DC), Texas A&M University, College Station, TX and the University of Utah in Salt Lake City, UT, Other scholars and researchers working in the same area as myself who helped and advised me informally included Dr Geoff Charlston (late of the NRO and now at the US Department of State), Dr Dwayne Day at the National Academy of Sciences in Washington DC, Mr R Cargill Hall (late of the NRO), Dr Peter Hays (late of the Air University, Maxwell AFB and at the Pentagon in various capacities), Mr Jack Neufeld (late of the USAF History Support Office), Mr Neil Sheehan, Professor Asif Siddiqui (Fordham University, New York), Dr Rick Sturdevant ((US) Air Force Space Command) and Dr Harry Waldron ((US) Air Force Space and Missile Center).

Relatives and personal friends of General Schriever who were happy to discuss his work and in some cases invite me into their homes included Colonel Gerry Schriever, USAF (Retd), Mrs Joni Schriever (Joni James), Mr Brett Schriever, Mr Ted Moeller and Major Michael Schriever USAF, Mr George Stine and Ms Patti Milauckus.

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I am grateful to Dr Mark Owen for the discussions about ‘pentathlon’ frameworks and related terminology, which led to the analytic framework proposed in Chapter 1 and utilised throughout this thesis.

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LIST OF FIGURES AND TABLES

CHAPTER 1 – TECHNICALLY ADVANCED SYSTEMS

The relationship between technology and strategy is complex, and at times the descriptions of each suggest they are at cross-purposes. Strategic thought seeks to identify timeless principles with wide applicability, while mastery of technology seems to require study of intricate and specific detail, which is only temporarily useful. Even when the discussion is limited to *military* strategy and *militarily useful* technology, the tension remains. But somehow the two are inter-related; the concept of maritime strategy and the geopolitical view it enables, for example, is abstract without access to ships and some knowledge of marine engineering.

The post World War 2 revolution in military technology which exploited nuclear weaponry, developments in electronics and computing power and the dawn of the space-age had profound implications for strategic thinkers, if only through what it made possible. This thesis seeks to illustrate an aspect of this paradigm shift by considering the role of one man – General Bernard Adolf Schriever USAF – and the part he played in developing the capabilities of the United States of America in this respect. His name may be recognised by some, and he gained a measure of public recognition during his life. Nonetheless, it is the author's contention that few recognise the nature of his contributions across the fields of missile and space technology, or understand why he acted as he did, and that filling this gap in the literature is a worthwhile endeavour. Even fewer have pondered how he achieved the results he did, and this thesis also proposes a scheme for evaluating his methods.

Without doubt, Schriever's service career and personal talents propelled him to a position where he exerted great influence on the nature and capabilities of the United States Air Force (USAF) in the 1950s and 1960s. In this period, he directed the development and introduction of intercontinental ballistic missiles (ICBMs) into the USAF inventory. Starting at about the same time, he was also involved in managing the early attempts to harness space capability. The engineering management techniques that Schriever used to direct the ICBM programme, and the associated organisational changes he promoted within the USAF have been the subject of study, but beyond this, little detailed analysis of his life, work or motivations has been undertaken. Given the respective impacts of the ICBM and the military space programme, this is a surprising omission.

The crux of the lacuna is as follows: the availability of the ICBM had a profound impact on the conduct of the Cold War and consequently, on strategic thought then and since. Schriever's contribution to that project was significant and has been analysed in considerable depth. Yet at the same time, Schriever was intimately involved in the birth of the military space programme. At the time it began, it was not only the *military* space programme, but also a large portion of the *national* space programme of the United States. Schriever had clear ideas about the utility of spaceflight as a domain for military activity – some were inherently practical and subsequently borne out by events, others much less so. Yet analysis of his ideas about spaceflight, space capability and their impact on national security is notably thinner than that relating to ICBMs. There are some passing acknowledgements of their importance; within the USAF, Schriever would be described informally as 'the father of the ICBM', and the reunion gatherings of his colleagues after his retirement were referred to as meetings of 'missileers'.[4] But the lasting tribute paid to him by the USAF when they renamed Falcon AFB, Colorado, as 'Schriever AFB' in 1998 is tied to spaceflight, not missiles. The major resident unit on the base is the USAF's 50th Space Wing (motto 'Masters of Space'), which has a declared mission to 'Command satellites to deliver decisive global effects'.[5] Thus, he was implicitly being honoured in respect of true space capability rather than for ICBMs and strategic deterrence. It seems reasonable to at least wonder why the analysis of his life dwells so much on the ICBM and to such a limited extent on his thinking on space power. A major part of the answer may lie in access to relevant source material.

During the course of his life, Schriever left behind significant clues as to what mattered to him and how he saw the world around him; he testified on several occasions to Congressional committees, he spoke in public at conferences, he wrote and inspired many short articles for the military and general press and supplied forewords to several books. It is only recently, however, that a large portion of his work has become accessible, principally through the conservation and curating of his personal archive at the Library of Congress in Washington DC and also as a product of official declassification activity conducted by the National Reconnaissance Office and other agencies. Using these sources, this thesis seeks

[4] See, for example, Lt Col Keith Amburgey, 'Space and Missile Pioneers Meet: General Schriever's Old Timers Reunion', *Association of Air Force Missileers Newsletter* Vol 11 No 2 (June 2003).
[5] 'Schriever AFB', <http://www.schriever.af.mil>, accessed 3 February 2013 .

to explain what Bernard Schriever thought was the potential of military spaceflight, both manned and unmanned, why he did so, and to what extent his reasoning was sound.

An initial impetus for Schriever's work was the realization among the victorious Allies after World War 2 of the technical progress achieved in several fields by Germany. Key German personnel and examples of their work were brought to the west principally by the United States and the United Kingdom. Some of those personnel were subsequently offered resettlement in the United States under the 'Paperclip' programme, and contributed to continuation of their work. Prominent among them were Wernher von Braun, Walter Dornberger and Hubertus Strughold.[6] This thesis will examine their relationships, such as they were, with Schriever in later chapters.[7] The United States also instituted an ambitious programme to assemble, adapt and test German V2 missiles at White Sands Missile Range in New Mexico. The knowledge gleaned, and circulated, coupled with theoretical studies, principally those conducted by the RAND Corporation, highlighted great possibilities. However, peacetime budgetary constraints and some unsolved technical problems served to limit early progress in capitalizing on this work. At a later stage, the complex division of responsibility between the US Army, the US Air Force, the Defense Advanced Research Projects Agency (DARPA) and NASA for elements of the space programme further obscured whose work influenced who.

Immediately after its formation in 1947, the USAF began pondering the utility of military spaceflight in various roles. One role was simply the demonstration of the capability; while missiles and space-launchers are separate systems, possession of an orbital spaceflight capability convincingly demonstrated the potential also to develop an ICBM. In the other direction, while possession of ICBMs did not guarantee a working space launch capability, they certainly demonstrated that such was potentially within reach of the demonstrating country. Demonstration of either capability was inextricably linked to issues of national

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[6] There are several published histories of the Paperclip programme; most tend to view the programme widely, covering scientists of all persuasions and fields, and often dwelling on the moral quandaries of the officials involved, and the various efforts during the 1970s and 1980s to extradite or expel individuals as evidence of their alleged culpability in war crimes emerged. None dwell exclusively on the rocket programme participants, but Eric Lichtblau, *The Nazis Next Door: How America became a safe haven for Hitler's Men* (Boston: Mariner Books, 2015) and Tom Bower, *The Paperclip Conspiracy* (London: Paladin, Grafton Books, 1988) provide significant details of the named individuals. There is also coverage from the viewpoint of a participant in the biography of von Braun by Michael Neufeld. See Michael J. Neufeld, *Von Braun : Dreamer of space, engineer of war* (New York: A.A. Knopf, 2007).

[7] See Chapters 2, 5 and 6 (pp 32, 125 and 155) of this thesis.

prestige and technical competence. The next roles exploited were independent of, but delivered in support of ICBMs: satellite reconnaissance to support targeting and inform deterrence strategies, surveillance for weather detection and forecasting in support of early reconnaissance systems, and missile launch detection for warning and response. Satellite communications emerged at about the same time to support a variety of terrestrial missions. But it also seemed plausible at the time that space itself would become an arena for combat and manoeuvre, and possibly for the direct attack of surface targets. As will be shown, Schriever was involved with significant portions of the associated debates and had clear ideas of his own about their practicality.

This thesis thus addresses the fundamental question "What did Bernard Schriever believe to be the utility of military spaceflight (manned and unmanned), how did he act to realize those beliefs, and to what extent were his views justified?" To do this, it notes that one common feature of all Schriever's attempts to exploit space and missile breakthroughs were that they were innovative, technically challenging and complex. To facilitate analysis of such programmes, it proposes a methodology positing that development is essential both against technical and 'systems' goals. The nature of systems goals, and the administrative underpinnings that are essential to such progress are outlined, drawing on contemporary military procurement methodologies. This analytic scheme is then applied to Schriever's attempts to exploit military spaceflight.

In this chapter we will develop this analytic methodology to study the maturation of a technically innovative defence or security system. This will then serve to analyse Schriever's strategy for developing USAF space capabilities and to explain the successes and failures he experienced.

A Methodology for Assessing Technically Innovative Systems

During his post-World War 2 career, Schriever laboured to introduce various novel systems into USAF service. Plainly some of his initiatives were objectively successful; he introduced the Atlas ICBM into service quickly, and at the same time he laid the foundations for the successor systems Titan and Minuteman, and a complimentary intermediate range system – Thor. He also recognised the close links between successful ICBMs and their potential use as satellite launchers. He had a sound awareness of the possible roles of military satellites and led the development of the first successful reconnaissance system – Discoverer. In other areas, however, his results were less convincingly successful.

Although Schriever recognised the potential of communications and navigation satellites as aids to military operations, and participated in their development, they were less obviously USAF-related initiatives. Finally, there were specific projects that he advocated for which never came to fruition. Notable among these were his ideas about orbital bombardment systems and about military manned spaceflight.

To truly analyse his work and draw meaningful conclusions, we must therefore have some analytic methodology, ideally, a single analysis that explains both the successes and failures consistently and accurately. It is the author's contention that the basis for such a system has emerged in recent times, couched as a recommended methodology to be adopted for the development of complex systems, and that it can be elaborated to support analysis of Schriever's work. It can be demonstrated that where Schriever followed its dictates, his projects prospered and where he did not, they stumbled and failed. Whether Schriever had a conscious understanding of this methodology is moot – he does not appear to have spoken or written on the subject – but he does seem to have applied it intuitively. We will now explain this proposed method and describe its component parts.

Constructing (and modifying) the 'Pentathlon' Framework

The first step in the explanation is to distinguish between two different kinds of progress towards implementing a complex initiative, one relating to hardware, the other to 'softer' issues. As described, the model is a development of Professor Keith Goffin's 'Pentathlon' framework for innovation promotion.[8] The development that inspired the author was carried out by Dr Mark Owen, an employee of MBDA Ltd, under the auspices of the '*Materials and Components for Missiles, Innovation and Technology Partnership',* an Anglo-French defence partnership.[9] Dr Owens' (unpublished) work posits that a technologically advanced system must develop from an initial conceptual or experimental idea to a mature system capable of delivering some useful effect in a finite, usually strictly defined, time period; this might be the duration of a contract, an end-date for some system to be replaced, or a target date where a requirement can be defined.

[8] For an overview of the Pentathlon framework, see Keith Goffin and Rick Mitchell, *Innovation management : strategy and implementation using the pentathlon framework* (2nd edn.: Palgrave Macmillan, 2010) Chapter 1.
[9] For details of the MCM ITP and its activities, see 'Welcome To MCM ITP', <http://www.mcmitp.com>, accessed 29 May 2014

Progress can be determined in two conceptually separate intellectual spaces: the first is 'technology space', where progress is measured by the technical performance of the system and its rate of improvement. The second is 'systems space' or 'concept space', where the intended use of the system, its tactical or strategic implications and other less tangible factors are assessed; the aspects being measured in each space will be defined shortly. Owen's model envisages relatively stringent conditions being met within the time constraints in both spaces, with improving performance being required in each, in parallel.

Capability Management Frameworks

The next step is to enumerate the different factors that might be measured in this way, and to consider how they could be allocated to each space. A potential template for this already exists; Capability Management Frameworks have been developed by several countries to ensure that in the introduction of a new capability, no critical aspect of development is ignored or neglected. Three examples will serve to demonstrate the general principle: in the UK, Defence Lines of Development (DLODs) provide a taxonomy, in the USA, the Department of Defence 'DOTMLPF' framework is used,[10] and in Australia, the scheme is known as 'Fundamental Inputs to Capability' (FICs). All owe some debt to John Zachman's Information Systems framework, though since that was developed strictly in the context of software development, the adaptations are extensive.[11]

The UK DLOD scheme is promulgated as part of the MOD Acquisition Operating Framework (AOF). It consists of 8 separate factors that must be taken into account when planning for the introduction of a new or enhanced capability within defence. The factors are usually listed in the order corresponding to the acronym 'TEPID OIL', denoting Training, Equipment, Personnel, Information, Doctrine (and Concepts), Organisation, Infrastructure and Logistics.[12] Each factor represents an area of activity that must be accounted for within capability management. Three of the categories – Equipment, Infrastructure and Logistics – lean towards the technical aspects of the programme, but the others capture non-equipment requirements, which cannot be neglected. Some connections are

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[10] Commonly pronounced 'dot-mil-pee-eff'.

[11] For Zachman's original formulation, see John A Zachman, 'A Framework for Information Systems Architecture', *IBM Systems Journal,* Vol 26 No 3 1987). Zachman has updated the framework several times since publishing this article.

[12] The latest version of the AOF is accessible via UK MOD AOF Team, 'Guidance: Acquisition Operating Framework', (updated 17 March 2014) <https://www.gov.uk/acquisition-operating-framework>, accessed 1 June 2014 . The framework is unclassified and publicly accessible, though would-be readers must register their personal details and create an account to access it.

obvious; a new system will probably require training or re-training of operators, so there must be a plausible and affordable training solution developed in parallel with development of the hardware. Infrastructure issues may be equally obvious – docks or harbours for ships, hangars and runways for aircraft, launch facilities for space systems. But the other danger that the DLODs are designed to avoid is that more subtle dependencies, such as motivating personnel to operate the system or the need to incorporate the capability within doctrine, are overlooked. It is worth stating what each is intended to encompass; the AOF provides a brief summary of them, included at Table 1 below.

DLOD	**Explanation**[13]
Training	The provision of the means to practice, develop and validate, within constraints, the practical application of a common military doctrine to deliver a military capability.
Equipment	The provision of military platforms, systems and weapons, (expendable and non-expendable, including updates to legacy systems) needed to outfit/equip an individual, group or organization.
Personnel	The timely provision of sufficient, capable and motivated personnel to deliver Defence outputs, now and in the future.
Information	The provision of a coherent development of data, information and knowledge requirements for capabilities and all processes designed to gather and handle data, information and knowledge. Data is defined as raw facts, without inherent meaning, used by humans and systems. Information is defined as data placed in context. Knowledge is Information applied to a particular situation.
Doctrine & Concepts	A Concept is an expression of the capabilities that are likely to be used to accomplish an activity in the future. Doctrine is an expression of the principles by which military forces guide their actions and is a codification of how activity is conducted today. It is authoritative, but requires judgment in application.
Organization	Relates to the operational and non-operational organizational relationships of people. It typically includes military force structures, MOD civilian organizational structures and Defence contractors providing support.
Infrastructure	The acquisition, development, management and disposal of all fixed, permanent buildings and structures, land, utilities and facility management services (both Hard and Soft facility management (FM)) in support of Defence capabilities. It includes estate development and structures that support military and civilian personnel.
Logistics	Logistics is the science of planning and carrying out the operational movement and maintenance of forces. In its most comprehensive sense, it relates to the aspects of military operations that deal with; the design and development, acquisition, storage, transport, distribution, maintenance, evacuation and disposition of materiel; the transport of personnel; the acquisition, construction, maintenance, operation, and disposition of facilities; the acquisition or furnishing of services, medical and health service support.

Table 1 – DLOD Summary.

[13] Extracted from the AOF as published in May 2014, reordered and edited slightly to conform to the TEPIDOIL acronym.

Having described the UK DLODs in some detail, we now turn to their US and Australian equivalents. Descriptions here will be brief, simply to underscore that frameworks of this nature are used elsewhere, and are thus not a particularly British trait.

The US DOTMLPF taxonomy is very similar in concept to DLODs, and appears to date from about 2001.[14] It sits within the 'DoD Architecture Framework', a similar construct to the UK AOF. The acronym decodes as: Doctrine, Organization, Training, Materiel, Leadership, Education, Personnel, and Facilities. This reflects the US distinction between 'training' and 'education', separation of 'leadership' as a distinct line of development (in the UK, it would probably be subsumed into 'Organization'), a slightly more encompassing 'Materiel' to cover Infrastructure and Logistics areas and the fact that the framework is seen as an adjunct to the equipment itself, which is not, therefore, listed.

The Australian 'Fundamental Inputs to Capability' are listed as Command and Management, Organization, Major Systems, Personnel, Supplies, Support, Facilities and Collective Training.[15] Again, the similarities are obvious, reflecting only minor differences and national preferences. There is thus a broad consensus on the factors thought important in management of capability development, at least in the context of the 1990s and 2000s. But which factors sit in technology space, which in concept space, and are any others are needed?

Populating Technology and Systems Space

Technology space is relatively easy to populate; for a technically novel and/or complex system, the equipment, its design, development and performance will undoubtedly sit here. The other technology-related areas from the DLOD 'menu' would appear at least potentially to be logistics and infrastructure – logistics because of the likely challenges of maintaining a complex system, and infrastructure because of the demands of supporting and operating it. In the case of satellites and missiles, infrastructure would, for example, include the provision of launch sites, range facilities for testing and the global communications and tracking sites required for on-orbit operation.

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[14] The DOTMLPF acronym features in the current US DoD Dictionary: Director, Joint Staff (promulgating authority), Joint Publication 1-02: Department of Defense Dictionary of Military and Associated Terms, (As amended 15 March 2014. Washington DC, 2010)
[15] Promulgated at: Chief, Capability Development Group, *Defence Capability Development Manual* (Canberra, ACT: Defence Publishing Service, 2006), 4.

Beyond these factors lies concept space, where the emphasis shifts to non-mechanical or non-technical factors – personnel, training, information relevant to operations, and the doctrine and concepts underpinning use of the system.

Two additional factors added are not traditionally included in national-level analysis – legality, and policy compliance. In contemporary acquisition management, the tools and schemas outlined are designed to manage a programme on the pre-supposition that it complies with government policy and is underpinned by a robust legal regime. There have been instances where policy shifts or emerging legal opinions have rendered defence systems non-compliant or potentially illegal, but at least in the West, and in recent times, where this has happened, the governments concerned have moved to withdraw and dispose of the offending systems.[16] In Schriever's time and in the USA, the single services enjoyed greater autonomy to propose and lobby for systems independent of each other and the administration than would be customary now, and the international legal regime surrounding nuclear weapons, space systems and arms control was much less mature than today. Consequently, although both factors could possibly be accommodated in a slightly more expansive definition of 'Doctrine and Concepts', it will serve analysis better if they are examined separately where necessary.

The next requirement for a useful model is to consider whether progress against any or all of these categories can be measured, or at least graded. This is not a trivial distinction; measurement implies some agreed scoring system, implicitly leading to a numerical value, while grading simply implies a ranking. Thus 'cold', 'warm' and 'hot', or 'small', 'big' and 'enormous' are gradings, while '212°' or '1.73m' are measurements. Recent developments in capability management offer a measurement system for technology space, and a grading system for concept space is proposed here. These have implications for any attempt to present an assessment model graphically.

Codifying Progress - Technology Risk Levels

Systems of metrics for levels of risk – specifically technical risk – do exist, and are used regularly. The system of Technology Risk Levels (TRLs) is a

[16] Examples of this happening include the prohibition of anti-personnel mines and other similar munitions, chemical weapons destruction agreements and implementation of various nuclear disarmament treaties.

relatively recent development, introduced initially by NASA in 1988 and widely adapted and developed since then.[17] It was first proposed by 3 NASA researchers – S R Sadin, F P Povinelli and R Rosen – and initially comprised a seven-point scale. Later variations on this original scheme have proliferated, though the current consensus is broadly for a nine-point scale. Sadin *et al's* original classification is described below in Table 2.[18]

TRL	Description
1	A phenomenon is observed, or an effect demonstrated, which might potentially have a useful application.
2	An application of the phenomenon is proposed and validated
3	Some analysis of the practical application is begun and/or experimentation to demonstrate or validate it is carried out. (This is often taken to mark the start of formal research and development).
4	Laboratory demonstration of components of a practical system.
5	Components or sub-assemblies are demonstrated either in the laboratory or in the 'real' environment (i.e. in space for a space-based system, albeit still perhaps not as a finished system).
6	The finished system is validated in a laboratory or simulated environment
7	The system (albeit probably a prototype) performs to expectations in a real environment

Table 2. Technology Risk Levels Explained.

As noted above, later elaborations of the scheme have increased the usual scale from seven to nine points. NASA itself, for example, now uses a nine-point scale, as do the European Space Agency and the US Department of Defense.[19] Since this thesis does not attempt to assign specific TRLs to individual systems retrospectively, there is no need to describe the nature of the refinements or the specific differences between scales. All follow broadly comparable principles; they

[17] In the author's professional experience, practitioners in the field are surprised to learn that formalisation of TRLs occurred so recently; there seems to be a perception that they have been in use for much longer. Nevertheless, the citation immediately following is definitely the earliest formal attempt to define them.

[18] Stanley R Sadin, Frederick P Povinelli, and Robert Rosen, 'The NASA technology push towards future space mission systems (IAF 88-033)', *39th International Astronautical Congress* (Bangalore, India, 1988). The descriptions paraphrased in Table 1 are at p3.

[19] See John C Mankins, *TECHNOLOGY READINESS LEVELS: A White Paper* (Advanced Concepts Office, Office of Space Access and Technology: NASA, 1995) for the latest NASA scale, 'STRATEGIC READINESS LEVEL: The ESA Science Technology Development Route', (updated 13 February 2012) <http://sci.esa.int/jump.cfm?oid=37710>, accessed 31 May 2014 for the ESA scheme and Assistant Secretary of Defense for Research and Engineering (ASD(R&E)), *Technology Readiness Assessment (TRA) Guidance* (13 May 2011 edn. Washington DC: US Department of Defense, 2011) for the DoD scheme.

chart the progress of an idea from its earliest demonstration, perhaps at that stage with no envisaged useful purpose, through to a practical demonstration of its utility.

Codifying Progress - System Risk Levels

While the metrics applied in technology space are relatively well formalised and accepted, the situation in 'systems space' is a little less clear. 'Systems Readiness Levels' are defined in the literature, but there is not the same international consensus on meaning and applicability. In the USA, the work of researchers at the Stevens Institute of Technology in Hoboken, NJ in defining a 'Systems Readiness Level' is noteworthy, though it is principally concerned with the challenges of integrating discrete components into an operational system.[20] Schriever faced challenges like this in his work, for example integrating warheads onto missiles and satellites onto launchers, but regarded those as being part of the overall technological effort. 'Systems risk' needs a more broadly based analysis.

In the UK, there is also a System Readiness Level process. It attempts to address broader aspects of programme management. As currently used, the process assigns an overall 'Red/Amber/Green' maturity to a programme, based on assessment against multiple equipment and non-equipment factors. These are still biased towards technical and equipment issues, though they do acknowledge wider aspects of programme delivery. The currently promulgated list comprises: training, safety and environmental factors, reliability and maintainability, human factors, software, information systems, airworthiness and maritime-specific factors if applicable, and an arbitrary number of project-specific factors identified by an individual project management team. Within this list, a variety of performance criteria can be applied to measure progress.[21] Factors such as these plainly help widen the scope of analysis, but they still lean heavily on technical progress. What is needed is a broader and more consistent way of considering non-equipment or non-technical factors in analysing progress.

It would be tempting to try to develop a multi-point scoring system for a 'System Readiness Level' analogous to TRLs and reflecting these factors, but the

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[20] For details of the Stevens Institute scale, see Brian Sauser et al., 'From TRL to SRL: The Concept of Systems Readiness Levels', *Conference on Systems Engineering Research* (Los Angeles, CA, 2006). Sauser's paper also provides a brief historical review of other similar initiatives including a comparison between USA and UK practice.

[21] The complete 'System Readiness Level' taxonomy, which includes the suggested performance levels for different stages of a project and various assessment tools, can be found at UK MOD, 'What are System Readiness Levels (SRLs) ?', <https://www.aof.mod.uk/aofcontent/tactical/techman/content/srl_whatarethey.htm>, accessed 13 Mar 15 2015.

subjective nature of any measurement applied to them militates against this. It does seem possible, however, to apply a grading and Table 3 below attempts to codify this. The existing DLOD framework is slightly expanded and combined with the 'traffic light' scoring of System Readiness levels, to yield an assessment matrix applicable to any technically advanced system. The author's indicative scoring descriptions are included.

Line of Development	**Marginal (Red)**	**Questionable (Amber)**	**Mature (Green)**
Non-equipment Factors from DLODs – "Systems/Concept Space"			
Training	Difficult, Un-validated, High failure rate, Expensive.	Adequate.	Successful, Validated, Self-sustaining.
Personnel	Unattractive, Over selective.	Manageable.	Popular, Over-subscribed role.
Doctrine & Concepts	No validated role or concept of employment (CONEMP).	Recognition that system may address identified capability gap.	Endorsed requirement, Clear CONEMP.
Organisation	No adequate structure for employment.	Muddled, Contested, Ambiguous.	Clear command structure.
Information	Requirement-ignorance, Poorly understood flows.	Challenging requirement – bulk of data, excessive, complexity, information control at odds with system progress.	Effective information-management.
Additional Non-Equipment Factors – "Systems/Concept Space"			
Legality	Clearly challenges existing laws or norms.	Tacit/local acceptance only or novel to the point of paradigm shift or establishment.	Widely accepted or already tested at law
Policy	Contrary to established policy or opposing an incipient policy shift.	Challenges established policy, presupposes policy shift.	Supports or enables established or emerging government policy.

<u>Table 3 – 'Traffic Light' Non-Equipment Scoring Scheme (proposed).</u>

Administrative Control

So far, this thesis has proposed measures and gradings for individual elements of progress in a programme or project leading to the finished system. But there is one further avenue along which progress must be made to deliver a technically advanced system, and that is the avenue, or 'axis' of administrative, or programme, control. This axis yields at best minimal concrete benefit to the programme itself (this, in fact, will identify where it is being addressed), but is nonetheless essential if progress is to be made. It thus encompasses sufficient administrative or command authority for managers to ensure that progress is made, prioritization sufficient to ensure success in a competitive setting, responsive budgetary mechanisms with appropriate financial controls and oversight, and the management of relationships with other organisations crucial to delivery of the system in question.

There is plainly a danger of a circular definition emerging here: ‘this category of things is important/essential because it comprises things held to be important or essential’, but on the other hand it reflects a clear reality. A technically plausible system that addresses a clear need stands little chance of success if unfunded, or supported only by those with no responsibility for acquisition, or if it is reliant on assistance from those unwilling or unable to provide it.[22] None of the remedies to these problems – funds, authority or assistance – will contribute directly to the finished system if addressed, but if they are absent, success is difficult or impossible. Recalling our graph of converging technical and systems readiness at the start of this chapter, if risk reduction is what drives the lines closer to the central axis, administrative control is what enables this movement and allows progress with the passage of time. Schriever gave great attention to this aspect of programme management, and achieved some of his most notable successes directly through the manipulation of factors in this area.

From the Framework to a Graph

We now have all the components needed to measure and evaluate progress, and thus to construct the analytic model. The working of the model can be presented as a graph, with progress represented on two vertical axes and time on the horizontal axis. An example of such a graph is given below at Figure 1.

[22] Lest this seem a fanciful analysis, it is worth noting it fits the experiences of Hermann Oberth, the German rocket engineer, during World War 1. Serving on the Eastern Front as a medical orderly, he proposed a primitive ballistic missile as an operational improvement on conventional artillery to German Higher Command during 1917. His plans were technically feasible, albeit it is unlikely that they were realistically within the capacity of German industry at the time, but the immediate cause of their failure was that Oberth had no recognised standing as a technician at the time, and the proposals were rejected out of hand by prevailing expert opinion. See Helen B. Walters, *Hermann Oberth : father of space travel* (New York: Macmillan, 1962) Chapter 5.

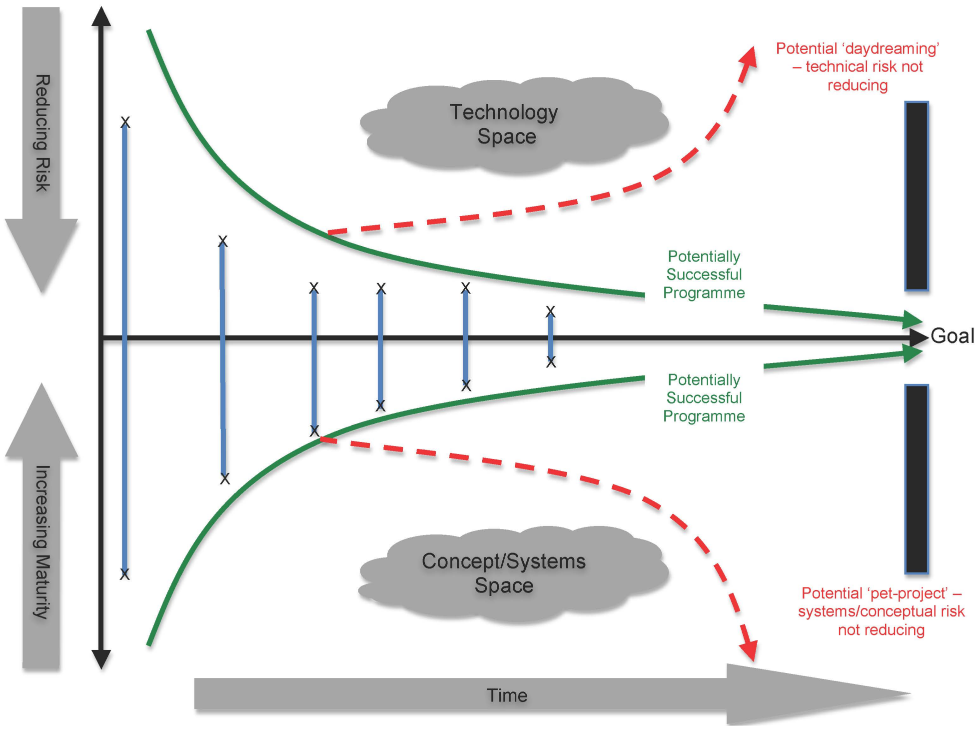

Figure 1 – A Diagram of Owen's Model.

The diagram in Figure 1 is not a conventional graph, in that the portions above and below the x-axis are not positive and negative values of a single variable; rather they are two distinct quantities being plotted above and below the axis along a common passage of time to allow a comparison between them. A line above the x-axis would show the outstanding risk of technical failure (as measured by a TRL) at a given point in time, the accompanying line below the x-axis shows the grading of the outstanding risk of system failure at the same time. Values far from the axis either above or below (large absolute values) indicate high levels of risk, and values near the axis (small absolute values) indicate low risk or increased maturity. At any instant in time, a project or system will have a given level of risk in both directions, and on the graph, the total risk inherent in the system could be shown as a vertical line between the two measurements or gradings. A system will be required to achieve a designated level of both technical and system maturity after a set period of time, and this can be imagined as some sort of wall or barrier with an opening through which the project must pass at a given time. The original graph used showed the barrier as two brick walls, straddling the x-axis. The gap through which the project is required to pass was referred to colloquially within the MCM IPT as the "valley of death", and successful passage would denote the system either entering productive service, or at least completing a given stage of its development to a level where it could begin the process again, perhaps integrated with some other sub-system. Finally, span of administrative control is not charted directly, but can be inferred from progress along the horizontal (time) axis.

Failure Cases

As well as this virtuous path, there are failure cases. In the first instance, imagine a programme beset by technical issues, but where the systems need is urgent, well understood and integrated with the relevant policy or strategy. Progress has thus been made solely in systems-space, while technical risk reduction has stalled. Whatever the reason for the technical failure – over-optimism, fundamental issues not fully understood or poor performance on the part of technicians to cite but three examples – the situation was described within the MCM ITP as 'concepting' – pursuit of a conceptually elegant solution to the exclusion of concern for technical progress. In conversation with team members, the situation was also described to the author as 'wishful thinking' or 'daydreaming', and it is this last (more descriptive) term that will be used within this thesis.

The opposite failure case also occurs. Here technical progress is being made, but against delivery of a product of questionable utility. There are plausible reasons for such situations; an evolving strategic crisis that has rendered an ongoing programme obsolete, or perhaps emergence of a technical alternative of greater promise. Whatever the cause, the term adopted within the MCM IPT for these situations was a 'pet-project'. The casual description of 'something that was too much fun to give up' cannot be taken too literally, but it captures the spirit of the underlying problem well.

In later chapters, this thesis will show that Schriever had experience of both 'pet-projects' and 'daydreams' as he explored the utility of military space systems, specifically in respect of military manned spaceflight and space weaponization. Regarding military manned spaceflight, Schriever saw a clear role for the military astronaut in space conducting military missions, but he also believed that such missions would have to be validated by a graduated series of experiments. Accordingly, he led the development of the USAF Manned Orbiting Laboratory project. There are good reasons for believing that solutions to the technical challenges of such missions were within his grasp, but he struggled unsuccessfully to protect the experimental nature of its mission against competing operational demands. Having allowed the programme to become subverted by these demands, he was then unable to protect it against rapid changes in operational requirements. He also struggled to develop it in a timely fashion and within budget. Thus, it represents a 'pet-project' – a technically plausible project, but ultimately one where the conceptual and doctrinal requirement could not be sustained. In the case of space weaponization, Schriever had what he believed were sound reasons for promoting the concept, particularly relating to orbital bombardment systems; he also saw a clear need for such a capability. But when it came to developing or introducing an orbital bombardment system, Schriever faced multiple problems; such a development would have been at cross-purposes with the US Government's policy ambitions, and with the emerging consensus on treaty limitations of space activity. More to the point, however, it would also have faced severe technical problems, relating to launch reliability, targeting and safe through-life operation of the platforms. Nonetheless, at least in Schriever's mind, such a system was a 'day-dream'; a conceptually sound system with technical challenges to be faced down. Schriever never got the chance to evaluate the technical challenges in detail, since he failed to convince the administration of the need for such systems – arguably a failure of span of administrative control. But taken overall, the importance of all three of our lines of activity are emphasised.

Summary – Technical, System and Administrative Readiness

This has been a rather winding path to arrive at the proposed methodology for evaluating the development of technically complex systems, and specifically those managed by Bernard Schriever. The initial premise is that of Owen: progress is essential in respect of both technical risk reduction and systems risk reduction, usually within a defined time-period and towards defined goals. Formal measurement of technical risk is actually a fairly recent (1988) innovation, but the measurement scales in use have achieved rapid and relatively wide adoption. However, it is worth noting that this thesis will not attempt to apply one of the scales retrospectively in respect of Schriever's work, firstly because to do so would require unachievable access to programme records and test results, in order to establish when a particular milestone was, or was not, reached. As will be shown, Schriever was not a 'hands-on' engineer or innovator; rather he was a technically adept engineer-manager, who oversaw complex programmes that were implemented at his direction and under his control in a variety of government and non-government (industrial) settings. The data that would be required for retrospective TRL analysis is not held within Schriever's papers, and it is not clear whether the records required to apply such assessment after the fact even exist.

Secondly, even if retrospective measurement was possible, for any system that eventually entered service, it is axiomatic that an acceptable TRL was in fact reached. This thesis will highlight where technical factors contributed to or caused programme failure, but in the main, it will simply demonstrate Schriever's understanding of the technical challenges he faced, noting his determination to monitor progress and manage his project teams in order to deliver systems into service. In this way, it can confidently assert where he was forcing risk levels down and steering his projects towards the 'valley of death', and where he failed to do so, even if it is not possible to assign exact numerical measures to progress at any given instant.

Measurement of systems risk is a less exact science than that of measuring technical risk; there is less consensus on the factors to be measured, and implicitly more subjective judgements are involved. Some measurements, such as training effectiveness, can also only be measured across an extended period. Nonetheless, they need attention throughout the acquisition process, and a pragmatic assessment seems possible. There are limitations in the promulgated lists of systems risk or readiness factors, but the UK DLOD categorization, augmented by consideration of legality and national policy compliance, provides a proposed assessment tool. Given the subjective nature of many of the judgements, and the extended period over which they would need to be applied, this thesis will resort to qualitative identification only of reasons for failure or success. Once again, this will suffice to indicate the direction of progress even if its exact rate is unknown. It will also be sufficient to indicate where Schriever fell victim to the lure of daydreaming or the attractions of pet-projects.

Finally, there are factors that are essential to programme management without contributing directly to the end product; factors such as budgetary authority and span of administrative control. Schriever was an experienced practitioner in this respect, identifying blockages to progress and taking effective steps to mitigate them. In terms of Owen's graph and model, they enable movement and progress within it rather than featuring directly on it.

Having proposed this model, we now move to set the context where we will apply it. In the next chapter, this thesis will outline Schriever's life and the circumstances in which he worked. This will serve to assert the significance of the work he undertook and of his part in it, without becoming trapped in detail of individual programmes. In Chapter 3, this will lead to an outline of the studies already undertaken and the relevant literature relating to his work, in order to allow identification of the gaps in the literature that it attempts to fill. In Chapter 4, we will then look at two areas where Schriever achieved conspicuous success – the introduction of the ICBM into service and development of early reconnaissance satellites. In Chapters 5 and 6, we will examine projects where Schriever achieved less; specifically, we will look at his ideas relating to space weaponization and to military manned spaceflight. Taken together, this will show that where he achieved success, it was demonstrably through careful attention to each of our chosen directions of progress, and where he failed, at least one had been neglected. This falls some way short of strict causation, but no claims of such are intended; it is exceedingly unlikely that there is a 'magic formula' which guarantees success in programme management if adhered to and failure if not. Rather, it attempts to validate an analytic tool that can be applied retrospectively within its limitations. It yields a framework for review and analysis to highlight reasons for success and failure, and as such, helps structure the analysis. But first, we should situate Schriever's life in its historical context.

CHAPTER 2 – BERNARD ADOLF SCHRIEVER

The criteria for entitlement to military honours and interment at Arlington National Cemetery are simply stated, yet subtly graduated to indicate the status of the deceased.[23] The principal beneficiaries are those who have served in the Armed Forces of the United States; some are interred having died on active service, others long after their retirement. There was therefore nothing inherently unusual about the funeral of a retired Air Force Officer that took place on Tuesday 12th July 2005, though a close observer would have deduced from the ceremonial that a senior figure was being commemorated. The honor guard of the United States Air Force (USAF) participated, according full military honours, and music was provided by the Air Force ceremonial band.[24] The religious observances were led by a Roman Catholic Bishop of the Military Archdiocese, who had himself served as an Air Force Chaplain for over 30 years. In themselves, these details would simply confirm that a senior retired officer had passed on; but among those that made this ceremony significantly more unusual was the presence not just of the Chief of Staff of the Air Force, the Chairman of the Joint Chiefs and most of the Air Force's cadre of four-star generals, but the additional presence as a mourner of the Secretary of Defense, The Honorable Donald Rumsfeld, attending in a private capacity. Plainly, the deceased had been a man of some significance, and his passing had been deemed worthy of note, yet his origins would have given little indication of what lay in store for him later in life.

The deceased on this occasion was Bernard Adolf Schriever. He served in the Armed Forces of the United States for 33 of the 35 years between 1931 and 1966, rising to high rank, and achieving a measure of public and professional recognition. But although his name would be recognised by students of 20th Century American Air and Space Power History, his motivations and lines of reasoning remain more obscure. Few relevant histories of the period omit his

[23] Department of the Army - Arlington National Cemetery, 'ADMINISTRATIVE GUIDE TO INFORMATION AND BURIAL AT ARLINGTON NATIONAL CEMETERY', (updated 17 May 12) <http://www.arlingtoncemetery.mil/docs/FuneralInfo_AdminGuide2InfoBurial_ANC.pdf>, accessed 15 Jul 12

[24] Webmaster: Michael Robert Patterson, 'Bernard Adolph Schriever, General, United States Air Force', (updated 23 August 2006) <http://www.arlingtoncemetery.net/baschriever.htm>. See also Neil Sheehan's account of attendance at Schriever's funeral contained in Sheehan, *A Fiery Peace* . Finally, some details here come from the Order of Service for the funeral (copy in the author's possession).

name, but equally few attempt to explain why he acted as he did, except by describing the strategic imperatives affecting the USA at the time generally.

Recent exposure of his writing and thinking offer a new route into his work and thought, but in order to provide context, it is necessary to begin by describing his life, and particularly his Service career. This chapter will therefore describe his circumstances, upbringing and interactions with significant contemporaries, particularly General Henry 'Hap' Arnold, who was effectively Schriever's patron for the first half of his service. The aim of this is to establish Schriever's significance in the field as a character worthy of detailed study.

First then, it must establish who Schriever was and what he did. This need not be a comprehensive biography; Chapter 3 will describe existing and easily accessible sources for much of his life. But the arguments expounded in later chapters will depend on specific details of his life, so the summary that follows provides the essentials of his life overall, and more specific details of his service career from 1931 to 1966.

Coming to America

Like several other notable Americans of the 20th Century, Bernard Adolf Schriever was an immigrant.[25] In his case, his name correctly suggested his origins – he was born in Bremen, Germany in 1910, and came to live in the United States in 1917 after his father – a ship's engineer on a German merchantman – was interned in New York in 1916.[26] The Schriever parents both had family connections in the United States, and when Mrs Schriever and their two sons, Bernard ('Benny') the elder by some 2 years, and his younger brother Gerhard ('Gerry'), managed to escape Europe and reach the USA to join her husband, it was natural that the family moved to New Braunfels, Texas, where they already had friends and acquaintances.[27] Released from internment, 'Papa' Schriever found work trading on his qualifications as an engineer, but his death before the end of World War 1 in an industrial accident in San Antonio must have come as both a shock and a blow. The nadir of both the Schriever boys' childhood came when their mother was forced to place them in an orphanage on a temporary basis

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[25] US Air Force, 'General Bernard Adolph Schriever (Official USAF Biography)', <http://www.af.mil/information/bios/bio.asp?bioID=7069>, accessed 21 July 2012

[26] Sheehan, *A Fiery Peace* 3. Jacob Neufeld, 'Bernard A Schriever: Challenging the Unknown', in John L Frisbee (ed.), *Makers of the United States Air Force* (Washington DC: Air Force History and Museums Project, 1996), 281-82.

[27]Sheehan, *A Fiery Peace* 4-5,US Air Force, 'General Bernard Adolph Schriever (Official USAF Biography)', Of note, the growing anti-German sentiment in and around New York as the United States pondered entering the war would have favoured the move to Texas.

while she worked to support herself and establish a family home as a widow. This hurdle was overcome when she found work as a housekeeper, and subsequently as the proprietor of a snack-bar on a golf course, but the need for financial security, and Bernard's putative role as breadwinner for the Schrievers influenced his future career choices.[28]

Bernard attended high school in San Antonio, and in 1927 entered the 'Agricultural and Mechanical College of Texas' – known today as Texas A&M University. He graduated in 1931 with a BS degree in structural architecture.[29] On graduating, he faced an unusual career choice; on one hand, while an undergraduate he had shown exceptional talent as a golfer, having won the San Antonio City and Texas state junior amateur championships in his graduating year. This led to him being offered a position as the golf professional at a local course on a respectable salary.[30] On the other hand, he had attended a military college; the Agricultural and Mechanical College was in those days all-male, and all those who were medically fit were enrolled in the Reserve Officer Training Corps programme. Schriever had thus been in uniform for the duration of his undergraduate course. He was not obliged to proceed into the Army, but chose to do so, and was commissioned into the Army Reserve as a Second Lieutenant in the Artillery.[31]

Despite entering the artillery, it is clear that Schriever felt little attraction for that branch of the service; rather it was a route into the Army overall, and subsequently a way to apply for transfer to the Army Air Corps.[32] San Antonio was not just a military town; it was also the home at the time of US Army Aviation. In the cuts to military facilities that had followed the end of World War 1, the Army Air Corps had reduced its training real-estate to two airfields – 'Kelly Field' and 'Brooks Field', both in the San Antonio Area.[33] Schriever was too young on graduation to

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[28] Sheehan, *A Fiery Peace* 6-9
[29] Texas A&M University College of Architecture, 'Bernard A Schriever (Outstanding Alumni)', <http://www.arch.tamu.edu/community/formerstudents/outstanding-alumni/past-honorees/70/>, accessed 21 July 2012 Sheehan, *A Fiery Peace* 12 US Air Force, 'General Bernard Adolph Schriever (Official USAF Biography)',
[30] Sheehan, *A Fiery Peace* 12
[31] http://www.arch.tamu.edu/community/formerstudents/outstanding-alumni/past-honorees/70/ , Sheehan, *A Fiery Peace* 13. Sheehan attributes Schriever's career choice to the influence of his mother, who did not consider golf a 'respectable' occupation. In a conversation with the author in August 2011, Bernard's brother Gerry cited the greater perceived job security that Bernard felt went with a military career, coupled with the potential need to support his family, as another significant factor in his decision.
[32] Sheehan, *A Fiery Peace* 14
[33] US National Parks Service, 'Kelly Field Historic District', <http://www.nps.gov/nr/travel/aviation/kel.htm>, accessed 21 July 2012 . Kelly and Brooks Fields activities were further consolidated and re-located with the opening of Randolph Field, in San Antonio in 1931. Schriever undertook his primary training at Randolph, transferring to Kelly Field for advanced training after 8 months. Sheehan, *A Fiery Peace* 15 For dates relating to Schriever's Service career, see also Laura Kells et al., *Bernard A. Schriever Papers; A Finding Aid to the*

apply for entry into flying training, but on attaining the requisite 21 years of age, he applied to transfer to the Army Air Corps. He was accepted and undertook flying training during 1932-33. The typical 'washout' rate in flying training at the time was of the order of 50%, but Schriever survived (two of his classmates were killed in training), graduated, and moved to productive service at March Field in Riverside, California as a bomber pilot. There he made an acquaintance that would have a significant influence on his future: Lieutenant Colonel (later General of the USAF) Henry 'Hap' Arnold.

'Hap' Arnold and the Air Mail Debacle

Hap Arnold had been a career soldier since before World War 1, and had been one of the first US Army officers to learn to fly.[34] During World War 1, he had risen to be Assistant Director of the Office of Military Aeronautics, with responsibility for overseeing the build-up of the US aircraft industry to support the military effort.[35] While Schriever was growing up in Texas during the 1920s, Arnold had become embroiled in the controversies surrounding the conduct of Colonel 'Billy' Mitchell and the latter's court-martial for insubordination in 1925. He survived with his own career if not intact, at least still underway, and by 1931 was Commanding Officer at March Field, where Schriever arrived in 1933 to join the 9th Bombardment Squadron.[36] By this time, both Mrs Schriever and Gerry were dependent on Bernard's support. The Great Depression was underway, the business that Mrs Schriever had built up at the golf course in San Antonio had failed, and the collapse of a bank had robbed them of their savings. Consequently, they followed Bernard to Riverside. Here, a happy coincidence arose; Mrs Arnold, who was about the same age as Mrs Schriever, had spent 3 years in her youth living in Germany, and enjoyed conversing in German. She made friends with Mrs Schriever, who became known socially on the base as a consequence. Lt Schriever thus became much more closely acquainted with his commanding officer than might otherwise have been the case.

Besides the social connection across the ranks, Arnold and Schriever were both technical visionaries. Later in their respective careers, they were both

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Collection in the Library of Congress (Washington DC: Manuscript Division, Library of Congress, 2011) 2-4

[34] US Air Force, 'General Henry H Arnold (Offical USAF Biography)', <http://www.af.mil/information/bios/bio.asp?bioID=4551>, accessed 21 July 2012

[35] Sheehan, *A Fiery Peace* 17-18 There are echoes of Schriever's later responsibilities in configuring US Industry to support the missile and space age with those held by Arnold in respect of conventional aviation.

[36] Arnold had narrowly escaped court-martial himself, for misuse of Government Property in that he was alleged to have used military printing facilities to lobby Congressmen on Mitchell's behalf.

convinced of the close connection between the practical aspects of the employment of air and space power, and the state of technological progress. Arnold's remark to Schriever in 1946 that:

> World War I was won by brawn,… World War II was won by logistics,… World War III will be won by brains,[37]

summarises the growing connection that both would make between the emerging capability of military aviation and its reliance on the 'high technology' of the day.

In 1934, the state of that military technological capability was brutally exposed at March Field; the US Army Air Corps undertook to supply the trans-continental airmail service, and in particular to bridge the gap across the Rocky Mountains. President Roosevelt had suspended commercial contracts for air-mail placed by the US Postal Service on the suspicion of fraud, and prior to the suspension had asked the Chief of the US Army Air Corps, General Benjamin Fulois, whether the Air Corps would be able to substitute their services pending an honest re-letting of the contracts. Fulois had replied in the affirmative, and consequently orders arrived at subordinate units to take station during the winter of 1933-34. Lt Col Arnold was appointed to a headquarters in Salt Lake City, and Schriever was one of the pilots thrown into the breach. The ensuing carnage immediately cost the lives of two of Schriever's classmates from training, along with others, as the inability of the Air Corps to match the performance of the commercial airlines in flying in poor weather and at night was laid bare. Notwithstanding any contract irregularities, the commercial pilots had significantly greater proficiency, and were significantly better equipped, for such operations. The importance of technical capability was unmistakably emphasised.[38]

Notwithstanding his dedication to the Corps and his performance in post, between 1935 and 1939, Schriever struggled to convert his Reserve Commission into a permanent appointment.[39] He managed to extend his service in the Air Corps in the aftermath of the Air Mail debacle for eight months, but eventually returned to civilian life in 1935. He re-entered service for a year to run a Civilian Conservation Corps camp in New Mexico, then gained a further reserve appointment in the Panama Canal Zone. During his time there, he became

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[37] This remark by Arnold to Schriever was widely quoted by the latter, including in an oral history interview within the NASA oral history program. It is also quoted in Stephen B. Johnson, 'Bernard Schriever and the Scientific Vision', *Air Power History,* Vol 49 No 1 2002).
[38] Neufeld, 'Bernard A Schriever: Challenging the Unknown', in Frisbee (ed),*Makers of the United States Air Force*, 283.
[39] Sheehan, *A Fiery Peace* 22-27.

acquainted with the daughter of the Air Corps Commander for the Zone – Brigadier General George Brett – when he was appointed one of the General's two aides. Schriever's relationship with Dora Brett developed, but when they became engaged, Schriever had to accept that a Reserve Lieutenant's position was too tenuous to support a wife. He left active service with the Reserves, and took up employment with Northwest Airlines in Seattle as an airline co-pilot. This lasted until the second half of 1938, when Hap Arnold, by now a Brigadier General, visited Seattle, partly to confer with the president of Boeing Aircraft there, but also, over a game of golf, to invite Schriever to apply for a regular commission in the Air Corps in the forthcoming competitive exam. Sheehan cites Arnold's wish to attract qualified airline pilots into the Air Corps, partly informed by the need to bolster its ability to operate at night and in bad weather, and coloured by the experience of the Air Mail fiasco:

> He explained that he wanted to create an all-weather air arm and therefore needed to get as many airline pilots who were Reservists as possible back into the Air Corps on a permanent basis, because they had the knowledge and experience for instrument flying. Decades later, Schriever remained astonished at Arnold's ability to look into the future.[40]

Rejoining the military and World War 2

Schriever returned to duty as a bomber pilot, but soon transferred to test-flying duty at Wright Field in Dayton, Ohio. This was not entirely unconnected with his father-in-law, Brigadier General Brett, now being head of the Materiel Division of the Air Corps, of which Wright Field formed part. It provided him not only with an opportunity to broaden his flying experience as a test pilot, but also to enter the Air Corps Engineering School for its one-year course in general aeronautical engineering, which he did in July 1940.[41] He did well in his studies, achieving an academic grading of 'superior', and consequently was selected to proceed on graduation from the Air Corps School to Stanford University to undertake a Masters degree in Aeronautical Engineering. He entered Stanford in September 1941 and was studying there on 7th December 1941 when the Japanese attack on Pearl Harbor brought the United States into World War 2. Despite his expectation that he would be recalled to active duty immediately, he was ordered not to proceed until he had completed his studies; consequently, it was June 1942 before he left for Australia and wartime assignments, first with the 19th Bombardment Group and

[40] Sheehan, *A Fiery Peace* 26
[41] Kells et al., *BAS Papers; Finding Aid* 2-4

then at the Headquarters of the Army Air Services, Southwest Pacific Area, the maintenance and engineering headquarters for the region.[42]

For the duration of the Second World War, Schriever combined the role of combat pilot with that of maintenance officer, capitalizing on the training he had received at the Engineering School. He flew combat missions regularly in B-17 bombers with the 19th Bombardment Group, and then rose in rank and responsibility within Headquarters Far East Air Service Command. At the end of the war, he was Commander of the Advanced Headquarters of the Far East Air Service Command on Okinawa. He remained in service in the Pacific Theatre until after the Japanese surrender, returning to the United States in September 1945 as a full Colonel in the United States Army Air Forces. He had flown 38 combat missions, gained a Distinguished Service Medal, a Legion of Merit award and a Purple Heart.[43] His erstwhile patron had also advanced. Hap Arnold was now a General and Chief of Army Air Forces, and Schriever's first appointment following the war was to be at Headquarters, Army Air Forces in the Pentagon.

The Roots of Post-war American Innovation and the birth of the USAF

To understand the significance of what follows in Schriever's career, it is necessary to take a step back, and reconsider Arnold's remark quoted above about brawn, logistics and brains. The United States ended the war victorious, with its industrial capacity and access to natural resources largely intact, and uniquely, in possession of atomic weapons. Yet for the astute analyst, there were still signs to give cause for concern. Particularly in the Air Forces, the war had been technically conservative; most combat aircraft in service in significant numbers at the war's end had been in service at its outbreak.[44] This could be contrasted with both the Royal Air Force and the Luftwaffe in Europe, where combat fleets had been comprehensively renewed during the war. Partly, this was a consequence of the United States later entry into the war. A type introduced in the United States in July 1941 (say) would still be considered 'pre-war'. Yet Arnold looked across the Atlantic and pondered that the jet engine had been invented in the United Kingdom,

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[42] Jacob Neufeld, *Bernard A. Schriever : challenging the unknown* (100th anniversary commemorative edn. Washington, D.C.: Office of Air Force History, 2005) 5

[43] Although later awarded solely to those wounded or injured in service, during World War 2 it was used as an award for meritorious service.

[44] The most obvious exceptions to this were the B-29 Superfortress bomber that had delivered the atomic weapons at Hiroshima and Nagasaki, and the P-61 night-fighter; the B-29 met a genuine US requirement, but the P-61 was an attempt to harness British breakthroughs in airborne radar and night-fighter design specifications Of the other notable types introduced to the AAF during the war, the P-51 had originally been designed before the war to fill an RAF, not an AAF, requirement, and the C-47 transport was a development of a pre-war civilian transport design.

and introduced into service both there and in Germany. Both antagonists had made significant steps in introducing and exploiting radar (another British invention). The only advantage that the United States had comprehensively exploited had been in atomic weapon design, and even that had been considered by Germany. As for rocket and missile technology, Germany had built up a significant lead in the field, which the victorious allies were struggling to comprehend and exploit. And all these advances had come in the face of large-scale bombing, blockades, shortages of resources and active ground campaigns. Arnold himself had pondered this conundrum during 1944 and had taken the first steps towards addressing it.

Arnold's first exposure to the potential of rocket technology had come much earlier in his career, and it came about through a prolonged and recurring friendship. Despite an academically inauspicious start to his military career at West Point, by the time he transferred from the infantry to aviation, Arnold had developed an interest in technical innovation, for example in the development of the 'Flying Bug', a very early attempt at developing a cruise missile. Through this, he had come into contact with noted academics whose names will recur later in this account; most notable of these were Robert Millikan and Theodor von Karman.[45] Millikan was the inaugural Chairman of the Executive Council of the California Institute of Technology (Caltech).[46] Having won the Nobel Prize for Physics in 1923, he subsequently applied himself to investigating the properties of cosmic rays; the experiments he was conducting required apparatus to be flown to altitude. Millikan had collaborated with Arnold during 1917-18 in the development of the Flying Bug, and in 1931 he asked Arnold, as Commander of March Field, which lay about 40 miles from Caltech, for reciprocal assistance. Historian Dik Daso suggests that Arnold had little comprehension of the nature of the physics being undertaken, but nevertheless he willingly co-operated, and the ensuing flights took March Field aircraft from Canada to Mexico.[47] Through his continuing friendship with Millikan, Arnold also met Theodore von Karman, a Hungarian émigré academic who had arrived at Caltech in April 1930.[48] By 1935, von Karman was

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[45] For an account of Arnold's involvement in the development of the Flying Bug towards the end of World War 1, see Dik A. Daso, *Architects of American air supremacy: Gen Hap Arnold and Dr. Theodore von Karman* (May 2003 digitization of January 2003 reprint edn.: Maxwell Air Force Base, Ala. : Air University Press, 2003) 18-19

[46] Millikan was never 'President' of Caltech, but held the equivalent post when the Institute grew out of Throop University. See California Institute of Technology Archives, 'Fast Facts about Caltech History', (updated 27 June 12) <http://archives.caltech.edu/about/fastfacts.html>, accessed 28 July 2012 .

[47] Daso, *Architects of American air supremacy* 25

[48] Daso, *Architects of American air supremacy* 27

director of the Guggenheim Aeronautical Laboratory within Caltech, and by 1938, he was collaborating with (now) Brigadier General Arnold in developing rocket-assisted take-off devices for the Army Air Force to aid the take-off of heavy bombers.[49] Arnold had thus spotted at least one potential use for rocket technology at an early stage.

During the Second World War, Arnold rose in rank and responsibility commanding the Army Air Forces, while von Karman continued to provide advice and analysis to the American military community. In 1943, reports reached the USA via the United Kingdom of German interest in developing long-range missiles. Von Karman was asked to analyse these reports and provide comments on their feasibility.[50] Arnold pursued other technological advances in American aviation too – the rocket-assisted take-off experiments of 1938, coupled with growing awareness of European successes on both sides of the conflict with jet-engined aircraft led to Arnold's championing of the development of the Bell XP-59A – the first American jet-powered aircraft.[51]

By 1944, Arnold was convinced of two things: that the war would soon be over, with America among the victorious allies, and that the development and exploitation of military aviation would depend on close cooperation with (civilian) scientists on topics of research interest.[52] He discussed a way ahead on the latter challenge with von Karman in a famous conversation that took place in a car parked on the apron at La Guardia airport in New York early in August 1944.[53]

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[49] Perversely, although they were pure rockets, such devices were known as 'Jet' assisted take-off (JATO) systems. This was not for reasons of security – the use of what would now be known as 'jet' engines lay in the future, and in later years, jet engines would be added to piston-engined aircraft to improve take-off performance. One plausible rationale for the nomenclature cites the undesirable image created in academia by 'rocket' research – see Loyd S Swenson, Jr, James M Grimwood, and Charles C Alexander, *This New Ocean: A History of Project Mercury (NASA SP-4201)* (Washington DC: NASA, 1998) For an account of Arnold's commissioning of the research, inspired by intelligence reports from the Spanish Civil War, see Daso, *Architects of American air supremacy* 58

[50] James W Bragg, *Historical Mongraph No 4 - DEVELOPMENT OF THE CORPORAL: THE EMBRYO OF THE ARMY MISSILE PROGRAM* (Redstone Arsenal, AL: Reports and Historical Branch Control Office, Army Ballistic Missile Agency, 1961) 4

[51] For an account of Arnold and von Karman's involvement in the development of the XP-59A, see Daso, *Architects of American air supremacy* 66-69 Notwithstanding Arnold's championing of it, it did not lead to an American operational jet-powered combat aircraft during World War 2.

[52] Daso, *Architects of American air supremacy* 76 See also Flint O. DuPre, *Hap Arnold: architect of American air power* (Air Force Academy series; New York: Macmillan, 1972) 113-14

[53] Despite its significance, even the date of the meeting is not entirely clear. It took place at La Guardia because von Karman was seriously ill, having just undergone surgery for cancer, and while keen for him to travel to the Pentagon to meet him, Arnold felt it safer to travel north to New York. The meeting took place with no others present, so accounts of its content rely on later recollections and interviews, and there is uncertainty about some of the dates that von Karman cited at what was plainly a stressful period. See Daso, *Architects of American air supremacy* 127-28 and Michael H. Gorn, *Harnessing the genie : science and technology forecasting for the Air Force, 1944-1986* (Air staff historical study Washington DC: Office of Air Force History, U.S. Air Force, 1988) 12

Implementing the proposed solution would bring Schriever back to the centre of the story.

Arnold proposed to von Karman that he should lead an investigation into the technical steps required to secure American air-supremacy into the future. This was to be a far-reaching study, with little regarded as off-limits:

> Arnold wanted the scientists assembled at the Pentagon to "forget the past; regard the equipment now available only as the basis for the boldest predictions"…Forced for four years to think in incremental terms, General Arnold now sought the best people in the scientific community to spur air power technology far beyond present limitations[54]

He had already canvassed Millikan as to von Karman's suitability (and Millikan and Caltech's willingness to lose his services for an indeterminate time). Von Karman's only reservations were unwillingness to work at the Pentagon and to have to give and receive orders. Arnold assured him this would not be necessary; he would be von Karman's 'boss' and would issue the orders on his behalf where necessary. This opened up the opportunity for Schriever to become involved in the follow-on to the program. In the meantime, von Karman was appointed General Arnold's Scientific Adviser on 23rd October 1944. By December 1944, the organisation he was directing was officially titled the 'AAF Scientific Advisory Group'.[55] In its two-year existence, it produced an immediate interim report entitled 'Where We Stand', and a twelve-volume substantive report (incorporating the content of 'Where We Stand', and including a first volume called 'Science, the Key to Air Supremacy) entitled 'Towards New Horizons'.[56] By the time this report was being circulated among AAF leaders in late-1945, Arnold was further convinced of the need to institutionalize the process by which it had been produced. 'Towards New Horizons' had included detailed surveys and recommendations for specific technical area, including high-speed aerodynamics, propulsion, missile design and guidance mechanisms and radio and radar systems, but in many ways the most far-reaching recommendations had been about the relationship between the scientific, industrial and military communities contained in von Karman's original 'Science, the Key to Air Supremacy' volume.[57] In this, von Karman outlined the steps he saw as necessary to perpetuate the capability to generate reports such as his own. His summary saw the following as essential:

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[54] Gorn, *Harnessing the genie* 14
[55] Gorn, *Harnessing the genie* 19
[56] AAF Scientific Advisory Group Towards New Horizons, (Washington DC: AAF Scientific Advisory Group, 1945) Although initially classified 'Secret', by 1960 the report had been declassified *in-toto.*
[57] See AAF Scientific Advisory Group, Short Towards New Horizons contents page for the individual subject areas covered. These are at pp xix-xxiii in 'Volume 0' and repeated in subsequent volumes.

> It is imperative from this point of view that the Air Forces continue and expand their present direct relations. spiritual and contractual, with various universities, research laboratories, and individual scientists. None of the central organizations existing now and to be established should be the only source of information and the sole intermediary agency between science and the Air Forces. The Air Forces should have the freedom to call on institutions and individuals whose assistance they deem to be of the greatest benefit for their program.[58]

Schriever thus arrived in Washington DC in December 1945 just in time to witness and participate in three initiatives championed by Arnold: the establishment of the Scientific Advisory Board (leading to Schriever's associated responsibilities as 'Scientific Liaison Officer'), building links with another Arnold project – Project RAND (soon to become the RAND Corporation) and involvement with the work of the Joint Research and Development Board. In each case, however, Schriever's precise duties and responsibilities are hard to define. It seems plausible that in each case they never were formally stated. Schriever himself commented later on this ambiguity:

> In December 1945, therefore, I was given a job called scientific liaison in a brand new office by that name in the Pentagon. I was at first uncertain exactly what to do about scientific liaison. Well, 'liaison' meant the Air Force working with the scientific community in this case, and that is what I set out to do.[59]

Thus he embarked on what he later described in an Oral History interview conducted under the auspices of a joint programme by the National Air and Space Museum of the Smithsonian Institution and the RAND Corporation as: "the genesis of the creation of the Scientific Liaison Office for that rather general objective of establishing and maintaining that interaction with the scientific community".[60]

Meanwhile, realizing that, because of his failing health, his time as head of the Air Forces would probably be short, Arnold set out to institutionalise some of his wartime initiatives and implement the content of von Karman's advice. The institutional change he saw as most urgent involved making the Scientific Advisory Group a permanent feature of the Air Force landscape, but the charter of the

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[58] AAF Scientific Advisory Group, Short Towards New Horizons Volume 0, p85
[59] Schriever speaking in 1995 at a Symposium on the US Air Force and Space. See R. Cargill Hall and Neufeld Jacob (eds.), *The U.S. Air Force in space : 1945 to the twenty-first century* (Air Force Historical Foundation Symposium, Andrews AFB, Maryland, September 21-22, 1995: Washington, D.C. : USAF History and Museums Program, U.S. Air Force, 1995) 13
[60] Bernard Schriever, interviewed by Martin J. Collins on 18 May and 5 September, 1990 for Joint Oral History Project on the History of the RAND Corporation. National Air and Space Museum Library, Smithsonian Institution

existing Group expired on 6 February 1946. Complicating the otherwise straightforward step of re-constituting it as a standing body was Arnold’s resignation from the command of the Army Air Forces on 9 February 1946 on the grounds of ill-health. A tussle ensued between General Curtis LeMay and General Carl Spaatz about reporting arrangements, eventually adjudicated by General Dwight D Eisenhower in favour of Arnold’s original vision. The successor organisation – the Scientific Advisory Board – was constituted on 17 June 1946, though even after that it continued to face threats to its existence.[61]

While building the fledgling relationships alluded to, Schriever also gained early exposure to another of Arnold’s initiatives to develop scientific capability: the formation of the RAND Corporation. Arnold had exploited friendship with Edward Bowles, another academic contact, though this time from MIT rather than Caltech. Bowles had been a consultant to the Secretary of War, and this in turn had led to involvement of others, including Donald Douglas, President of Douglas Aircraft Company, Arthur Raymond, Chief Engineer at Douglas and Franklin Collbohm, Raymond's assistant.

On October 1, 1945, Arnold, Bowles, Douglas, Raymond, and Collbohm met at Hamilton Field, California, to set up Project RAND under a special contract with the Douglas Aircraft Company. Project RAND got under way in December 1945. On March 2, 1946, a letter contract was executed that put Project RAND under Frank Collbohm's direction.[62] The first task laid on the Project RAND team by the Air Force was a study of the potential of earth-orbiting satellites, so by involvement with RAND’s work at an early stage, Schriever was gaining entry to a very early American consideration of the military potential of spaceflight.[63] He was clear about the distinction between the work of RAND and the work of the Scientific Advisory Board: “The Scientific Advisory Board would use some of RAND’s analytical studies, but they never themselves went into that kind of detail. Their interests were, I would say, at a level higher with respect to technologies, and even further out in the future than RAND”.[64]

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[61] For details of the struggle over reporting arrangements and Eisenhower’s role in resolving the dispute, see Gorn, *Harnessing the genie* 44-46 LeMay sought to subordinate the Board to his own role as Deputy Chief of Staff for Research and Development, rather than allowing direct access to the Commander of the Army Air Forces (Spaatz). It fell to Eisenhower as Army Chief of Staff (prior to the creation of the Air Force as an independent armed service) to resolve the debate. See also Chapter 4, p86 of this thesis.
[62] The RAND Corporation, 'History and Mission', <http://www.rand.org/about/history.html>, accessed 31 July 2012
[63] The US Navy had in fact seized a short-term lead in military thinking about satellites, but it is unclear if Schriever was aware of this work at all.
[64] Schriever, 'RAND Oral History Interview', 11.

Finally, Schriever had to reconcile these responsibilities with the activities of the Joint Research and Development Board. This was an organisation in transition, on its way to being reconstituted during 1947-49 with the creation of the Department of Defense and the establishment of the United States Air Force as an independent service. Schriever's duties involved preparing briefing material for the members of the Board; he summarized:

> They set up that Joint Research and Development Board …so it was set up in '46 and I was also the briefing officer to our representatives on the JRDB. So it was the Joint Research and Development Board, the Air Force Scientific Advisory Board and the RAND Corporation that I dealt with, interfaced with, as far as the air staff was concerned. That's how I became involved with the scientific community to the extent that I did…[65]

One significant Schriever initiative from this period has been recorded; the introduction of Development Planning Objectives (DPOs). These sought to match military requirements with R&D activity. Working at the Air Force Historical Studies Office, Jacob Neufeld later described them thus:

> For the first time, operational matters were considered within the broad context of technological advancement. DPOs were prepared for all major elements of air power: strategic, tactical and logistical elements; air mobility; space systems; air defense; intelligence; and reconnaissance. Schriever became a strong proponent of the efficacy of "technology push", an approach that allowed free rein to technological R&D as the means for solving complex operational needs.[66]

Schriever continued in this vein until he was selected to attend the National War College (NWC) in 1949. This was at the time a 'young' organisation, created in 1945 to prepare its graduates for the highest ranks in their service: "The College is concerned with grand strategy and the utilization of the national resources necessary to implement that strategy... Its graduates will exercise a great influence on the formulation of national and foreign policy in both peace and war...".[67] Selection to attend thus marked Schriever as an officer with potential for high rank. The curriculum for the course was wide ranging, covering 'International Affairs' in the first semester, and shorter series of lectures on 'Elements of National War Planning', 'Strategy Analysis', 'Strategic Concepts of Operations and Outline

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[65] Schriever, 'RAND Oral History Interview', 9.
[66] Jacob Neufeld, 'Gen Bernard A Schriever: A Tribute', in Jacob Neufeld (ed.), *A century of air power leadership : past, present, and future* (College Station, TX: Air Force History and Museums Program, 2003), 176-77.
[67] Quotation by Lieutenant General Leonard T Gerow, President of the Board that recommended creation of the NWC, cited in National War College of the National Defence University, *60th Anniversary Brochure* (NDU Washington DC, 2005) Downloaded from http://www.ndu.edu/nwc/AboutNWC/NWC_60th_Anniversary_Brochure.pdf on 3 August 2012.

Plans', National Security issues in various guises, technology and logistics in the second. Among the series on 'Weapons Capabilities and Prospective Developments' was one on 'Guided Missiles' – delivered by Dr Frederick Hovde, who was then the President of Purdue University, but who had been Chief of Rocket Ordnance Research for the National Defense Research Committee during World War 2.[68] Schriever produced a term paper during his time at the College, thought its title offers little suggestion of an interest in technology and its applications ("The Economic Commission for Europe: Its Purpose, Organisation and Accomplishments"). It is, however, unclear whether the students had any say in choosing their own topics.

A significant development had taken place during Schriever's time at NWC. In response to the Ridenour Committee's report in 1949 (on work undertaken partly at the prompting of Dr von Karman, and partly at the direction of General Putt, who had been Schriever's Commanding Officer during his time as Scientific Liaison Officer), the USAF had created a new command – Air Research and Development Command (ARDC) – in an attempt to separate research and development activity from the procurement and support of current combat equipment.[69] ARDC would play a significant role in Schriever's later career.

The 'Paperclip' Cadre and the V2 – the Foundations of the US Missile Programme

Since this period (broadly 1946-1950) marked the time when the 'Paperclip' émigrés referred to at the start of Chapter 1 were settling in the US, this is probably a good place to summarise Schriever's relationship with them. The first thing to note is that the Paperclip cadre was substantial and not exclusively concerned with rocketry and missiles. At least 1500 individuals were potentially involved with the programme (along with their families, which took the total cadre size to well over 3000), although a lower number were eventually assimilated into the USA.[70]

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[68] Lecture Schedule for Academic Year 1949/50 for the National War College – NDU Archive; Purdue University Libraries, 'A Biography of Frederick L. Hovde', <http://www.lib.purdue.edu/spcol/?page=hovde>, accessed 4 August 2012

[69] The Ridenour Report was not the only report that had proposed the foundation of an R&D Command – there had been an earlier report by the Office of the Air Force Assistant for Atomic Energy in October 1947 which had made essentially similar recommendations, but which remained un-implemented. Additionally, General Hoyt Vandenberg, the then Chief of Staff of the Air Force subjected the Ridenour Report's findings to review by a further panel chaired by General Anderson of the Air University. See MIchael H Gorn, *Vulcan's forge : the making of an Air Force Command for weapons acquisition, (1950-1985)* (University of Michigan Library Reprint edn. Andrews Air Force Base, Md.: Office of History, Headquarters, Air Force Systems Command, 1989) 12-14, and Major Arthur D Tubbs, 'Establishing Air Research and Development Command: Two Civilian Scientists Played Key Roles', (Air Command and Staff College, Maxwell AFB, Alabama 1986 7.

[70] The US National Archive holds the personal files of over 1500 potential Paperclip émigrés , and admits that this is not a complete collection – notably, they do not hold the dossier for Wernher von

Nonetheless, the rocket scientists attracted most attention, and it is clear that as early as 1946 there were at least 100 of them working in the USA.[71] The most prominent of them were undoubtedly Wernher von Braun and Walter Dornberger, both key figures in the development and production of the V2 missile.

Work to develop what would become the V2 had occupied Wernher von Braun and Walter Dornberger from about 1930, until just before the German surrender in 1945. Both men had been active in the 'Verein für Raumschiffahrt' (VfR), an amateur rocket development society founded in Germany in 1927 and absorbed into the Third Reich's military expansion by 1934.[72] Wernher von Braun (1912-1977) had joined the VfR in 1930, having previously been strongly influenced by the work of Hermann Oberth; von Braun was the principal designer of the V2 missile. After World War 2, he emigrated to the USA and was assimilated under the 'Paperclip' programme prior to work firstly with the US Army and latterly with NASA where he played a key role in the Apollo moon-landing programme. Dr Walter Dornberger (1895-1980) was initially a German artillery officer. He served in World War 1 then qualified as an engineer. He was the military patron of the VfR and was the director of the V1 and V2 programmes at Peenemunde during World War 2. He too emigrated to the USA post-war, and worked briefly for the USAF (where he came into some conflict with von Braun's work with the Army). He later worked for the Bell Aircraft Company, prior to retirement and eventual return to Germany.

Initially, the German rockets were referred to as being part of the 'Aggregat' series, commonly referred to as A-1, A-2 etc. One of the last things attached to the programme was the 'V2' designation.[73] This was imposed when operational use approached by Goebbels' Propaganda Ministry, standing for 'Vergeltungswaffen' or 'reprisal/vengeance weapon', and indicating their perceived effect, rather than any development lineage across the 'V-series'.[74] Compared to the other rocket

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Braun. For the National Archive finding guide, see http://www.archives.gov/iwg/declassified-records/rg-330-defense-secretary/ (accessed 3 May 2016). Total numbers credited to the programme vary with different authors – partly because in the earlier stages of the programme, the rate at which Germans were allowed entry was controlled carefully, and not all émigrés stayed permanently in the United States. Eric Lichtblau mentions a figure of "…up to a thousand scientists…". Lichtblau, *Nazis Next Door* 24.

[71] Walter J Boyne, 'Project Paperclip', *Air Force Magazine,* Vol 90 No 6 June 2007)

[72] German development of rocket technology had not been proscribed by the Versailles Treaty, and was consequently attractive to the re-arming Reich as a potential means of developing long-range attack capability.

[73] The original convention of writers on the topic appears to be to hyphenate the 'A' designations, and not to hyphenate 'V2'. Particularly in the US, use of 'V-2' became common after the war. This thesis adheres to the original convention, though use in the literature now appears to be mixed.

[74] To illustrate the diverse nature of the 'V' the 'V1' was a winged, pulse-jet powered bomb (in modern times it would be described as a cruise missile), developed by the Fieseler aircraft company, the V2

pioneers, the German team had the advantages of governmental support, good facilities and adequate funding, but despite this, they still struggled with technical problems throughout development, and indeed since they made more progress than any other team, they encountered more of them. The A-1 to A-3 models were developed between 1933 and 1937, and represented at best technology demonstrators, rather than operational systems. The only A-1, despite successful ground engine tests failed at launch, two A-2 models were successfully demonstrated to army officers in December 1934, and the A-3, a much larger design (about half the size of the eventual V2), was a severe disappointment to its designers, suffering several failures during December 1937. The A-4 design had already been begun, but was temporarily halted while the A-5 emerged as a re-designed A-3, still half-size, used to iron out design flaws. After repeated successful A-5 tests, the A-4 was first flown in 1942, and the design became the V2, first used operationally in 1944.[75] It is worth noting the achievements that the design team had accomplished at this stage.

The V2 was a true ballistic missile; after launch, it accelerated along a pre-determined path while the rocket motor ran, and at a designated instant, the fuel supply was cut off. The rocket was then unguided, its point of impact determined solely by its position, speed and attitude at the instant the engine cut out. The airframe could withstand the strains of powered flight and acceleration, a rapid climb into space (at a maximum altitude typically just above 50 miles) and the reverse process as it fell in free flight, stabilised aerodynamically but not guided, towards its target. Total flight duration was typically about 5 minutes, and its peak speed was about Mach 4.0 – four times the speed of sound. This was fast enough to pose problems of transonic and supersonic stability, complicated by the centre of gravity of the missile shifting dramatically as it consumed its fuel. Initially, the missile required both stability (i.e. the prevention of tumbling or tipping relative to the flight path) and guidance (i.e. direction along the required course to hit the target). During powered flight, it was steered by a combination of aerodynamic control via moveable surfaces at the bottom of its fins, similar to those of an aircraft, and by varying the direction of the rocket exhaust by moving graphite

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was the ballistic missile described, and the V3 was a fixed, multiple-charge staged-firing cannon pointed at London from near Calais. Development of both V1 and V2 systems took place at Peenemunde, partly on grounds of secrecy and partly because of access to test ranges and facilities, but in all other respects, they were separate systems.

[75] There are several accounts of the development of the A-series rockets, but one of the most concise is Walter Dornberger's own, originally published in 1952. See Walter Dornberger, *V-2*, trans. James Cleugh and Geoffrey Halliday (New York, NY: The Viking Press, 1979). Each of the 'Aggregat' rockets is cited individually in the index.

vanes positioned in the exhaust flow. These latter surfaces were in fact relatively inefficient; since they were permanently in the exhaust stream, they restricted its flow, even when no steering command was required.[76] Guidance was provided by a variety of systems, but at its heart, the V2 contained a rudimentary inertial guidance system.

In its modern form, such a system consists of a physical platform held in a fixed orientation in space, independent of the vehicle in which it is mounted, stabilised by gyroscopes. On the platform, accelerometers measure any accelerations imposed on it by the carrying vehicle in each of 3 dimensions, and by continuously summing those accelerations, and knowing the original start position of the system, they measure its present position.[77] The V2 implemented several rudimentary forms of this system; one variant used three gyroscopes in an attempt to stabilise a platform in three dimensions.[78] Most used a two-axis platform, supplemented by a compass system, and in some variants a radio navigation beam to aid initial steering. In all cases, the essence of the system was to accelerate the missile along a pre-determined heading and a fixed attitude, a balance between vertical ascent and horizontal acceleration. Launch sites were surveyed so that the required heading and distance to reach the target was known, and the missile either computed its speed by measuring acceleration until the required value was reached, or was tracked by radar from the ground and commanded to shut the engine down at the correct instant. Because of its rudimentary design and tolerances in manufacture, accuracy was not very high; precise figures are hard to determine due to the operational circumstances surrounding most launches, but they were of the order of 3-5 miles over about 150 miles of flight range. An operational V2 carried about 1000 kilograms of high explosive.[79] This was a substantial warhead, but accuracy in the 3-5 mile range was nowhere near enough to use single missiles against strategic targets. Rather, the V2s were employed against area targets, principally cities such as London and

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[76] One modification of a V2 that yielded immediate performance gains in post-war American flight tests was to remove the vanes from the exhaust, and substitute it with a moveable ('gimballed') engine which could tilt the entire thrust-line. Although more complex than the vanes, and incurring a weight penalty, the performance improvement over the previous system was substantial.

[77] In its most modern form, an inertial navigation unit my dispense with the fixed platform and measure displacement by optical means, relying on interference between light beams travelling in opposite directions to each other, but the principle remains similar to the original gyroscopically stabilised platform.

[78] Practical three-axis inertial navigation systems rely on exquisite measurement of acceleration within very fine mechanical tolerances; it is fair to say that V2 systems did not approach the standards of accuracy that would be required e.g. to monitor the missile flight path and correct it continuously in three dimensions. Nonetheless, it would be hard to describe them without stating that they embodied some of the characteristics of such systems.

[79] Norman Longmate, *Hitler's Rockets: the story of the V2s* (London: Hutchinson and Co, 1985) 103.

Antwerp. Where they were used against precision targets (for example when aimed at the town of Remagen in Germany in 1945 in an attempt to limit use of the bridge over the Rhine there), they were of questionable effectiveness.[80]

At the heart of the V2 lay its engine, probably its designer's most significant technical achievement. It used industrial alcohol as fuel and liquid oxygen as an oxidiser. This choice of fuel was not the most powerful imaginable; The Russian rocket theoretician Tsiolkovsky had correctly identified liquid hydrogen as a more powerful alternative some 40 years earlier. However alcohol was easily obtainable and transportable, was more dense (so occupied a smaller space within the missile for a given weight) and avoided the challenges of storing and pumping a liquid at a temperature of -250 degrees Celsius; the challenges posed by liquid oxygen at about -200 degrees Celsius were severe enough. A significant breakthrough for the designers was their ability to pump large quantities of fuel and oxidiser to the combustion chamber; the pumps themselves drew on fire-engine pump technology (though they faced adaptation challenges to reach the performance required for use in the V2), but the mechanism of driving them on a common shaft, powered by decomposing hydrogen peroxide with a catalyst and using the resulting steam to drive a turbine was novel. A greater problem had been achieving stable combustion within the engine – early engines had been plagued by unstable combustion leading to explosions or failures within the chamber. This was overcome by extensive design and redesign of the fuel and oxidiser mixing arrangements, involving elaborate nozzles and careful component layout. Finally, the combustion chamber was protected from extreme temperatures firstly by circulation of fuel around it as a coolant prior to injection into the chamber, and secondly by arranging for a small proportion of un-burnt fuel to form a thin film or boundary layer inside the combustion chamber.[81]

Subsequent V2 Refinements

The V2 was thus a mature ballistic missile, but it is worth noting that although it represented the 'state of the art' in 1945, it left plenty of room for

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[80] Historians debate whether nearby V2 impacts contributed to the collapse of the bridge, already significantly weakened by prior demolition attempts by retreating German forces. Certainly V2 impacts occurred in its vicinity on the day of its collapse, but none of the missiles hit it, and attribution must remain uncertain. Hitler nonetheless sent personal congratulations to the battery commander responsible for the barrage on the following day. For an account which concludes that V2s were not responsible, see Ken Hechler, *The Bridge at Remagen* (Presidio War Classic New York, NY: Presidio (Mass Market Edn), 1975) 198-201. See also http://www.v2rocket.com/start/deployment/v2s-on-remagen.html (accessed 17 July 2016) for further detail and references.

[81] A comprehensive description of the development of the A-4/V2 rocket motor can be found at J. D. Hunley, *Preludes to U.S. space-launch vehicle technology : Goddard rockets to Minuteman III* (Gainesville, FL: University Press of Florida, 2008) 102-12

Schriever and others to improve upon in the following decades. By some measures, its development had stalled at precisely the 'right' stage for Schriever to capitalize on it subsequently. On one hand it was too small, short range, inaccurate and inefficient for intercontinental use as a nuclear-tipped missile. But on the other, and particularly after the American experience exploiting captured V2s post-war proved that improvements were possible, they had achieved their (limited) aims and it was impossible to argue that they were impractical. Plainly, however, there were areas ripe for improvement.

The first improvement required was to scale the missile up; a V2 was too small and too short range for intercontinental use. Early ICBMs were substantially larger than the V2, both to allow carriage of a larger warhead and to increase fuel capacity to give intercontinental range. This brought into focus the possibility of requiring a multi-stage rocket, as originally posited by Tsiolkovsky. Both the Atlas ICBM and its Soviet counterpart 'fudged' this design issue by adopting a hybrid 'stage and a half' design, where multiple rocket engines were ignited at launch, and some later discarded soon after launch, but subsequent missiles would employ true multi-stage principles.[82] As well as the theoretical advantages outlined by Tsiolkovsky regarding discarding useless weight, multi-stage rockets also allowed the various stages to be optimised for operation in the atmosphere (for first stages) or in the vacuum of space (later stages).[83] Initial fears about the difficulty of igniting a second or subsequent stage in-flight were addressed by the development of 'hypergolic' fuels and oxidisers, which reacted spontaneously when mixed. The principle could also be applied to first stages, in the interests of launch reliability for missiles on alert, though at a price regarding safety in a silo due to the toxicity and spontaneous explosiveness of the substances involved.[84]

The next improvement was to guidance systems. These needed to be matched to the yield of the warhead (greater yield requiring less accuracy to achieve effect on a given target). Dornberger is clear in his memoir that inertial

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[82] Chuck Walker and Joel Powell, *Atlas : the ultimate weapon : by those who built it* (Burlington, Ont.: Apogee Books, 2005) 24-25

[83] This factor had a significant impact on the design of the rocket engine exhaust nozzle, since it determined whether the engine had to operate against atmospheric pressure outside the chamber, or whether it was exhausting into a vacuum.

[84] The worst silo accident in USAF service killed 53 workers in 1965 when a fire started in a silo in Searcey, Arkansas, although this instance did not directly involve missile fuel. Other silo accidents involving hypergolic propellants did occur, however, including one in 1980 at Damascus, Arkansas involving a Titan II ICBM, which killed a member of the silo crew and ejected the (live) nuclear warhead from the silo. The warhead safety features worked as advertised and no detonation or release of nuclear materials occurred. For further details of Titan incidents and accidents, see David K. Stumpf, *Titan II : a history of a Cold War missile program* (Fayetteville: University of Arkansas Press, 2000) Chapter IX.

systems had been preferable to the radio beams used for some early V2 flights (at least partly because of their immunity to jamming and other countermeasures), and von Braun had also favoured them from the outset during V2 development.[85] When Schriever came to developing the Atlas ICBM, he was initially limited by the accuracy of inertial guidance available, and the first Atlas missiles used a hybrid system with gyroscopic information on the missile being combined with radar tracking data from the ground; later variants employed purely inertial systems.[86]

A further problem to be addressed was re-entry; although the V2 coasted clear of most of the atmosphere at the top of its trajectory, and thus had to travel back through it to reach its target, it did so at a relatively modest speed for a missile. Although fast enough to induce problems with transonic stability during re-entry, and to impose significant stresses on the missile airframe, it still managed to avoid significant issues with frictional heating during its descent.[87] To reach ranges typical for ICBMs, however, the missile must be accelerated to much greater speeds, and problems of frictional heating during re-entry became significant. A variety of solutions were explored, broadly speaking along two lines of development. One was to build a massive cover for the warhead, large enough to absorb the heat generated without damage to the payload. The other utilised ablative coatings – materials capable of exposure to high-temperatures which would gradually burn away during re-entry, and which were made thick enough for the inner layers to survive.[88]

There were, of course, other significant problems to overcome; for example, the V2 had employed mobile launchers capable of launching a missile from a rural location after it had been fuelled in the field. At least initially, this was impractical for an ICBM due to its size. The early use of cryogenic (ultra low-temperature) oxidisers such as liquid oxygen required that the missile be fuelled immediately before launch (otherwise the liquid oxygen would boil within its tank and either evaporate if it was not continuously topped up, or gradually chill the entire rocket if it was) limited operational employment. Even assuming a fixed launch site, which

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[85] See Dornberger, *V-2* 245 for his views on jamming, and Neufeld, *Von Braun* 103 for von Braun's views on inertial guidance. References within Neufeld on this topic refer to ''Krieselgerate" – the firm who manufactured the gyro platforms.
[86] Walker and Powell, *Atlas* 54.
[87] German engineers suspected that some of the in-flight failures experienced by operational V2s were due to frictional heating during descent igniting un-burnt fuel in the missile's tanks, but this does not appear to have been proven conclusively, nor was it a limiting factor in the use of the V2. J D Hunley, *Preludes to US Space-Launch Vehicle Technology: Goddard Rockets to Minuteman III* (Gainseville, FL: University Press of Florida, 2008) 80.
[88] Walker and Powell, *Atlas* 58. Ernest G. Schwiebert, *A history of the U.S. Air Force ballistic missiles* (New York: Frederick A Praeger, 1964) 61-62.

could produce sufficient liquid oxygen, the civil engineering challenges of protecting 'on alert' missiles either in silos or in surface revetments was substantial.[89]

Schriever and the Paperclip Cadre

Alongside von Braun and Dornberger was another member of the Paperclip cadre – not an engineer, but rather a medical doctor – called Hubertus Strughold, who would influence Schriever's thought significantly. Initially, Strughold had the greater influence on Schriever, principally because on arrival in the United States, von Braun and Dornberger were sequestered by the US Army, rather than the Air Force. The Army initially confined them in Fort Bliss, Texas, and later moved them to Huntsville Arsenal in Alabama where they worked on the Army's Redstone (short range) missile programme.[90] Strughold, meanwhile, who had been located in Germany by a USAF doctor and aviation medicine specialist, Colonel (later Major General) Harry Armstrong, started work immediately at the USAF School of Aviation Medicine at Randolph Field, Texas.[91] The author can find no direct evidence of interaction between Schriever on one hand and Armstrong or Strughold on the other in these early days, but given Schriever's wide-ranging duties at the time, it seems likely he was aware of the work being undertaken at Randolph Field. Schriever quickly identified the importance of aero-medical research to enable manned spaceflight, and there is later evidence of the continuing relationships between the Paperclip cadre collectively and Schriever.[92]

As for von Braun and Dornberger, neither ever worked directly for or with Schriever, yet plainly their work influenced him. Dornberger was detained by British forces at the end of World War 2 and was held in the UK for 2 years before travelling to the USA. After working briefly for the USAF at Wright-Patterson AFB

[89] Dr John Lonnquest, whose PhD thesis deals extensively with Schriever, and which will be discussed in the next Chapter, later became the Historian of the US Army Corps of Engineers. He has written the definitive history of the civil engineering and infrastructure aspects of the ICBM programme, which expands at length on this topic. See John C. Lonnquest and David F. Winkler, *To defend and deter : the legacy of the United States cold war missile program* (USACERL special report 97/01 Washington, DC: Dept. of Defense, Legacy Resource Management Program, Cold War Project, 1996)
[90] Bower, *Paperclip* 308-09. Boyne, 'Project Paperclip', (*Air Force Magazine)*.
[91] Mark R. Campbell et al., 'Hubertus Strughold: The "Father of Space Medicine"', *Aviation, Space, and Environmental Medicine,* Vol 78 No 7 (July 2007). Although written over 20 years after Strughold's death in 1986, this article takes an uncritical view of him; he died a US citizen, in good standing with the USAF, but his posthumous reputation has been severely tarnished by the previously overlooked connection between much of his post-war work and experiments carried out on human subjects in concentration camps during World War 2. The 'Hubertus Strughold Award' mentioned by Campbell is no longer awarded under that title. Both Bower and Lichtblau devote chapters of their books to Strughold – see Bower, *Paperclip* Chapter 11 and Lichtblau, *Nazis Next Door* Chapter 6. Details of Armstrong's rise to prominence as a USAF medical officer can be found in Annie Jacobsen, *Operation Paperclip: the secret intelligence program that brought Nazi scientists to America* (New York: Little, Brown & Co, 2014) 204-16.
[92] See Chapter 5, p125 of this thesis.

(while Schriever was working in the Pentagon) he was employed by Bell Aerospace. He worked on the X-15 and X-20 Dyna-Soar programmes – the latter of particular interest to Schriever – before returning to Germany in retirement, via a brief sojourn in Mexico.[93] Von Braun stayed in US Army service at Fort Bliss and later at Huntsville prior to transferring to NASA and playing a leading role in the Apollo programme. His views on the potential utility of space weapons became prominent in the late 1950s, and in this context became more relevant to Schriever's work.[94]

Schriever in the early-1950s – 'Development Planning'

On graduation from the NWC, Schriever resumed duties within the broad area of technology, research and development. His new appointment was as Assistant for Development Planning on the Air Staff. Here, his responsibility was to try to move weapons development planning forward to incorporate technologies not in service at that instant. He described it thus:

> …the main purpose of that particular office was to try to factor in, as an extremely important element in the determination of initiating new weapon system developments, the technologies that would be available downstream…I mean five to ten years in the future, although that meant the technology was already being worked with in laboratories…to try to give a balance to the nearer term operational commands and the longer term technology…[95]

Once again, Schriever's precise duties remained loosely defined – he himself described them as "ad-hockery". He rose within the Development Planning Office from 'Assistant for Evaluation', to 'Assistant for Development Planning' to Office Director.[96] He had the advantage of support for his activities from the then Chief of Staff of the Air Force, General Nathan Twining, who knew Schriever from previous service.[97] Schriever had control of a budget of $10 million to fund his activities, and one of the uses he put this to was having personnel seconded to his

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93 Roy Houchin describes Dornberger's role in the X-20 in his monograph Roy F. Houchin, *US hypersonic research and development : the rise and fall of Dyna-Soar, 1944-1963* (New York: Routledge, 2006) 7-9, 16. Jacobsen also notes Dornberger's employment history in her 'guide to characters' *(op cit).*

94 See Chapter 6, p155 of this thesis.

95 Schriever, 'RAND Oral History Interview', 20.

96 These promotions owe at least something to the departure of Dr Ivan Getting, the founding Director, from public service to join the Raytheon Corporation. Getting would interact with Schriever again in the 1960s via The Aerospace Corporation.

97 Twining had undertaken his flying training at the same Texas bases as Schriever, but about 8 years earlier. He had then among other things served as an engineering officer in Chicago during the Air Corps 'Air Mail' operations in 1934, and in the Pacific Theatre at Air Forces Headquarters during World War 2 until 1943.

office for fixed term projects. He thus was able to seek assistance from various organisations such as the Air University at Maxwell AFB, Alabama, and more notably from the RAND Corporation.[98] Schriever's days of 'ad-hockery' were coming to an end, however, as technological developments in atomic weapons were about to force a period of massive re-equipment on the United States armed forces.

After the initial breakthrough in developing atomic weapons during World War 2, the United States had to contemplate other countries doing likewise. The Soviet Union duly achieved effective nuclear parity in 1949. At that point, both countries could only usefully deliver such weapons by air, as bombs dropped from aircraft. Although vastly more powerful than conventional alternatives, the yield of early fission weapons was still sufficiently limited that they required relatively accurate delivery, which at that time could only be achieved by aiming them from an aircraft near the target, to assure its destruction. The potential of missiles as a delivery system had obvious advantages from the standpoint of speed and reduced vulnerability, but the limits on the accuracy of missiles of sufficient range to serve as deterrent systems meant they were impractical. Solving this conundrum would either require more accurate missiles or more powerful nuclear warheads. In 1951-52, successful American tests of thermonuclear devices, which relied on nuclear fusion rather than fission, and which offered much higher yields than fission devices, suggested that the latter breakthrough was more likely. Determining whether this was in fact the case was referred to a special panel of the Scientific Advisory Board, under the direction of Professor John von Neumann. Their report in 1953 confirmed the feasibility of a thermo-nuclear tipped ICBM, and the USAF, which had maintained a fitful interest in their development over the years since World War 2 re-assessed priorities and established an organisation specifically to deliver an operational version.[99]

The Western Development Division and the Dawn of the Space Age

Schriever thus became the first (and only) commander of the Western Development Division (WDD), a component of ARDC established in Inglewood, California on 1 July 1954.[100] Schriever directed the WDD with the prime aim of

[98] Schriever, 'RAND Oral History Interview', 21.
[99] Edmund Beard, *Developing the ICBM : a study in bureaucratic politics* (New York: Columbia University Press, 1976) 170.
[100] The Western Development Division was re-named the Air Force Ballistic Missile Division in 1957 (while still commanded by Schriever), and was eventually split into a 'missile' division and a 'space

delivering an operational ICBM as quickly as possible. The resulting weapon system was known as the 'Atlas'; it achieved operational status in September 1959. To produce an operational system as quickly as possible, Schriever developed a variety of techniques of programme management, collectively known as 'concurrency'. The natures of these techniques, the reasons for their success and the ways that Schriever employed them have been the subject of much study. For now, it is sufficient to note that concurrency circumvented the limitations of sequential development, where one component of a complex system was developed essentially to completion before the next stage was commenced. During the five-year duration of Atlas development, Schriever coordinated the design, testing and production of the complete missile airframe and propulsion system, its integration with the warhead, the construction of infrastructure at the operational bases for the missile and the training of personnel to operate it. Within his management activities, Schriever also undertook significant innovation in the introduction of management information systems and development of internal review and communications processes. Finally, he had to cope with shifting budget priorities and changing perceptions of American preparedness compared to that of the Soviet Union, particularly following the launch of Sputnik 1, the first artificial Earth-orbiting satellite, by the Soviets in 1957. Schriever accepted that in taking on the appointment at WDD he was perhaps limiting his career; the discussion he had with General Power, the Chief of Staff of the Air Force, in July 1954 was commented on at some length by a close colleague, Colonel Vince Ford.[101] Evidently, however, he felt any sacrifices were worth the inherent penalty.

It was during this period that Schriever also became directly involved with exploring the utility of military space systems. The US Army and Navy had both been considering their potential since 1945, and Schriever would have been aware of the content of the RAND Report of 1946 that had outlined their likely roles, and the subsequent work commissioned by the USAF.[102] On 15 February 1956, one of the consequences of that work was that responsibility for the USAF satellite programme, previously vested in the Wright Air Development Center (WADC) at

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systems' division in 1961 within Air Force Systems Command, which Schriever had risen to command at that time.

[101] Evidence for this interview can be found in the Schriever/Ford manuscript, which will be described in detail in Chapter 3 of this thesis.

[102] The original RAND report was published as Project Rand, *Preliminary design of an experimental world-circling spaceship* (Santa Monica, CA: Project Rand, within the Douglas Aircraft Company Engineering Division, 1946) There were numerous follow-on reports, commissioned by the USAF from the RAND Organisation in the intervening years.

Wright-Paterson AFB in Dayton, Ohio,[103] was transferred to WDD.[104] The principal focus of this work was the launch of a reconnaissance satellite as soon as possible. Presented to the public as 'Discoverer', a series of scientific satellites to characterise the upper atmosphere and perform other aspects of basic research, the program could actually trace its origins to the DPO for reconnaissance that Schriever had promoted while working at the Pentagon. Also in hand, and in fact orbited before a successful Discoverer mission was achieved, was Project SCORE, an early communications satellite. Both programmes relied on technology developed by WDD for launch – the SCORE vehicle was in fact an ATLAS missile airframe launched into orbit in its entirety. Thus Schriever played a key role in the earliest US military space systems. When Discoverer at last achieved the most challenging part of its rather tortuous development, the safe recovery of a capsule ejected by the orbiting reconnaissance system, the payload exposed to public view was a US flag that had been successfully launched and recovered. With some fanfare (as part of the public explanation of the mission), the flag was taken to the White House and presented to President Eisenhower. The scene was photographed, but whether by accident or design, no extant photograph seems to show the whole grouping around the President; nonetheless, standing close behind the President when the flag was presented was Major General Schriever.

Air Force System Command

In 1959, with Atlas entering service, and with two more missile systems initiated by Schriever – Titan and Minuteman – in development, he was promoted to the rank of Lieutenant General and given command of WDD's parent organisation, AFBMD. He guided it through a major reorganisation that he himself initiated, and was rewarded by his final promotion to the rank of (4-star) General to command one of its fruits, Air Force Systems Command (AFSC). This gave him wider management responsibilities; after 8 years of almost exclusive involvement in space and missile systems, he took responsibility for the management of aircraft procurement for the USAF too. Between 1961 and 1966, the bumpy paths of the C-5A transport aircraft and F-111 multi-role fast-jet aircraft into service fell under his remit. At the same time, however, he retained his involvement in space and missile systems development. The USAF had for some time been lobbying and

[103] The same location and essentially the same organisation as Schriever had served at in 1940.
[104] John T. Greenwood, *Space and Missile Systems Organization : a chronology, 1954-1979* (AFSC historical publication Washington, D.C.: Air Force Chief of Staff, History Office, 1979) 34 Significantly, the WADC retained responsibility for the X-15 experimental rocket-powered aircraft, and would similarly control the development of the X-20 Dyna-Soar program.

manoeuvring for a coherent military manned spaceflight programme, and Schriever had been involved with it throughout. Early proposals for an overtly military manned spaceflight programme, referred to variously as 'Military Man in Space' (MMIS), and 'Man in Space Soonest' (MISS) had come to naught, and the transformation of the National Advisory Committee on Aeronautics (NACA) into the National Aeronautics and Space Administration (NASA), and parallel creation of the Defence Advanced Research Program Agency (DARPA) had frustrated USAF ambitions in this area. NASA still relied heavily on all the Services for its own programmes; Mercury capsules were launched atop (Army) Redstone and (Air Force) Atlas launchers, each derived from the equivalent missile. Gemini capsules similarly relied on (Air Force) Titan-derived launchers; it was not until the Apollo program that NASA launched a manned capsule atop a non-missile derived launcher, the Saturn – and even that was developed by a team originating from the US Army missile programme. The US Navy played a critical role in recovery of all NASA astronauts when they returned to 'splash-down' in the ocean, and the four Services supplied the bulk of the astronaut corps, most of which were former military test pilots.

While Schriever was commanding AFSC, several further attempts at a military manned spaceflight programme were undertaken, though only the last of these involved him directly. This was the attempt to develop an early 'space station' known as the 'Manned Orbiting Laboratory' (MOL); Schriever himself was the MOL project director in the last 2 years of his service career.[105]

[105] Since these programmes provide a very clear insight into Schriever's view of the importance of military manned spaceflight, their natures are described in more detail in Chapter 5, and specifically at pp 122-24.

Retirement

In 1966, Schriever retired from the USAF. He had commanded at 4-star rank, and the only logical further appointment for him would have been as Chief of Staff of the Air Force, but with no combat experience in South-East Asia and no experience on jet-powered aircraft he would have been a surprise choice. His retirement from Air Force service did not mark the end of his working life, however. Some of his ventures were commercial; he established a Washington-DC based consultancy firm with an Air Force colleague, General Fulton McKee, and served as an adviser to many corporate bodies. Others were in the public sector; among them, he advised the Department of Transportation on the regulation of civil air transport, he served on (President Nixon's) Presidential Advisory Council on Management Improvement in the early 1970s, was involved in President Reagan's election campaign and subsequently his transition to the presidency, and under President Reagan served on various advisory bodies relating to the Strategic Defence Initiative, foreign intelligence and defence management and policy.[106]

He also began to garner honours marking his career achievements; he was awarded an honorary doctorate by Utah State University in 1995.[107] The United States Air Force Academy at Colorado Springs endowed a chair in Space Systems Engineering in his honour in 2005. He was elected an honorary fellow of the American Institute of Astronautics and Aeronautics and a member of the National Academy of Engineering. He received a 'Lifetime Achievement' award from the Air Force Association in 2003, and was honoured three times by the Air University at Maxwell AFB, Alabama within its 'Gathering of Eagles' programme.[108] He was inducted into the National Aviation Hall of Fame in 1980. The Headquarters of the USAF found its own unique way to honour Schriever; in 1998, Falcon AFB, Colorado was renamed Schriever AFB. This was the first (and only as of 2012) time that a USAF base has been named for a living serviceman, and Schriever had the unique experience of attending its dedication ceremony.[109] Falcon AFB was at

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[106] See Sheehan, *A Fiery Peace* 471 for a description of some of Schriever's retirement activities. Further details can be deduced from the chronology and container titles in Kells et al., *BAS Papers; Finding Aid* , especially pages 3-4 and 20-34.

[107] The award by Utah State was probably in recognition of the relationship between the US military space community and the Space Dynamics Laboratory created at Utah State in the wake of US experiments with V2 missiles following World War 2. Schriever had also been awarded an honorary degree by Rider University in New Jersey in 1958, while still serving in the USAF.

[108] In 1987, 1997 and 2001.

[109] Some cite the naming in 1927 of Wright Field (later Wright-Patterson AFB) in Ohio for the Wright Brothers, one of whom was still alive at the time of the naming, as a counter example, but neither brother had ever served in any branch of military service, and no attempts seems to have been made to link the dedication with the surviving brother.

the time a relatively ‘young’ base, having been established in 1983 and supporting only space-related units throughout its history.[110] His final years passed in this general aura of recognition and gratitude from the USAF for his career endeavours. He maintained close links with his former colleagues; the body known as ‘Schriever’s Old-Timers’ met regularly, with close support and encouragement from the senior leadership of the USAF. Its last formal gathering appears to have been its 15th Anniversary reunion at Bolling AFB in Washington DC held 23-27 April 2003. Among those present to honour Schriever and his colleagues were the Chairman of the Joint Chiefs, the Chief of Staff of the Air Force, the Commanders of Air Force Space Command and Materiel Command, and of US Strategic Command.[111] His death, aged 94 in June 2005, led to the funeral honours noted at the start of this chapter.

Schriever’s life spanned the 20th Century and the rise of air and space capability and power. From respectable but modest origins in Germany, he made a new life in the United States. His military career up to 1945, while honourable, would not have suggested the respect he eventually gained, though his technical qualifications might have been thought noteworthy. Rather, his renown arose from its latter half. In the next chapter, this thesis looks at how other scholars and writers have gauged his importance and why. Suffice it for now to note that for the remainder of this study we will concentrate on his contribution to the development of space and missile capability within the USAF. Plainly his part was significant, but to do him justice, we should establish why.

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110 See Randolph J. Saunders and Meredith Cooper, *Master of the Sky to Master of Space: A Brief History of the 50th Space Wing* (Schriever AFB, Colorado: 50SW History Office, 2011) Appendix 7 for the history of the base and its renaming.

111 Amburgey, 'Space and Missile Pioneers Meet: General Schriever's Old Timers Reunion', (*Association of Air Force Missileers Newsletter*)

CHAPTER 3 – LITERATURE, SOURCES AND THEIR PROVENANCE

Students of the history of the early US Space and Missiles programme cannot claim to be short of material to study; many of the key players realised the seminal nature of their work and took steps to record parts of it, institutional historians were established in key departments, and the longevity of many of the principals allowed relatively generous recording of their outlooks and memories. Yet there are challenges in this field of study, and whether for these reasons or otherwise, it is the author's contention that there is a significant gap in the analysis of Schriever and his motivation. This chapter firstly surveys the existing academic analysis and popular accounts of Schriever's life and work. The limitations of such sources will be highlighted, in order to indicate where further work might be profitable. It is followed by an examination of the provenance of the Schriever Archive at the Library of Congress, in order to lay a foundation for the analysis of Schriever's thoughts that it contains. In particular, it examines the three book-length manuscripts found there to establish their authorship and Schriever's relationship with each.

Categorisation of Existing Analysis

In his lifetime, Schriever's work attracted interest for at least three reasons. The most obvious was the interest generated by the end product – delivery of an operational ICBM system. The histories deriving from this period form a major part of the technical underpinnings of the copious literature on nuclear deterrence, as well as documenting the various engineering and scientific challenges and breakthroughs faced and achieved by missile developers. A second strand of literature covers the management techniques employed by Schriever to marshal the resources placed at his disposal during his work developing ICBMs, and in particular his time at the head of the Western Development Division. The context of this literature is the emergence of Systems Engineering as a distinct discipline within engineering, a process promoted by increasing complexity, particularly of military systems. Within this analysis, Schriever's proponency of 'concurrency' is thought notable, and is consequently the subject of much study. Thirdly, there are straightforward biographies of Schriever, some written as matters of scholarly record, others with more popular aspirations. As will be demonstrated, there are still gaps in his life-story; filling some of the gaps in this area is one goal of this thesis.

The Histories of the ICBM Program

The ICBM was developed in the United States within a severely compressed timeline, driven by competition between the United States and the USSR.[112] Notwithstanding the original analysis of space systems and their utility undertaken in the immediate post-World War 2 period by the RAND Corporation and the related work undertaken on behalf of the US Navy, the driver for the development of systems such as Atlas and its immediate successors, was missile-related, specifically the dominant wish to deliver thermo-nuclear warheads at inter-continental ranges with acceptable levels of accuracy. The connection between achievement of that capability and gaining access to space was, however, evident. Thus early literature both sought to explain to the American public what a missile capability would confer and to describe the systems being developed to achieve it. Each emerging system was presented to the general public through a variety of official statements and announcements, but each also led to the writing of books detailing the programmes, from both a policy/strategy perspective and also from the standpoint of technical history.[113] Thus, for example, by 1960, the Atlas missile had been described in detail by John L Chapman, an American journalist and wartime member of the US AAC.[114]

Chapman's book was written for a general audience, and thus eschews scholarly conventions of footnoting and referencing. It was written, however, to get the story of the missile into the public domain as rapidly as possible – the Preface is dated 'August 1959', about the same time as the first detachment of Atlas missiles achieved operational status.[115] Chapman acknowledges significant assistance from the Convair Corporation in the preparation of his work, and this in itself gives it some value; gaps in corporate records and archives form a significant lacuna in early spaceflight and missile history. It also, however, must raise questions as to its impartiality and objectivity. The core of his work is a journalist's

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[112] No attempt will be made within this thesis to describe the development of ICBM or space-flight capability in the USSR or any other nation. Thus description of an event or sequence of events here should not be taken to imply primacy in the field; plainly, the USSR was the first nation to demonstrate achievement of earth orbit and of manned spaceflight, in 1957 and 1961 respectively.

[113] This thesis will look specifically at two works, one on the Atlas missile, the other on Thor. There was also a popular history of the Minuteman missile, though Titan does not seem to have been described until much later in its service life.

[114] John L. Chapman, *Atlas : the story of a missile* (New York: Harper Brothers, 1960). The end-matter notes Chapman's service in the US AAC in a bomber squadron in India and China.

[115] Chapman, *Atlas* xii, 144. There are conflicting dates in the literature for the activation of the first Atlas missile detachment, but it is plain that Chapman's book was published at about the same time.

linear narrative of the Atlas programme. It begins with an explanatory section on the nature of an ICBM, its flight profile and the principles behind rocket propulsion. It then summarises the immediate post-war proposals for missile development arising out of captured German technology, the significance of the development of thermo-nuclear payloads and the various technical obstacles encountered and overcome along the way. It concludes by discussing the utility of Atlas as a satellite launcher, describing its use in Project SCORE, the early communications satellite mentioned previously and its (then) forthcoming use as a launcher for the orbital flights within the NASA Mercury manned spaceflight programme. Thus it served to underscore the connection in the USA between missile technology and spaceflight, though by 1959 the Soviet Union had already demonstrated the general principle beyond any conceivable doubt. It acknowledges Schriever's contribution to the development of Atlas, noting his early proponency of ballistic missiles, though also his previous realisation that until the thermo-nuclear breakthrough of 1952-3, air-breathing and rocket propelled cruise missiles offered a more promising line of development.[116] It also notes his involvement in the decision to impose external management on the Atlas project.[117] At the time of the book's writing, Schriever was heavily engaged in the early 'Discoverer' missions, and he is not noted in the acknowledgements for contributing directly to the text; considerations of security would probably also have intruded in any such attempt.[118] The principal source employed by Chapman appears to be Karel ('Charlie') Bossart of the Convair Corporation, though the credit extended to Colonel B L Boatman USAF, who was closely associated with Schriever, is also noteworthy.[119]

In the following year (1961), an equivalent book describing the Thor missile gained endorsement by Schriever when he contributed the Foreword.[120] Entitled 'The Mighty Thor: Missile in Readiness', this too was written by a journalist – Julian Hartt.[121] Schriever highlights what he saw as important in his brief foreword; Thor had been developed in an even more compressed timescale than Atlas, and like Atlas it had contributed significantly to early spaceflight, being the first launcher

[116] Chapman, *Atlas* 59, 71
[117] Chapman, *Atlas* 74-5
[118] A total of 7 Discoverer launches took place between January and August 1959
[119] Chapman, *Atlas* Acknowledgements, 185
[120] Julian Norris Hartt, *The mighty Thor : missile in readiness* (New York: Duell, Sloan and Pearce, 1961)
[121] Again written for a popular readership, this volume omits both references, and an index.

employed at the USAF launch facility at Vandenberg AFB, California, and the first to lift a US payload into a polar orbit.[122] As he notes:

> The story of Thor is a story of 'firsts'...[details of Vandenberg and polar orbit]…First long range missile in the Free World to record one hundred launchings. The story of Thor is more- it is a story of space. Thor has earned the title of 'Workhorse of the Space Age'.[123]

Hartt also makes early reference to Schriever's use of concurrency in project management.[124] This is not, however, a detailed analysis of the concept, and there are better accounts elsewhere.

After these first popular volumes, academia sought to catch up. The first thesis concentrating on the development of ICBMs was submitted by Claude Johns at the University of North Carolina at Chapel Hill in 1964.[125] It opens with a conventional history of early ICBM development: the achievements of Germany during World War 2, the early service proposals for long range missiles in the immediate post-war period and the importance of the development of thermo-nuclear warheads in allowing ICBM development to start in earnest. It posits three principle architects of the early US ICBM programme – John von Neumann, via the work of the Scientific Advisory Board panel described in Chapter 2, Trevor Gardner, who was the Secretary of the Air Force's Special Assistant for Research and Development when Atlas development began in earnest and was later appointed Assistant Secretary of the Air Force, and lastly Bernard Schriever.[126] Johns concentrates his analysis on two aspects of the subsequent effort: the organisation of the various committees that oversaw ICBM development, and Schriever's concept of concurrency.

The chapter on committees focuses on Gardner's work, but that on USAF organisation understandably concentrates on Schriever and on concurrency. There are other works that explain the detail of concurrency as envisaged by Schriever, but for now it is sufficient to note that Johns provided a very early attribution of the technique to him.

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[122] These latter achievements are in fact related, and bear on military spaceflight applications. A polar orbit – one where the satellite passes over both North and South poles of the Earth – is essential for a mission overflying all areas of the Earth's surface, and this in turn is a common requirement for reconnaissance satellites. Launches to achieve such orbits are constrained in populated areas by the need for an unobstructed north or south facing path over water or similar unpopulated terrain, which was the USAF motivation for developing Vandenberg AFB on the Californian coast as a launch site.
[123] Hartt, *The mighty Thor* xi
[124] Hartt, *The mighty Thor* 52-3
[125] Claude J. Johns, 'The United States Air Force intercontinental ballistic missile program, 1954-1959: technological change and organizational innovation', (University of North Carolina, Chapel Hill, NC 1964 PhD),
[126] Johns, 'The USAF ICBM program', 39

Johns also describes the quartet of Air Force organisations that between them developed Atlas: the Ballistic Missile Division (BMD) of Air Research and Development Command (commanded by Schriever as the WDD in 1954-57, and latterly under the BMD title), the Ballistic Missile Center (BMC) – sometimes confusingly referred to as the Ballistic Missile Office (BMO), but in either case a component of Air Materiel Command, from which ARDC had been 'spun off', 'SAC-Mike', a component of Strategic Air Command (SAC) responsible for developing operational doctrine and strategy for ICBM employment, and the Space Technology Laboratories (STL).[127] This last organisation was established to assist with oversight of the development program, since it was felt that Convair, as prime contractor for the Atlas airframe, lacked the managerial experience and ability to supervise such a large project. This decision ultimately drew censure on Schriever, since STL was a division of the Ramo-Wooldridge Corporation, which was involved in aerospace engineering. This was felt to confer an unfair advantage on Ramo-Wooldridge, by giving them an oversight role in a program they would later be able to participate in as a supplier. Johns outlines alternative supervisory arrangements that could have been employed, but in the end concludes that there were certainly strong grounds for making the decisions taken and some for believing that they were in effect the only practical ones.

Overall, Johns' achievement was in being the first to place the management of the ICBM and its development in the academic domain. Working at the time he did, it is deeply regrettable that he did not interview the protagonists to any significant extent, citing time constraints and travel costs for this omission rather than any considerations of classification or security.[128] His conclusions do not dwell on any particular contribution of Schriever, instead simply noting the strategic imperatives consequent on the Soviet's demonstration of ICBM capability before that of the USA. They can also be criticized for introducing material not supported anywhere else in the thesis – in his final chapter he notes 'military conservatism' consequent on the introduction of ICBMs into service and their impact on established military roles. His observations may be valid, and are certainly interesting, but do not draw on any of the preceding analysis or sources cited. His work constitutes a valuable first step in recording the history of the ICBM systematically, and of highlighting Schriever's role, but the critique of Schriever's motives must be sought elsewhere.

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[127] Johns, 'The USAF ICBM program', 53-62
[128] Johns, 'The USAF ICBM program', 12

There are other composite histories of the USAF missile program. In 1964 (at the same time as Johns was submitting his thesis at UNC), the official historian of Air Force Systems Command (AFSC), Dr Ernest Schwiebert, was writing a contemporary history of the ICBM program.[129] This work mixes interviews with authored chapters, and includes an interview transcript between Schriever and John F Loosbrock, an aviation journalist and commentator. In the interview, Schriever validates the contributions of von Neumann and Gardner that Johns had identified.[130] He also notes the changing strategic imperatives evident even at the time of the interview. He connects the fact that the first-generation ICBMs on the US side had already been supplanted by second-generation systems such as Minuteman with an enduring western superiority over the USSR, but notes that this must be maintained through active development efforts, and that the diminishing prospect of a nuclear exchange between the great powers did not in itself preclude regional conflict.[131] The bulk of the text is a comprehensive history of the development of the ICBM, with von Neumann's and Gardner's contributions highlighted. Separate chapters dwell on management and organisational aspects, and the denouement of the STL/Ramo-Wooldridge issue alluded to above, in which Ramo-Wooldridge became 'the Aerospace Corporation' (a non-profit entity).[132] Critical opinion of the work is divided. John Lonnquest, whose thesis will be considered shortly, describes it as being 'of questionable reliability', but Davis Dyer, in his history of TRW describes it as 'authoritative'; frustratingly (particularly in Lonnquest's case), neither substantiates their critique with examples or justification.[133]

Lastly in the canon of ICBM histories, mention must be made of the work of Jacob Neufeld. Neufeld worked as an official historian within the US Air Force History Support Office in Washington DC for many years. He knew Schriever well, and was instrumental in the securing of the Schriever Archive now deposited at the

[129] Schwiebert, *A history of the U.S. Air Force ballistic missiles* . AFSC was commanded at this time by General Schriever.
[130] John F Loosbrock, 'A look back - a look ahead: An interview with General Bernard A Schriever', in Ernest G. Schwiebert (ed.), *A History of the US Air Force Ballistic Missiles* (New York, NY: Frederick A Praeger, 1964).
[131] Schriever cites the continuing importance of tactical air power and the need for global mobility (at the time when AFSC was, under his direction, managing the development of the F/FB-111 and C-5 aircraft for those respective roles for the USAF) without mentioning 'Vietnam' or 'SE Asia' at any stage. He relies rather on mention of Korea; this seems a noteworthy omission in an interview given in 1964.
[132] Claude Witze, 'The USAF Missile Program: A Management Milestone', in Ernest G. Schwiebert (ed.), *A History of the USAF Ballistic Missiles* (New York: Praeger, 1965), 167-83.
[133] See John C. Lonnquest, 'The face of Atlas : General Bernard Schriever and the development of the Atlas intercontinental ballistic missile, 1953-1960', (Duke University, Durham, NC 1996 PhD), 288 and Davis Dyer, *TRW : pioneering technology and innovation since 1900* (Boston: Harvard Business School Press, 1998) 441

Library of Congress. He wrote several works relating to Schriever, including journal articles, book chapters and longer works. His contribution to the descriptions of the ICBM program was developed between 1976 and 1989.[134] It deliberately restricts its scope to the Atlas, Titan and Thor programmes in the timescales stated in order to allow a suitable depth of coverage.

Neufeld had direct assistance from Schriever, and from Colonel Vince Ford, who worked closely with Schriever, in writing his history.[135] It begins by noting the early interest in pilotless aircraft, precision (free-fall) munitions and variants of the cruise missile within the US armed forces before recapping the origins of liquid-fuelled rocketry in the USA. It provides a detailed account of the original attempt to initiate a ballistic missile programme in the 1940s, before attention turned to developing nuclear cruise missiles. Once again, it identifies von Neumann, Gardner and Schriever as the founding fathers of the ICBM program. It provides clear detail of the impact that successive funding cutbacks and restorations had on the ICBM programme, and of the power struggle between the US Army and Air Force for primacy in the use of missiles in various forms. It relates the potential conflict between development of Atlas as an ICBM and Thor as an IRBM (implying a difference in range), and of Schriever's concerns that IRBM production would delay the rapid development of ICBMs. It is thus a comprehensive technical history, written by a knowledgeable author with good access to primary material. John Lonnquest summarises it as a missed opportunity, noting that Neufeld "studiously avoided many of the critical issues…skirted the subject of concurrency, avoided …Ramo-Wooldridge's controversial role, glossed over the all-important political dimension and did not adequately develop Schriever's character".[136] While Neufeld does indeed omit or minimize coverage of the issues cited, this is harsh criticism; his work stands on its merits as a technical history, makes no unfulfilled claims, and as will be shown, Neufeld dealt elsewhere with Schriever's character and personality.

Schriever and Concurrency

The second route into study of Schriever and his work is through his exploitation of concurrency. This challenge to contemporary wisdom regarding the management of complex programmes is widely associated with Schriever, and

[134] Jacob Neufeld, *The development of ballistic missiles in the United States Air Force, 1945-1960* (Washington, D.C.: Office of Air Force History, United States Air Force, 1989)
[135] Neufeld, *Ballistic Missiles* vi
[136] Lonnquest, 'The face of Atlas', 5-6.

consequently needs some explanation. At its simplest, concurrency is the superposition of the research and development, testing, and production phases of a project. Where applicable, it can also encompass the training of operators, construction of ground facilities and installations and development of operational doctrine and plans, or in other words, the incorporation of non-materiel as well as materiel factors. In the literature, it is identified as an American innovation.

One early example is held to be the development of the depth charge by the US Navy during World War 1. This exemplar is covered in some detail by Captain Wayne Foote's MSc thesis, submitted to the Air Force Institute of Technology at Wright-Patterson AFB in 1986. Foote's definition of concurrency notes that the term itself was not used initially, but that the essential characteristics of what was then a 'crash program' were:

> A significant threat demanding rapid response, great latitude in managing the crash program, unrestricted resources to draw upon and very close cooperation between the Program Office and highly motivated contractors.[137]

Foote then charts the development of the concept during World War 2, including the differences between applying it to a 'conventional' challenge, such as aircraft procurement, and to programs where the weapons systems itself is innovative, as in the case of the Manhattan Project to develop nuclear weapons.[138] He finally makes the connection between these programs and Schriever's work developing Atlas, crediting him with inventing the term 'concurrency' in 1958.

Another author, G Harry Stine, also credits Schriever with pioneering work in concurrency, which he connects with a report entitled 'Combat Ready Aircraft', compiled by Schriever in 1950-51 when he was serving as Deputy Chief of Staff for Development at USAF HQ.[139] In this report, Schriever addressed problems the USAF was then experiencing with the introduction into service of new combat aircraft. In some cases (Schriever does not state which types he is referring to),

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[137] Wayne C Foote, 'History of Concurrency: The Controversy of Military Acquisition Program Schedule Compression', (Air Force Institute of Technology, Wright-Patterson AFB, Dayton, Ohio 1986 MSc in Logistics Management), 31.

[138] Foote cites the freedom from conventional management constraints as the most 'concurrent' aspect of the Manhattan Project. In a later work compiled for the Congressional Budget Office, G Wayne Glass (who cites Foote) notes the simultaneous experimentation, development and production aspects as being the essential feature from a concurrency point of view. See G. Wayne Glass, *Concurrent weapons development and production* (CBO study. Washington, D.C.: Congress of the U.S., Congressional Budget Office, 1988) 20.

[139] G. Harry Stine, *ICBM : the making of the weapon that changed the world* (New York: Orion Books, 1991) 172. USAF DCS/Development (Bernard Schriever) (ed.), (1951), *Combat Ready Aircraft*, *Demler Collection*; (Maxwell AFB, AL).

‘crash’ production of new types had led to their introduction to front-line units before initial design problems had been resolved. These were remedied (expensively) by modification, sometimes requiring return of the aircraft to the factory. Schriever’s report is notable for two reasons; firstly it is the earliest instance in USAF analysis where the development not just of hardware, but also of support infrastructure, and the training of personnel to operate the new system are regarded as multiple facets of one problem.[140] Secondly, in the case of hardware, it notes the need to deal equally with the design, test and in-service engineering staffs and their problems. In tones reminiscent of a discussion of Clausewitzian friction, it concedes that no system is ever likely to be perfectly conceived and delivered, but that while endless test cycles are potentially as undesirable as front-line modification of defective systems, it is more likely than not that problems will need to be addressed at some stage.

> [concurrency] could delay delivery of the aircraft to combat units if the new design were found to be technically faultless and tests uncovered no functional deficiencies. This is an extremely unlikely condition. The reverse is probably more apt to occur.[141]

Just as with the histories of the missiles themselves, concurrency, and specifically Schriever’s concept of it, has been the subject of substantial study in academia.

The first doctoral thesis to address the issue fully was submitted at Duke University in North Carolina by John Lonnquest in 1996.[142] To this day, it is still the thesis providing the most personal and detailed treatment of Schriever and his work. Lonnquest begins by outlining the history of the USAF ballistic missile programme, starting with the initial studies in 1946, proceeding to the rejuvenated programme of the early 1950s, and from there moves to the thermo-nuclear breakthrough of 1953 and Schriever’s appointment to command the Western Development Division. In a more detailed fashion than previously mentioned sources, he relates the relationships between Gardner, Schriever and von Neumann. He highlights as pivotal Gardner’s nomination in 1953 to the newly created (and non-statutory) post of Special Assistant to the Secretary of the Air Force for Research and Development by Harold Talbott, the incoming Secretary of the Air Force in the first Eisenhower administration. Gardner was then introduced to Schriever by Col Vince Ford. Von Neumann was induced to confirm the

[140] In other words, incorporation of factors lying in ‘concept space’ as well as in ‘technology space’, as differentiated in Chapter 1.
[141] USAF DCS/Development (Bernard Schriever), 'Combat Ready Aircraft', 19.
[142] Lonnquest, 'The face of Atlas',

estimates of size and weight for a thermonuclear warhead that had made the ICBM possible via a meeting at Princeton with Schriever on 8th May 1953.[143] After describing the establishment of the Western Development Division of Air Research and Development Command under Schriever's leadership, he details the alternatives to the Convair/Ramo-Wooldridge structure that were considered and rejected. He devotes a chapter to the history of Ramo-Wooldridge before turning to the main subject of his analysis; Schriever and concurrency.[144]

Lonnquest begins this portion of his work by noting that Schriever was already severely constrained by timescales and deadlines when he accepted management of the Atlas missile programme; delivery of an operational system was required by 1960. Thus the first criteria for use of concurrency – a 'crash' timescale – had already been met. Simple calculation, working backwards from the required in-service date, mandated concurrent design, development and production of hardware. Given the extent of the infrastructure that would also be required to store, prepare and launch Atlas missiles, its construction would also need to be underway relatively early in the programme. Thus, another criterion for concurrency had been met.[145]

Lonnquest then pauses to review the history of concurrency, noting the World War 1 depth-charge example cited earlier, and instances of its use during World War 2 in aircraft production, conventional and nuclear weapons development. He notes that its prevalence reduced after World War 2, being replaced by a constrained version known as the Cooke-Craigie Plan. He then describes how Schriever combined features of this Plan with the contents of the 'Combat Ready Aircraft' study. The consequence of this was recognition of the importance of developing a weapon-system as a single entity, rather than as separate component parts.[146] In the context of the ICBM, this plainly could apply to connection of development of the thermonuclear warhead and the missile to carry it. The missing component of concurrency as understood at the time was a 'flat' management structure with the unrestricted resources identified by Foote and others. Schriever now moved to attain these.

Lonnquest notes the challenges faced by WDD, and specifically by Schriever and Gardner included competition from other projects. Being a high

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143 Lonnquest, 'The face of Atlas', 43, 47, 77. Fred Kaplan also describes the implications of this meeting. See Fred M. Kaplan, *The wizards of Armageddon* (New York: Simon and Schuster, 1983) 115

144 Lonnquest, 'The face of Atlas',Chapter VII, 144-59

145 Lonnquest, 'The face of Atlas', 161-2

146 Lonnquest, 'The face of Atlas', 172

priority in the USAF did not translate into defence-wide priority. Additionally, Schriever still needed budgetary approval from outside his organisation for major purchases; "as a result, on average it took just under nine months to get a major facilities contract approved".[147] Schriever's solution, aided and abetted in its delivery by Gardner and by some of his trusted circle of colleagues including Colonels Ford and Boatman, was multi-faceted. Ford began briefing Senator Henry 'Scoop' Jackson, who chaired the military affairs sub-committee of the Joint Committee on Atomic Energy on the delays faced by the programme. He also gained access to the Science Advisory Committee and through it the Technical Capabilities Panel, which prepared briefings for the National Security Council. Through these mechanisms, Senator Jackson's sub-committee recommended to the President that the ICBM programme be placed on a wartime footing, the State Department's interest was secured and eventually, President Eisenhower requested a briefing on the issue.[148] The briefing, on 28th July 1955, was delivered by Gardner, Schriever and von Neumann. The President appreciated the gravity of its content, and consequent to it, an NSC Directive was crafted and approved, stating in respect of the program that:

> …(3) The U.S. ICBM program is therefore a program of the highest priority above all others, except as directed by the President.
>
> (4) The Secretary of Defense will prosecute the program with all practicable speed, and all other Executive departments and agencies will assist the Department of Defense as required…[149]

With this mandate in hand, the WDD sought to secure the last piece of the jigsaw in respect of a concurrent approach to ICBMs – clearing the bureaucracy of the procurement process. On the same day as NSC Action No 1433 was signed-off, Trevor Gardner asked the USAF Deputy for Budget and Program Management, Mr Hyde Gillette, to prepare simplified accounting and management procedures to reduce delays to the ICBM. The so called 'Gillette Procedures' became a critical process by which Schriever circumvented the snail's-pace methods of allocating funds to projects. Lonnquest notes that application of them did not increase the Air Force's overall budget allotment, but rather protected the Atlas program from

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[147] Lonnquest, 'The face of Atlas', 176-7

[148] Lonnquest, 'The face of Atlas', 179-86. Lonnquest cites interviews conducted with both Schriever and Ford in his reconstruction of events. As will be shown, there are irretrievable gaps in some of the official archives relating to these meetings and briefings, so participants testimonies are in some cases the only available sources.

[149] NSC Action No 1433, (addendum to the Memorandum of Discussion at the 258th Meeting of the NSC), text available at http://www.history.state.gov/historicaldocuments/frus1955-57v19/d34#fnref4 downloaded on 22 Oct 12. (cited in the same context by Lonnquest at p191).

reallocation of funds to other projects, and allowed for approval of whole year's spending programmes as one transaction, rather than piecemeal.[150]

Lonnquest's next chapter analyses Schriever's application of the procedures outlined above, dwelling in particular on the management structures he created.[151] He then describes the initial production and testing of the Atlas missile, and the early construction of support infrastructure. He notes Schriever's insistence on making the management structures of the programme's contractors compatible with his own, down to exchanging liaison officers in the military fashion with key contractors. He notes the impact of the growth of the ICBM programme to incorporate the Titan and Minuteman missiles, and its expansion to incorporate the intermediate-range Thor; he also remarks on the expansion of the WDD to encompass space-related programmes. He says something about Schriever's personality and management style, noting the contrast between his public persona (youthful, athletic, at times heroic) and his staff's perception of him (aloof, distant, visionary but disorganised). In making this assessment, Lonnquest leans both on contemporary public accounts of him, driven in part by US public concerns about the 'missile race', in part by Schriever's realization that there was a public-relations aspect to his work, and on testimony from his contemporaries and colleagues regarding his attitude to his work.

The following chapter expands on the public-relations aspect of management, and on two specific tools employed by Schriever: the Management Control System and Program Control Room (MCS/PCR), and the Commander's Monthly Internal Management Conferences (more commonly known as 'Black Saturday' meetings). The former was a combination of extensive communications and data gathering systems, reaching out from WDD HQ to contractor's installations, and feeding elaborate displays in a secure room back at the headquarters. The latter was a related process, where senior leadership was briefed monthly on problems and proposed resolutions. As Lonnquest notes, "The AFBMD's management control system, which included the PCR and Black Saturday, supplied Schriever and his managers with the information and management review structure that enabled them to use concurrency."[152] Lonnquest then turns to analysis of concurrency as a concept, and to consideration of its overall efficiency. The first significant observation is that Schriever's concept

[150] Lonnquest, 'The face of Atlas', 199-200
[151] Lonnquest, 'The face of Atlas',Chapter X, pp 201-225
[152] Lonnquest, 'The face of Atlas', 231, citing an AFBMD response to a General Accounting Office inquiry about the management of the ICBM programme.

of a weapon system as an entity, rather than simply thinking about a piece of hardware in isolation, is an essential pre-condition; the former is what gives the manager separate strands of work in infrastructure, training, command and control and suchlike to develop concurrently. He credits Schriever with first using the term in early 1955, though not with originating it personally, holding however that it originated in AFBMD.[153] The AFBMD view of what it consisted of was that

> …concurrency was not a single, unified management approach. Concurrency was only one element of the AFBMD's management approach. In order for concurrency to be effective, the program management office also needed a high degree of centralized authority, adequate resources and a talented staff. Concurrency did not exist separately, it was the sum of its parts.[154]

Lonnquest credits Schriever with this narrow understanding of concurrency too – the overlapping of the development and production processes but not the other contributing factors. This simple explanation served Schriever's public relations purposes, in that it gave external stakeholders, such as members of Congress, quick reassurance that an effective management process was in place, even though the technical explanation of what was being managed was beyond their grasp. Internally, its radical differentiation from other USAF processes reminded uninvolved colleagues of the ICBM programme's privileged status.

Concurrency also courted controversy when costs were examined. As Lonnquest notes, "The commonly shared sentiment among senior officials was that concurrency did indeed save time, but at a frightful cost. In 1961, Lt Gen Roscoe Wilson…described concurrency as 'useful but very wasteful' and said that the Air Force could only afford one or two such programs."[155] Schriever held the opposing view: that concurrency saved both time and money. It did this, he held, by reducing overheads incurred during the shortened development cycle, and by delivering the weapon system sooner, which allowed it to achieve a longer in-service life, and thus improved its overall value for money. Lonnquest finds these assertions unproved; the evidence for concurrency's effectiveness in reducing development time is comprehensive and conclusive, but the value for money assertion is both dubious and un-provable, not least because of the subsequent destruction of many of the AFBMD financial records.[156] Schriever himself recognised the vulnerability of his claims and in 1957 asked the Air Force Inspector General to audit the AFBMD management processes. At best, they received a

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[153] Lonnquest, 'The face of Atlas', 232-33
[154] Lonnquest, 'The face of Atlas', 233
[155] Lonnquest, 'The face of Atlas', 237, citing Gen Wilson's testimony before Congress.
[156] Lonnquest, 'The face of Atlas', 241-43

‘qualified pass’, with praise for the management information aspects, but with censure for serious flaws within budget control and the management of contracts.

Lonnquest’s final substantive chapter tells the story of the conclusion of Atlas development in the historical context of the early space-age, Atlas’ relatively brief in-service life, Schriever’s further career and his attempts to perpetuate aspects of concurrency. In terms of missile development, one of the most important implications of the launch of Sputnik 1 in 1957 was the credibility it leant to Soviet assertions that they had a functioning ICBM in service, a claim not previously accepted by the USA. At that point, Schriever conducted a further review of the Atlas programme to see if it could be accelerated in any way. Perhaps he would have been disappointed if any easy gains had emerged, but none were, in fact, forthcoming. The ensuing public controversy about ‘the missile gap’ is well recorded elsewhere, and Lonnquest dwells on Schriever’s public testimony which became caught up with the 1960 presidential election, where candidates Nixon and Kennedy traded assertions about it, based on partial and selective access to relevant information. He also expands on the questions asked about TRW’s involvement in Atlas, which finally played out in front of several Congressional investigations in 1958-9.

Atlas came into service in 1959. Lonnquest, having described the various test flights leading up to this milestone, records Atlas Series D being placed on alert in 1959, but rapidly being supplanted by Atlas-E (improved guidance system) and Atlas-F (capable of being located in and launched from above silos), before being withdrawn entirely by the end of 1965 as the Titan and Minuteman systems matured. In keeping with the weapons-system view of Atlas, he notes also the infrastructure implications of moving from Atlas-D, which operated unprotected from an open launch-structure, through Atlas-E, which was stored in a ‘semi-hardened’ bunkers but launched from an upright, exposed position, to the silo-capable (for storage) Atlas-F.

As Lonnquest notes, Schriever’s career progressed as the Atlas programme matured. In April 1959 he assumed command of ARDC and in 1961, he moved on again to command AFSC in four-star rank, the post he held until his retirement in 1966. In 1960, Lonnquest notes that Schriever was on the receiving end of Air Staff criticism of the Gillette procedures he had employed at WDD. These had excluded the Air Staff almost *in-toto* from traditional oversight of the ICBM programme, and Schriever had to watch while they dismantled them. Lonnquest now notes that Schriever claimed they were an essential element of concurrency, ("Despite Schriever's protests that the Gillette procedures were an essential ingredient of concurrency…")[157]. This sits uneasily with his previous narrow definition of concurrency as overlapping development and production only. He also had to contend with opposition to the 'weapons system' doctrine; AFSC – Schriever's new command – and its predecessor Air Materiel Command (AMC) – had traditionally overseen support functions for all in-service equipment, while other organisations had had responsibility for development. Re-arranging 'vertically', by system, instead of 'horizontally' by function was a challenge to the existence of AFSC and AMC, with all the vested interests that could be expected already attached. Schriever participated in a panel review to reconcile this conflict. As might be expected, he took the 'systems' view, in opposition to the Commander of AMC, who embraced a functional view. Also perhaps unsurprisingly, a compromise emerged, brokered by the third member of the panel. It was implemented in the 1960-63 period, initially as the 'designated systems approach', later as the '375-series regulations'. Lonnquest (and Schriever) feel, (and felt), that these failed, lacking two key elements of the Concurrency/Gillette process. Firstly, although empowered compared to their predecessors, programme managers lacked the almost total autonomy that Schriever had enjoyed, leading to an increasing number of decisions being referred upwards for approval. Secondly, the new procedures did not allow the bulk-approval of budget decisions, so the system struggled to keep pace with the piecemeal allocation of funds. By 1962-3 even Schriever noted that he had been unable to replicate the WDD model on a widespread basis.[158] This is the basis for Lonnquest's concluding assertion that the concurrency model, defined narrowly, is not the overall reason for Schriever's success in developing Atlas. Rather it was the consequence of a unique and unrepeatable combination of circumstances. Since it is not clear that Schriever himself was totally committed to the narrow definition of concurrency (his retrospective defence of the Gillette procedures being a case in point), this is not the only interpretation that can be laid here. Another interpretation would be that concurrency broadly understood – including 'crash' priority, is not sustainable in routine circumstances, when not all programmes can be afforded such status.

Lonnquest's thesis remains the most complete in its treatment of Schriever and the ICBM programme. Based on comprehensive access to informed individuals it offers genuine insight on Schriever's personality. It provides a

[157] Lonnquest, 'The face of Atlas', 274
[158] See Bernard Schriever, 'The Role of Management in Technological Conflict', *Air University Quarterly,* Vol XIV No 1 and 2 1962-3), and particularly pp 24-5.

comprehensible explanation of concurrency for the non-specialist and avoids the entanglements of detailed technical treatments of missile engineering. It is thus an invaluable source for the Schriever scholar. However, for understandable reasons, not least of length and scope, it omits consideration of large parts of Schriever's work. Within these limitations, it is immensely useful, but it still left gaps for further study.

Stephen Johnson wrote his thesis from the standpoint of a professionally-qualified engineer turning to writing a history of project management within highly-complex systems.[159] He thus has a particular interest in the sociology of relationships between factions involved in development work, and in the management techniques appropriate to different projects. His thesis, submitted at the University of Minnesota in 1997, is a comparative analysis of the management of the ICBM programme under Schriever, the management methods adopted within the NASA manned spaceflight programme and those of the European organisations ELDO and ESRO which attempted to develop a European launcher and satellite capability prior to the establishment of the European Space Agency.[160] Johnson describes his intent as being threefold: "the extension of the discussion of executive and managerial control from office...and shop floor workers in manufacturing to scientists, engineers and R&D, the extension of discussion of industrial research and organisation into the cold-war period, and the aerospace industry…and [analysis of] the effect of organization and management upon the technologies that these organizations create".[161] His status as a qualified engineer gives his work unique insight into Schriever, the challenges he faced and the solutions he adopted.

Johnson considers Schriever's work specifically in two chapters of his thesis. Firstly, he makes a more comprehensive effort to identify Schriever's technical mentors than Lonnquest did. In his Chapter 3 he establishes the history of procurement within the US Forces, and specifically the Army Air Corps, from the

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[159] Stephen B. Johnson, 'Insuring the future : the development and diffusion of systems management in the American and European space programs', (University of Minnesota, Minneapolis, MN 1997 PhD),

[160] The European Launcher Development Organization (ELDO) was a collaborative venture between the United Kingdom, France, Germany and Italy, formed in 1961 and dissolved in 1974. It attempted (unsuccessfully) to develop a European three-stage satellite launcher by combining separate national projects from the United Kingdom, France and Germany. All the test flights of system failed to some extent; most were total failures. After the dissolution of ELDO, its assets were subsumed into the parallel European Satellite Research Organisation (ESRO), which had comparatively been much more successful. Re-brigaded as the European Space Agency, it went on to develop the *Ariane* series of commercial launchers under French leadership. This has, in contrast to ELDO attempts, been a resounding success. For more details, see, for example, Michael Sheehan, *The international politics of space* (New York: Routledge, 2007) Chapter 5.

[161] Johnson, 'Insuring the Future', 10-11.

first interaction with the Wright Brothers in 1908 until the end of World War 2. He highlights the technical vision of General Hap Arnold and his interaction with Theodore von Karman during World War 2, and Arnold's realization of the inevitable dependence of US Air Forces upon scientific and technical development in the future. He sees Schriever's first tangible contribution to the advancement of management technique as the introduction of Development Planning Objectives while working in the Pentagon Development Planning Office, and the harnessing of scientists as opposed to front-line commanders in defining them.[162] He also highlights the roles of Lt Gen Donald Putt USAF and Dr Louis Ridenour in establishing Air Research and Development Command (ARDC) – the organization within which Schriever would achieve most of his managerial breakthroughs. He notes the radically changed strategic outlook faced by the United States at the war's end; developments in long-range systems, particularly missiles and jet-powered aircraft meant that it would no longer be able to mobilize protected by the Atlantic and Pacific Oceans. Johnson also identifies the poor state of preparedness at the outbreak of the Korean War, which prompted Schriever's 'Combat Ready Aircraft' study and, due to the increased complexity of the new types, validated the 'weapons system' approach. He once again identifies the familiar trinity of Gardner, von Neumann and Schriever as architects of the ICBM programme and describes the establishment of the WDD and the involvement of Ramo-Wooldridge in the management function.[163]

As might be expected, given his intent on teasing out the implications of Schriever's management techniques, he identifies the impracticality of extant budgetary processes and the importance of the Gillette Procedures that supplanted them. He also situates their impact on the rest of the Air Staff in some detail. He notes the information-management importance of the PCR and 'Black Saturdays', but also their utility as public-relations tools. Finally in Chapter 3, he notes the pre-conditions he identifies as essential to Schriever's methods: the perception of an imminent national threat, and continuing technical progress within the ICBM programme.[164] In his summary, however, he notes that "the Soviets would not be quite as fearsome as Americans feared, and his [Schriever's] scientific friends would fail to deliver a technological marvel".[165]

[162] Johnson, 'Insuring the Future', 34, 75.
[163] Johnson, 'Insuring the Future', 68-89
[164] Johnson, 'Insuring the Future', 95.
[165] Johnson, 'Insuring the Future', 97.

In Chapter 4, Johnson teases out his analysis of Schriever's understanding of concurrency. In his estimation, a key feature of concurrency was duplication of technology wherever doubts about practicality arose, as well as concurrent development of components within a single system. Specifically, he is emphasising linkages with the Manhattan project, where two completely separate designs of fission weapon were developed in parallel and subsequently employed. In a similar fashion, he infers, Schriever oversaw development of Atlas and Titan missiles in an overlapping manner. This is not a particularly convincing definition; Atlas was designed with the intent of entering service as soon as possible, and consequently eschewed the technically more efficient multi-stage design of Titan. The latter was always intended to be a follow-on system, though it also provided a guarantee against a critical failure of Atlas. Johnson situates Schriever's development of concurrency within general progress in systems engineering, noting individual aspect's origins in earlier management methods, but crediting Schriever with their consistent application. He provides deeper analysis of the problems encountered with the contract management aspects of concurrency, noting that although Schriever tried to brush aside the cost overruns that resulted, he eventually had to yield to accounting scrutiny.[166] Johnson's next insight into concurrency is perhaps his most valuable – when technical problems arose in development, the implied costs of the concurrent approach became very apparent. Additionally, although such failures were not unexpected for such a technically novel and challenging enterprise, the dramatic outcomes gave critics of the project, who had been rebuffed previously by Schriever's demonstrations via the PCR of technically competent management, an easy path of attack. At the same time as these difficulties were arising, Schriever was facing the controversy relating to TRW's oversight role. Johnson's summary is that the employment of concurrency achieved the speed of development that Schriever hoped for, but that the costs were enormous and contributed to the later dismantling of many of Schriever's innovations. The difficulties encountered also highlighted the difference between pure science, relevant to establishing the theoretical basics of ICBMs, and the engineering challenges inherent in bringing them into service.[167]

Johnson's last contribution to the analysis of Schriever and concurrency is an examination of how it morphed into the 375-series regulations described by Lonnquest, and how those regulations addressed Secretary McNamara's concerns

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[166] Johnson, 'Insuring the Future', 103.
[167] Johnson, 'Insuring the Future', 107-08. He returns to the analysis of the contrasting roles of scientists and engineers in pp 116-7

about cost control. Johnson's assessment is that Schriever was responsive to McNamara's concerns, made particular efforts to train Air Force officers in the management techniques required to implement the new systems effectively and that McNamara was suitably impressed. It is the author's belief that while Schriever saw the centralising tendencies of McNamara as irksome, he had acquired wider management responsibilities with his assumption of command at AFSC, and he was forced to recognise that McNamara was implementing on a larger scale the practices of centralization and tight control of information flows that he himself had imposed on the Atlas programme a few years earlier.[168]

Other theses have covered similar ground; Schriever's status as the father of the USAF Ballistic Missile programme, and the honours accorded to him during his lifetime ensure his work is a recurring topic at the various US Staff and Warfare colleges, and also for military officers pursuing qualifications elsewhere. Since many of the related courses are accredited at Masters level, the associated theses enter the academic mainstream. Wayne Foote's work cited above was one such effort. Another is due to Captain Scott Mattson USAF, who submitted an MA thesis at Kansas State University in 1994 on Schriever and the ICBM programme.[169] Mattson's work covers similar ground to the other theses detailed, but adds some insights not found elsewhere. He favours a broad definition of 'concurrency', to include not only overlapping development and production, but also 'parallel development', which he describes as the dual sourcing of critical components from two separate suppliers. He also cites development of the Titan missile as a further expansion of parallelism, noting that it shared some components with Atlas, but provided an alternate route to the ICBM if Atlas had failed.[170] At the end of his thesis Mattson also ponders whether Schriever had a realistic chance of rising to head the USAF at the end of his tour at AFSC in 1966. His conclusion encompasses the political context of the USAF at the time, with Secretary McNamara still firmly in control of the Defense Department, employing management techniques that Schriever disagreed with to pursue goals he was uneasy with. Mattson concludes that Schriever had the latent ability to discharge the role, but would have been unsuitable to serve under McNamara. He notes, however, the significance of Schriever rising to a major command in 4-star rank on

[168] Johnson, 'Insuring the Future', 134-36.
[169] Scott D. Mattson, 'Technological innovation in the Cold War : General Bernard A. Schriever and the closing of the missile gap', (Kansas State University, Manhattan, KS 1994 MA),
[170] Mattson, 'Technological innovation', 47

the strength of technical, rather than operational performance and acumen, and the important message that sent to other like-minded souls.

Two other PhD theses are worthy of brief mention; both are histories of particular systems or concepts, rather than personal analyses, but Schriever features tangentially in both. The first is a history of the X-20 Dyna-Soar programme, which was an unsuccessful attempt by the USAF to develop initially a sub-orbital and then orbital hypersonic space-plane. Roy Houchin described the project in his PhD thesis submitted at Auburn University, Alabama in 1995.[171] He notes that the project made good technical progress, and could reasonably have been expected to succeed, but that it failed for political and economic reasons. Schriever was involved with the programme almost from its inception to its demise. Houchin draws no direct conclusions about Schriever in either of his works, though elements of Schriever's actions as he describes them, particularly where he was simultaneously progressing the ground infrastructure, the design of the platform and aspects of crew training and selection, echo his adoption of concurrency as a management tool for ICBMs.[172]

The last PhD thesis to be considered here is that of Stephen Rothstein, at the Fletcher School of Law and Diplomacy at Tufts University, Massachusetts in 2006.[173] His subject is the development of the 'aerospace' concept within the USAF. Rothstein's thesis is that ideas develop within organizations in the same way that institutions do within cultures. He illustrates and demonstrates this by taking as a case study how the concept of 'aerospace' – air and space forming a single graduated, but indivisible operating medium – arose and developed within the USAF. Again Schriever is essential to the exposition of the thesis, without being its central subject. Rothstein notes Schriever's changing attitude to manned spaceflight, seen initially as a distraction that might delay introduction of operational ICBMs, but later becoming much more important in his conception of how the USAF might harness emerging technologies to develop new roles. He was not a quick convert to the idea, however, and his views brought him into conflict with General White, the Chief of Staff of the Air Force, in 1959.[174] These conflicts played out particularly when Schriever was contemplating military manned

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[171] Roy Franklin Houchin, II, 'The rise and fall of Dyna-Soar : a history of Air Force hypersonic R & D, 1944-1963', (Auburn U, Auburn, AL 1995 PhD),
[172] Houchin, 'The rise and fall of Dyna-Soar', 482, 529
[173] Stephen M. Rothstein, 'Ideas as institutions: Explaining the Air Force's struggle with its aerospace concept', (Tufts U, Medford, MA 2006 PhD), Rothstein had previously addressed this topic as a Masters-level thesis *("Dead on Arrival?: the development of the Aerospace Concept 1944-58")* at the School of Advanced Airpower Studies within the Air University at Maxwell AFB, Alabama in 2000.
[174] Rothstein, 'Ideas as Institutions', 237.

space missions, and to an extent in his involvement with space weaponization; they are discussed further in Chapter 5 and 6.[175] Rothstein also analyses how Schriever's struggles with organizational structures, and the debate between 'horizontal' and 'vertical' divisions of responsibility that played out when Schriever took command of AFSC interacted with the development of a new operational concept. Ultimately, Rothstein's aim is to summarise the status of the aerospace concept by analysing its origins:

> The bifurcation of air and space was first thrust upon the service in 1958 as a national security policy *choice*. Its structural and technological bifurcation—within the Air Force—was complete by the close of 1963. Cultural bifurcation emerged slowly and more fully thereafter over time. Conceptually, however, a significant connection between air and space remains. Aerospace is not the current picture in the Air Force's head; the concept currently fails to capture the way the service sees the world and does not significantly influence the organization's actions. Nor, however, has the idea disappeared.[176]

Schriever's importance to this debate lies principally in the posts he held and the projects he oversaw in the critical 1958-63 period, which played out against the backdrop of the cultural background that Rothstein describes.

Theses – A Summary

The three PhD theses described in detail each discuss important aspects of Schriever's work, but even taken together, still leave significant aspects untouched. Claude Johns produced an early summary of the first-generation (1954-59) ICBM programme, and became the first writer to identify the 'trinity' of von Neumann, Gardner and Schriever as its architects in the USA. He analysed the administrative arrangements surrounding the programme, highlighting Gardner's role, and gave an early analysis of how concurrency had been used within it, focussing on Schriever's part. Beyond that, he says little about any other aspect of Schriever's conceptual or strategic viewpoints, and in the author's opinion, missed a unique opportunity to interview key participants while their memories were fresh. Nonetheless, his description of Schriever's understanding of concurrency was apposite and timely.

John Lonnquest's thesis stands as that giving the most personal treatment of Schriever and his work, but again it dwells exclusively on the ICBM programme and to a great extent on concurrency and its implications. It fills some of the gaps

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[175] For further comment on this disagreement between Schriever and White, see Chapter 5, p138 and Chapter 6, p166.
[176] Rothstein, 'Ideas as Institutions', 383

left by Johns, validates the roles of von Neumann, Gardner and Schriever, provides more depth in its analysis of concurrency and Schriever's understanding of it, and says more than any other about how Schriever implemented it. Finally, it provides some analysis of the Atlas missile and its short operational life. But it says little about any other aspect of Schriever's work, nor of the relationship between ICBMs and space systems, and in its own literature analysis, it occasionally fails to substantiate its criticisms.

Stephen Johnson's thesis covers Schriever's work as one case study among three, again dwelling on the ICBM programme and its management challenges. As might be expected of a thesis coming from an engineer, he offers significant insights on Schriever's technical influences before once again concentrating on engineering management issues.

Finally at PhD level, Roy Houchin and Stephen Rothstein both broaden the analysis by moving beyond consideration of ICBMs, in Houchin's case by writing an account of the X-20 Dyna-Soar programme, in which Schriever played a part without ever having direct control of it. Its most valuable contribution to a study of Schriever is that it shows that the ideas underpinning concurrency had applications beyond the ICBM. Rothstein analyses the 'aerospace' concept as the USAF understood it at various times, and his work offers a useful insight on Schriever's changing attitude to military spaceflight.

In summary, Schriever's work has received significant academic study, but the preponderance of it considers solely the ICBM programme and the management concept of concurrency. There has been no attempt to study the interactions between the ICBM programme and the USAF's manned and unmanned spaceflight aspirations, nor any of the strategic thought or conceptual development surrounding that interaction. This thesis attempts to fill that gap.

Schriever Biographies

Outside academia, Schriever's status as a significant actor in the field of military spaceflight was recognised early in his career. There are consequently several volumes of straightforward biography of him available. The earliest, and one which immediately attests to the significance of his work, is by Shirley Thomas (1920-2005).[177] Shirley Thomas wrote 8 volumes of essay-style biographies of

[177] Shirley Thomas route to writing technical biographies must surely be one of the strangest imaginable. In the 1950s, she worked as a celebrity journalist in and around Hollywood, reporting on 'red-carpet' events for NBC and CBS. Smitten by the dawn of the space age, she came to the United

space pioneers between 1960 and 1968. She was guided in the selection of her subjects by an advisory committee, which included such luminaries as Lieutenant General 'Jimmy' Doolittle USAF, Major General Holger Toftoy (who led US Army Intelligence exploitation of captured German V2 technology at the end of World War 2 and who later commanded the US Army Space Programme at Redstone Arsenal), Senator Stuart Symington (the first Secretary of the Air Force (1947-50) and later member of the Senate Armed Services and Foreign Affairs Committees) and Mr Harry Guggenheim (Director of the Guggenheim Foundation, which had funded Robert Goddard's experiments in rocketry in the 1930s and Theodore von Karman's researches at the California Institute of Technology)). In Volume 1, alongside such pioneering figures as Goddard, Tsiolkovsky and von Braun she included an essay on Bernard Schriever.[178] Her work is an amalgam of 'human interest' biography on one hand, revisiting the story of Schriever's birth in Germany, emigration during World War 1 and naturalisation in the United States as recounted here in Chapter 2, supplanted with details of Schriever's marriage and family life, and on the other an examination of the context of his work on ICBMs. Concurrency is mentioned, as is the connection between parallel sourcing of Atlas components and the ability to develop the Titan missile as a follow-on design in relatively short order. Thomas also notes the role of TRW in Atlas management, and near the end of her account comments on Project SCORE – use of an Atlas missile airframe as a very early communication satellite. She asserts that only 88 people knew the nature and intended function of the launch, including Schriever, but not including the officer who actually fired it.[179] Thomas' study of Schriever is not in itself critical to an understanding of him,[180] but its significance is that it demonstrates that in the earliest days of the space age, an eminent selection committee rated his contribution worthy of immediate inclusion in an extended series of studies, alongside the luminaries listed above and others such as John von Neumann, Theodore von Karman and Professor James van Allen.

Kingdom, enrolled at the University of Sussex, achieving a BA (1960) and PhD (1967) in Communications. She founded the Aerospace Historical Society in the USA and taught Technical Writing at the University of Southern California; there is a memorial scholarship endowed in her honour at the California Institute of Technology in Aerospace Engineering. See Karen Begen, 'Remembering Dr Shirley Thomas', *The Technograph (Newsletter of the LA Chapter of the Society for Technical Communciation),* Vol No October 2005) at 5-6, and Valerie J Nelson, 'Shirley Thomas Perkins: Obituary', *LA Times,* 6 August 2005, sec. Obituaries.

[178] Shirley Thomas, *Men of space; profiles of the leaders in space research, development, and exploration Vol 1* (Philadelphia: Chilton Co., Book Division, 1960) 47-65

[179] Thomas, *Men of space* 65

[180] The insights recorded by Thomas do seem to be unique in that they draw on the only traceable interview with the first Mrs Schriever (Dora Brett), albeit those contribute to the domestic, rather than the technical content.

Fifty years after Thomas began her series of studies, another journalist produced the first (and so far only) full-length biography of Schriever. After service in the US Army, Neil Sheehan worked in SE Asia as a journalist with UPI before joining the staff of the New York Times in 1964 and moving to Washington DC. In 1971 he obtained and published the 'Pentagon Papers', relating to a secret Pentagon history of the Vietnam War, and followed it with a biography of John Paul Vann, commenced in 1972 but not published until 1988.[181] He recounts his decision to focus his next book on the Cold War, and how a chance encounter in the library of the Air Forces Association in Arlington, Virginia inspired him:

> …it's about 1994 now. I was over at the Air Force Association in Arlington ... And I was in their library and they keep files on prominent Air Force figures.
>
> And someone said to me you ought to look up Bernard Schriever. So I asked the librarian for the file…opened it up. And right there…was a photograph of this General leaning up against a table with all of these missiles around him…And I said this guy looks interesting.
>
> …he was famous within the Air Force, but not outside...So I called him and arranged to go over and talk to him and it began the first of 52 interviews with him. And then I realized that this man had stood at a pivotal point in the Cold War.[182]

Sheehan's biography of Schriever (which provided the background to Schriever's funeral in Chapter 2) was published in 2009.[183] As Sheehan alludes above, it was based on extensive access to Schriever and to his papers in the latter years of his life, as well as numerous interviews with colleagues and acquaintances. It is a lengthy work (534pp) and is valuable not only for the extensive research that underpins it, but also because it combines a study of Schriever's contribution to ICBMs with mention of his later work in military satellites, a subject almost totally ignored until now. That is not to say that it does not pose problems of its own. Sheehan's research was voluminous and probing, including 52 interviews with Schriever, interviews with 117 other named contributors, access to Schriever's diary and numerous other papers in his possession, as well as visits to all the significant archives and presidential libraries in the USA that bear on Schriever and his work. Frustratingly, however, Sheehan omits footnotes and close referencing conventions. Each chapter of his book has a list of sources and there is a detailed bibliography, but there is no mechanism to

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[181] Neil Sheehan, *A bright shining lie : John Paul Vann and America in Vietnam* (New York: Random House, 1988). Sheehan was awarded the Pulitzer Prize for general non-fiction for this work in 1989.
[182] Neil Sheehan, interviewed by Brian Lamb on 15 September 2009 for CSPAN Transcript available at http://www.c-span.org/video/transcript/?id=8195
[183] Sheehan, *A Fiery Peace*

connect individual assertions in the book with the primary sources. All the tapes and other papers accumulated in its writing have been deposited at the Library of Congress, but they are currently in storage pending processing and conservation, and were initially withheld from researchers in the belief that Mr Sheehan wished the archive to remain closed (presumably during his lifetime). This misapprehension on the part of the Library was resolved by the author in 2014. The collection has now been opened to researchers on a limited basis, and though at present it is un-catalogued by the Library, it reflects Sheehan's methodical organisation. Once organised, it will provide the means to test some of the assertions in Sheehan's book.

Sheehan's book itself straddles the material already alluded to in academic analysis and the personal narrative found elsewhere. On one hand, he offers valuable insights into the character traits of many of the key figures, and outlines their working methods. And although he says nothing about concurrency, he provides context for many of the difficulties encountered in making it work. But on the other, its eclectic structure and digressions limit its usefulness to readers seeking more precise details of Schriever's work; two of its seven major sections barely mention Schriever at all.[184]

In the end, his book must stand on its merits; it was written for a popular readership, it uses Schriever to provide a central narrative around which a history of the early Cold War and the Air Forces' part in stabilizing it can be woven. When its primary sources become more generally accessible, it may be possible to exploit them further; for now it appears to be a sincere but selective attempt to introduce Schriever and his career to a wider public.

The Schriever Archive at the Library of Congress

This completes a summary of the extant literature relating to Schriever and his work. In many cases, the authors were able to draw on primary sources; a number of them came from a USAF or other US military background, and would have had access to official archives and other reference material such as contemporary histories and policy documents. Some were also able to interview Schriever and/or his contemporaries. Throughout their research, however, there

[184] 'Book II' within Sheehan's volume is an account of Soviet intelligence agency penetration of the Manhattan project and the USSR's subsequent attainment of nuclear weapons technology. 'Book VII', which relates unique details about Schriever's involvement in early reconnaissance satellites, devotes about half its length to an account of the Cuban missile crisis – a significant event regarding the use of nuclear missiles, but one in which satellite reconnaissance played no publicly acknowledged part.

was another major source of Schriever material that remained inaccessible. Its recent unlocking has provided scope for new insights and conclusions.

US and UK government security regulations are prescriptive about the secure storage of classified material. Depending on precise classification, minimum standards of physical security are required, along with a regime of accountability and audit to ensure that material is not lost or misplaced. Although there are exceptions for specific circumstances, a serving officer would not normally be allowed to retain classified material at home. This was plainly not the case during Schriever's career; he appears to have retained a significant holding of official papers, both classified and unclassified, during his active duty and afterwards.

As recounted to the author by Mr Jacob Neufeld, who at one time directed the USAF Historical Studies Office (AFHSO), shortly after Schriever retired from the USAF, his first marriage ended. The parting with Mrs Schriever was reasonably amicable, and initially, she remained resident in the former family home. When she eventually decided to move to a smaller property, his collection of official papers was discovered. To prevent their dispersal or disposal, and to protect any classified content, the archive was collected by the AFHSO based at Bolling AFB in Washington DC. Subsequently, the papers were stored first at Bolling, then latterly at the Air Force Historical Research Agency (AFHRA) at Maxwell AFB in Alabama.[185] AFHRA offered them to Texas A&M University (Schriever's *alma-mater*) for archiving.[186] Negotiations were never satisfactorily concluded, and in 2005 and 2007 the archive was donated to the Library of Congress by General Schriever and his second wife.[187] It was collated and conserved, and finally opened to researchers in 2011.

The Schriever archive is extensive, comprising some 87,000 items in 250 containers.[188] From 1931 onwards (when Schriever graduated from university and entered the US Army), it includes content from every decade of his life. Within it, there are, broadly speaking, six categories of unclassified material. A portion consists of copies of Schriever's record of service in the USAF and related documents, including his working diaries for the period 1954-66. The biggest

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[185] Conversation with the author at Mr Neufeld's home, 29 March 2009. Mr Neufeld had taken part in the retrieval of the archive and was at the time researching a biography of Schriever (so far unpublished).

[186] As recounted by current Texas A&M University Library staff (personal communication with the author during 2010). The last reference held at Texas A&M to the possible donation is dated in 1990.

[187] Kells et al., *BAS Papers; Finding Aid* 1.

[188] Kells et al., *BAS Papers; Finding Aid*

portion consists principally of duplicate copies of official correspondence produced under his supervision. After that, there is a section devoted to Schriever's speeches and writings, and a section relating to the co-authored work produced with Colonel Vince Ford (details of Ford's career and relationship with Schriever appear below). There are copies of two other book-length manuscripts, and there is finally a section of papers mainly relating to Schriever's post-USAF business interests.

The desk diaries are almost entirely hand-written by Schriever; while he was completing them, he appears to have been punctilious about keeping a record of his duty location, his major daily appointments and 'action lists' of matters on his mind, either as issues to resolve during a day or topics for discussion during meetings or appointments. Typically, they were written on office notepad pages, but occasionally they were written on whatever scrap paper was to hand.[189] There are gaps in the record, though since the diary is unbound, it is generally impossible to tell if there are missing pages, or if no entries were written on a given day.[190] Many entries are simply 'bullet-point' lists, but some are more discursive. They are almost entirely devoid of personal opinions, views or comments; a singular, but arguably marginal, exception is on 8 February 1957 when Schriever notes simply "JVN Died" – a reference, without further comment, to the passing of his close friend and collaborator John von Neumann. An indication of how exceptional this is comes from the fact that there are no pages or entries for 4 October 1957, 12 April 1961 or 22 November 1963.[191]

Much of the correspondence in the archive takes the form of retained carbon copies of letters or memoranda. In many cases the intended recipient has to be inferred, since the carbon copy was removed before the salutation and signature were added to the letter. In some cases the recipient's name or job title is included, but others, particularly inter-office memoranda, are addressed using an impenetrable telegraphic code system which assigned a three to five-letter code for

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[189] Some of the later diary entries are lists typed by secretaries on which Schriever made notes – for example the diary entry for 2 January 1964 is a typed list of appointments, subsequently annotated by Schriever; his diary entry for the following day is then handwritten by him on the remainder of the page. *Box 7 Folder 1, Bernard A. Schriever Papers, Manuscript Division, Library of Congress, Washington, D.C.* – hereafter abbreviated '*Schriever Archive, Box x, Folder y'*.

[190] Occasionally, Schriever inserted dated blank pages in his diary, but either these are exceptional, or a very large number of such pages have at some point been removed from the collection. An example of their inclusion is 9-11 June 1959 (*Schriever Archive Box 6, Folder 1).*

[191] The dates for the launch of Sputnik 1, Yuri Gagarin's orbital spaceflight and the assassination of President John F Kennedy respectively. Again it is possible that all 3 pages existed but are now missing, or conceivably that they are classified and remain in one of the closed containers of the archive, but there is no comment or acknowledgement relating specifically to any of those events in adjacent pages.

the appointment held by the recipient. The codes do not give any obvious indication of sender or recipient – they are not in any way acronyms. Thus, for example, in September 1959, Major General Ritland, then Commander of Air Force Ballistic Missile Division, prepared a draft policy statement on 'Military Operations in the Aerospace Medium'.[192] The drafting was led by Colonel Soper from the Plans and Policy Office, and a covering letter from General Ritland to General Schriever was also prepared. However, within the Archive copy, Colonel Soper refers to his own office as 'WDGP', he is drafting for General Ritland ('WDGV?') to send to General Schriever ('RDG'), and the draft is to be coordinated (i.e. reviewed and approved) by 'WDGN', 'WDS', 'WDT', WDZ' and 'WDGE'. In some cases, the codes are reused or annotated often enough to be recognisable, but in many others they remain completely obscure. It seems a reasonable inference that the bulk of the unattributed correspondence was either drafted by or for Schriever – he would have been unlikely to have retained many carbon copies from another office or department that did not involve him, and the received correspondence in the Archive appears usually to be a signed copy addressed to Schriever or his organisation. There remains the residual risk either that drafted correspondence was not in the end sent or that occasional external drafts have been included, but this does not seem likely to be a common occurrence.[193]

The next major sub-division within the Archive comprises the unpublished book manuscripts. At least three times in his life Schriever collaborated with others to produce manuscripts intended for publication; in no case was the aim realised, but the manuscripts survive and are valuable for the insights they provide into Schriever's thinking. One never approached completion, and as it stands is plainly unpublishable, but the others are mature and essentially ready for publication; there follows a description of their contents and such context as can be reconstructed.

The Vince Ford Manuscript

Colonel Vincent Thaddeus ('Vince') Ford (1907-2001) was a contemporary and colleague of Schriever's. Born in Connecticut, he graduated with an engineering degree from UCLA and joined the Air Corps two years before Schriever. He trained as a pilot, was seriously injured in a flying accident, left the

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[192] *Schriever Archive, Box 25, Folder 6*

[193] There are instances where a given draft was not used, or was sent back to the drafter for amendment, but since in many such cases the amendment or instruction to amend is in Schriever's handwriting, this strengthens rather than weakens the provenance of that item.

Air Corps and worked in Hollywood for Universal Studios prior to rejoining the Air Corps, and later the USAF. He did this twice – once during World War 2, and again post-war when he completed regular service between 1948 and 1964. In this latter period he served as Military Executive to Mr Trevor Gardner, the Assistant Secretary of the Air Force for Research and Development, and as Executive Officer to Schriever at WDD and ARDC.[194]

At some time in 1957-58, Ford decided to attempt to record the history of the ICBM project 'from the inside', concentrating on events during 1954-57. His intent was to record the personal history of the ICBM project rather than the technical detail, through the activities of three key players he knew or had known well: John von Neumann, Trevor Gardner and Bernard Schriever. One driver for this was probably his awareness of the fragility of personal recollection – John von Neumann had died before he started the work, Trevor Gardner had resigned from the Eisenhower administration in 1956 and was taking no part in the ICBM programme at that point, and only Schriever remained actively engaged.[195] The precise start date for this effort cannot be proven, but it can be bounded. The first papers in the Schriever archive relating to the work are dated 1 March 1958, but they are included in a bundle reviewed by Ford in 1968 and marked as '1957-58' work.[196] It is obvious from the draft text that Ford had been closely attached to John von Neumann and had been deeply affected by his death; in later drafts he describes von Neumann's final illness and death in great detail. It therefore seems plausible to the author that von Neumann's death in February 1957 acted as a spur to him to record his memories and experiences. Finally, a very early draft of the opening chapter mentions the launch of Sputnik 1 (4 October 1957), which provides another bound.[197]

A further prompt for publication may have come a little later. In January 1960, Schriever was anticipating public reaction to the publication of the General Accounting Office (GAO) inquiry into management of the ICBM Program. On 13 January 1960 he includes in his diary a draft White Paper outline, responding to the report. In it, after noting that the entry into service of the Atlas ICBM had only been

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[194] Biographical details from a copy in the author's possession of the eulogy given at Ford's funeral by another colleague, Mr George Stein, from the introduction to the archive copy of the Ford manuscript held by the Dwight D Eisenhower Presidential Library (Dwight D Eisenhower Presidential Library, Abilene, Kansas; *Accession Number A92-17*) and from a copy of Ford's service record.
[195] Trevor Gardner would return to public service in 1960 under the Kennedy Administration serving as an adviser on Space policy before his death in office in September 1963, aged only 47.
[196] Schriever Archive Box 184, Folder 7. The annotation is in Ford's handwriting.
[197] *Ibid*.

achieved through the efforts of identifiable individuals, he then proposed recording the history of the program:

> "One important element supporting such a course of action [a robust response to the GAO report] is the preparation of a factual document which tells the Air Force Ballistic Missile Story…The why, the where, the <u>who</u> and the how of each major milestone should be included."[198]

Schriever expands on this idea over several pages, culminating by proposing nine people to undertake the task: from HQ ARDC, Colonels Boatman and Ford, from BMC Colonel Bishop, from STL Dr Mettler and five 'Helper Historians' – 'Williams, Rockerfeller, Ramo, Gardner and Schriever'.[199] By this stage, Ford's work, as noted above, was almost certainly underway, but this initiative may well have been both an impetus and a degree of official endorsement for it.

There is one further cryptic diary reference of note – on 13 May 1960, Schriever's diary includes the entry: "VTF Book (Medaris), Position USAF".[200] VTF are Ford's initials, often used by Schriever in his diaries. The Medaris reference probably relates to Major General John Medaris's unsuccessful attempt to publish his book 'Countdown for Decision' while still serving in the US Army, related in the foreword of that book.[201] Whether this entry refers to Ford's own book, or whether in fact he was being asked to explore the USAF's attitude to Schriever's co-authoring of his book with Sam Cohen, remains unclear.

The bulk of the drafting by Ford appears to have taken place over two periods. The first consisted of Ford dictating his source material for transcription and the subsequent editing of those transcripts. This material survives in the Archive as transcriptions of dictation 'belts' numbered from 82 to 138.[202] (It is not clear whether belts 1-81 existed or were related to this project). The main source for Ford's work is Schriever's 1954-57 diaries. Much of the transcription is plainly from Ford reading pages of Schriever's diaries verbatim, in their characteristic 'date, location, short/one-word topic' style; in some cases, the dates match and the

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198 Schriever diary entry 13 January 1960, *Schriever Archive Box 6, Folder 4*. Underlining in original.

199 Along with Ford, Colonel Beryl L 'Boat' Boatman was a close colleague of Schriever. 'Colonel Bishop' is probably Samuel W Bishop, who worked at the Ballistic Missile Center and who later contributed to other missile history projects. 'Dr Mettler' is Ruben F Mettler, who worked for Trevor Gardner among others as a Department of Defense consultant. 'Williams' is unidentified, 'Rockefeller' was probably Dr Alfred Rockefeller, the WDD Historian, while Ramo, Gardner and Schriever are obvious protagonists in the story.

200 Schriever Archive, Box 6, Folder 5.

201 John B. Medaris and Arthur Gordon, *Countdown for decision* (New York: Paperback Library, 1961)

202 Ford appears to have used a 'Dictabelt' dictation machine, which recorded dictation via a stylus onto a plastic belt – consequently the pages of transcript in the archive are referred to by 'belt number'. *Schriever Archive, Box 184, Folders 8-10*

contents correspond exactly with the diary originals elsewhere in the Archive. Ford also refers explicitly to access to Schriever's diary: "I wish to continue with Benny's personal notes because while this may seem a useless exercise in chronologizing a series of disconnected, single word think-pieces, they actually tell a coherent and continuous story…"[203] Where Ford could not make sense of Schriever's thoughts or ideas, he would dictate the ensuing question ('Ask Bennie to explain this', 'Check the date', and suchlike) straight into the transcript. Frustratingly, if he found an answer to the question he does not appear to have gone back to the transcription to note it. In other places, Ford cites Trevor Gardner's notes,[204] and in yet other places, he is dictating his own recollections, in relatively continuous prose but without great 'diary style' detail.[205] Apart from the limit on the start of the drafting implied by the dated handwritten notes, there are very few clues as to when the dictation and transcription took place. In one place, Ford has handwritten a note dated August 1965 referring to a 'Herald-Tribune' news article bearing on his work into the transcript.[206] This sets an effective end-date on the dictation and transcription, albeit a conservative one.[207]

The second round of dictation occurred during 1965. Ford had retired by this time, and was plainly trying to move the book forward. He conducted a variety of interviews to supplant missing information, and was also working on a detailed and complete draft of his text. Topics covered included bioastronautics, the medical implications of manned spaceflight and the presentational aspects of military personnel flying as NASA astronauts. He also conducted a substantial interview with Major General 'Ozzie' Ritland, and added some reminiscences of his own.[208] Judging by the tape numbers cited, there are almost certainly gaps in the Archive regarding Ford's work at this point.[209]

Ford's work ultimately yielded a very long text of some 1200 pages. He admitted that it was un-publishable as drafted, but hoped that with editing it would

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[203] Belt Number 102, Page 2, contained in *ibid.*
[204] For example Belt 106, Page 3 in *ibid*.
[205] The Schriever diary entries predominate in Belts 82-118, Ford's narrative becomes more continuous after that to the end of the Belt sequence.
[206] Belt Number 122, Page 2 – *Schriever Archive Box 184, Folder 10*.
[207] There are no references to any events in the period 1958-65 in the transcripts, so it would seem likely that they were dictated and transcribed earlier in that period than later.
[208] For the Ritland Interview(s), see *Schriever Archive Box 188, Folder 4*. Most of the interview material seems to have been gathered on 23-24 November 1965, though there are references to earlier meetings.
[209] Ford's manuscript is cited directly only rarely in this thesis – chiefly due to the florid nature of the original dictation and the multiple edits, many un-attributable, that it contains. Nevertheless, it provides the best description of some of the personal dynamics within Schriever's immediate circle, and in particular, it informs as background the first half of Chapter 4 of this thesis relating to ICBM development.

yield a publishable text. There is no evidence that Ford, either alone or with Schriever, sought to have it published during the 1950s or most of the 1960s. However beginning in 1968, Ford sought professional help with editing. Frustratingly, much of Ford's correspondence relating to this survives only in (heavily amended) drafts, with the recipient identified by initials or nickname. Some can be identified with confidence, but other attributions are more speculative. The first letter about publication, dated 14 May 1968, is addressed to 'Sam', and describes the draft text in detail (which at that stage comprised 578 pages).[210] 'Sam' is possibly Samuel S Vaughan, at that time an editor with Doubleday Inc.[211] Ford shared portions of the manuscript with an apparent friend of Trevor Gardner ('Muriel') for proof reading, and had apparently opened negotiations about publication with Doubleday Inc.[212] This first round of Doubleday negotiations ended in apparent acrimony in October 1969.[213] An approach to Harper-Row Inc followed, but again appears to have foundered.[214] In 1969, Ford was in correspondence about arranging publication with 'JD' – John Ducas, a publicity consultant who had worked for TRW in connection with the ICBM programme when it had first been made public, and was also known to Schriever.[215] [216] [217] He dealt with a Ms Betty Oswald at the RAND Corporation during March and April 1971, but RAND appear to have been unable to assist in publication.[218] In August 1972, correspondence resumed with Doubleday Inc via an attorney. Mr Samuel S Vaughn, by then the President of Doubleday, met the attorney (a Mr Botte) and assigned a professional editor to the project. However, for reasons that remain

[210] Draft letter from Col Vince Ford (VTF) to 'Sam' dated Tuesday, 14 May 1968. *Schriever Archive, Box 184, Folder 4.*

[211] Bruce Weber, 'Samuel S. Vaughan, Publisher at Doubleday, Dies at 83', *New York Times,* 1 February 2012, sec. Obituaries.

[212] Correspondence between VTF and 'Muriel' dated November 1968 – October 1969. Reference is made to 'Mr Vaughan', believed to be Mr Samuel S Vaughan and connected to August 1972 correspondence described below. Muriel was acting both as proof-reader and copy typist at this point. *Ibid.*

[213] Note VTF to Muriel dated 22 October 1969: "Maybe we are still trying to recover from the traumatic Vaughn-Doubleday thing where we believed, in good faith". *Ibid.*

[214] VTF, in a note to John Ducas, states that "…the Harper-Row correspondence tells its own story…", which was presumably a negative one. This correspondence appears now to be lost. *Schriever Archive, Box 184, Folder 5.*

[215] For some biographical details of Mr Ducas, see New York Times, 'John Ducas, 70, Dies; A Publicity Consultant', *New York Times,* 29 September 1990, sec. Obituaries.

[216] There is one previous, typically cryptic, reference to J Ducas in the Schriever diaries. Ten years earlier, on 14 October 1959, the entry 'Jake-VTF-J Ducas' appears, without further detail, as business to be discussed on a trip to New York. *Schriever Archive, Box 6 Folder 2.*

[217] Ford submitted a draft of the book to Ducas on 1 April 1970, and exchanged correspondence with him until 18 February 1971 when he noted his frustration at the lack of progress in 9 ½ months: "-NOTHING! NO REASON, NO EXPLANATION, NO COMMENT, PRO OR CON—NOTHING!!" VTF note (possibly to self), capitalization in original. *Schriever Archive, Box 184 Folder 5.*

[218] Correspondence VTF/Betty Oswald, *Schriever Archive, Box 184, Folder 6*. Some of the letters were sent to Betty Oswald's home address, suggesting that she may have been assisting in a personal rather than commercial capacity. The RAND archive has no record of the project.

unclear, Doubleday rejected the manuscript in November 1972.[219] Meanwhile, Ford sent sections of the text to a 'Hi' Hertell in September 1972 (another unidentified correspondent).[220] The last documented attempt at this time to find a publisher occurred in 1973, when Mr Charles J V Murphy acted as a potential ghost-writer to secure sponsorship from the Foreign Policy Research Institute in Philadelphia for re-writing/editing the manuscript. Murphy wrote to Schriever laying down the terms on which he would undertake the work, but no record exists in the Archive of how and why this effort foundered like all the others.[221] It is possible that the sum to be advanced by the authors - $25 000 – to cover costs served as a deterrent. Allowing for inflation, this equates to about $140 000 in 2014 values.

Finally in 1984-85, Ford made one last attempt to publish: archive details are sketchy – they consist of a stock covering letter explaining the book and its intent, and separately a list of potential publishers.[222] Reflecting changing times, the list of possible publishers includes several academic institutions: The University Presses at Brown University, Cornell, Chicago, Illinois, Ohio, Oklahoma, Texas and Wayne State, as well as the Smithsonian, Hoover and Naval Institute Presses and other commercial publishers are all listed. There is no confirmation which, if any of the listed institutions was in fact approached, nor record of any results.

Ford had already expressed his frustration many times with delays in bringing the publication to fruition, and stated frequently that it was principally loyalty to Schriever, Gardner and von Neumann (most explicitly Gardner) that had kept him engaged. At the end of his efforts, Ford was left with his 1258 page text, still at his own estimation desperately in need of editing.

Ford changed his mind several times about the title of the proposed book: "It's My Country Too", "24 Minutes to Checkmate" and "Three Special Immigrants" are all cited at various times, and he makes mention of having further possible titles in mind. At the end of the project, at least 3 copies of the 12-1300 page manuscript existed. One copy was held by Schriever, one ended up in the hands of another

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[219] Letter from Mr Bill Henderson (Doubleday editor) to Mr David Botte dated 30 November 1972. *Schriever Archive, Box 184 Folder 6.*
[220] Single page memo by VTF (draft?) to 'Hi' Hertell. It is possible that 'Hi' was a Doubleday contact and that this was therefore not a further attempt to secure a publisher. There was also a publishing company active at the time in Chicago – the John H Hertell Co – but it appears to have concentrated solely on publishing Christian Bibles, so seems an unlikely outlet for Ford's manuscript. Ford was, however, a devout Catholic, so it is possible that he knew someone at the company. *Ibid.*
[221] Letters from Mr Bill Kiltner (FPRI) to Bill Richardson and from Charles Murphy to Schriever dated 15 March and 17 May 1973. Brigadier Gen Bill Richardson III was another associate of Schriever who had served under him at AF Systems Command and later worked for Schriever's consultancy firm. The letter from Mr Kiltner to General Richardson makes it clear that they were close colleagues, already collaborating on other writing and publishing projects. *Ibid.*
[222] *Schriever Archive, Box 181, Folder 11.*

friend of Ford and Schriever – Mr George Stein of Manassas, Virginia, and Ford gifted his own copy to the Dwight D Eisenhower Presidential Library in Abilene, Kansas. Schriever's copy passed into the hands of his family, but deteriorated due to damp storage conditions and was eventually disposed of.[223] Ford's copy remains in the library in Abilene and is available for consultation. Mr Stein gifted his copy to the Library of Congress via the author during 2013 and after conservation it will become available for study as part of the Schriever Archive.[224] A brief examination of the copy in Abilene in 2013, limited by time available in the archive, suggested that both surviving copies are very similar if not identical. Unfortunately, both are photocopies of a hand-amended original; and the photocopying has served to make attributing the corrections excessively difficult. Nevertheless, the text is readable as drafted and is presumably roughly in the form that Ford would have wanted it to appear.

Ultimately, the value of the Ford manuscript is twofold. It compares portions of Schriever's diaries over three years with other records, and it also synthesises Ford's own experiences as a participant in the events described, and his research through interview and conversation with other participants. Ford contributed significantly to Neil Sheehan's research for his Schriever biography, and in some places, Sheehan's debt to Ford is obvious (and is acknowledged).[225] To that extent, portions of Sheehan represent what Ford had always sought – a professionally edited version of his draft text.

'ICBM: The Prelude to Survival and Space' - The Samuel Cohen Manuscript

In contrast to the rambling narrative of Ford, the other book-project that Schriever definitely collaborated on is much shorter, more focussed and would have been much more suitable for immediate publication as a co-authored work. Sometime, probably in late 1959, Schriever and his co-author Sam Cohen set out to write a book about the military implications of the ICBM and of military spaceflight.

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[223] As recounted by Mr Ted Moeller (Schriever's son-in-law) in conversation with the author during a family visit in 2011.

[224] There are numerous draft chapters of Ford's book in the Library of Congress, developed across an extended time-frame. It would probably be possible to assemble a copy of the manuscript as Ford envisaged it at various dates in its history, making reasonable guesses as to an editor's direction, but Stein's copy donated via the author is probably the most complete and consistent with the author's intent at a given (unknown) instant.

[225] A particularly clear example is Ford's account of the meeting in May 1954 where Trevor Gardner offered Schriever management of the ICBM effort over lunch. This forms Chapter 36 of Sheehan, and draws directly on Ford's account in his manuscript. See Sheehan, *A Fiery Peace* 225-28 and *Schriever Archive, Box 181, Folder 10,* 'Sample Chapter 13'.

Samuel Theodore Cohen (1921-2010) was born in New York City and studied mathematics and physics at UCLA prior to joining the US Army in the aftermath of Pearl Harbor. He was recruited into the Manhattan Project and worked on weapons design at Los Alamos. After World War 2, he joined the RAND Corporation where he remained for the rest of his working life. He invented and publicized the concept of the enhanced radiation (nuclear) warhead (the so-called 'Neutron Bomb') and was a proponent of the use of nuclear weapons for tactical purposes in limited conflicts, in contrast to the prevailing orthodoxy of their use solely as a strategic deterrent.

Unlike the relatively well-documented genesis of the Schriever-Ford manuscript and the accounts of attempts to secure a publisher, there is little in the Schriever Archive to explain the context, motivation or background to Schriever and Cohen's work.[226] Nor is there any explicit hint of who the intended publisher was, nor why the publication did not go ahead. What does exist is a draft of a work entitled "ICBM: The Prelude to Survival and Space". The text is almost complete and ready to publish, all that appears to be missing are diagrams and/or illustrations, which are referred to in the text but not included in the draft. The manuscript is in 11 Chapters and a Preface, and the copy held in the Archive (the only known copy) was compiled during August 1960, and acknowledged as complete by Schriever on 27 August 1960.[227] The covering note detailing its compilation acknowledges the existence of illustrations – the unknown secretary(?) who compiled the covering note asks to be taken along on Schriever's next visit to Air Force Ballistic Missiles Division to complete collation of photographs and illustrations. The illustrations would in fact have been few in number and the text is perfectly readable without them.

The writing of the Schriever-Cohen manuscript can be dated fairly accurately.[228] Plainly it was complete by August 1960, the date of the draft's covering note. The text of Chapter 1 seems to imply that ICBMs were already in service: "using today's technical capabilities it is possible for an ICBM to throw a

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[226] Cohen died at the age of 89 in 2010, and appears not to have left any papers. The RAND Corporation and UCLA (the two most obvious likely recipients) confirmed to the author that they hold no deposit of his papers, and surviving members of his family have stated that they are unaware of his having held any significant quantity of correspondence or other papers requiring curating.

[227] The complete text, including the covering note, fills a single container within the Archive. *Schriever Archive, Box 180.*

[228] There is a possible fleeting reference to Cohen and book-writing in Schriever's diary for 24 October 1957. Schriever's handwriting is untypically difficult to read, but he may have discussed 'Book interest – Cohen' with Vince Ford and with 'T Walk' (unidentified). *Schriever Archive, Box 5, Folder 3.* This is an isolated entry, and does not seem to have been followed up at the time.

sizeable nuclear payload to any point on earth."[229] Since the Atlas ICBM was the first such missile to enter service (on 31 October 1959), the draft would appear to have been written either after this date, or at least close enough to it to anticipate successful entry to service. In Chapter 2, there are also references to Soviet successes with 'Luniks' (plural) which, since they would have been hard for an American to anticipate, suggest that writing occurred after September 1959.[230] Thus, sometime between November 1959 and July 1960 are the most likely dates for its authoring.

Schriever's diaries provide almost no corroboration of the proposed drafting dates. In line with his strict demarcation between his personal and official lives, and the consequent almost total lack of personal appointments or business in his diaries, there are no references to co-authoring anywhere in the diaries for January-August 1960 – not even a mention on 27 August, the date when the draft text was acknowledged as complete. The only specific references to Cohen in the diary at this time are on 14 October 1959, when he notes "Sue for Book (Sam Cohen)", on 10 November 1959, when he made notes about low-yield nuclear warheads against Cohen's name, and on 14 June 1960, when Schriever notes: "Get Cohen to rewrite and cut-down Cella paper and thus get to the right people".[231]

Although Sam Cohen died in 2010, Mrs Cohen survives, and through her family, the author attempted to establish any memories she had about the rationale for authorship or any other context for its writing that she could offer. Sadly, although Mrs Cohen has memories of her husband collaborating with Schriever on the text, she could recall little except that it was written in Washington in Schriever's apartment.[232] Thus the detail above is probably all that will ever be established about its provenance, barring discovery of any new correspondence.

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[229] *"ICBM: the Key to Survival and Space", Chapter 1, Page 16.* Unpublished draft by B A Schriever and S T Cohen, contained in *Schriever Archive, Box 180*. Hereafter referred to as if a published volume, and referenced by Chapter and Page Number as reflected in the draft (pages are numbered separately within each Chapter).

[230] *Bernard Schriever and Samuel. T. Cohen, ICBM: The Key to Survival and Space (Library of Congress MSS Collection, 1959-60) Chapter 2 Page 13*. The Soviet Luna 2 and Luna 3 missions achieved their goals (impact on the Moon's surface for Luna 2, and photography of the far side of the Moon for Luna 3) on 12 September and 4 October 1959 respectively.

[231] *Schriever Archive, Box 6, Folder 4 and 5*. There is no clue who 'Sue' is or what her connection with the manuscript is; Colonel (later Brigadier General) Richard Cella worked for Schriever on several occasions, but it is unclear what paper is being referred to, nor, why Schriever wanted Cohen to do this work.

[232] "Though my mother does remember the time during which my father worked on the book you'd mentioned - she said my father had taken time away from work and he and General Schriever had holed up in his apartment for days at a time - I'm afraid she has no knowledge of what exactly the project entailed." *Mrs Carla Nagler (daughter of Sam Cohen) in correspondence with the author, August 2012.*

An attempt was made through textual analysis software to deduce whether authorship of individual chapters could be established, but due to the (statistically) small sample of text, and perhaps due to mutual editing or the involvement of other un-acknowledged contributors, the results were inconclusive. The text must therefore be taken to be the joint opinions of both authors.

The Schriever-Cohen manuscript is a statement of the joint views of the authors on the utility of ICBMs, their history, the relationship between ICBMs and space capabilities and the utility of military spaceflight. The dominant theme changes sharply between chapters and at times the analysis is laboured; the opening chapter about 'The Evolution of Weapons' includes an analysis of human combat starting in pre-historic times with hand-held weapons, while Chapter 4 about 'The Committees' begins with a philosophical discussion on the nature of committees in general before dwelling on the workings of the various committees that oversaw the ICBM programme, including the (US) Scientific Advisory Board and the Strategic Missiles Evaluation Committee (the Von Neumann 'Teapot' Committee). In yet other places, the text presents startlingly original analysis of strategic issues, including advocacy for space weaponization that will be examined in detail later. Overall, and despite occasional textual thickets, the text provides a rare insight into Schriever and Cohen's thoughts about the strategic implications of their work.

Another Schriever-Cohen Manuscript?

In 2000, Sam Cohen wrote and self-published his memoirs.[233] In these, he recounts his friendship with Schriever, which lasted from 1950 until Schriever's death in 2005. Much of his writing provides context for the relationship between Schriever, von Neumann, Ford and Cohen, and he also provides a caustic assessment of Schriever's relationship with Robert McNamara. But there is no mention of the 'ICBM' manuscript of 1959-60 or its authorship. However, tucked away within it is mention of another Schriever-Cohen publishing venture, dated after Schriever's retirement in 1966. Cohen recounts: "[Schriever] requested of me that I write a book for him, which would express our common views on what had to be done about our defense, particularly the tactical nuclear part of the equation. I wrote the book…However, for his personal and political reasons, he backed away, and it never saw the light of day."[234] In the Schriever Archive, there is another book

[233] S. T. Cohen, *Shame : confessions of the father of the neutron bomb* ([United States]: Xlibris, 2000)
[234] Cohen, *Shame* 437

length manuscript, which appears to be the text of this book, now finally in the public domain.[235] It has no title and no attribution or accompanying correspondence, thus identification of this MSS as the text referred to by Cohen is speculative. Like the acknowledged Schriever-Cohen manuscript, it is a late draft, plainly almost ready for publication. Compared to the 'ICBM' manuscript, there is slightly more handwritten amendment in this later(?) text, and what there is appears to be in the same handwriting in both. The style of layout and typing is similar in both, and finally the later text deals at some length with tactical nuclear issues as described by Cohen. In the absence of a title page, or any supporting correspondence or cross-references elsewhere in the Archive, the reader cannot be sure of its identity, but it does not seem fanciful to make the connection.

Conclusion

This chapter establishes that Schriever's life and work have already been studied, in some aspects extensively. But with a few notable exceptions, academic research has concentrated on the ICBM programme, and within that the concept of 'concurrency'. Popular biographies have used Schriever's status as an 'immigrant boy made good' for human interest and his work with ICBMs as a prism through which to refract the history of the Early Cold War. While these are worthy goals, they omit interesting facets of his life and work. There are good reasons for this, principally the paucity of source material, and the blanket of classification and other security issues that covered large parts of the early space age. The opening of the Schriever archive for research and the aggressive (and very welcome) programme of declassification pursued by the National Reconnaissance Office have provided a new opportunity to assess his work and to attribute reasons for his successes and failures. So what has been covered and what is left to say about Schriever and his work?

Schriever's contribution to the development of ICBMs was recognised as significant while it was still underway, and many scholars have made reference to it. He is seen by some as architect of an essential element of Cold War deterrence theory, and by others as a significant figure within the discipline of systems management of complex projects. It is worth noting that in both cases, but particularly the latter, he was a technically-competent manager rather than a working engineer, and in this respect he stands in contrast to other comparable figures such as Wernher von Braun, who began his career in pre-war Germany

[235] *Schriever Archive, Box 183, Folders 8 and 9.*

actually designing and building rockets. His understanding of bureaucratic organization and the lobbying that can be required to gain support for a project is also of note; the accounts of his leadership of the WDD are full of records of him flying regularly between California and Washington DC to brief and lobby leadership and administration figures in the Pentagon and elsewhere. He also had a keen appreciation of the need to demonstrate the effectiveness of his management skills to Congress. His name is inextricably linked with the concept of concurrency, although analysis of its effectiveness as a technique is inevitably clouded by competing definitions; taken as a narrow concept it is not in itself sufficient to account for the ICBM project's success, but interpreted more broadly, it is hard to see that it was applied consistently.

What is missing in all this is an analysis of Schriever's motivations and understanding of where missile and space forces fitted conceptually and doctrinally into the Air Force of his times. There has also been very little attempt to analyse his understanding of the significance of space systems, both manned and unmanned, to relate those to developing air-power understanding and to see if the techniques Schriever employed in developing such systems bear any relationship to those employed in ICBM and IRBM development. This thesis attempts to fill that gap, using the methodology outlined in Chapter 1. Chapters 4, 5 and 6 look in turn at test cases to apply the methodology and analyse Schriever's successes and failures.

CHAPTER 4 – THE VIRTUOUS PATH: ICBMS AND RECONNAISSANCE SATELLITES

Introduction

As was noted in Chapter 2, Bernard Schriever is widely identified today as the 'Father of the ICBM' within the USAF. He also played a significant part in the introduction of reconnaissance satellites into USAF service. His management of the Atlas ICBM project brought it into operational service within the stipulated time limits, and although Atlas had a relatively short service life, it met the demands placed on it until more advanced systems became available. The first part of this chapter looks at Bernard Schriever's involvement with the ICBM programme from its earliest days, and analyses it against his technical, policy and administrative achievements. The second part, repeats the analysis as it applies to reconnaissance satellites.

ICBMs and the USAF

The first formal display of interest by the US Armed Forces in ballistic missiles predated Bernard Schriever's entry into the Scientific Research and Development field, but only by a slim margin. The initial impetus in fact came from General of the Army 'Hap' H Arnold, who, as was shown in Chapter 2, was well acquainted with Schriever. At the end of his US Army Air Forces career, and inspired by the report on future technologies prepared for him during 1944-45 by the Scientific Advisory Group (SAG) under the leadership of Professor Theodore von Karman, [236] Arnold began to advocate for the development of long-range missiles and satellites.[237] Schriever arrived at the Pentagon in January 1946, and almost immediately, Arnold summoned him and put him to work. His first appointment was in the Scientific Liaison Office of HQ AAF (soon to become HQ USAF). This office provided the secretariat function to the Science Advisory Board (SAB), the successor organisation to Von Karman's SAG, but after General Arnold's retirement, the SAB was soon fighting for survival. Schriever recalls

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[236] Theodore Von Karman, *Towards New Horizons: A Report to General of the Army H H Arnold submitted on behalf of the AAF Scientific Advisory Group by Theodore von Karman* (Washington DC: AAF SAG, 1946) was requested by Arnold in December 1944, completed in December 1945 and circulated as a classified document in 1946. Three volumes (vv 8-10) bear on various facets of missile technology.

[237] Robert Perry dates Arnold's first statement to this effect to November 1945, though also noting the US Navy's parallel interest in a satellite programme during October 1945. See Robert L Perry, *Origins of the USAF Space Program 1945-1956 (AFSC Historical Publications Series 62-4-10)* (Air Force Systems Command, 1962) vi

attending the first SAB meeting in 1946 and supporting its work during 1946-49; it would be some time, however, before ICBMs were its most pressing concern; several attempts were made within the USAF to dissolve it.[238] The early USAF enthusiasm for ballistic missile development soon waned, as evidenced by the cancellation on budgetary grounds of the Convair MX-774 missile programme in 1947 – at the time, the only long-range ballistic missile in development.[239] After his year's attendance at the National War College in 1950, Schriever returned to the Pentagon to direct the Development Planning Office, to find the outcomes of the (civilian) Ridenour report of 1949 and the parallel (military) report of the Anderson committee had led to major rearrangements within the Research and Development community. Air Research and Development Command (ARDC) had been created during 1950-51 to provide a focus for research and development activity within the USAF. Additionally, in 1951 the USAF re-entered ICBM development with the initiation of the MX-1593 programme, a low-rate development contract placed with the Consolidated-Vultee (Convair) Corporation. Although this was a significant milestone, the technical problems that had beset the MX-774 had not been solved and progress was slow.[240]

In 1954, the USAF ICBM programme received a boost from the complementary outcomes of the committee referred to as the 'Teapot' committee headed by John von Neumann, (properly, the Strategic Missiles Evaluation Committee and sometimes also simply known as the 'von Neumann Committee'), and a parallel report from the RAND Corporation. Their validation of the practicality of an ICBM, and the need to develop one as soon as possible, led to the formation of the Western Development Division (WDD) within ARDC. Charged with developing a practical ICBM as soon as possible, Schriever was appointed its inaugural, and only, commander.[241]

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[238] For Schriever's role in the early SAB, see Jacob Neufeld (ed.), *Reflections on Research and Development: An interview with General Bernard A. Schriever and Generals Samuel C. Phillips, Robert T. Marsh, and James H. Doolittle, and Dr. Ivan A. Getting, conducted by Dr Richard H Kohn* (Washington DC: Center for Air Force History, 1993) 35. For details of the early tribulations of the SAB, see Gorn, *Harnessing the genie* 45-50.

[239] In 1947, the principal technical reason cited for cancellation of MX-744 was the inability to develop a missile of sufficient range using then available fuels and rocket motors, but in 1957-58, Schriever testified to Congress that concerns over warhead yield and weight were major problems. For an outline of the cancellation of MX-744, and the state of ICBM research during this period more generally, see Beard, *Developing the ICBM* 53-57

[240] Ridenour and Anderson were not the only committee chairmen advocating for improved R&D organisation. The work of the Findlater Commission was also notable. For more details of the genesis of ARDC, see Dennis J. Disch Minnie L. Weaver John J. Stanley, *An Air Force command for R & D, 1949-1976* (Office of History, Air Force Systems Command, 1976) 8-11

[241] John L. Frisbee (ed.), *Makers of the United States Air Force* (USAF 50th Anniversary edn., USAF warrior studies, Washington, D.C.: Office of Air Force History, U.S. Air Force, 1995) 290

Ballistic Missiles – Schriever and Technical Progress

On 2 August 1958, Schriever's desk diary gives a rare insight into his personal feelings. The entry in its totality reads:

"1. 4 Years complete today.

2. Atlas 4B. 17:16EST successful – we did it.

First Free World ICBM – all three engines."

Schriever is commenting on the successful Atlas test flight achieved that day, the tenth such attempt (preceded by six failures and three that had reached their limited goals) of the programme.[242] It was the first time, as he notes, that an Atlas had been successfully launched in its intended three-engined configuration.

It would be another year before the Atlas would be declared operational in USAF service, but as far as Schriever was concerned, his major effort relating to it was now complete.[243] He had achieved his goal significantly quicker than the Teapot Committee had thought possible, and even slightly ahead of the RAND estimate.[244] Meanwhile, the WDD had simultaneously assumed responsibility for delivery of the Thor Intermediate Range Ballistic Missile, which entered service in late 1958, initiated development of the Titan ICBM as a backup system in the event of Atlas failure and begun work on the Minuteman solid-fuel ICBM as a follow-on system. Under Schriever, it had also assumed responsibility for much of the early US military space effort. Against this backdrop, let us now examine Schriever's contributions in detail.

The first thing to make clear is that in running the Atlas programme, Schriever was managing a family of industrial contractors. He was quite sure not only that this was the case, but that it was in essence a good thing. Speaking in 1957, he noted:

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[242] Diary entry for 2 August 1958 in *Schriever Archive, Box 5, Folder 9.* Underlining in the original. For a listing of Atlas test firings, see Walker and Powell, *Atlas* 265.

[243] It is telling that on the date that Atlas was declared operational (31 October 1959), Schriever appears to have made no comment in his diary. There is no diary page with that date, and the dominant themes for the October entries that do exist are future aerospace systems – Atlas is not mentioned at all. *Schriever Archive, Box 3, Folder 9.*

[244] The Teapot Committee had predicted development to take 6-8 years, i.e. likely to deliver an operational missile during 1960-62 while internal USAF estimates in 1953 had suggested 1965 as a goal. The RAND report suggested that 1960 was achievable. The full Teapot Committee report has not been declassified, but an extract from it containing the time estimate is in a pack of historical documents prepared for Schriever in 1956. The RAND report is publicly available. See *Schriever Archive, Box 3 Folder 7* and Bruno W. Augenstein, *A Revised Development Program for the ICBM (RAND Report SM-21)* (Santa Monica, CA: The RAND Corporation, 1954) 34

> "...Close cooperation with industry is especially important for the Air Force. Being the youngest of the three Services and operating equipment whose military history spans only a few decades, the Air Force does not have establishments like the arsenals, gun factories, ship yards and similar facilities created by the other Services long ago to develop and manufacture much of their material. Instead, the Air Force has relied, from the very start, on the assistance of industry and has, thereby, gained considerable experience in the management and monitoring of weapon systems contracts.
>
> To keep pace with the rapid expansion of its responsibilities , the Air Forces has commensurately expanded its special organizations for the management of its research and development contracts – the Air Research and Development Command -… Above all, the Air Force's system of contracting for most of its research and development projects has greatly stimulated the growth of research facilities in the aircraft and allied industries, increased the capabilities of industrial and scientific organizations, and enhanced the scientific efforts of educational as similar institutions. All of these factors are now paying off in the Air Force's pursuit of its Guided Missiles effort."[245]

Schriever had in fact placed himself one step further removed from direct involvement with the programme than this quote might suggest. His greatest innovation as an engineering manager at WDD was to insert the Ramo-Wooldridge (R-W) Corporation as a 'system integrator' to manage the overall development programme. His management role thus became monitoring progress against USAF and DoD timelines, including overseeing R-W's work. He faced significant criticism for this innovation for a variety of reasons, but persisted with it.[246] Central to effective prosecution of this role, however, is an engineer's understanding of the problems being addressed, and Schriever's technical grasp of the demands of the ICBM programme was clear from the start.

Sometime in 1954, possibly in September or October, Schriever drafted a briefing to be delivered in 'Rome', outlining WDD's intent in the management of the Atlas programme.[247] Schriever's handwritten notes make clear his understanding

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245 Bernard Schriever, Presentation at the Annual Meeting of the Institute of Aeronautical Sciences, New York, NY, 29 January 1957. Press copy of speech in *'Earlybird' Files, Boxes 110-111, National Security Archive, George Washington University, Washington DC*.

246 For details of the concerns relating to R-W's position within Atlas, see for example Neufeld, *Ballistic Missiles* 125-26

247 *Schriever Archive, Box 3, Folder 9.* From the briefing content, it can be inferred it was intended to take place at the Rome Air Development Center at Rome, NY rather than in Italy. The notes are simply titled 'Rome Presentation', and are mainly undated, although reference is made within them to a meeting during 21-22 September 1954. There are plainly several drafts of the same presentation contained in the source, as Schriever adjusted its content; the analysis here draws on a composite of the various drafts. Schriever's desk diary does not indicate when the presentation was delivered, but it does highlight a meeting of the Scientific Advisory Board in Omaha, Nebraska during 27-29 September 1954, where Schriever also spoke on similar lines. It appears possible either that the

of the technical challenges that would be faced by the programme. He outlined the technical advances that had led to the initiation of Atlas, and the further advances that would be required. The principal enabler had been the breakthrough in lightweight thermonuclear warheads that had occurred in 1953. Specific warhead weights are not discussed in the Rome presentation, but Schriever has made the marginal note '23 000ft/sec' in this section. This is the speed required of a ballistic missile with a range of about 6000 miles, a speed/range combination in common discussion at the time. The implication is that Atlas would require the performance to accelerate one of the new lightweight warheads to this speed. He notes the advantages of the lightweight warhead, improved missile performance from the reduced weight for a given yield and the relaxation in accuracy allowed because of the greater yields possible. He discusses the speed profile of warhead re-entry into the atmosphere and how it would affect heat-dissipation. CEP figures of 3 and 5 miles are noted as significant and Schriever analyses the total number of weapons that would be required for different levels of damage to a reference target set at each CEP.[248] Schriever also notes that supplies of fissile material should be adequate to support the warheads by 1958-60.

Schriever then lists technical challenges still to be overcome. Within the various drafts in his notes, he highlights the need to settle on the configuration of the missile (number of stages and number of engines per stage), the problems of nosecone design (referring to protecting the warhead from heating during atmospheric re-entry), the guidance options that might be possible (he compares 'radio/radar' with 'control', a synonym for an early inertial navigation system, and the various hybrid solutions that these might yield), the challenges posed by engine and missile body design and the need for various test facilities during the development programme. This is a comprehensive list of the challenges he would face over the next few years and it is plain that he had a good grasp of how they might be addressed.

One way of measuring Schriever's engineering involvement in Atlas development is to look at the periodic reports he gave to his senior management and other audiences during the development period. We will review the administrative methods by which Schriever gave direction to his team shortly, but for now we will establish that throughout, he retained a sound understanding of the

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meeting venue changed or that Schriever delivered essentially the same presentation twice. For the desk diary, see *Schriever Archive, Box 3, Folder 17.*

[248] 'Circular Error Probable' (CEP) is a common metric for weapon accuracy; it represents the radius within which 50% of the weapons fired at a given target will lie. Smaller CEPs equate to more accurate systems.

technical issues being addressed. He invariably prepared the briefings personally – almost all the notes for them are in his own handwriting. They plainly show his grasp of the engineering challenges being faced during this complex project. During April and May 1955, Schriever delivered two significant progress reviews.[249] The first may have been to the Secretary of the Air Force (the draft presentation notes only include the date, not the recipient and Schriever's desk diary is very sparse in 1954-55), the second was definitely to the Air Force Council, but taken together they provide a comprehensive review of progress. Schriever notes the analysis conducted of 'stage and a half' versus two-stage missiles,[250] early work on warhead design, scoping of the requirements for test facilities later in the programme, and early work on guidance systems. Again Schriever is plainly in command of his brief, but he makes his own philosophy clear in a note for the April presentation: "Judgement factors – I'm not the expert – hundreds of man-months by high caliber teams".[251] He then lists the multiple service and commercial organisations forming his community of experts.

In 1956, Schriever continues his round of reports and presentations. The record of these briefings becomes harder to read for four reasons. Firstly, classification of the records in the Schriever archive occasionally intrudes – most of the notes relating to a briefing to the von Neumann Committee in January 1956 are withheld due to their content. Secondly, the briefings appear to have become slightly more elaborate, and make greater use of charts displayed to the audience, copies of which do not survive. Thus analysis of the content of the briefings becomes harder, being based only on Schriever's speaking notes (though these are plainly still his own work). Thirdly, the programme itself had become more complex, involving parallel development of Atlas and Titan missiles, and the addition of the Thor intermediate range ballistic missile to the WDD portfolio. Finally, Schriever devoted increasing time and effort to briefing other projects, specifically the early space systems that were beginning to show military promise.

A good snapshot of the state of the overall effort was delivered to the Air War College at Maxwell AFB by Schriever on 16 May 1956. Here Schriever is

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[249] *Schriever Archive, Box 3 Folder 10.*

[250] Despite the clear theoretical advantages, there was considerable technical concern at the time about igniting a second stage during flight. Atlas therefore employed a 'stage and a half' design where the missile launched with three engines running (a 'sustainer' and two 'booster' engines). During flight, the two booster engines were jettisoned to save weight, leaving the sustainer engine to power the missile for the remainder of its powered phase. The Titan missile, introduced as an alternative to Atlas to provide design redundancy, employed a true two-stage configuration. See also Chapter 2, p37.

[251] *Schriever Archive, Box 3, Folder 10.*

plainly speaking as an engineering manager, rather than a hands-on engineer. Aside from the recent acceleration of the missile programme, Schriever identifies four stages that he sees the missile programme passing through. The first had been completed in 1954 and comprised the organisation of WDD and analysis of missile requirements. The second phase, also completed by this date, had been the selection of contractors to lead on development of the various components of the missiles. The third stage, which he describes as the 'hi-level' stage had been the evaluation leading to the acceleration of the programme during the summer of 1955. The fourth stage, which was in progress as he was speaking, was fabrication and test of the missiles themselves, which would lead to operational capability.[252] Schriever says little in his notes about the performance of the missiles under test at the time, but he does expand on the technical features of the design that would enhance the operational utility of the missiles. Towards the end of his address, he discusses the possible vulnerabilities of a missile barrage. Having noted its short time of flight overall, he remarks that detection of a missile attack would nonetheless be possible. He then points out that incorporation of 'chaff' – strips of aluminised foil designed to confuse radar to be deployed by the incoming warheads – or blowing up the redundant missile bodies or varying the re-entry trajectories would all act as potential protection for the warheads.[253]

By 1957, the tone of Schriever's briefings are changing and the technical content relating to missiles is reduced in favour of comment on other topics such as the space programme. Nonetheless, he was still briefing at the highest level. On 11 January 1957 he participated in the annual Department of Defense briefing to the National Security Council (NSC) on the Ballistic Missile Program. The NSC minutes are a rare record of a briefing by Schriever compiled by the recipients.[254] The technical portion of Schriever's contribution was evidently brief, and is summarised as follows in the NSC Minutes:

> "…General Schriever then indicated the major difference between the Atlas 1 ½ -stage missile and the 2-stage Titan missile. In so doing he was assisted by models of these missiles, which had been brought to the Cabinet Room. He predicted that the first launching of the flight test missile Atlas would occur in the course of 1957. The first guided flight of an Atlas missile was likely to occur in 1958. The corresponding dates for tests of the Titan missile would be approximately one year behind the similar

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252 Schriever elaborated on this series of necessary steps, increasing it to a five-step series in a nearly contemporary article. Bernard Schriever, 'The USAF Ballistic Missile Program', *Air University Quarterly,* Vol IX No 3 1957)

253 *Schriever Archive, Box 3, Folder 11.*

254 *Schriever Archive, Box 3, Folder 12*. Memorandum of Discussion at the 309th Meeting of the National Security Council, Washington, January 11, 1957, (Washington DC: www.history.state.gov)

developments on Atlas…"

The correspondence between Schriever's notes and the NSC minutes is very clear, giving confidence that his notes were used as drafted.

From this point on, Schriever's briefing notes become sparse and then non-existent in the Library of Congress archive – there are no briefing folders retained for 1958-60. He was, however, making an increasing number of public speeches, but without his technical grasp of the programme, and his determination to meet engineering deadlines to schedule waning in any way.

The last aspect of Schriever's engineering management techniques to be considered is how he co-ordinated the development effort and tracked progress across the country. As noted briefly in Chapter 2, Schriever made two major innovations in his technical management techniques to meet the demands of the programme: he instituted a comprehensive management information system to feed data from the burgeoning number of contractors involved in the programme directly to his Headquarters in California, and he orchestrated monthly review meetings (informally known as 'Black Saturday' meetings since they invariably took place on the first or last Saturday of a calendar month). Schriever plainly recognised their innovative nature and described them thus:

> "The ballistic missile program is nationwide in all aspects. The work of seventeen major system contractors located in every part of the United States has to be coordinated and kept in phase… To keep abreast of the entire program, the efforts of all the members of the management-development team are closely monitored in a central place in BMD-the Program Control Room. The joint BMD-BMO-R-W Program Control Room is the nerve center for the project. As a management tool it provides "management visibility" by displaying information on the status of every aspect of the project in graphic form. This management information is provided through frequent visits with contractors, use of the extensive communications network between BMD and the contractors and field offices, written reports, and periodic meetings of BMD, BMO, and R-W personnel…Any problems are spotted early and acted upon quickly. [They] are given immediate treatment and their status is considered each time a review is made.
>
> The program pulse is felt continuously. It is presented formally once a month to key members of the management team. In these presentations, the rule of "management by exception" is followed. There are hundreds of items that could be considered. It would take several days to treat them all. Instead, only progress and problem areas are noted and discussed."[255]

[255] Schriever, 'The USAF Ballistic Missile Program', (*Air University Quarterly)*.

The importance of both processes was underlined much later, when Stephen Johnson interviewed both Schriever and one of his staff, Charles Terhune, for a journal article.[256] Johnson noted:

> [fn46] "Some historians have questioned whether the Control Room served any real purpose aside from showmanship. My conclusion is that some of the people in the WDD saw that it had "show" purpose, and concluded that is all that it was for. After interviewing Charles Terhune, I am convinced that it served a real purpose in giving the "official" status at any time, and in providing a central information repository. [See Charles Terhune, telephone interviews with author, Sep.-Oct. 1998, Washington, D.C.; Schriever, interview with author, Mar. 4, 1999].
>
> [fn47] Schriever believes that the idea for "Black Saturday" was his own, because his philosophy of management was to dig out the problems, and not to spend time on the status of things that were going well. He did not recall seeing this practice on any other programs at that time. It also seems that Schriever's group soon began to use computers for the MCS, with Ramo-Wooldridge. This would make it one of the earliest applications of computers to management communication." [257]

Johnson's assertion about Schriever's early use of computers can also be validated from WDD correspondence. On 25 January 1955, Schriever noted in a memo to Colonel Morris (the head of the Air Materiel Command Purchasing Office at WDD) "I wish to make a matter of record the importance of proceeding with the computer facility…as rapidly as possible". On 28 January in a subsequent memo on the same topic to him he added "I have discussed [the construction of the computer facility] in detail with Dr Wooldridge…".[258] This is a relatively visionary view of the utility of IT in a management environment for early 1955.

Ballistic Missiles – Schriever, Policy and Strategy

In this section this thesis looks at Schriever's attitudes to the policy issues relating to nuclear weapons and ICBMs, to his understanding of their broader context, and to his recognition of wider issues falling within 'policy space' as defined in Chapter 1.

On the date of its creation in September 1947, the USAF was the only nuclear-capable armed service in the world, but even by then, the CIA was predicting that the USSR would achieve an equivalent capability sometime in the 1950s. There was thus no significant debate within the fledgling USAF about the need for nuclear weapons, only about the means for their delivery. The evident

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[256] This is the same Stephen Johnson who's PhD thesis was referred to in Chapter 3.
[257] Johnson, 'Bernard Schriever and the Scientific Vision', (*Air Power History)*.
[258] Both memos in *Schriever Archive, Box 3, Folder 6.*

potential of ballistic missiles had been demonstrated during World War 2, validated in US eyes by the Von Karman studies and others, and only the difficulty of matching nuclear warhead dimensions and weight to a missile of sufficient range alluded to above, hindered their development. There was, however, the issue of which service or services should possess and manage them. This debate was largely fought out during 1945-47 as defence staffs struggled to migrate from a wartime to a peacetime (but Cold War focussed) footing, as the USAF emerged from the Army's shadow and, from Schriever's point of view, as they sought to learn the scientific and technical lessons of the preceding conflict.[259]

Schriever was evidently unsure about some of the decisions made; he made his own feelings clear about both the strategic insights and follies of this period during his USAF oral history interview in 1973, when he reflected on the balance between nuclear and conventional force development (what he describes as equipping for 'limited wars') at the time:

> "…during the period right after World War II when we had a monopoly on the nuclear bomb and we certainly could deter the rest of the world with our strategic forces, we became completely nuclear weapon minded and oriented, I think that was quite natural…We really didn't get to seriously thinking about [limited wars] until Korea hit us. We had a period where we were thinking about air defense against a strike from the Soviet Union and our own strategic strike capability against the Soviet Union, and that's about all."[260]

Schriever had no doubts, however about the need for deterrence. Later in the same interview, he noted:

> "I don't see that there was any arms spiral on account of that. Each country had to build its ICBM force. I think the ICBM plus the submarine launched missiles really have created a standoff so to speak in strategic forces, which, from the standpoint of an all-out war, has reduced, I think, the probability of an all-out war by a very significant factor. The fact that we can't knock out their forces and they can't knock out ours without suffering a very, very devastating retaliatory strike has a tremendous deterrent significance. I said during the time of the ICBM program that the reason I feel the ICBM is so important and the program is so important is because it's the weapon system that is most likely not to be used. That's what deterrence is all about."[261]

This is hardly an unexpected view from someone in Schriever's position; it reflects the conventional views on nuclear deterrence from the time. Even Air

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[259] For a clear exposition of the positions taken by the various parties in this period, see Neufeld, *Ballistic Missiles* 17-23

[260] Bernard Schriever, interviewed by Major Lyn R Officer and Dr James C Hasdorff on 20 June 1973 for Office of Air Force History, HQ USAF

[261] Schriever, 'USAF Oral History Interview', 10..

Force Doctrine provided support for Schriever's wish to develop the ICBM within the USAF. Within its 17 (total) pages, the first edition of AFM 1-2 'USAF Basic Doctrine' noted:

> "Weapons systems must be constantly evaluated in terms of their capability of exercising a decisive influence toward the accomplishment of the objective…The effect of the advent of this [modern air] force in the conduct of war is to make modern war total – Its threat more imminent, its impact more sudden, its expanse more extensive, and destruction exceedingly more devastating…The establishment of adequate air forces in-being calculated to be decisive is, therefore, the paramount consideration for the security of the United States."[262]

Essentially, Schriever was pushing on an open door in seeking to develop the USAF's deterrent capability, though fierce arguments nonetheless raged internally as to what form that deterrent force should take. Schriever had, however, much more work to do in other areas of policy space. He had to face the demands placed on him to deliver a potentially operational system, not just a piece of hardware. This was felt important enough to be mentioned explicitly in direction issued to ARDC and WDD. Firstly, General White, Vice Chief of Staff of the USAF directed Lieutenant General Power, the commander of ARDC:

> 1. The immediate goal of the ICBM effort is the earliest possible attainment of an initial operational capability. To achieve this goal, it is essential that a Development Plan, specifically encompassing an initial operational capability, be implemented with minimum delay.
>
> 2. An initial operational capability is envisaged as one which would provide a capability of operationally employing prototype weapons during the latter phase of the development program. It should include one or two prototype operational bases.
>
> 3. Initially, the ICBM will probably incorporate certain marginal technical features. Early systems undoubtedly will undergo a great deal of revision and change as the development progresses. These developmental considerations will have direct influence over many aspects of operations, training, logistics, etc., as related to the initial operational capability. For these reasons, early implementation of the Development Plan mentioned above will require flexibility of action and singular direction. It is believed this direction can best be provided by your Command.[263]

General Power then passed on this direction to Schriever:

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[262] AFM 1-2 USAF Basic Doctrine, (Washington DC: US Government Printing Office, 1953) 5-6, 17. This first, brief, publication was not well received – for a critique of early USAF doctrine writing efforts, see Dr James A Mowbray, 'Air Force Doctrine Problems: 1926-present', *Air Power Journal,* Vol 9 No 4 Winter 1995). However, note that this 'basic' (at the time the accepted term for 'strategic') publication clearly supported possession and maintenance of 'decisive' systems such as ICBMs.

[263] Memo dated 18 November 1955 VCAS USAF to Cdr ARDC. *Schriever Archive, Box 3, Folder 7.*

> My memorandum to you dated 29 July 1954 stated that the primary mission of the Western Development Division is, "To manage the development program for the Atlas Weapon System, including ground support and the development of operational, logistic, and personnel concepts." As a result of recent letter instructions received from Headquarters USAF, this mission is expanded to include the development of an initial operational capability for the Intercontinental Ballistic Missile (ICBM) as soon as practicable.[264]

Thus he needed to regard the ICBM system as the sum of all its component parts and capability, even including initial basing. Writing in 1957, Schriever remarked:

> "from the beginning BMD [the Ballistic Missile Division] had a staff to study system operational planning. With the addition of the responsibility for initial operational capability, the operational planning staff has expanded considerably in both size and mission. From it have come not only the operations, personnel, logistics, and installations concepts which furnish the guidelines for the organization and employment of the IOC force, but the actual detailed plans that are at the present time being put into effect"[265]

He also noted the innovative nature of these arrangements:

> "Assignment of the initial operational capability to WDD was, therefore, a departure from the established procedure of limiting ARDC's responsibilities to research, development and testing of new weapon systems."[266]

He held to this view in later life too. During an interview in 1977, he noted in the context of ICBM development:

> "and, of course, paralleling all of this is the need to train people way in advance of when they go in the field, the need to develop your whole logistic system, your support system and so forth … supply and maintenance and the whole works."

Schriever also confirmed the equal status of 'soft' issues in his eyes by noting that he charted their progress alongside hardware development:

> "It was a chart about as long as this room and we had all of the important elements in it. The R&D, then we had each one of the major subsystems, the logistics. the training - everything in a conceptual. way with objective dates on the chart that we would have to achieve certain things in order to reach the end objective of having an initial operational capability."[267]

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[264] Memo dated 14 December 1955, Cdr ARDC to Cdr WDD. *Schriever Archive, Box 3, Folder 7.*
[265] Schriever, 'The USAF Ballistic Missile Program', *(Air University Quarterly)*
[266] Bernard Schriever, Presentation at the Annual Meeting of the Institute of Aeronautical Sciences, New York, NY, 29 January 1957. Press copy of speech in *'Earlybird' Files, Boxes 110-111, National Security Archive, George Washington University, Washington DC*.
[267] Both quotes from Bernard Schriever, interviewed by Dr Edgar F Puryear Jr on 29 June 1977 for USAF Oral History Program.

This highlights that Schriever had to pay particular attention to the non-hardware aspects of the ICBM program, and that he regarded such issues as equally important in fulfilment of USAF strategy and national policy.

Ballistic Missiles – Schriever and Administrative Action

The final aspect of Schriever's management of the Ballistic Missile Programme that should be considered is how he shaped administrative practices and procedures to achieve an early operational capability. The areas he manipulated to achieve this overlap to a significant extent – they include Schriever's engineering management techniques and use of management information systems, which arguably straddle the policy space and the administrative space. The administrative areas worthy of study in this section are Schriever's understanding of 'concurrency' (as described in Chapter 3), the action taken by Schriever to prioritize the ICBM programme within government, and the specific budgetary processes (the 'Gillette' procedures also outlined in Chapter 3) that provided the means for Schriever to circumvent bureaucracy and achieve timely progress. According to Major General Osmond Ritland, who served as Schriever's deputy on several occasions during his career, these actions were presaged by the recommendations of the Von Neumann (Teapot) Committee report of 1954. He wrote:

> "the Von Neumann Committee made certain recommendations, which the US Air Force adopted: (1) to take maximum advantage of the scientific and technical state of the missile art and advance that art as quickly as possible, (2) to proceed with all aspects of the program concurrently, and (3) to streamline administrative and control procedures".[268]

As shown in Chapter 3, the origins of the term 'concurrency' are contested; Foote asserts that Schriever was the first to use the term explicitly, and certainly, his 'Combat Ready Aircraft' study of 1951 is significant in its development and application within the USAF. The Von Neumann recommendation promoted a concurrent approach due to the extremely stringent time constraints associated with the programme, but Schriever also saw it as a cost-saving measure:

> "I contend that we did the missile programs at a lot less cost under the concurrency management approach than if it had been done by the so-called fly-before-you-buy thing. The indirect costs that are associated with these major programs always run as high as direct cost, so you've got an even split, and your indirect costs keep going. There is no way that you can save money by having your programs stretched out by two, three or four

[268] Osmond J Ritland, 'Concurrency', *Air University Quarterly,* Vol XII No Winter-Spring 1960-61 1960) at 241

years."[269]

Whatever the ultimate rationale, Schriever supervised the concurrent approach to system development throughout his management of ICBMs. Perhaps underlining the budgetary effect he attributed to it, he made specific mention of it when briefing the 'Mahon' committee (the Department of Defense sub-committee of the House Appropriations Committee) on 26 February 1957: "Earliest Attainment – REAL OBJECTIVE FORCE IN BEING – How [?], Integration with development…Concurrent Planning and Actions".[270] He spoke at greater length on the topic in January 1958, when he addressed the Economic Club of New York, devoting about half of his remarks to explaining 'concurrency' to a business-oriented (but not defence-focused) audience. He began by emphasizing the difference between concurrency and the customary sequential approach, presenting it as a 'break with tradition'. He noted that it applied equally to hardware development, eschewing "fashion[ing] hand-wrought prototypes before venturing into production tooling", and to integrating the other aspects of the system "spending our funds, concurrently, on research, development, testing, production, manpower training, base construction and other phases of our program". He also emphasized the synergistic nature of this concurrency; "all elements act, interact and re-act with each other, with constant feedbacks from each element of the cycle to all other elements."[271]

Schriever's endorsement of concurrency thus seems consistent through the critical development period of the ICBM. But that period was critical for other defense systems too. Without prioritization, ICBM development would have fought for resources against other programs, and progress would have been slowed to an unacceptable extent. Although the greatest credit for the acceleration and prioritization of the ICBM program lay with civilians, Schriever played a key part too.

As noted earlier, much of the impetus for the ICBM program had come from the report of the Teapot Committee. That Committee had been established in 1953 by the Undersecretary of the Air Force for Research and Development, Trevor

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[269] Schriever, 'USAF Oral History Interview', 11.. Schriever's response was given in the interview in relation to the Minuteman ICBM, which followed Atlas and Titan into service, but its context makes clear that he applied it overall to the ICBM programme.

[270] Schriever's briefing notes for the meeting (emphasis in original), *Schriever Archive, Box 3, Folder 12*.

[271] Bernard Schriever, *'Equation for Survival: Lead Time equals Leadership'*. Presentation at the Annual Meeting of the Economic Club of New York, NY, 21 January 1958. Press copy of speech in *'Earlybird' Files, Boxes 110-111, National Security Archive, George Washington University, Washington DC*.

Gardner, who then oversaw the revival and acceleration of the Atlas programme, including the creation of WDD. Roughly simultaneously, President Eisenhower commissioned a report from the Technological Capabilities Panel, chaired by Mr. James Killian, his Special Assistant for Science and Technology into a variety of Defense issues.[272] When its report emerged in February 1955, it included two specific recommendations that would affect Schriever and the affairs of WDD. Firstly, it recommended "that the development of an intercontinental ballistic missile (with about 5500 nautical mile range and megaton warhead) continue to receive the very substantial support necessary to complete it at the earliest possible date". Secondly, it recommended that "There be developed a ballistic missile (with about 1500 nautical mile range and megaton warhead) for strategic bombardment…"[273]. This in itself said little about the priority of the ICBM, so in July 1955, Gardner, von Neumann and Schriever briefed the President on the status of the ICBM programme. The precise content of Schriever's briefing cannot be determined – but he gave an outline of its context in an oral history interview:

> Trev Gardner, and myself and Johnny von Neumann briefed the President on the program. ... And the President was aware of the program but had never really been given [a] detailed briefing on the thing and the briefing was supposed to have taken an hour, I think and we were there for damn near two hours with all the questions and everything. The President had quite a few questions…Well, the result of that was the program was given the highest national priority. … so we got the priority, [Secretary of Defense] Wilson approved a streamlined procedure in the Pentagon in which he set up the Ballistic Missile Review Committee, … where he himself sat as chairman.[274]

Schriever also testified to the importance he attached to the briefing when he gave an interview in 1964 to Mr. John Loosbrock, which was included in Dr Ernest Schwiebert's quasi-official history of the ICBM programme. In response to the question "From your own memory of ten years of involvement with the ballistic-

[272] For details of the interaction between the various reports and committees on this topic see Max Rosenberg, *Plans and Policies for the Ballistic Missile Initial Operational Capability Program* (Washington DC: USAF Historical Division Liaison Office, 1960) iii-iv

[273] "*Meeting the Threat of Surprise Attack*". Report by the Technological Capabilities Panel of the Science Advisory Committee to the President of the United States (The Killian Committee Report) dated 14 February 1955. Available at www.history.state.gov , accessed 2 May 2015.

[274] Schriever, 'Interview with General Bernard A. Schriever, USAF (Retd) 29 June 1977', 4-5.. Although not quoted directly here, Vince Ford's account of this meeting in his putative memoir of ICBM development also provides context. In particular, Ford noted that Vice-President Nixon took a keen interest in the briefing, and advocated for development of a rapid operational capability. Ford additionally cites an encounter between Nixon and Schriever in Nixon's office in the Capitol where he questioned Schriever keenly to improve his understanding of the ICBM programme. *Schriever Archive, Box 184, Folder 10 (Ford MS, Dictabelt 123).* Many years later, in 1979, President Nixon would offer the post of NASA Administrator to Schriever (among others). See T. A. Heppenheimer, *The space shuttle decision: NASA's search for a reusable space vehicle (NASA SP-4221)* (Washington, DC: National Aeronautics and Space Administration, NASA History Office, Office of Policy and Plans, 1999) 115.

missile program, is there a single event or a single person that stands out in your mind?", Schriever, after nominating John von Neumann and Trevor Gardner as persons of note said: "The one event that stands out is the first time that von Neumann and Gardner and I briefed President Eisenhower in July 1955. This briefing led to the establishment of the ballistic-missile program as the number-one national-priority effort…"[275]

The only significant concern that Schriever had about the outcome of the Killian report was the priority it also attached to other projects, most specifically the Intermediate Range ballistic missile – this was the 1500-mile range weapon mentioned above which would become the Thor. Schriever had made his initial concerns clear in a memo for the record composed in December 1954. This paraphrased a message he had sent that day to General Power, in which he said he voiced concern about dilution of industrial expertise and fostering undesirable competition for test facilities between various missile projects. He also feared that this expansion of the programme would allow other services, or even the DoD centrally, to exert undue influence on its execution.[276] He then expanded on these ideas in a memo that he probably composed to General Power in September 1955.[277] The author refers to general concerns about 'interference' between the ICBM and other programmes, specifically mentioning the TBM (tactical ballistic missile), though the memo later expands on range and categories of missiles, the 'scientific satellite' and the 'military satellite'. He then expands on the nature of that interference, seen as most likely to be conflict over access to industrial contractors and to test facilities needed by multiple programmes, rather than through dilution of management. Schriever was eventually forced to manage the Thor programme in parallel with Atlas; since it could be brought into service quicker than the ICBM it served to address US security needs, even though its restricted range meant it would have to be based in Europe to threaten targets in the USSR.

Schriever's final significant achievement relating to ICBMs in administrative terms was the introduction of devolved budgetary arrangements to streamline procurement. He described the pressing need for such arrangements and the means he employed to achieve them:

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[275] Schwiebert, *A history of the U.S. Air Force ballistic missiles* 21-22. At the time of writing, Dr Schwiebert was the official historian of Air Force Systems Command, and Schriever was its Commander.

[276] Bernard Schriever Memorandum for Record *Interaction of TBMS with ICBM* dated 20 December 1954. *Schriever Archive, Box 3, Folder 5.*

[277] The memo, in *Schriever Archive, Box 1, Folder 8,* is held only in carbon copy and cannot definitely be attributed to Schriever – it is addressed to General Power, dated 23 September 1955, but is unsigned. Context and style, however, suggest that it is Schriever's.

"From a management sense, we had to clear away the bureaucracy above us or else we never would have been able to get it done. And it was obvious to me that after we got the program started, there was no way that I could get the job done unless we streamlined the decision making process and I made … a flow chart of all the places I had to touch base and get decisions made in connection with the program. I brought that briefing in, I gave it myself with the little document which showed all the places I had to go and briefed Trev Gardner… he said, come on we're going down to see Don Quarles. Don Quarles was at that time Assistant Secretary for R&D at OSD level …We went down to his office … We didn't have a previous appointment. But Trev went storming in there and said, Don you've got to see this and Quarles never sat down. I … went through the whole chart exercise and he stood there and didn't say anything … and Quarles said we've got to do something about it. So, he said, Trev, you set up a study group to go into this thing in detail and come up with specific recommendations as to what should be done. Well he set up Hyde Gillette … who was a deputy assistant secretary on the financial side Well we loaded the study group pretty much with people who knew and who would come up with the right answers and it was called the Hyde Gillette study, they came up and it took them a couple of months to do it but it was done very rapidly."[278]

Jacob Neufeld confirms both their origins and effects:

> "…Gardner established a committee under Hyde Gillette, the Air Force deputy for budget. With Schriever's guidance, the committee devised a set of administrative procedures which made WDD solely responsible for planning, programming, and directing ICBM development…Overall, these Gillette Procedures cut the number of review levels from 42 to 10."[279]

Ballistic Missiles and Schriever – an Assessment

This account of Schriever and the ICBM began by noting Schriever's reaction to a major milestone in ICBM development – the first successful flight-test of an Atlas missile in its intended operational configuration. Within 18 months, Atlas had become an operational system, delivered in accordance with the system for operational capability development that Schriever had pioneered. Although Atlas had a relatively short operational life, the Thor IRBM had entered service alongside it (actually slightly before it), and the Titan and Minuteman follow-on systems represented more operationally useful and long-lived successor systems forming the backbone of the US deterrent capability throughout the 1960s and early 1970s; Minuteman in a modified form remains in service today. By any objective standard, Schriever succeeded in his task.

[278] Schriever, 'Interview with General Bernard A. Schriever, USAF (Retd) 29 June 1977'.
[279] Neufeld, *Ballistic Missiles* 120

Note also that Schriever understood the technical challenges he faced in developing the ICBM, and that within the constraints of the 'system integrator' construct he implemented, he remained engaged with them as a professional engineer, monitoring each issue until it was driven to resolution. Thus it seems reasonable to assert that he oversaw the reduction of technical risk throughout the programme.

Regarding policy and conceptual risk reduction, Schriever did not face significant doctrinal or conceptual challenges in introducing the ICBM into USAF service; conceptually, it was seen as a highly efficient, high-speed, long-range delivery system for nuclear warheads – a 'more effective bomber'. He did, however, understand and address the challenges of maturing the experimental nature of the Atlas into an operational system. This included the challenges of addressing personnel and training requirements, infrastructure design and logistic support issues alongside technical risk reduction. He accorded these matters equal priority alongside technical development, charting their progress in parallel with engineering development and applying his understanding of concurrency as a management tool.

Finally, Schriever shaped the administrative space to enable rapid development of the ICBM capability. He briefed and lobbied at the highest levels to gain the prioritization necessary for his project, having shaped the creation of the organization that would deliver it. He then recognized the administrative friction that would have precluded timely development, and manoeuvred to introduce the Gillette Procedures that gave him the budgetary and administrative autonomy needed to deliver Atlas. Intuitively, he applied pressure along all three of the axes of development identified in Chapter 1 and success ensued. In the second part of this chapter, this analysis will be repeated for another successful initiative, the early USAF attempts to develop a reconnaissance satellite.

The USAF and Early Satellite Reconnaissance

It is not the intent in this thesis to analyse the history of American Reconnaissance Satellites in detail for its own sake. Rather, it seeks to explore Schriever's involvement with the technical, conceptual and organisational aspects of their development. However, this analysis will be much easier to follow after a short summary of how the USAF entered the space-based reconnaissance era.

Theodore von Karman's 'New Horizons' report of 1944 laid much of the groundwork for later ICBM development, but it says nothing about reconnaissance satellites. Nevertheless, early interest by the US AAF and USAF is not hard to find. The first major study into potential satellites – the 1946 'Preliminary Design of an Experimental World-Circling Spaceship' RAND Study – noted that "It should also be remarked that the satellite offers an observation aircraft which cannot be brought down by an enemy who has not mastered similar techniques".[280] The first Secretary of Defense, James Forrestal, made a passing reference to satellites in his first Annual Report, but this was primarily directed at the potential of a geo-stationary bombardment satellite.[281] At about the same time, General Hoyt Vandenberg, then Vice Chief of Staff of the Air Force, staked the USAF's claim to ownership of military satellites: "The USAF, as the service dealing primarily with air weapons -- especially strategic – has logical responsibility for the satellite".[282] Follow-up RAND studies explored many applications of satellites; the most notable for reconnaissance satellites was that produced in 1951 by J E Lipp and others; it analyzed possible orbits and observation mechanisms and likely targets.[283] This work led to a further commission for RAND, which became the Project FEED BACK study.[284] The FEED BACK study made a firm recommendation that the USAF should move to develop a reconnaissance satellite as soon as possible:

> "The over-all conclusion to be drawn …is that reconnaissance data of considerable value can be obtained, and that complete coverage of Soviet territory with such pictures will result in a major reversal of our strategic intelligence posture with respect to the Soviets. RAND has been working on the satellite vehicle for 8 years. During this period the metamorphosis from a feasibility concept to a useful reconnaissance purpose has occurred. Cognizance is now being turned over to the Air Force with recommendation that the program be continued on a full-scale basis."[285]

Submission of Lipp's report in 1951 had coincided with the USAF establishing Air Research and Development Command (ARDC – the 'parent' organization from which WDD would be established in 1954). Among the activities they undertook during 1951-54 were investigations of how a satellite could be powered and of possible sensors for a reconnaissance payload.[286] By December

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[280] Project Rand, *Preliminary design of an experimental world-circling spaceship* 10
[281] Noted in Robert Silverberg, *First American Into Space* (Derby, CT: Monarch Books, 1961) 23
[282] 'Statement of Policy for a Satellite Vehicle' General Hoyt S Vandenberg dated 12 January 1948. Reproduced in David N. Spires, *Orbital futures : selected documents in Air Force space history* (Peterson Air Force Base, CO: Air Force Space Command, United States Air Force, 2004) Vol 1, 19-20
[283] J. E. Lipp et al., *R-217 Utility of a Satellite Vehicle for Reconnaissance* (Santa Monica, CA: The RAND Corporation, 1951)
[284] Perry, *Origins* 33
[285] The RAND Corporation, *R-262 Project FEED BACK Summary Report* (RAND, 1954) Summary, p vii
[286] Perry, *Origins* 34

1953, concerned that multiple studies and research efforts were becoming uncoordinated, ARDC brought them together into one programme, initially known as Project 409-40, the 'Satellite Component Study'. Although there is a clear conceptual link from the original RAND report of 1946 to the operational systems deployed in the late 1950s, the transition from conceptual study to actual project took place in late 1954 and early 1955.

ARDC issued a Systems Requirement Document for an 'Advanced Reconnaissance System' in late 1954, followed on 16 March 1955 by the publication of 'General Operational Requirement No 80' (GOR 80).[287] [288] Three companies submitted proposals in response to this requirement (Lockheed, RCA and Martin; a submission was also sought from Bell Aerospace, but they declined to submit one).[289] The responses formed the basis of 'Weapons System No 117L' (WS-117L)[290] and WDD was assigned responsibility delivering against this requirement; there would be technical overlap with the USAF's missile program, but Schriever was already involved and had already anticipated some of the issues that would arise in developing a useful capability. Ultimately, WS-117L would yield several satellite systems, of which "Corona" would become the most successful. In 1956, ARDC placed a contract with Lockheed for development of WS-117L and the programme began in earnest.

In 1958, WS-117L activity underwent a re-organization with the establishment of the Advanced Research Projects Agency (ARPA). ARPA was formed in February 1958 in order to:

> "[create] a U.S. capability to launch and use spacecraft, after the Soviet Sputnik launch. Subsequently it was given a broader charter, to advance defense technology in many critical areas and to help the DoD create military capabilities of a character that the Military Services and

[287] There are several sources for publication dates for the various ARS/GOR80/WS-117L documents. This section draws on dates in declassified documents published online by the National Reconnaissance Office at http://www.nro.mil/foia/declass/WS117L_Records.html . There are over 1000 numbered documents published in the WS-117L archive (as at August 2015), and many thousands more in related lists, each allocated a unique reference number; they are hereafter referred to as '*NRO WS-117L Archive, Document XX*'. The chronology used here is at *NRO WS-117L Archive, Document 2.* The 1955 draft of GOR 80 is at …*Document 260.* Related NRO archives will be referenced in similar ways – all are accessible via the NRO FOIA website.

[288] Nomenclature for the early systems is complicated by these shifts. 'ARS' is used in some literature to denote the satellite systems prior to assignment of the WS-117L designation, and (less correctly), WS-117L is occasionally taken to refer to the film-return photo reconnaissance satellite only, presumably on the grounds that it was the most successful and best known of the early systems. Further confusion is then engendered by the 'Discoverer' name and cover-story applied to the 'Corona' payload.

[289] *"Chronology of Air Force Space Activity"* (undated)*, NRO WS-117L Archive, Document 52, "WS-117L Development Plan"* dated 2 April 1956*, NRO WS-117L Archive, Document 52.*

[290] The 'WS' designation was a fruit of an ARDC study in 1950 by Major General Gordon Saville, which provided a stepping stone to concurrency. See Daso, *Architects of American air supremacy* 168 The Atlas missile was 'WS-107A'.

Departments were not able or willing to develop"[291]

Management of WS-117L was transferred to ARPA, but, since ARPA lacked (and was not intended to possess) extensive development facilities, it directed the USAF to submit a management plan for WS-117L, and in effect became the top-level management agency for the programme, with the USAF acting as their executive agent to carry out the activity.[292] One of ARPA's first actions (on 28 February 1958, a mere 3 weeks after its formation) was to split the Corona photographic reconnaissance satellite off from the overall WS-117L programme for separate development.[293] Several technical configurations were considered for Corona, but eventually development centred on use of the Thor IRBM as the first stage, with a booster stage developed by Lockheed with a Bell Aerospace rocket motor – collectively known as 'Agena' – incorporating the reconnaissance payload. The system worked by exposing conventional photographic film, which was then ejected from the main satellite in a small body designed to survive re-entry. This re-entry body deployed a parachute and was intended for pick-up in mid-air by a suitably equipped aircraft, which recovered the capsule and its film cargo for subsequent development and processing. The cover story promulgated about the satellite was that it was intended to develop a capability to return objects from space (in 1958, no satellite had ever returned intact from orbit), and utilized the name 'Discoverer'.

Prototype Discoverer/Corona launches took place from January 1959 until August 1960, carrying a variety of payloads, including an early attempt to fly a camera, and two flights carrying experimental sensors for the MIDAS missile-warning programme. Eventually, a capsule was successfully recovered from orbit on 11 August 1960 (carrying only a US Flag, which was then presented to President Eisenhower in the incident described in Chapter 2). The next Discoverer

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[291] Sidney G. Rees, Richard H. Van Atta, and Seymour J. Deitchman, *DARPA Technical Accomplishments: An Historical Review Of Selected DARPA Projects Volume 1* (Alexandria, VA: Institute for Defense Analyses, 1990) 1

[292] *"Chronology of Air Force Space Activities", NRO WS-117L Archive, Document 52, 6.*

[293] Some of the transfer activity was to provide an apparent cover within the USAF for the nature of the satellite mission. Announcements were made about the cancellation of elements of WS-117L which provoked indignation and outrage within the USAF. The programme was then 'reinvented', ostensibly as a scientific research satellite, with the original (but now covert) reconnaissance mission restored. See Kenneth E Greer, 'Corona', *Studies in Intelligence (supplement),* Vol No 17 (Spring 1973), reproduced in Kevin C Ruffner, *Corona : America's first satellite program* (CIA Cold War records; Washington, D.C.: History Staff, Center for the Study of Intelligence, Central Intelligence Agency, 1995). For a detailed description of the development of the cover story, and an evaluation of alternative Corona configurations, see Robert L Perry, *A History of Satellite Reconnaissance: Volume I Corona* (National Reconnaissance Office, 1973). He describes the cover mission and its rationale at p xiii-xiv and the various satellite configurations at 34-39.

mission (launched on 18 August 1960 with the capsule recovered on 20 August 1960) yielded useful photographic coverage of the USSR and Corona became an operational system.

ARPA also promoted the other elements of the WS-117L programme, with varying degrees of success. “MIDAS” was an attempt to supplant the warning of an incoming ICBM attack on the USA provide by ground-based radar with a satellite based launch-warning capability. This used the infra-red emission from an ICBM launch to return a warning to a ground station. Even though no part of the system had to return to Earth, the technical challenges were substantial. Unlike a reconnaissance satellite, which could be launched at a planned time to follow a predictable orbital path and ground track against nominated ground targets, an infra-red warning satellite had to maintain continuous coverage over a large area for an indeterminate time. Along with the limitations of infra-red sensors, which were, and which remain, technically challenging, there were significant engineering challenges in designing a surveillance satellite for the intended mission profile. MIDAS payloads required the greater performance of an Atlas first-stage, coupled with the Agena upper stage developed for Discoverer/Corona. The first (unsuccessful) test launch was attempted on 26 February 1960, and the first operational MIDAS mission took place during May-June 1963. Although it did deliver a limited capability, it never fulfilled its design goals, and effective launch warning was not achieved until the advent of the ‘Defense Support Program’ satellites in the early 1970s.[294]

The other WS-117L system – “SAMOS” – had originally seemed more promising than Corona.[295] It was a reconnaissance system based on onboard development of photographic film and subsequent broadcast of a TV picture of the negative from space directly to a ground station. Although in theory this was quicker and less accident-prone than the capsule recovery technique, there were operational limits imposed by the quality of broadcast achievable. SAMOS launches spanned October 1960 – November 1962, and some carried electronic intelligence payloads as well as cameras. There was a proposed alternative use

[294] Although focussed on the DSP family, Jeffrey Richelson, *America's space sentinels : DSP satellites and national security* (Modern war studies; Lawrence, KS: University Press of Kansas, 1999) provides some background on MIDAS. A detailed description of the technical and management travails of the MIDAS program was given by Dr Rick Sturdevant at the 2010 AIAA Conference. Rick W. Sturdevant, 'AIAA 2010-8812 - From the Pied Piper Infrared Reconnaissance Subsystem to the Missile Defense Alarm System: Space-Based Early Warning Research and Development, 1955-1970', *AIAA SPACE 2010 Conference & Exposition* (Anaheim, CA, 2010).

[295] This was certainly the view of ARPA policy makers. See Robert F Piper, *WDD-AFBMD-SSD Space Programs 1954 - 1966* (AFBMD History Division, 1966) 9

for SAMOS capsules, but although in principle more technically advanced as a reconnaissance system, SAMOS was outclassed and outperformed by Corona as an operational capability.[296]

Satellite Reconnaissance – Schriever and Technical Progress

In many respects, the technical management of the satellite programme mirrored that of the ICBM – indeed for much of the period under discussion, they also overlapped. Schriever was appointed to head the WDD in July 1954, and delivered the Thor missile into service during the winter of 1958-59; Atlas had followed in October 1959. Both missiles, when adapted for use as launchers, were key to the reconnaissance programme as noted above. But there were significant additional challenges posed by satellite operations, and it is reasonable to ask about the understanding Schriever had of them.

The first indication of Schriever's grasp of his new responsibilities is found in a draft memo in his archive. Markings suggest it was prepared by Schriever for Lieutenant General Tommy Power – the commander of ARDC – sometime in late 1955. In it, Schriever highlights both the synergies between the ICBM and satellite programmes (for example the small increase in velocity required to inject a payload into orbit) and the areas where the requirements diverged (specifically highlighting controlling the flight-path of the satellite and supplying power for its payload).[297]

A further indication of Schriever's grasp of the technical challenges he faced is included in the development plan for WS-117L, which he issued on 2 April 1956.[298] Schriever signed the foreword as Commander of the WDD of Air Research and Development Command, and recommended strongly that it be adopted. The plan outlines the specification for a satellite system (at that time favouring the TV-readout method of returning pictures). It includes a proposed schedule for construction and test, a list of facilities required and estimated costs. A few specific examples from this document will emphasize Schriever's grasp of the technical challenges.

Schriever envisaged a graduated development of WS-117L capability. There was plainly an operational imperative to deliver a limited reconnaissance capability as quickly as possible, but he also envisaged what mature systems could deliver and outlined a reasoned and graduated means of achieving it. He saw,

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[296] For details of the alternative use, see Chapter 5 p126 *et seq* in this thesis.
[297] Memorandum for Lt General Power, "*Interactions Amongst Ballistic Missile and Satellite Programs." Schriever Archive, Box 1 Folder 2.*
[298] *NRO WS-117L Archive, Document 858.*

overall, 8 programs nested within WS-117L. Program I was solely experimental, intended to gather environmental data and prove concepts, but Program II would yield "…a Visual Reconnaissance [system] with a capability of mapping…features at a ground resolution of 100 ft and a locational accuracy of one-half mile…". Program III would introduce an electronic intelligence capability (referred to at the time generically as a 'Ferret' capability), Program IV would be a more accurate photographic system and Program V an advanced 'Ferret'. Program VI would be the definitive TV-based system (Program II had admitted the possibility of a film-return system yielding quicker results), Program VII was intended to be the first infra-red missile launch system and Program VIII a surveillance (i.e. wide-area coverage) 'Ferret'.[299] The plan also described in detail the various sub-systems – the 'spaceframe', propulsion systems, an auxiliary power supply to run the sensor payload (this would be tailored to the needs of the varying sensors, and envisaged eventual development of solar cells to charge batteries, and on-board nuclear power generation for higher-power systems). He noted the challenges of guidance systems, and that the capability required was currently beyond the capacity of the ICBM guidance-system programme, and finally outlined the various sensor payloads that would yield the required target data.[300] He had scoped the potential of the Thor and Atlas first-stage launchers (although at this stage the choice of the upper-stage booster was left open, he had a clear idea of the performance it would need to deliver). Lastly he outlined the launch flight path, including a calculation of the impact point for the discarded first-stage and envisaged the orbital altitudes best suited to data collection and rebroadcast.[301] Although it is most unlikely that Schriever developed the entire plan personally, his foreword to it indicates his technical understanding of the challenges it contained. Two days after he issued it, he began a series of briefings on the WS-117L programme, speaking at the Air Force Missile Test Center at Cape Canaveral on 5 April, discussing aspects of it with the Ramo-Wooldridge Corporation on 10 April and briefing the Science Advisory Board (the von Neumann Committee) on 12 April; this final meeting was attended by General Nathan Twining, then Chief of Staff of the Air Force. Comment on these events in Schriever's diary gives some insight on the content of these briefings. For the SAB briefing, he had to balance technical detail with

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[299] Schriever envisaged these programs as forming an 'Experimental' system (Program I), a 'Pioneer' System (II-III), an 'Advanced' System (IV-V) and an 'Ultimate' System (VI-VIII). *Ibid, Pages B-7 and C-2*
[300] *Ibid, Page B-9.*
[301] *Ibid, Pages B-5, B-6.*

explanation of his intended management strategy for the programme, but his notes again make clear his grasp of the technical challenges he faced.[302]

Subsequent diary entries reveal both Schriever's continuing technical awareness of WS-117L and his personal involvement in its progress. On 28 or 29 May 1958 (there is a one-day discrepancy of dates in his diary), a briefing was delivered to the National Security Council Planning Board about WS-117L. The diary includes an outline of a 117L briefing by Schriever, which covers the multiple collection strategies of the overall WS-117L programme (visual/photographic, 'ferret' and IR missile launch detection), likely detection ranges, revisit times or coverage areas for the various systems and the operational advantage they would confer.[303] At the end of 1958, he was involved in chartering a scientific review of WS-117L (the precise nature of the problem that provoked it is unclear, other than that from the identity of those involved – 'Din' Land and Dick Leghorn, who's work in reconnaissance is considered further below – it probably originated with the photographic payload); Schriever's belief in the urgency of the situation is clear.[304] Just prior to assuming command of ARDC, in March 1959, he is taking an interest in the progress of Discoverer/Corona launches. He notes on 16 March 1959 the delivery of a 'Support Package (Photographic)' for Discoverer at Point Arguello – this may well be the payload for Discoverer 4, the first to carry a camera, launched from Point Arguello (later renamed Vandenberg AFB) on 25 June 1959.[305] With his appointment to command ARDC in April 1959, his diary entries and correspondence on reconnaissance issues moves to a more managerial focus, but it is clear that Schriever understood the technical aspects of satellite design and development in a very similar way to his understanding of ICBMs. Both SAMOS, with its reliance on downloading imagery, and MIDAS with its reliance on immature sensors would be hard to describe as successes; MIDAS eventually matured into DSP, and SAMOS was simply overshadowed by the unexpected success after initial difficulties of Corona, but Schriever's understanding of the issues involved is clear.

[302] *Schriever Archive, Box 4 Folder 11.*
[303] *Schriever Archive, Box 5 Folder 7.*
[304] Diary entry 6 December 1958, *Schriever Archive, Box 5 Folder 10.*
[305] Diary entry 16 March 1959, *Schriever Archive, Box 5 Folder 13.*

Satellite Reconnaissance – Schriever, Doctrine, Policy and Strategy

Perhaps surprisingly, Bernard Schriever faced significant conceptual and doctrinal challenges in introducing his ideas for strategic reconnaissance into the USAF. An indication of the problem can be found in the USAF's foundational doctrine. At the instant of its foundation, the USAF employed doctrine codified during World War 2. Army Field Manual FM 100-20 '*Command and Employment of Air Power'* had been written in 1942-43 based on experiences of air campaigning in North Africa.[306] This document, unsurprisingly given its origins, dwells on the integration of air and land offensive operations. It admits the possibility of (strategic) aerial reconnaissance, but says little about its practicalities.[307] After the formation of the USAF, it took until 1953 for it to supplant FM 100-20. The first (1953) edition of the manual 'United States Air Force Basic Doctrine' makes barely a mention of the potential of Air Reconnaissance or Surveillance, noting simply that "[the air medium]…permits unparalleled observation of any point on the earth's surface", spending the rest of the document largely emphasising the merits of destruction of the enemy and his equipment.[308] Subsequent revisions (in 1954, 1955 and 1959) made little change to this – in fact, the 1959 edition added emphasis to the importance of 'firepower delivery'.[309] Yet outside the formal doctrine process, informed thinking had already begun to reconsider the role of strategic reconnaissance in the Cold War.[310] Writing in 1963 for a reissue of a 1940s collection of papers, Amrom Katz of the RAND Corporation outlined the problem elegantly:

> …reconnaissance is essentially uncivilized. We have no current literature, no standing references, no time-proved and -tested philosophy.
>
> It is not that we do not write. We ozalid, multilith, photostat, and print - we staple, bind, and fold. We deal with documents of all sizes, shapes, and formats. We give them "Standard Distribution" and we proceed immediately to file, lose, retire, and bury these thoughts, proposals, and discussions.

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[306] Mowbray, 'Doctrine Problems', (*Air Power Journal)*
[307] FM 100-20 defers consideration of reconnaissance to a subsidiary publication – FM 1-20, which in turn places strategic reconnaissance (which it refers to as 'Air Reconnaissance for Air Force Aviation') among the range of options, without ever admitting its primacy.
[308] United States Air Force, *AFM 1-2 United States Air Forces Basic Doctrine* (Department of the Air Force, 1953) 7. The third edition (1955) rewords the line on reconnaissance, but again subordinates it to other roles.
[309] Jonny R Jones, *Development of Air Force Basic Doctrine 1947-1992* (Maxwell AFB, AL: Air University Press CADRE Papers, 1997) 8, 48.
[310] It should not be imagined that the USAF was ignorant of the purpose and requirement for strategic reconnaissance; plainly both conventional and nuclear bombing campaigns during World War 2 had required appropriate reconnaissance to develop targets and plan missions. However, there is no evidence from doctrine that the AAC ever considered gathering such information as a core role of Air Power.

> As a result, anyone who chooses or is forced to enter this field of reconnaissance has to go through agonizing rediscovery, redefinition, resifting, and regurgitation of old thoughts and basic ideas. Restatement, not reaction, is the order of the day.[311]

A key thinker in this respect was Colonel Dick Leghorn, closely followed in USAF circles by Colonel Richard Philbrick. Both contributed papers to Katz's work cited above. In Leghorn's opinion, perhaps due to its roots in the Army Air Corps, and the associated emphasis on Close Air Support in many campaigns during World War 2, reconnaissance as a role was seen as essentially tactical in nature, being commonly associated with immediate post-strike battle damage assessment.[312]

Leghorn had served in Europe as a (tactical) reconnaissance pilot in the AAF, having been called up for service when working for Eastman Kodak. He was intent on returning to them when he was demobilized, but was persuaded to return to active service in 1946 to direct the photography of the US Atomic Bomb tests (Project CROSSROADS) at Bikini Atoll in the Pacific. Experience of the destructive potential of nuclear weapons, together with access to the post World War 2 assessment of bombing campaigns, convinced Leghorn of the emerging importance of true strategic reconnaissance.[313] He outlined his ideas in an address at the opening of the Boston University Optical Research Laboratory in December 1946. Although much of his thought had gone into relatively technical improvements in aerial reconnaissance, such as merging radar and photographic information, he also spoke more generally about the importance of strategic information gathering. Speaking about proposed research objectives for reconnaissance, he said, "strategic operations will dominate the military scene and will assume the position of prime importance…long-range reconnaissance of any place on earth from established bases will be a necessity".[314] Although he had not

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[311] Amrom H. Katz, *Selected readings in aerial reconnaissance : a reissue of a collection of papers from 1946 and 1948 (P-2762)* (Santa Monica, CA: RAND Corporation, 1963) vi 'Introduction to the 1963 Edition'.

[312] Leghorn outlined these ideas in conversation with the author at a social meeting in April 2011. He expanded on Schriever's interpretation of them, and recorded these ideas more formally in an interview with Mr Neil Sheehan. See Footnote 342 immediately below.

[313] For details of Leghorn's World War 2 service record and subsequent recalls to active duty, see the biographical summary published online by US Air Force Space Command (http://www.afspc.af.mil/library/biographies/bio.asp?id=9942 accessed 2 February 2015). For details of the influence of his experiences at Bikini Atoll, see R Cargill Hall, 'From Concept to National Policy: Strategic Reconnaissance in the Cold War', *Prologue: the Journal of the [US] National Archives,* Vol 28 No 2 - Summer 1996)

[314] Richard S Leghorn, 'Objectives for Research and Development in Military Aerial Reconnaissance', in Amrom H. Katz (ed.), *P-2762 Selected Readings in Aerial Reconnaissance: A Reissue of a Collection of Papers from 1946 and 1948* (Santa Barbara, CA: The RAND Corporation, 1963), 54.

yet foreseen the potential of space-based systems, he was outlining the circumstances in which they would be useful. In 1947, he was talking to Schriever about similar matters. He recounted their meeting in an interview with Neil Sheehan, which contributed to Sheehan's biography of Schriever:

> "Leghorn said Schriever was one of a few in the Air Force and Government who saw the need for aerial and satellite reconnaissance. He said when Schriever summoned him to Washington, he [Schriever] drew a 'graph' of four squares on a sheet of paper in his Pentagon office. The top two squares represented the USA, and the bottom two the USSR. BAS put 'P+' in left top square because, he said, US clearly outweighed the Soviets in power. But then put 'I-' in right top square because he said the US was lacking gravely in information about the Soviets; their country was closed to us, with little of use published or broadcast, very difficult to penetrate with agents, and thus extremely hard to get any reliable intelligence. In the bottom left square Schriever wrote 'P-' for the Soviets because they were behind the US in power, but then wrote 'I+' for them in the bottom right square because they able to learn so much about us -- our society was open, they were able to penetrate with agents, much of use was published and broadcast. Leghorn's mission, Schriever was thus saying, was to come up with methods of reconnaissance that would overcome the information gap."[315]

Leghorn returned to industry, initially working at Kodak, but in 1951 was recalled to active duty in connection with the Korean War. He was soon working indirectly for Schriever, who by now was Assistant for Air Force Development Planning. Initially their work centred on forthcoming reconnaissance aircraft such as the U-2, which Schriever was involved in developing; Leghorn was unconvinced of the practicality of satellites as reconnaissance platforms, although Richard Philbrick (mentioned briefly above) had been an early advocate for them, and had been in regular contact with Leghorn.[316] Hall asserts this endured until Sputnik was launched ("only the launch of an artificial Earth satellite in 1957 prompted them [Leghorn and 'Din' Land] to change their minds and actively support direct development of reconnaissance satellites").[317] A more nuanced change of heart, in

[315] Transcribed and expanded by the author from Sheehan's verbatim notes at the Library of Congress. *Sheehan Archive, Box 7;* the interview between Sheehan and Leghorn took place on 15 November 1996 at the National Air and Space Museum in Washington DC. Col Leghorn recounted substantially the same story in conversation with the author in April 2011, at which point he indicated the 1947 date for the encounter.

[316] See Richard Philbrick, 'Trends in Reconnaissance - a paper presented to the reconnaissance symposium (Topeka, KS, Tuesday, November 23, 1948)', in Amrom H. Katz (ed.), *P-2762 Selected readings in aerial reconnaissance : a reissue of a collection of papers from 1946 and 1948* (Santa Monica, CA: RAND Corporation 1948). Katz footnotes Philbrick's remark about satellite reconnaissance specifically at page 4.

[317] Hall, 'Concept to National Policy', (*Prologue: the Journal of the [US] National Archives)*. Leghorn had participated in an Air Staff study (the 'Beacon Hill' study/report) in 1951-52 seen by some as another counter to early enthusiasm for satellite reconnaissance. It plainly identified the need for strategic reconnaissance – often referred to in the literature as 'pre-D-Day' reconnaissance, but did

conjunction with Schriever, is claimed by Robert Perry.[318] However the change of mind occurred, the combination of Leghorn and Schriever's shared enthusiasm for strategic reconnaissance, and Schriever's exposure to research work coming from the RAND Corporation gave him the impetus to advocate for satellite reconnaissance as soon as the establishment of the ICBM programme made launchers an accessible goal; in doing so, however, he was moving well beyond established doctrine.

A rare piece of evidence of Schriever's persistent interest in satellite reconnaissance survives from 1951. In the wake of a RAND corporation proposal for further research and development on satellite reconnaissance, Schriever arranged a conference on 16 February 1951 to determine the types of information from a satellite that would be useful to end users; the USAF Director of Intelligence, Major General Cabell, responded directly to Schriever with a list of indicative targets.[319] It seems likely that Schriever and Leghorn captured their shared ideas during January 1953 in the Development Planning Objectives for Intelligence and Reconnaissance. Sadly, no contemporary historians seem to have been able to locate a copy of the finalized Objective, and it is possible that no copy survives, but Cargill Hall cites Leghorn's description of it to him in 1995 correspondence: "The DPO called for high-altitude balloons and eventually Earth satellites to provide wide-area search of the Soviet Union, with close-area surveillance provided by high altitude airplanes and second-generation satellites".[320] Schriever departed the Pentagon shortly afterwards to take command of WDD and the early ICBM work, but the establishment of the Eisenhower administration during 1953 and the subsequent transfer of responsibility for satellite development to WDD in 1955 must have been welcome to Schriever. It certainly guaranteed that development was entrusted to someone who was already persuaded of their utility.

Subsequent activity by Schriever ensured that other non-equipment lines of development were also pursued enthusiastically. The WS-117L development plan

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not believe that satellites would be practical soon enough to be a useful contributor, preferring instead more advanced aircraft and exploration of balloon platforms. See Merton E. Davies and William R. Harris, *Rand's role in the evolution of balloon and satellite observation systems and related U.S. space technology (R-3692)* (Santa Monica, CA.: Rand Corp., 1988) 30-39

[318] Perry asserts a connection to Leghorn's support for 'inspected disarmament', and a belief that the Soviets would not cooperate in the necessary inspection regime. See Perry, *History, Vol 1* 10-11, 14.

[319] Memo from Director, USAF Intelligence to Colonel Schriever dated 17 March 1951 "*Research and Development on Proposed RAND Satellite Reconnaissance Report"*. The National Security Archive, George Washington University, Washington DC, Bernard Schriever collection.

[320] Hall, 'Concept to National Policy', (*Prologue: the Journal of the [US] National Archives)*. Hall notes the probable loss of the DPO in footnote 41. Schriever made a direct reference to it in an interview with Neil Sheehan in connection with Sheehan's biography of him – Sheehan noting: "Never formally put out DPO on Reconnaissance. Wanted to hold close. Was highly classified" *Sheehan Archive, Box 3.*

referenced above details not only the technical specification of the platform, but also looks at the required ground infrastructure, such as locations for down-link stations for reception of images transmitted by the satellite (in the SAMOS concept of operations) and also considers what analytic capability might be needed within the Intelligence staff to process the new information gained, and the training requirements for personnel assigned to the programme. Throughout, Schriever was convinced of the importance of reconnaissance activity to the USAF, and the potential of satellites in supplying a significant part of it. Cautious advocacy and liaison between the RAND corporation and the Pentagon, for example in the formulating of the Intelligence and Reconnaissance DPO, served to put him in the best place to deliver the capability once others were persuaded of the need.

Satellite Reconnaissance – Schriever and Administrative Action

When WDD were charged with developing the ARS in 1955, Schriever's first concern was that it should not interfere with ICBM and IRBM development. Within that stipulation, the programme could be run on similar lines to the concurrent ICBM programme. He made his concerns clear in a memo to General Power on 23 September 1955, in particular the need for satellite and ICBM programmes to share specialized test facilities such as engine test stands, of which only a few existed nationally, and the effect of proliferating programmes on the ability of industry to retain sufficient qualified personnel – another national shortage. He used this latter concern to justify defending the USAF primacy in both missile and space systems development; if programmes were dispersed between the Services, the necessary coordination with the industrial base would become much harder.[321] However, two major administrative factors were about to complicate Schriever's delivery of an effective satellite reconnaissance capability. The first was a serious clash between the uniformed and civilian management of the Air Force relating to funding, the second was the interposition of the Advanced Research Projects Agency (ARPA) as a new layer of programme management.

Secretary Donald Quarles was installed as Secretary of the Air Force on 14 August 1955, having previously served as Assistant Secretary of Defense for Research and Development. He was also the first Secretary to come from an overtly technical background, and could thus be hoped by the Air Force to be sympathetic to aspirations for acquisition of advanced weapons and systems such

[321] Memorandum for Gen Power – *'ICBM-TBM Interference and Dilution of Manpower' Schriever Archive, Box 1 Folder8.* The title suggests concern about interference between the ICBM and other missiles, but the main text makes clear that it applies to satellites too.

as satellites.[322] Yet almost immediately, he was issuing direction to reduce such activity and extend development times, principally to serve the needs of the Eisenhower Administration's budgetary policies. While reference to the capability of satellites to deliver 'pre D-Day intelligence' might have played to the President's wartime experiences, his drive to achieve savings in defence expenditure, which strongly influenced his second term in office, led to public clashes between the Administration and the USAF Senior Leadership. (The resignation of Trevor Gardner as Assistant Secretary for Research and Development mentioned in Chapter 3 was a direct result of the public disagreements played out around this issue). In 1957, Quarles turned his attention to the USAF satellite programme. In the NRO Archive of WS-117L documents there is a 4-page summary of the influence of Sputnik on WS-117L activity; although its context and date are unclear, it summarizes the situation succinctly, firstly outlining the relatively steady progress made up until October 1956 (when the WS-117L contract was awarded to Lockheed). It then describes Secretary Quarles' instructions of March 1957: "1. Slow down. 2. No orbital testing prior to January 1960. 3. Conduct development along conventional lines".[323] These restrictions were enforced by the withholding of funds from the program.

The best description of how Schriever circumvented these restrictions and delivered an operational capability during 1960 is in an NRO history of Corona. At least two of the authors were personally involved in the Corona program (and were well known to Schriever). One of them (Colonel Oder) played a leading role in management. The history was written at a high classification in 1987 before declassification in 1997.[324] Schriever first successfully persuaded AF Headquarters to sustain WS-117L funding for the remainder of the financial year while he sought a better solution. He then tried unsuccessfully to adapt WS-117L as a reserve system for the troubled 'Vanguard' scientific satellite being developed by the US Navy. This attempt foundered too on the double grounds of breaching budgetary ceilings and compromising the administration's 'Space for Peace' policy, but it laid the bedrock for the ultimately successful tactic. The scheme, known as

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[322] George M. Watson, *The Office of the Secretary of the Air Force 1947-1965* (Washington DC: Center for Air Force History, 1993) 149

[323] *NRO WS-117L Archive, Document 988.* The document is anonymous, though a handwritten note on the front page states 'Done for Col Evans, SSD'; it must date from after November 1959, and might form the outline for an essay, or possibly a speech. It includes further analysis of the influence of ARPA.

[324] Frederic C E Oder, James C Fitzpatrick, and Paul E Worthman, *The Corona Story* (Sunnyvale, CA: National Reconnaissance Office, 1987). Col Oder had been Schriever's head of the WS-117L program within WDD, and Col Worthman had headed development of the aerial recovery system developed for the balloon-based overflight reconnaissance system known as Project GENETRIX.

'Second Story', was devised principally by Col Frederic Oder, who worked on Schriever's staff in the WS-117L office; it is summarized succinctly in *The Corona Story:*

> "It involved an announced cancellation of the WS-117L program, an overt establishment of an Air Force scientific satellite project as a follow-on to the marginally-limited Vanguard, and covert reestablishment of the reconnaissance program under overall cognizance of the CIA, with the Western Development Division retaining technical management responsibilities."[325]

The authors also make clear that Schriever was intimately involved with this plan from the outset:

> "The 'Second Story; proposal had been entirely concocted within Schriever's own organization…Schriever scheduled a formal meeting with State and CIA for late September, by which time he planned to have the 'Second Story' proposal in a form suitable for official submission."[326]

Schriever thus proposed trading ultimate ownership of the programme for CIA sponsorship, and an opportunity to remain involved in its technical direction. Before this plan could be carried out, however, the Soviets launched Sputnik 1 and some of the objections to developing military satellite systems evaporated. Oder *et al* recount that initially, the Chief of Staff of the Air Force, General White, was told to plan on accelerating development of the reconnaissance satellite in response. The setback of disapproval from Donald Quarles and James Douglas (Deputy Secretary of Defense and Secretary of the Air Force respectively) was then overcome via a direct appeal to the Secretary of Defense, and an adequate level of funding was secured.[327]

Management issues were further complicated during 1958-59 by the interposition of ARPA as the military agency responsible for advanced research and development. ARPA initially took over direction of the military satellite programme, although the 'Second Story' plan took Corona out of their hands. Schriever and his colleagues seem never to have had a positive opinion of this Agency; in the memo prepared for Col Evans detailed above, the assessment of them is scathing. ARPA's influence is summarized:

> "ARPA Period of Management 19 May 1958 – 17 Nov 1959…constant requirements for new development plans…period characterized by

[325] Oder, Fitzpatrick, and Worthman, *The Corona Story* 12.
[326] Oder, Fitzpatrick, and Worthman, *The Corona Story* 12
[327] Oder, Fitzpatrick, and Worthman, *The Corona Story* 9-14

instability, indecision, rapid changes in objectives and funding"[328]

In February 1959, two of Schriever's staff shared views on the same issue:

> "I have no hope that ARPA can be turned into a useful activity of any kind... Somehow we should be able to make use of the fact that industrial interest has governed many of their actions to an extent almost – if not quite – illegal."[329]

That Schriever shared at least elements of this view is made clear in a letter he sent to General White (Chief of Staff of the Air Force), also during 1959, complaining about ARPA's influence on elements of the reconnaissance satellite effort. It is in essence a summary of the complaints alluded to above about rapid changes in objectives and funding, noting their effect on morale among the staff, and the wasteful nature of expending money on elements of the programme only to have them re-prioritized or abandoned before completion. He concluded by asking "that these matters be brought to the attention of ARPA …and that every effort be made to provide greater program stability in the future…"[330] Some of this confusion probably arose from the need to publicly cancel elements of the satellite programme then covertly reinstate them – a fact which was not always made clear to those criticizing ARPA. Schriever, however plainly saw the need for stable management as key to delivering effective systems. This view was also held outside Schriever's immediate circle; a similar sentiment can be found in an NRO summary of the period:

> "The only management problems of any consequence arose well outside the program structure, chiefly from ARPA's efforts to re-orient the covert program (now called CORONA) toward some rather variable objectives of its own choosing."[331]

As noted above, Schriever also incorporated elements of concurrency in Corona development; the direct appeal to the Secretary of Defense and the sponsorship of the CIA implied in 'Second Story' principles assured the programme of priority status, and the reconnaissance payloads could be developed in parallel with the missiles which were being adapted to become the first generation of space-launch vehicles.

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[328] *NRO WS-117L Archive, Document 988*
[329] 'Memorandum for Colonel Curtin: ARPA and NASA' by Colonel William Sheppard dated 7 February 1959. *Schriever Archive, Recently Declassified Documents folder.*
[330] Letter General Schriever to General White dated 1 August 1959, *Schriever Archive, Recently Declassified Documents folder.*
[331] Robert L Perry, *Management of the National Reconnaissance Program 1960-1965* (Langley, VA: The National Reconnaissance Office, 1969) 8

Reconnaissance Satellites and Schriever – an Assessment

Once again, Schriever managed the development of a technically challenging programme by paying attention to each of three strands of development: managing technical progress, developing conceptual maturity and assuring adequate management structures. His grasp of the technical issues at hand permeate much of his correspondence and diary entries, and although as in the ICBM programme he was an engineering manager rather than a working engineer, he plainly had a sound grasp of the principles involved. He also appreciated the non-equipment challenges he faced; his advocacy of the importance of access to information, dating back to 1947 and his initial foray into scientific and technological development is noteworthy both for its originality and for the succinctness with which he expressed it. Finally, he managed to chart a path through the management obstacles and uncertainty caused by the overt/covert duality of parts of the programme, the creation of ARPA and its involvement in satellite development and the changing priority attached to the programme at various times due to budgetary and political considerations.

In the next two chapters, this thesis examines instances where Schriever was unable to maintain progress along the three axes of development activity, and the fates that befell programmes where this happened.

CHAPTER 5 – MANNED SPACEFLIGHT: A PET PROJECT

Introduction

Chapter 4 looked at two specific projects where Schriever successfully led the development of an advanced capability through attention to three lines of effort: technical risk reduction, improving conceptual maturity and gaining administrative freedom of manoeuvre. Although it is impossible to prove categorically that it was only by attention to those factors that Schriever led the projects to successful conclusions, it is unmistakeable that he paid attention to them and ultimately delivered a useful capability. In this chapter and the next, this thesis considers the opposite case; two aspects of military spaceflight where Schriever held strong views as to their value, and in each case took action to develop them, but also in each case with at best limited success. It will explore which of the three key aspects he worked for, and which he either ignored or paid insufficient attention to. In each case, it will show which aspect contributed to the failure. Again it is not conclusive proof that only these factors guarantee success for a project, but together, the two analyses will suggest their importance. In this chapter, it looks at Schriever and his advocacy for military manned spaceflight; in Chapter 6, it will look at space weaponization, and in particular, orbital bombardment systems.

Military Manned Spaceflight

Proposals for military manned spaceflight arose from the same sources as the coincident civil space programme; very early awareness of the possibilities of orbital motion, growing understanding of the challenges of sustaining human life in such circumstances and finally detailed technical study of their implications. The 1946 RAND report devoted a chapter to consideration of whether their analysis (aimed at proving the practicalities of an unmanned satellite) would also validate manned spaceflight possibilities, concluding in the affirmative.[332] Formal civil space planning was emerging at the same time. Wernher von Braun had indicated possibilities in the first debrief he gave to Allied intelligence officers in May 1945, not quite 3 weeks after the German surrender; in this he outlined his view that the

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[332] Project Rand, *Preliminary design of an experimental world-circling spaceship* Chapter 14, pp 211-12

state of the rocket and missile art in 1945 resembled that of the manned bomber at the end of World War 1, and that equivalent rapid growth in spaceflight possibilities could be anticipated.[333] As was noted in Chapters 1 and 2, von Braun's earliest work in the United States was in the service of the US Army, rather than of the Air Force; additionally, the circumstances of the late 1940s rewarded a certain discretion on the part of the Paperclip cadre – there were plenty of senior officials in the United States who felt that too many Germans had entered the United States, or failing that, that the 'wrong' ones had been allowed in. As Tom Bower notes:

> "…the military wanted the Germans to be given immigration visas so that they and their families could eventually become American citizens.…attempts inside the State Department to mediate between conflicting officials had failed…[State Department] feared that for America to admit indoctrinated Nazis, while publicly demanding that Argentina and other Latin American countries should expel Nazis…would place the department in an indefensible position…But the War Department had no time for State Department bickering."[334]

Annie Jacobsen provides more detail both on the attempts to indict various members of the Paperclip cadre, and on their conditions and morale while settling in the United States.[335] Although she notes that they assimilated into the pleasures of life in America relatively quickly, there were still limits to the degree of public profile they could adopt. Ten years later, there would be far fewer constraints on what they could do, and von Braun would play his part as an advocate for military manned spaceflight, but at this stage, their public personae were more closely controlled.

The USAF recognised that many of the particular challenges of manned spaceflight would come from the nature of the human occupants, rather than the dynamic aspects of orbital flight. In 1948, the Air Force School of Aviation Medicine at San Antonio, Texas, arranged a pioneering panel discussion on 'Aeromedical Problems of Spaceflight', and in 1949, the Air Force established a Department of Space Medicine within the Aeromedical Laboratory at Wright-Patterson Air Force Base, with Hubertus Strughold as Head of Department, and

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[333] Neufeld, *Von Braun* 204-5

[334] This is a grossly simplified account of the complex conflict between the State Department, the Justice Department and the Department of Defense that occupies most of Chapter 10 of Bower, *Paperclip* .

[335] Jacobsen also provides an account of the various attempts to investigate Paperclip personnel for war crimes during the late-1940s – see Jacobsen, *Operation Paperclip* , Chapters 12-14.

subsequently Professor of Space Medicine. Subsequent symposia and other events arranged by this Department and related organisations testify to an enduring interest in the challenges of human spaceflight, culminating in Schriever appearing with von Braun, Dornberger and Strughold at a Symposium in 1958.[336]

The next formal interest in manned military spaceflight was probably the 1956 announcement by Air Research and Development Command of a study into a 'Manned Ballistic Rocket Research System'. This study spawned a plethora of follow up studies and proposals.[337] Eventually, these formed a basis for the activities of the 'Man-in-Space Task Force' at Air Force Ballistic Missile Division HQ. During 1958, they devised a multi-phase approach known as the 'Air Force Manned Military Space System Development Plan' to enable their stated goal of a manned lunar mission.[338] The first phase of this plan was referred to as 'Man-in-Space Soonest', often abbreviated 'MISS'. It envisaged progress through unmanned, then primate-occupied and then man-carrying flights in Earth orbit. It would be followed by three later phases spanning a developed orbital spacecraft, unmanned lunar reconnaissance and manned flights first around and then onto the lunar surface. Notwithstanding the national imperatives to achieve parity with, and then superiority over, the Soviet Union (who had achieved orbital flight with Sputnik 1 just as this planning activity ramped up) in the 'Space Race', the reliance on anticipated developments of advanced launch boosters, including use of exotic propellants, must have made the intended lunar landing in 1965 appear somewhat optimistic even then.

The creation of NASA out of NACA in 1958 and the award to them of the national manned spaceflight programme did little to dampen the USAF's enthusiasm for spaceflight, although in the first instance, the MISS programme was folded into the NASA Mercury programme. As well as its deep involvement in aspects of the NASA manned space programme, the USAF was also busy developing the X-15 experimental hypersonic aircraft (in conjunction with NACA and then NASA) and the proposed 'Dyna-Soar' X-20 boost-glide platform. Both

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[336] The 1948 panel is described in, Swenson, Grimwood, and Alexander, *This New Ocean* 34 The formation of the Department of Space Medicine is noted in David N. Spires, *Beyond horizons : a history of the Air Force in space, 1947-2007* (2nd edn. Peterson Air Force Base, Colo.: United States Air Force, 2007) 73. An example of early interest in human spaceflight being explored publicly is the 1951 symposium sponsored by the Air University School of Aviation Medicine – see 'Physics and medicine of the upper atmosphere; a study of the aeropause. ', Clayton S. White and Otis O Benson, Proceedings of '*Physics and medicine of the upper atmosphere*' at Air University School of Aviation Medicine, Randolph Field, TX on 6-9 November 1951. This later symposium will be described shortly.

[337] Office of Information Historical Division, SSD AFSC USAF Chronology of Early Air Force Man-in-Space Activity, 1955-1960, (Los Angeles AFS, Los Angeles, CA, 1965)

[338] Swenson, Grimwood, and Alexander, *This New Ocean* 91.

these latter, and especially the X-15, had their origins in extending the performance of conventional aircraft, but alongside this, the USAF continued to plan for the implementation of a true military manned spaceflight capability.

Firstly the USAF proposed a series of military manned missions using Air Force procured Gemini capsules, which were sometimes referred to as 'Blue Gemini' missions.[339] These came to naught, though the US military services were allowed to sponsor experiments to be carried on NASA Gemini missions. Next, the USAF pursued the experimental single-seat recoverable prototype mentioned above, known alternatively as the X-20 and the Dyna-Soar. X-20 would have been launched atop an Atlas booster, and would have had unique (at the time) abilities to manoeuvre in orbit.[340]

In 1963, the X-20 Dyna-Soar programme was cancelled by the Department of Defense, while construction of the first prototype was underway. In its place, the USAF was given a new programme intended to determine the true utility of manned spaceflight as it related to military tasks – the Manned Orbiting Laboratory (MOL). It was the culmination of USAF studies going back to 1958, which had led to a formal proposal in early 1963. When the X-20 programme was cancelled, MOL was seen as its replacement.[341] As proposed, it envisaged the launch of a large experimental capsule, sandwiched between a developed Titan booster and a Gemini capsule carrying a crew of two Air Force astronauts. Capable of a 30-day mission, and providing a 'shirt-sleeve' environment in which the astronauts could conduct a variety of experiments to determine the limits of human capability in orbit, this ambitious mission would have given the USAF a true long-endurance manned spaceflight capability. For two years the MOL programme continued in the public eye; during 1965, however, its management changed such that its nature and objectives became hidden.[342] This was in fact because of the growing interest in the programme displayed by the National Reconnaissance Office (NRO), who

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[339] For a description of the Blue Gemini programme, see Barton C. Grimwood James M. Hacker, *On the Shoulders of Titans: a History of Project Gemini (NASA SP-4203)* (Washington, DC: National Aeronautics and Space Administration, Scientific and Technical Information Division, Office of Technology Utilization, 1978) Ch6-2

[340] The most comprehensive description of the Dyna-Soar programme is contained in Houchin, *The rise and fall of Dyna-Soar* There is also a substantial treatment of the programme in Mark Erickson, *Into the unknown together : the DOD, NASA, and early spaceflight* (Maxwell Air Force Base, Ala.: Air University Press, 2005) Chapter 4. Walter Dornberger was heavily involved in Dyna-Soar development via his employment as a consultant to the Bell Aerospace Corporation.

[341] Carl Berger, *History of the Manned Orbiting Laboratory Program (MOL)* (Washington DC: The Department of the Air Force MOL Program Office, 1970) 50-51 makes clear that '…the X-20 program would be cancelled in favour of the MOL Program'.

[342] In Schriever's papers at the Library of Congress is a press cutting from *Time* magazine (Time, 'Bioastronautics for Survival', *Time,* 6 August 1965 p. 58.) describing an account of MOL goals and progress given at a conference of the AIAA. Although short, it is succinct and demonstrates clearly that the MOL was still 'in the public domain' at that point. *Schriever Archive, Box 25, Folder 7.*

wanted to use it as a platform for an operational manned reconnaissance system. Since the very existence of the NRO was itself classified at that time, their involvement led to the MOL 'disappearing'. The MOL programme was in turn cancelled in 1969, before significant hardware had been produced.[343] With its cancellation, the USAF's last attempt to manage a manned space programme with bespoke hardware ended. The role of the USAF and the Department of Defense's involvement in the subsequent Space Transportation System (STS – the 'Space Shuttle') programme still lay in the future, but that was never a USAF-only project.

Military Manned Spaceflight – Schriever and Technical Progress

From his earliest reflections on the potential of military spaceflight, Bernard Schriever plainly envisaged it including manned missions. It was not, however, until the closing stages of his career that he was actually given a manned spaceflight programme to manage. Thus, for much of his working life his understanding of the technical challenges implicit in the role must be deduced from his writing and speaking, rather than from concrete results. Schriever's first major public statement on spaceflight, which at the time gained him a certain notoriety in Department of Defense circles, was to the Air Force Office of Scientific Research Astronautics Symposium at San Diego, California in February 1957.[344] In it, he identified clearly the extension of effort required to get from the envisaged unmanned satellite to a manned capability:

> …However, before man can be committed to space vehicles, a tremendous amount of human factors research will be necessary.

Later in the same speech, he amplified this. Referring specifically to extended duration missions, he said:

> …it will be necessary to extend the navigational program and the space medicine program characteristic of this type of sustained flight.[345]

The following year, Schriever publicly demonstrated his commitment to the scientific and technical effort to solve the problems of manned spaceflight; he co-

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[343] The Titan IIIM booster, which would have launched the MOL was developed within this time period, though MOL was not the only payload intended for it. On 3 November 1966, a Titan IIIC was launched on a sub-orbital flight carrying a mock-up of the MOL and an unmanned Gemini capsule (which would have been used to carry crews to the MOL) to prove the heatshield modifications to it intended for the MOL mission. Although not an operational MOL, this flight was the nearest that MOL hardware got to orbit.

[344] Schriever recounted in 1995 that: "…I made a speech concerning military space and indicated that space would play an important role for national security in the future. The next day, I received a wire from the Secretary of Defense's office: "Do not use the word 'space' in any of your speeches in the future"…" Text of Schriever's remarks reprinted in Spires, *Orbital Futures* 20-26 (Vol 1)

[345] Spires, *Orbital Futures* Document I-2, pp 20-26.

chaired a major session of a 3-day symposium at the USAF School of Aviation Medicine in San Antonio, and additionally presented a paper on 'The Weight Limitations and Capabilities in Rocket Propulsion for Manned Space Operations'. In it, he confirmed his belief that then current systems would be suitable for adaptation for man-carrying missions.[346]

Aside from the content of Schriever's paper, the San Antonio Symposium also confirms his continuing contact with the German 'Paperclip' cadre. The Symposium overall was co-chaired by Major General Otis Benson, a senior USAF medical officer, and Dr Hubertus Strughold. Schriever chaired the first session and delivered his paper on Day 2, alongside contributions in the same session from Wernher von Braun and Walter Dornberger. Dr Strughold chaired the closing session on Day 3, and delivered two papers during the Symposium.[347] A USAF film of highlights of the Symposium survives, showing a short extract from each of the speakers across the three days of the symposium. Apart from confirming the physical presence of the speakers listed to attend, the thanks extended to Schriever by Dornberger and von Braun appears sincere (von Braun and Dornberger spoke at the session chaired by Schriever).[348]

Within the year, the Soviet Union stole a lead on the USA in space by launching Sputnik. Part of the USAF reaction to this was the establishment of an ad-hoc committee of scientists by HQ Air Research and Development Command (ARDC) under the chairmanship of Dr Edmund Teller, in response to a DoD request for details of each of the Single Services' ongoing space programmes. The Committee appears to have met only once, over 22-23 October 1957, rendering its report some 5 days later. The USAF believed that the request for information was preparatory to one of the Services being nominated as lead-command for spaceflight within the DoD (and in plain expectation that the USAF would be the deserving winner). In fact, as David Spires recounts in his introduction to his reprinting of the Teller Committee final report, their hopes were dashed, and the

[346] 'Physics and medicine of the atmosphere and space', Otis O Benson and Dr Hubertus Strughold, Proceedings of '*2nd Int. Symp on Physics and Medicine of the Atmosphere and Space*' at San Antonio, TX on 10-12 November 1958

[347] A comprehensive summary of the Symposium was published by Cornelius A Tobias, 'Meetings - 'Space Science'', *Science (The Journal of the American Association for the Advancement of Science),* Vol 129 No 3353 (3 April 1959). The Symposium proceedings amplify and confirm Schriever's understanding of the challenges at hand, and demonstrate that he was working as an equal with researchers and engineers such as von Braun and Strughold.

[348] The original film is held by the US National Archives – posted online at 'Coverage of the second International Symposium by the School of Aviation Medicine showing scenes of the opening session.', <https://www.youtube.com/watch?v=1ewOQ_v30PA>, accessed 17 December 2016

DoD used the collected outcomes of their trawl for information to establish the Advanced Research Projects Agency (ARPA) as champion of DoD spaceflight.[349]

There is clear evidence that Schriever was aware of, and interested in, the Teller Committee deliberations, even though he did not attend their October meeting. At the time, he was fully occupied running the WDD component of ARDC developing ICBMs. Nonetheless, on 21 October (the day before the Teller Committee meeting opened), he spoke about it to a trusted colleague who would be an ARDC attendee at it, Colonel Paul Blasingame. Schriever notes in his diary for that day: "Paul Blasingame – Teller report to Douglas [James H Douglas, then Secretary of the Air Force]… 1. Why did it happen? 2. ICBM-IRBM, Impact permissible, Tech[nical] feasibility. 3. Scientific Flights – Hi[gh] alt[itude] research. 4. Missions with a purpose. 5. Spaceflight – unmanned, manned."[350] These seemingly disconnected headings mirror closely the final structure of the Teller Committee report. In particular, 'Why did it happen' is the title of the first section of the main report, it is closely followed by an analysis of the impact of ICBM and IRBM breakthroughs in the USSR, and following sections discuss the high-altitude research problems that spaceflight will pose. The phrase 'missions with a purpose' does not appear in the report, but the Summary within the report includes a reference to 'stunts with a purpose', and the report concludes with a strong endorsement of a proposed USAF manned spaceflight programme. Given that the conversation between Schriever and Blasingame took place prior to the Committee's meeting, the conclusion that they were shaping its discussions and outcomes is inescapable. It is also clear that Schriever was still identifying the specific challenges of manned spaceflight.[351] As noted, the Teller Committee report did not have the desired effect, at least from the USAF's point of view. ARPA's establishment followed, and the MISS project was incorporated into NASA's Project MERCURY.

It is at least possible that Schriever at this point tried to initiate another USAF manned spaceflight programme. This arose out of the SAMOS (unmanned)

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[349] Spires, *Orbital Futures* 216. The Committee Report follows immediately as Document II-6.
[350] *Schriever Archive, Box 5, Folder 3. Diary Entry for 21 October 1957.* In a marginal annotation in red crayon, Schriever then listed six people he believed to be 'Anti Ed Teller'. The names read 'Tuggs', 'Valley', 'Fitts', 'Spilhaus', 'Walk/Walh', 'Bulow'. The context is completely unclear, and most are unidentifiable, at least to the author. 'Spilhaus' may refer to A F Spilhaus, who was a science 'populariser' in the press at the time, 'Bulow' might be Senator J W Bulow/Buhlow, sometime governor of South Dakota who lived in Washington at the time, and 'Fitts' might be Paul M Fitts, a former USAF Lieutenant Colonel, then a psychologist at Ohio State University with interests in 'human factors' research and ergonomics.
[351] There is additionally an earlier entry in Schriever's diary for 15 September 1957 – 'Go see Edward Teller' – but the context is completely unclear. It is, however, at least possible that Schriever and Teller discussed the issue directly. *Schriever Archive, Box 5, Folder 3.*

reconnaissance satellite programme being developed by the USAF in parallel with the Discoverer/CORONA programme previously described. SAMOS-E, the visual reconnaissance element of the programme was technically a more advanced, and more challenging programme than CORONA, based on a heavier orbiting payload.

Sources for the description of the potential manned SAMOS capsule are elusive; the most complete account has been assembled by Dwayne Day, who published his account in a three-part article in 'Spaceflight' journal in 2002-3.[352] Day does not provide detailed references to his source material. Fragments of primary material relating to this programme are beginning to emerge through the ongoing NRO declassification efforts, validating Day's thesis, but there are still many gaps.[353] Consequently, the quotes from his article that follow are lengthy, and must be judged strictly on their merits as regards overall plausibility.[354] Plainly Day had access to some primary material, and he cites an interview he conducted with Mr Jack Herther, a key figure who worked for the Itek Corporation (the SAMOS camera design company) and who was heavily involved in the design of the SAMOS capsule.

At this time, technical requirements were driving up the size of the SAMOS reconnaissance capsule (Discoverer/CORONA sacrificed sophistication for early availability; SAMOS (at the time known as part of 'Sentry') was intended to deliver higher resolution imagery (implying a bigger camera), via film readout techniques (yielding quicker delivery of results and avoiding the need for a recoverable film capsule). The 'large camera' version of SAMOS/Sentry would be known as SAMOS E-5. Day asserts that:

> …members of the Air Force Ballistic Missile Division (BMD) office that managed the Sentry satellite program in Los Angeles became convinced that they also needed to develop a recoverable satellite, like CORONA. Unlike
> CORONA, however, whose recovery vehicle was so small that it could fit

[352] Dwayne A Day, 'A sheep in wolf's clothing: the Samos E-5 recoverable satellite -- Part 1', *Spaceflight,* Vol 44 No 10 (October 2002), Dwayne A Day, 'A square peg in a cone-shaped hole: The Samos E-5 recoverable satellite -- (part 2)', *Spaceflight,* Vol 45 No 2 (February 2003), Dwayne A Day, 'From cameras to monkeys to men: The Samos E-5 recoverable satellite -- Part 3 ', *Spaceflight,* Vol 45 No 9 (September 2003). The articles were reprinted online via *The Space Review* in 2009.
[353] One document validating the core of Day's thesis explicitly has reached public circulation. A 1965 CIA Summary Memorandum, almost certainly written by Mr Albert 'Bud' Wheelon, the CIA Director for Science and Technology, notes "For instance, one version of SAMOS (E-5) was warped around to provide a space vehicle capable of supporting a military man into space". 'A Summary of the National Reconnaissance Problem', A D Wheelon (?), 13 May 1965. *NRO Declassified Collection of CORONA, ARGON and LANYARD Records, Document 1D0008, p10.*
[354] Perhaps the most useful volume for correlating Day's account is *History of Discoverer Volume II - Rough Draft* (Los Angeles, CA: Air Force Systems Command SAMSO History Office, 1959), recently declassified by the NRO. Despite its 'draft' status, with no identified author and an uncertain publication date, it provides a comprehensive description of the Biomedical programme associated with Discoverer and further evidence of Schriever's interest in the problems it addressed.

inside an oil drum, they wanted a larger recovery vehicle.
Exactly why the members of BMD decided in the fall of 1958 that they needed a large recoverable capsule is unknown and remains one of the important mysteries in this story. …. It may have been a coincidence that this new requirement for a large recoverable capsule emerged only a few weeks after Lockheed proposed a manned recoverable capsule and at the same time that NASA was undertaking Mercury. But the leaders of the Air Force space program, particularly Lieutenant General Bernard Schriever, clearly coveted the manned spaceflight role, and it is hard to avoid the conclusion that they were covertly trying to develop their own manned spaceflight capability. In late September 1958, Air Force leaders in Washington, DC, undoubtedly acting upon the advice of members of BMD in Los Angeles, issued a directive that:

> "consideration… be given to the use of a recoverable satellite in order to achieve maximum accuracy, information content, reliability of receipt of collected data, and reuse where economically feasible."

…By January 1959, with ARPA approval, the Ballistic Missile Division issued a revised "development plan" for Sentry that formally established the goal of developing a large recovery capsule, … Surprisingly, the BMD plan mentioned almost nothing about actual reconnaissance requirements, ... In many ways the Air Force was designing this new reconnaissance system backwards—defining the capsule first and leaving out the details of what would fit inside of it and, most importantly, what it would do.
At some point in this process, Lockheed added another requirement to the spacecraft design—the capsule had to be pressurized. "It was just specified: Thou shalt pressurize," camera designer Jack Herther explained. This requirement had nothing to do with the reconnaissance mission, and whether Lockheed had added it at Air Force request or not remains unclear. Pressurization only complicated the spacecraft design. The pressure shell and pressurization system added weight that could have been devoted to payload.
Pressurization was unneeded for a film return system - CORONA was unpressurized. But the pressurization requirement allowed the Air Force, and Lockheed, to develop key technologies necessary to a manned space program. Although the Air Force was out of the manned space effort, building a large pressurized capsule capable of carrying a man kept the Air Force half a step behind NASA—as opposed to out of the race completely. They were cloaking a manned spacecraft program in the veil of a military reconnaissance satellite.[355]

And later:

Herther had never been happy with the compromises forced upon Itek by the need to fit their camera inside a volume constrained, pressurized re-entry vehicle, but he had no idea why the Air Force had imposed those requirements. It was only four decades later, with the declassification of information concerning the E-5 and its origins in Lockheed's October 1958 Sentry Man in Space proposal, that Herther thought the pieces started to fall into place. The Air Force was less interested in the reconnaissance mission than it was in developing the capabilities necessary for manned space flight. "I was naïve," he admitted. But in retrospect, it all made sense.

[355] Day, 'A sheep in wolf's clothing', (*Spaceflight)*

"As I now look at the documentation and understand why they wanted the camera to come back, which caused the pressurization, which caused the mirror, which caused the configuration to be so favorable to the recovery process that it compromised the reconnaissance 'stable table' notion…" Herther shook his head in disbelief. "It's just the hard way to do it," he said…[356]

In June 1959, ARPA (who retained control of finances) cancelled SAMOS E-5. Day reports Schriever's outrage at this and the subsequent coordinated protests from the USAF.

> Air Force officials were incensed by ARPA's decision to terminate the E-5 camera. Major General Osmond Ritland, Commander of Ballistic Missile Division, complained to Air Force Chief of Staff General Thomas D. White. Lieutenant General Bernard Schriever, the head of BMD's parent organization, Air Research and Development Command, and a former commander of BMD, also complained to White. Schriever sent a letter to White declaring "should the ARPA decline to continue the recovery program… it is recommended that the Air Force immediately support this urgent development." In other words, with ARPA refusing to fund the Sentry E-5, the Air Force would have to find additional money in its own budget to pay for the recoverable capsule, taking it from another non-space program. Responding for General White, Deputy Chief of Staff General Curtis LeMay told Schriever "I am completely sympathetic with your point of view and have taken action through Secretarial channels to restate the Air Force requirement to the director of ARPA and request reconsideration of its support in FY 60."[357]

SAMOS E-5 was thus reinstated. Day then provides extensive description and analysis of the camera systems developed for the various SAMOS variants and the conflicts and overlaps between these options and the extant CORONA systems (including the compromises required to fit them within the E-5 pressurized capsule); these need not concern us here. He next recounts that after several unsuccessful test launches, SAMOS E-5 was cancelled (for the second and final time) in December 1961, with the sole exception that a test flight already scheduled for March 1962 could proceed as planned. In Day's account, towards the end of the programme (perhaps with cancellation already threatened), Schriever made one final attempt to capitalize on the SAMOS E-5 pressurized capsule, and the available SAMOS E-5 hardware to further military manned spaceflight:

> Air Force officials were also proposing other missions for their new pressurized spacecraft. These missions had nothing to do with gathering

[356] Day, 'From cameras to monkeys to men', (*Spaceflight)*
[357] Day, 'A sheep in wolf's clothing', (*Spaceflight)*. Note in this instance also that Curtis LeMay, who had not had the easiest relationship with Schriever regarding introduction of ICBMs, is completely supportive of his concerns.

> intelligence.
> …
> In October 1960 General Schriever proposed flying a primate on a Discoverer mission. Mice and monkeys had been part of the original Discoverer program, to mask the true mission of the satellites with their CORONA reconnaissance cameras. But the primate life support system had been delayed and once the CORONA cameras became available in June 1959 the CIA immediately incorporated them into the satellite. Program directors abandoned the life sciences research and work on the primate life support system for Discoverer was canceled.
> Now, with CORONA operating and the primate cover story dormant, General Schriever wanted to fly the small primate mission to obtain information for future manned missions… In December 1960 the Office of the Assistant for Bioastronautics at the Air Force's Air Research and Development Command—ARDC, the Command that General Schriever was in charge of—issued a report titled "Bioastronautics Capability and Requirements for Manned Space Operations." The report was a justification for putting a military man in space.
> In April 1961, only three days after Yuri Gagarin became the first man to orbit the earth, the Air Force Systems Command, which Schriever had recently molded from ARDC and Materiel Command, issued a proposal for a "Bioastronautics Orbital Space System," or BOSS. BOSS was to use a Samos E-5 capsule to carry a large primate— a chimpanzee—on a long-duration flight up to fourteen days. It would be a relatively high altitude mission, exposing the test subject to the radiation belts. According to the proposal, a mockup of the life support system was already built and the Air Force was seeking permission to build a test vehicle. Lieutenant Colonel Charles W. Craven wrote the report and General Schriever and Major General Osmond J. Ritland, the commander of Space Division, which was part of Systems Command, approved it. In the introduction Craven bluntly stated: "This is not a biomedical program to collect a great amount of data from animal orbital flights, but is a system to determine the feasibility of manned military operations in space."
> …The BOSS proposal was apparently approved in June 1961—probably by the Air Force's military leadership and not its civilian leadership. But BOSS was not funded and it went nowhere… And by late 1961, Undersecretary Charyk cancelled the Samos E-5.[358]

This long account of the travails of Samos E-5, if accurate, shows Schriever's enduring interest in human spaceflight. It is possible that he wished to protect the pressurised capsule for some misplaced technical concern relating to reconnaissance, but it seems more likely that he was, as Day suggests, preserving the capability to move quickly to deliver a manned USAF capsule if NASA's Mercury programme had encountered any major problems. The BOSS report, delivered with Schriever's endorsement, and his willingness firstly to actually execute the Discoverer 'cover story' and secondly to adapt the SAMOS E-5 to a primate flight via BOSS, links directly back to his initial enthusiasm as outlined in

[358] Day, 'From cameras to monkeys to men', (*Spaceflight)*

San Diego in 1957 for exploration of the bio-medical implications of manned spaceflight.

Schriever's next expression of understanding of the technical factors implied by manned spaceflight can be found in the USAF Space Plan for 1961. This plan, prepared at the request of the Secretary of Defense, appears to have been drafted at Schriever's direction or at least under his supervision.[359] Among other things, the report goes into considerable detail proposing a new family of rockets to provide a comprehensive range of launch capabilities graduated by payload and orbit requirements, then turns to the demands of manned spaceflight. It notes:

> The significant restrictions in payload capability have, to date, mediated seriously against adequate considerations being given to man's potential usefulness as an integral component in space vehicles regardless of their objective or mission. This negative approach has resulted in a most serious lack of a well conceived, integrated and supported program of space biology and medicine directed towards valid determination of man's basic needs, tolerance and performance capabilities in even the first generation of orbital vehicles with mission times not exceeding 2-7 days.[360]

The author has thus identified the constraints imposed on any manned spaceflight program beyond the most basic missions by the state, at the time, of the US space launcher capability. A subsequent, substantial, portion of the plan incorporates an ambitious (frankly over-ambitious) proposal to develop a military 'Lunar Expeditionary Force'; although there is no suggestion that any attempt was made to implement such a force, it is in itself a fine example of the connection in the US public eye between exploration and the peacetime role of the military –for now, it is sufficient to highlight that it might have faced legal challenge from the emerging consensus on permitted activity in space.[361]

He then makes the point that the Soviet Union had begun to fly biological specimens on their second orbital flight and maintained the momentum on

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[359] A complete copy of the plan, an associated intelligence threat brief, and a draft of a covering letter for the plan's submission to the Secretary of Defense is in *Schriever Archive, Recently Declassified Documents folder.* The covering letter would appear to have been written for signature by the Secretary of the Air Force, or possibly the Air Force Chief of Staff, and Schriever is the last copy recipient (often, but not infallibly, indicating the drafting office or appointment).

[360] "United States Air Force Proposed National Space Program", Section 4/page12. *Schriever Archive, Recently Declassified Documents folder.*

[361] The Lunar Expeditionary Force might well have fallen foul of Article IV of the Outer Space Treaty of 1967 – "The establishment of military bases, fortifications and installations, the testing of any kind of weapons and the conduct of military manoeuvres on celestial bodies shall be forbidden." (Outer Space Treaty text, Article IV). The lack of any attempt to implement the 1961 Policy made the point moot, and Schriever faced more serious challenges from the emerging legal regime relating to his ideas on space weaponization, but this is another example where he failed to note the emerging consensus.

successive missions, while the USA had yet to orbit a living organism (there had been numerous US sub-orbital flights carrying organisms ranging from fruit flies to monkeys and apes, dating back to V2 experiments in the 1940s, but none had yet achieved orbit). He concludes the section by proposing a graduated series of orbital flights of increasing duration carrying primates, and including exploration of tolerance of radiation during the longer-duration flights. This last objective is plainly a recycling of the BOSS proposal outlined above, and once again shows Schriever pursuing or directing the research necessary to develop a manned spaceflight capability.

In the spring of 1961, Schriever had a chance to influence the shape of the US National Space Programme, and made what may have been his final public intervention on policy issues alongside Wernher von Braun. The Soviet Union had launched Vostok 1 carrying Cosmonaut Yuri Gagarin on his orbital flight on 12 April 1961, about 3 weeks ahead of the planned manned launch of Lt Cdr Alan Shepherd on a sub-orbital 'Mercury' flight.[362] Between these two events, a fit of national recrimination seized the United States, much of it directed at the White House. As Craig Nelson summarizes:

> The shock of Gagarin's achievement convinced any American who wasn't already concerned about Sputnik, Luna and Strelka *[earlier Soviet satellites]* to now join the chorus of complaint against NASA and a drowsy Federal leadership.[363]

Alongside a renewed interest in the USAF Space Plan, the Kennedy administration took particular steps to consider what the military might contribute to the emerging manned space gap opening up with the Soviets. On 25 May 1961, President Kennedy would commit the USA to landing a man on the Moon within the decade.[364] Before doing so publicly, however, he conducted and directed considerable study into what might be possible. This began with a White House meeting chaired by President Kennedy on 14 April 1961 (two days after Gagarin's flight), and was followed by at least two sessions of interviews with key players by Vice President Johnson.[365] On 24 April 1961, both Schriever and von Braun spoke with the Vice President. Nelson reports that:

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[362] Swenson, Grimwood, and Alexander, *This New Ocean* 332-41
[363] Craig Nelson, *Rocket Men: the epic story of the first men on the Moon* (London: John Murray, 2009) 152. Nelson's book has been criticised both for errors of fact in reporting, particularly relating to the Apollo programme, and for its journalistic style, but this particular section of his prose appears accurate in principle.
[364] Swenson, Grimwood, and Alexander, *This New Ocean* 362
[365] Nelson, *Rocket Men* 153.

"Schriever stated that 'we need a major national space programme for prestige purposes', and pointed out that such a direction would help the aerospace industry at a time when many in the USAF believed that the ICBM missile race would be levelling off…At his meeting with the vice-president, von Braun concurred that 'we do not have a good chance of beating the Soviets to a manned 'laboratory in space'…we have a sporting chance of sending a three-man crew around the moon ahead of the Soviets…we have an excellent chance of beating the Soviets to the first landing of a crew on the moon…".[366]

It is worth pausing briefly and trying to explain why von Braun was quite so prescient in his discussion with the vice-president. Nelson himself offers no further analysis, but Neufeld's biography of von Braun provides some explanation. Firstly, the USSR's breakthroughs in orbital flight and manned spaceflight had demonstrated that they had mastered the technology for launching relatively large (heavy) satellites.[367] The step from Gagarin's *Vostok* capsule to a larger, longer-duration vehicle (von Braun's 'laboratory in space') in Earth orbit would have posed challenges, but there was nothing in early Soviet missions to cast doubt on their ability to meet them, and as a 'race' voluntarily entered, it was not clear that the USA would win. Conversely, lunar missions were a much more ambitious target, but the longer time that would be taken to achieve them gave the USA a better chance of catching up, hence von Braun's enthusiasm. As Neufeld notes: "..the technological leap was so large that it potentially cancelled out the Russian advantage in big boosters".[368] Lastly, the three-man circum-lunar mission (implicitly without a landing) had been an early overall goal of the Apollo program, albeit one that had been rejected by the Eisenhower administration. This early meeting with Kennedy's vice-president was an opportunity for von Braun to start lobbying for more ambitious Apollo goals.[369]

As far as Schriever's aspirations were concerned, a national space programme was plainly forthcoming, but it was a civilian programme, entrusted to NASA, in which von Braun would play a crucial part. The extent to which Schriever

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[366] Nelson, *Rocket Men* 156. Nelson does not credit the specific source for this account of Schriever and von Braun's interviews, though he lists multiple possible sources in his collected Bibliography, but the fact that 'space meetings' took place on the date cited is confirmed by Vice President Johnson's desk diary held at the JFK Presidential Library. Schriever's diary in his Archive contains no entry for the date in question.

[367] At launch in 1957, the Soviet *Sputnik 1* had weighed just over 180lbs, and *Sputnik 2*, which followed about a month later carrying a dog as an experimental payload, had weighed over 1000lbs. In comparison, the first US artificial satellite *Explorer 1*, launched in early 1958 (after *Sputnik 2)*, had only weighed about 30lbs. By 1961, Gagarin's spacecraft *Vostok* weighed about 5500lbs.

[368] Neufeld, *Von Braun* 356.

[369] Neufeld provides a more detailed summary of early Apollo options beginning with von Braun's early (and frankly over-ambitious) plans for lunar missions drawn up during 1958-60. Neufeld, *Von Braun* 357-58. For further discussion of the contrasting Eisenhower and Kennedy administrations attitudes to Apollo, see the first chapter of William David Compton, *Where no man has gone before: a history of Apollo lunar exploration missions (NASA SP-4214)* (Washington DC: NASA Office of Management, Scientific and Technical Information Division, 1989) 3-11.

saw this as usurpation of a valid military task is unclear; the content of the USAF 1961 Space Plan, and particularly the material relating to the Lunar Expeditionary Force, suggests that he saw at least some of this as valid work for the USAF, but there is nothing at the time in his diaries to indicate animosity with respect to NASA; on the contrary, the diary entries immediately following these meetings continue to reflect his concerns to keep ICBM development on track, to establish his authority over his new command at AFSC, to continue to grow Air Force Space capability at Vandenberg AFB in California (inextricably linked to the launch into polar orbit associated with reconnaissance missions, and by June 1961 to assisting NASA:

> "* Write letter to NASA offering our help
>
> * Establish SSD [Space Systems Division of AFSC] as focal point and direct that they set up office and work with NASA"[370]

Schriever's last foray into military manned spaceflight was his management of the Manned Orbiting Laboratory (MOL) programme. Rather than recap the lengthy technical history of that programme, this thesis will gauge Schriever's understanding of its technical demands and progress through the testimony he gave to the House Committee on Science and Astronautics on 8 February 1966. The context of the hearing was a Committee investigation into possible wasteful duplication between NASA and DoD space programmes, and Schriever's testimony followed that of Dr Seamans, the NASA Assistant Administrator.[371] The full text of Schriever's prepared remarks survives, and viewed overall, they demonstrate that he had a firm grasp of the technical factors that he was managing at the time.

Schriever explained to the Committee that a comparison had been made between adapting the NASA Saturn 1B launcher and developing a new variant of the Titan III launcher to launch the MOL, with the latter chosen for a variety of reasons. He also explained the relationship between the modifications proposed to the NASA Gemini capsule heat shield and the forthcoming test-flight in that configuration. He then discussed the demands posed by the proposed polar orbit to be adopted by the MOL for operational reasons, the reasons why that orbit then favoured launch from the West Coast rather than the East of the United States (to

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[370] Schriever (part of) Diary Entry for 19 June 1961. *Schriever Archive, Recently Declassified Documents folder.* Bulleting and underlining in original.
[371] Berger, *History of the MOL* 210-11.

avoid overflying land during launch), and the payload constraints that would ensue from any attempt to launch from the East Coast. He reviewed the technical challenges of designing a man-carrying craft for the extended mission duration intended for MOL (30 days), including environmental control, waste management, astronaut health and nutrition and the intended composition of the MOL atmosphere. He briefed the committee on the tracking and monitoring activity that would be needed to support an MOL launch, communication arrangements during an MOL mission and finally the arrangements for emergency and planned recovery of the crew during the mission. [372] Although the brief had been prepared for him by the MOL Program Office, the ability to deliver and defend it before the Committee suggests that Schriever was a man with a sound understanding of the technical constraints and possibilities of the programme he was managing.[373]

Military Manned Spaceflight – Schriever, Policy and Strategy

Some of the policy and strategy issues surrounding military manned spaceflight are actually implicit in the technical survey just completed. The earlier efforts to develop a military manned spaceflight capability were simply aimed at getting an (air)man into space, principally for reasons of national prestige. Developing the capability and demonstrating it was, in itself, a worthwhile mission. Later efforts such as Dyna-Soar and (especially) the MOL, sought to explore more enduringly useful missions and in the case of the MOL, evolved into a proposed operational system. But a persistent problem for all proponents of these schemes was identifying the mission to be undertaken; simply 'being in space' had significance as a national objective, but not to deliver positive military advantage. Thus, two strands of thought are detectable in the analysis of these missions. Firstly, some thought that once human exploration of space was underway, as surely as conflict had spread to the seas and skies with the invention of ships and aircraft, it would spread to space. Adoption of this hypothesis appealed naturally to those subscribing to the notion of 'aerospace'. If a nation was unwilling to contest control of space, it would yield to any space-based threats that might be posed

[372] Summary taken from 'Statement for the Record by General Bernard Schriever, Director Manned Orbiting Laboratory, Department of the Air Force, before the House Committee on Science and Astronautics, February 8, 1966'. *Schriever Archive, Recently Declassified Documents folder.*

[373] Berger notes the various difficulties imposed at this hearing by security constraints, including the fact that only a few members of the Committee staff had been indoctrinated as to the true nature of the MOL mission, and that consequently while Schriever could not mislead the committee, he could not discuss, for example, the reasons behind selection of a polar orbit. Thus multiple versions of the brief were prepared, and some of the Committee members took partisan positions in questioning – most notably the contributions of Florida-based members, who were incensed that MOL operations were being 'taken away' to the West Coast, but who could not have the full reasons behind the decisions explained to them. Berger, *History of the MOL* 208-13

against it, and since the Air Force's role was to protect against such threats from the air, it naturally extended to protection from and in space. An alternative view was that a human presence would enhance or enable the operation of existing unmanned systems, most probably by improving reliability or efficiency. Schriever's thinking was initially concerned with the first issue, but moved significantly towards the second with the passage of time.

A prime example of Schriever supporting the 'exploration' school of thought can be found in his address at San Diego in 1957:

> Several decades from now the important battles may not be sea battles or air battles, but space battles, and we should be spending a certain fraction of our national resources to ensure that we do not lag in obtaining space supremacy. Besides the direct military importance of space, our prestige as world leaders might well dictate that we undertake lunar expeditions and even interplanetary flight when the appropriate technological advances have been made and the time is ripe.[374]

At about the same time, Schriever's ultimate Air Force superior, General Thomas White, was making a related point in testimony to the House Appropriations Sub-Committee. He stated: "missiles are but one step in the evolution from manned high-performance aircraft to true manned spacecraft; and in the forces structure of the future…we will have all three systems."[375] What was not clear, however, from either White or Schriever, was an analysis of exactly how these new platforms would be integrated into this seamless aerospace force. As Stephen Rothstein perceptively notes in his first thesis on the history of the aerospace concept: "There is little evidence to indicate, however, that the considerations and implications of Air Force operations in space from an intellectual perspective were studied much at all."[376]

Two years later, there was evidence that some thought had gone into this issue. HQ ARDC drafted a proposed 'Air Force Space Mission' paper in 1959, which predicted that:

> Space weapons of the foreseeable future will be a family of satellite

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[374] Spires, *Orbital Futures* Document I-2, pp 20-26.
[375] General Thomas D White, Chief of Staff of the United States Air Force in testimony to the House Appropriations Sub-committee hearings on DOD Appropriations for 1958. Cited in Robert F Futrell, *Ideas, concepts, doctrine; Vol I: basic thinking in the United States Air Force* (Maxwell AFB, AL: Air University Press, 1989) 545-46. This is an early example of White expressing his developing concept of 'aerospace' without using the word explicitly. He (and Schriever) would return to public reference to this during 1959.
[376] Stephen M. Rothstein, 'Dead on arrival? : the development of the aerospace concept, 1944-58', (Air University, Maxwell Air Force Base, Ala. 2000 SAAS), 56. Rothstein attributes much of the early impetus for 'aerospace thinking' to White; Schriever's attitude to the concept is more ambiguous. Rothstein expanded greatly on this analysis in his subsequent PhD thesis, already discussed in Chapter 3 of this thesis, and explored in greater detail below.

> vehicles with common characteristics and will tend to become larger, carrying combined payloads as our capabilities progress. They will be manned or man maintained. Many of them will be general purpose type vehicles satisfying many requirements. These satellite forces will primarily satisfy strategic and air defense and support requirements. They must be integrated into a common system with the other strategic and air defense weapons. They must use a common communications and tracking system.[377]

Although this showed that the problem had achieved some attention, it also implies considerable faith in rapid technical progress. At least as far as the 'support' missions were concerned, there was also clearly emerging evidence of the potential of unmanned systems. By the end of 1959, Discoverer/CORONA was about half way through its series of development launches, the first 'TRANSIT' navigation satellite had been launched (albeit unsuccessfully) with a second nearing readiness for launch, and Project SCORE had demonstrated the potential of satellite communications in December 1958 – all by means of unmanned vehicles.

The shift in Schriever's thinking began to become apparent. Speaking to the National Geographic Society in Washington DC in 1958, he stated: "We can investigate by instruments alone a very large part of what there is to know about the other planets and about the conditions in space between planets. But sooner or later we will need more data that cannot be practically supplied by instruments. At this point man, even with all the equipment he must take with him, will become the cheapest, lightest, and most practical 'instrument' for obtaining and assessing that added data."[378] The thrust of his remarks is still based on exploration and 'being there', but the context is now man augmenting and supplanting an unmanned probe.

During 1959-61, Schriever grappled with the rapidly crystallising 'aerospace' concept. In its simplest form, this concept proposed that the operating mediums of 'air' and 'space' were essentially indivisible, and hence that the military service charged with overall responsibility for 'air' should acquire a parallel responsibility for 'space' as technology made it accessible. This emerging idea had powerful sponsors; although it had not originated with him, General White, the

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[377] *Schriever Archive, Box 6, Folder 3.* The author of the paper is unknown, as is its precise date, but the office routing indicator preserved on the copy in the Archive probably indicates a 1959 origin in an office immediately subordinate to Schriever's, and he retained a draft copy of it in his papers.

[378] 'Quotable Quotes From Congressional Testimony And Speeches By Lt. Gen. B. A. Schriever Commander, Air Research And Development Command', p37. *National Security Archive (George Washington University), Earlybird Files, Box 110.*

then-Chief of Staff of the Air Force, was a very early champion of it.[379] As Chief of Staff, he took the opportunity of promoting it as a vision for how the Air Force should configure itself, and via subordinate staff, he drove its use into doctrine.[380] Schriever, however, remained unconvinced as to its coherence and validity. In his eyes, 'air' and 'space' remained separate domains, albeit ones in which the Air Force could still logically (in his eyes) claim primacy. Stephen Rothstein dwells at some length on Schriever's ambiguous relationship with the term, noting, for example, that in his testimony before Congress in 1959, he failed to defend the 'indivisibility' argument, and in fact left scope for attack on the concept by the other Services. In subsequent public-speaking engagements, Schriever's commitment to the concept was again at best ambivalent.[381] Without becoming engrossed in the propriety of Schriever publicly questioning his superior's intent, one simple rationale for the discrepancy of thought may be that White was concerned to ensure that the statutory basis for an Air Force space programme was established via appeal to US Code Title 10, while Schriever, as a working engineer wished to highlight the technical challenges of operating in the extended (or new) medium; specifically, they would require different solutions to those addressing the difficulties of conventional air operations.[382]

By 1961, Schriever was plainly thinking more deeply about the implications of linking exploration with military activity. In an undated note/diary entry, he remarked in the context of the military space programme: "Lunar program could become political liability unless Nat[iona]l Security potential is identified".[383] That would never, in fact, happen, and the conceptual tide was running against such thoughts. Ultimately, the Outer Space Treaty of 1967 would explicitly prohibit a

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379 General Thomas D White was Vice-Chief of Staff of the Air Force during 1953-57, and Chief of Staff during 1957-61. Stephen Rothstein credits Dr Woodford A Heflin from the Air University with first use of 'aerospace' (citing a glossary quoted in Futrell's guide to Air Force Doctrine), and dismisses Frank Jennings' counter-claim to have been its originator. See Rothstein, 'Ideas as Institutions', 163, Futrell, *Ideas, concepts, doctrine; Vol I: basic thinking in the United States Air Force* 553, Frank W Jennings, 'Doctrinal conflict over the word aerospace', *Airpower Journal,* Vol 4 No 3 (Fall 1990)

380 Rothstein, *ibid.* Rothstein devotes several subsequent pages of his thesis to explaining General White's activities to promote the term as he gained confidence in its use.

381 Rothstein, 'Ideas as Institutions', 214-17, 37-40. Rothstein would seem to have had access to the series of internal USAF memoranda known as 'Air Force Information Policy Letters' when writing his thesis. These letters were used to convey Commanders' intent on a variety of topics to forces under his command, at this time notably expanding on the Air Force's role in space; this access makes Rothstein's analysis of the USAF internal tensions comprehensive and persuasive.

382 The US Services are generally very mindful of the legal justification for their activities, enshrined mainly in 'Title 10' of the US Code (of Laws). In that to this day, there is no agreed boundary between air and space, this would mean that arguing that air extended seamlessly into space could be seen as bringing it into the ambit of the Air Force's statutory permissions.

383 *Schriever Archive, Recently Declassified Documents folder.* The document is filed with other 1960-61 entries, and must post-date the Kennedy inauguration (January 1961), since the top of the page is marked prominently 'See VP Johnson'.

military mission on the Moon.[384] Prohibition of military activity was one of the last stipulations added to the succession of UN General Assembly Resolutions that preceded the Outer Space Treaty, but the general tenor of those resolutions gave less and less scope for extra-terrestrial military activity.

The March 1961 report of the USAF Space Study Committee (established by Schriever in October 1960) shifted the balance even further.[385] Its section on Manned Spaceflight opened with a reminder of the historical importance of the US Armed Services to exploration (the Lewis and Clark expedition to the Pacific Coast in 1804-05 is mentioned explicitly), but it moves rapidly to an analysis of manned spaceflight as an enhancer of unmanned or automatic activity: "…it is unlikely that information filtering in orbit can be performed more efficiently by machines than by man…The presence of a man to make and interpret tests, and to take corrective action, will make possible reliable continuous operation of much more complex systems…Flexibility of alternatives in perception and action can only be provided in automated devices at a great penalty in weight and reliability. Man can better provide this flexibility for some time to come."[386] The problem with this belief is that unmanned system reliability was improving rapidly. Accidents still occurred; nobody claimed '100% reliability', and where it was expected, for example in manned spaceflight or in the launch of payloads with nuclear materials, it was hard to achieve, but the experience that Schriever had endured of repeated launch and on-orbit failures, for example early in the Discoverer/CORONA programme was thankfully in the past.

Even if satellite systems were becoming steadily more reliable, there remained the question of whether they could be enhanced by the presence of a human operator, and a subsidiary question of whether there were any military tasks (not so far explored in the civilian space programme) that also required the presence of an astronaut. The MOL was an effort to resolve these questions. It arose out of an exchange between Vice-President Johnson and Secretary of Defense McNamara during 1963. Preliminary details of the intended programme were worked out by Dr Harold Brown, then Defense Director of Research and

[384] "The establishment of military bases, installations and fortifications, the testing of any type of weapons and the conduct of military manoeuvres on celestial bodies shall be forbidden." Treaty on Principles Governing the Activities of States in the Exploration and Use of Outer Space, including the Moon and Other Celestial Bodies, Article IV. Text available at www.unoosa.org

[385] Nothing in Schriever's diaries indicates the exact relationship between the March 1961 report of the Space Study Committee, and the April 1961 Space Plan alluded to earlier (such as whether the Committee report accelerated or shaped the subsequent plan) but the similarities between them are obvious, and further indicate a connection between Schriever and the April 'Plan'.

[386] 'Report of the Air Force Space Study Committee' dated 20 March 1961, pp11-12. *Schriever Archive, Box 140, Folder 6.*

Engineering. Carl Berger reports that: "According to his instructions, the basic purpose of the Manned Military Orbiting Laboratory (MMOL) was to 'assess the military utility of man in space'. Since man was not considered useful unless he performed a variety of tasks in space, MOL equipment was to be chosen both to support the astronaut and challenge his flexibility and judgment".[387] Headquarters USAF sent the full direction to Schriever and directed him to submit a development plan.[388] Development work was slightly hindered, however, when the USAF settled on experiments relating to reconnaissance systems, and early suggestions that the MOL might meet operational requirements as well as performing experimental studies. Those in higher headquarters who were aware of the existence of the National Reconnaissance Office (NRO) feared that MOL activity might compromise the tight security that surrounded NRO activities. Schriever nonetheless submitted an MOL development plan in January 1964, which included the proposal that he be appointed programme director.[389] The following month, "Schriever's and Ferguson's planners agreed that 'the objectives of the MOL should not be based on a single set of experiments aimed only at one mission, such as reconnaissance.'"[390] Despite opposing views, aimed at maintaining a reconnaissance emphasis, a balanced programme of experiments was agreed. The 15 topics still included reconnaissance elements, such as direct observations over land and sea, and tracking ground targets but also included physiological and bio-medical tests of the astronauts, experiments in building and maintaining equipment, spacecraft navigation and gaining 'spacewalk' experience.[391]

Unfortunately, alongside this effort to maintain a broad suite of experimental themes within the MOL programme, a parallel covert study into the possibility of deploying a very-high resolution reconnaissance system aboard the MOL was being conducted at the direction of Dr Brockway McMillan, who held the dual appointments of Undersecretary of the Air Force and Director of the NRO: 'On 7 June 1963 he instructed the Directorate of Special Projects (SAFSP), which developed and operated the unmanned reconnaissance systems, to undertake a study and simulations to determine man's ability to recognize "high priority targets" and to point "high resolution cameras so as to obtain coverage of these targets."' When its report was rendered, it suggested that man could indeed "make

[387] Berger, *History of the MOL* 52
[388] Berger, *History of the MOL* 54
[389] Berger, *History of the MOL* 56
[390] Berger, *History of the MOL* 61
[391] Berger, *History of the MOL* 69-70. One primary experiment is unknown as it remains classified in the released version of Berger.

substantial contributions to a satellite reconnaissance mission."[392] When this conclusion was applied to the MOL project-definition work then underway, the security implications (acknowledgement of the NRO's existence and the activity of satellite reconnaissance) rendered the MOL programme classified in itself; additionally, from then on, MOL designs incorporated provisions for the large camera and telescope envisaged by the study. Further studies explored whether such a camera could operate in an unmanned mode when the Laboratory was unoccupied, and ultimately whether there was a need for a man aboard at all. Schriever's view on this quickly became apparent: "General Schriever was worried about the effect the unmanned system might have on MOL development planning, being strongly opposed to any possible decision to eliminate the manned version. On 29 December, during a conversation with General Evans, he proposed they undertake an operational analysis of "manned and unmanned capabilities for reconnaissance." He was particularly interested in the "quantitative differences" in the reconnaissance "take" of the two modes and also in a qualitative comparison of the resolutions on specific targets and the reliability of the two configuration on a 30-day mission."[393] Notably absent from this discussion is any reference to 'experiments'. The MOL has morphed into a reconnaissance system to meet operational requirements.

Much of the pressure to re-focus the MOL on operational tasks had come from the Presidential Science Advisory Committee (PSAC), and much of the remainder of Schriever's time in charge of MOL was occupied with an attempt to reconcile tensions between PSAC, the Department of Defense, the NRO and the USAF. With the MOL rationale now only tenuously connected to the original experimentation plan, the DOD Budget Office were also evaluating the relative costs of the manned and unmanned missions – manned missions costing more.[394] During the summer of 1966, USAF justifications for retaining the man aboard centred on his ability to improve the quality of the reconnaissance results achieved, exploit 'targets of opportunity' and improve efficiency by eliminating photography marred by cloud cover. There is also, however, some evidence in Berger that some PSAC staff supported the original experimental MOL ethos: 'one of the PSAC members, Dr. Steininger, remarked during a visit with the NRO staff that the

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[392] Berger, *History of the MOL* 59, 93
[393] Berger, *History of the MOL* 171-72
[394] The released version of Berger's history does not reveal the physical resolution that the manned and unmanned camera systems were intended to achieve, thus the detail of his reporting of the cost comparisons are occasionally difficult to follow due to redaction. The general point he makes, however, is clear. Potentially unmanned missions could also have lasted longer (60-days proposed in lieu of 30-day manned missions), yielding more results for a given fixed cost.

DOD was "killing itself in attempts to justify the man." He said the man did not need to be justified to the panel, which accepted his presence. "MOL is an experiment in which man is the experimentor," he said. "We should keep it that way." Further, he stated 'that the panel insisted on automating all MOL functions so that the man "could stay loose and be an experimentor."'[395]

In August 1966, General Schriever retired from the USAF. As far as the MOL programme was concerned he made two 'valedictory' statements. One was the testimony to the House Committee on Science and Astronautics in February 1966 described above. The other was a memorandum delivered to the Secretary of the Air Force (Harold Brown) on the day he stood down. In it, he restates his belief in the importance of military manned spaceflight, and laments the conservative pace at which it has been developed. In the portion quoted by Berger, he makes specific comments on the MOL: "The inception of the Manned Orbiting Laboratory Program has given us the opportunity to bring into sharper focus a broader appreciation of the potentials of military space by now encompassing the uniqueness, flexibility, and responsiveness of man."[396] Schriever's faith in the utility of military manned spaceflight was now tied to militarily useful missions, but he had struggled throughout the MOL programme to protect its demonstration function. Had he succeeded, it might have validated this belief without concentration on a single mission (reconnaissance), where in fact unmanned automatic systems were becoming more reliable, efficient and productive.

Military Manned Spaceflight – Schriever and Administrative Action

Schriever's main administrative activities (and difficulties) relating to military manned spaceflight involved managing the USAF's relationships with other agencies, specifically ARPA and NASA. In this section they are examined, particularly that relating to NASA, in order to demonstrate that Schriever maintained far better relations with them than with ARPA, while still recognising the differing motivations behind the actions of each agency and the USAF.

[395] Berger, *History of the MOL* 186
[396] 'Manned Mil Missions in Space' (Schriever to Sec AF dated 31 August 1966), cited in Berger, *History of the MOL* 187. Schriever makes no specific comment in these remarks about the reconnaissance versus experimentation controversy, perhaps because the memorandum was unclassified. It is possible that Schriever intended that it be published, though the author can find no evidence that this happened. Sadly, no copy of the full text appears to survive in the Schriever archive either.

The creation of ARPA was coincident with the formation of NASA during the second Eisenhower administration. Multiple factors converged to drive the creation of the two agencies, and it is not the intent of this thesis to explore them.[397] Their result was that ARPA was created from scratch to manage a variety of advanced research programmes for the DoD on behalf of all the Services. This included military space programmes. Soon after, NASA – a reformation of the previous National Advisory Committee for Aeronautics – was given responsibility for management of the civilian space programme. At this point, the military unmanned space programme was transferred back to the Services to deliver, but with ARPA retaining an oversight role, while the manned portion was subsumed into NASA activity.

Schriever's relationship with ARPA was frequently antagonistic. By the end of 1958, ARPA was 'reaching into' the USAF to control the development of the Discoverer/SENTRY programme.[398] In February 1959, the tenor of the relationship between them can be gauged by a memorandum transmitted within Schriever's organisation. Colonel Sheppard, Assistant Deputy Commander for Military Space Systems, wrote to Colonel Curtin (a close colleague of Schriever's and at that time a staff officer within ARDC) stating: "…I have no hope that ARPA can be turned into a useful activity of any kind…Somehow we should be able to make use of the fact that industrial interest has governed many of their actions to an extent almost – if not quite – illegal…".[399] Sheppard goes on to propose sponsoring a history of ARPA activity compiled by external agencies and scholars. Matters appear to have culminated in August and September 1959, when Schriever twice wrote to the Chief of Staff of the Air Force about ARPA's performance.[400] As was mentioned earlier in this chapter in our analysis of Dwayne Day's account of SAMOS E-5 activity, Schriever even managed to enlist the support of General Curtis LeMay in his campaign to rescue the USAF space

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397 There are numerous accounts in the literature of the various lobbyists claims and counterclaims surrounding ARPA and NASA. For examples, see Walter A. McDougall, *The heavens and the earth : a political history of the space age* (Baltimore, MD: Johns Hopkins University Press, 1997) 167-69, Robert A. Divine, *The Sputnik challenge* (New York: Oxford University Press, 1993) 100-01

398 Letter dated 5 December 1958, ARPA to Undersecretary of the Air Force 'DISCOVERER-Thor project and SENTRY programs'. *Schriever Archive, Recently Declassified Documents folder.* Schriever also received an internal ARPA memorandum dated 17 December 1958 detailing ARPA intent for the SENTRY programme. *Same Folder.*

399 Memo Col Sheppard to Col Curtin dated 5 February 1959. A copy was furnished to Col Boatman, who was Schriever's aide at the time, and Schriever retained it in his archive. *Schriever Archive, Recently Declassified Documents folder.*

400 Letters General Schriever to General White (untitled – one relating to SAMOS, one to MIDAS) dated 15 September 1959. *Schriever Archive, Recently Declassified Documents folder.*

programme from ARPA attention. Eventually, Schriever prevailed, and the USAF regained adequate control of the military space programme.

Schriever's relationship with NASA was apparently more nuanced. The same internal memorandum referenced above relating to ARPA and its deficiencies adopted a more conciliatory, albeit still potentially mischievous, approach to NASA relations. Col Sheppard suggested:

> My current feeling is that the politic thing to do is not to try to draft up and sell in any high pressure, overt way a category of things which NASA should do as contrasted to the things the military departments should do. It seems to me that we can accomplish our objective with much less commotion by listing the countless requirements we have for scientific data relative to space and then laying these requirements firmly on NASA through all the channels we can find. This will insure that they are kept so busy doing the right things that, in general, they will be unable to intrude into genuine military business.[401]

There is again no record in the archive of Schriever's reaction to this suggestion but it certainly illustrates a view held in his HQ at the time. In 1961, a UPI news release (retained by Schriever in his papers) suggested that the DOD 'might seek to take over NASA', but beneath the sensationalist headline UPI were actually reporting a 1960 memorandum from General White, the Chief of Staff of the Air Force which had gone to Schriever among other recipients. They noted that General White had written: "It is obvious that NASA will play a large part in the national effort in this direction [spaceflight], and moreover, inevitably will be closely associated, if not eventually combined with the military…It is perfectly clear to men that particularly in these formative years for Air Force must [sic] for its own good as well as in the national interest, cooperate to a maximum extent with NASA to include the furnishing of key personnel even at the expense of some Air Force dilution of technical talent."[402] By 1961, Schriever appeared to have reached a working accommodation with NASA. In his diary for 10 September 1961, he noted in 'to do' list-style: "1. More comms between BAS and TG [Schriever and Trevor Gardner] with J Webb [the inaugural NASA administrator]. 2. Need for existing resources now. 3. Why NASA can't build and do at the same time. 4. Why USAF needs what it has – particularly life sciences. 5. Why full partnership is needed – mgt resources etc." On the reverse of the same page, Schriever summarises an

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[401] Memo Col Sheppard to Col Curtin dated 5 February 1959. *Schriever Archive, Recently Declassified Documents folder.*
[402] UPI Press Release by David Burnham, dated 7 March (1961 can be inferred from reference to General White's tenure as CSAF and a further reference to McNamara as Secretary of Defense). *Schriever Archive, Box 136, Folder 1.* The author has not been able to locate an original copy of General White's memorandum referred to.

action list for interaction with Mr Jerry Wiesner, President Kennedy's Presidential Scientific Adviser: "2. Jerry Wiesner *Get Together. *Down Range. *USAF/NASA – how do we work together – MLLP [Manned Lunar Landing Program?] (Wiesner had made a point of advocating for unmanned lunar and planetary exploration, notwithstanding President Kennedy's recent public commitment to put a man on the Moon within the decade.)[403] This shows Schriever simultaneously continuing to forge a constructive relationship with NASA, preserving USAF equity in life sciences research and influencing a notable critic of manned spaceflight, perhaps to NASA and the USAF's mutual benefit.

When the MOL was first proposed, the management context was an agreement of January 1963 between NASA Administrator Webb, and Secretary of Defense McNamara. This agreement related initially to military interest in the NASA Gemini programme. It committed the parties "…'to insure the most effective utilization of the Gemini program in the national interest.' It created a Gemini Program Planning Board (GPPB), one of whose aims was 'to avoid duplication of effort in the field of manned space flight and to insure maximum attainment of objectives of value to both the NASA and DOD.'" Additionally, it stated that "neither agency could initiate a major new manned space flight program in the near-earth environment without the other's consent."[404] Schriever was taking a close interest in NASA activities at this time, delegating Major Generals Keeling and Ritland special responsibility; in his diary for 5 January 1963, he notes "NASA *Procurement and Contract, Keeling and Ritland – formal procedures for monitoring above – for the record."[405]

In March that year, NASA announced their intention to proceed with a study of a manned (civilian) laboratory, orbiting the Earth, with the intent of deciding for or against construction in time for the FY65 budget submission. USAF officials reminded NASA of the terms of the January agreement, and Major General Ritland, presumably following his direction from January, advised Schriever that he thought NASA and the USAF should coordinate space station planning.[406] Discussions between DOD and NASA ensued during March and April, culminating

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[403] Diary page in *Schriever Archive, Recently Declassified Documents folder.* For comment on Wiesner's attitudes to manned exploration, see Arnold S Levine, *Managing NASA in the Apollo Era (NASA SP-4102)* (Washington DC: NASA Scientific and Technical Information Branch, 1982) Chapter 2. President Kennedy made his public commitment to manned lunar exploration before a joint session of Congress on 25 May 1961.
[404] Text of the 'Gemini Agreement', quoted in Berger, *History of the MOL* 19, 22.
[405] *Schriever Archive, Box 6, Folder 14.* Underlining and bulleting in original. 'Keeling' is almost certainly Major General Gerald Keeling, at that time Schriever's Deputy Chief of Staff for Procurement and Production at Air Force Systems Command.
[406] Berger, *History of the MOL* 22-23.

in a meeting between Secretary McNamara and James Webb on 27 April, in which they: "reached a compromise of sorts. That is, the space agency head agreed that funded space station studies "should be jointly sponsored by the Department of Defense and NASA." Webb also accepted the argument that DOD and NASA would proceed, with hardware development "only by mutual consent."'[407] The USAF moved to capitalize on DOD interest in the subject area, by offering their own plan for an orbiting space station.

> The USAF proposal struck a responsive chord in OSD. On 25 May Secretary McNamara advised Zuckert that he considered "the Orbital Space Station Program as one requiring a new national mission to be assigned by the President on behalf of all national interests." He agreed that since the lunar landing assignment previously had been given to NASA, "the near-earth interests of the DOD might be considered a logical reason for assigning to the DOD this undertaking … He expressed confidence that, if such an assignment were made to the DOD, "the. Air Force could carry out its management responsibilities cooperatively with the NASA."[408]

This direction plainly flowed down to Schriever. His diary suggests that as Commander Air Force Systems Command during May 1963, he was principally concerned with the travails of the RS-70 proposed reconnaissance aircraft, and with establishing the personnel who would conduct the Systems Command 'Project FORECAST' technology futures study, but it also shows that he kept a 'watching brief' on space station progress. Although its future probably did not appear bright, his diary also suggests enduring interest in the Dyna-Soar programme and its objectives.[409] By the end of the year, as recounted above, the Air Force had a proposed MOL programme with Schriever as its Director.

The last individual mention of relationships between NASA and the USAF in the context of MOL arise in 1965, near the end of Schriever's involvement with the programme. Without specific context, Berger reports that: "By 1965-1966 Air Force and NASA manned space programs had evolved to the point where the competition between the two agencies had manifestly declined… This period saw

[407] Letter Secretary McNamara to Mr Zuckert (Secretary of the Air Force) cited in Berger, *History of the MOL* 24.
[408] Berger, *History of the MOL* 24-25
[409] A good example is Schriever's diary entry for 28 October 1963, which indicates his interest both in an operational mission for Dyna-Soar and in the associated experiments and studies. On 28 November 1963, there is a further entry listing both 'X-20' (the alternate designation for Dyna-Soar) and 'Space Station – Source Selection' as matters of interest that day. It seems noteworthy that both topics arose on the same day, so close to the end of the Dyna-Soar programme, although the context of what was being discussed or analysed is unclear from the entries. Dyna-Soar was cancelled on 10 December 1963. Both entries in *Schriever Archive, Box 6, Folder 16.*

increasing coordination of the efforts of both agencies."[410] He then recounts, however, an instance where although Schriever worked to promote good relations with NASA, his methods were questioned from above. In September 1965, NASA and DOD senior leadership proposed "to establish an informal six-man DOD/NASA committee to review and solve manned space flight problems "not solvable by any other level." The committee, they agreed, would also serve to assure Congress that they were working closely together in the Gemini, Apollo Applications, and MOL areas". Schriever was unconvinced of the need for the committee: "General Schriever, however, had been attempting to establish a close personal working relationship with Dr. Mueller and felt that the proposed committee would undercut his current effort."[411] In the end, Schriever was overruled and the committee established, but by the time this happened, Schriever's involvement with MOL was coming to an end.

Throughout this account of Schriever's interaction with NASA, he was working to build a constructive relationship with them. He recognised the conceptual difference between military and civil/scientific spaceflight but also the technical similarities. He also understood the significance of their early efforts for national prestige and worked pragmatically to forge a productive relationship with them. After his retirement, he summarised it thus:

> I was never opposed to NASA as such. What I was concerned about was the constant reference to space for peaceful purposes and forgetting or implying that we weren't going to do anything in space for military or national security needs…My own feeling is that had we not had NASA that we would have proceeded much more rapidly in our military space programs… So it was not NASA per se that I objected to. It was the space for peaceful purposes baloney that kept cropping up all the time. If the President had just said, "We're also going to do those things necessary for national security," I would have been perfectly happy… All the time that I was running the Systems Command, I made available some of my best people to NASA …and, as far as I'm concerned, supported NASA with all the resources that we had available at all times in the most cooperative way.[412]

Despite the pressures he was working under, and the wider remit of programmes he had to manage as Commander of Air Force Systems Command compared to his previous appointments, it seems clear that he recognised and

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[410] Berger, *History of the MOL* 222.

[411] Both quotations from Berger, *History of the MOL* 238. There are, unfortunately, no diary entries from Schriever to corroborate or add context to Berger's assertion. This specific interaction is the last mention of Schriever in Berger's MOL History, which goes on to recount the rest of the programme until its cancellation in 1969.

[412] Schriever, 'USAF Oral History Interview', 42-43..

prioritised relations with external agencies appropriately in pursuit of the military manned spaceflight mission.

Military Manned Spaceflight – Summary

Schriever's faith in the utility of military manned spaceflight was sincere and enduring, but it must be evaluated against the steps he took to achieve it and the criterion of technical risk reduction, conceptual maturity and administrative span of control and competence. The analysis is complicated by the limited practical extent of the USAF's manned spaceflight programme; however, it can nonetheless be evaluated as to the likely outcomes by comparison with the NASA manned spaceflight programme, at least as far as technical aspects are concerned.

Schriever's first attempted forays into manned spaceflight – his advocacy for the MISS programme, and perhaps his preparation for a manned SAMOS capsule, closely mirrored the NASA Mercury programme. Schriever also had a proven understanding of many of the technical challenges of manned spaceflight, partly from studies he had promoted, and partly because the NASA Mercury and Gemini programmes relied on launchers based on USAF ICBMs.[413] It thus seems reasonable to surmise that MISS and manned-SAMOS would not have posed any insuperable technical challenges if they had proceeded to the hardware stage. Schriever took a close interest in the Dyna-Soar programme through most of its life, despite not having had significant involvement in it initially. He was also aware of its dependence on a Titan-variant launcher. The degree of technical risk associated with Dyna-Soar would have been substantially higher than with the earlier ballistic capsules, due to the challenging goal of ensuring its controllability for glide-landings to fixed landing sites, but there were some technical analogues such as the X-15 research aircraft then in use which might have suggested that Dyna-Soar was at least technically plausible.

When Dyna-Soar was eventually cancelled in favour of the MOL programme, Schriever became much more closely involved with its day-to-day management. From the technical standpoint, the MOL was an ambitious undertaking, but it reused elements of proven technology such as the Gemini capsule for crew transport and a developed Titan launcher. A convincing technical analogue for MOL was the subsequent Skylab mission flown by NASA in 1973-74. It utilised surplus Saturn and Apollo components after the last lunar mission, in a

413 The initial sub-orbital Mercury launches used a booster based on the US Army's 'Redstone' missile, but all orbital Mercury launches, and the entire Gemini programme relied on Atlas and Titan variants respectively.

technically similar configuration to MOLs use of Gemini and Titan parts. Although not without its problems (part of the laboratory was damaged on launch, necessitating improvised repairs by its crew), it yielded useful data and is generally regarded as successful. It could be argued that its impromptu repair by its crew was exactly the kind of capability intended for demonstration by MOL.[414] Thus Schriever's faith in the technical plausibility of MOL appears well founded. The technical plausibility of the manned reconnaissance mission remains unknown; all Schriever had to base his optimism on were some preliminary experiments and trials conducted on Earth, including use of a Lockheed simulation facility, but these were in themselves promising, and thus his endorsement of the proposal appears sound. Problems would, however, arise over the policy and conceptual implications of combining an experimental programme with an operational mission.

As was outlined above, Schriever's view about the conceptual relevance of manned spaceflight shifted from an initial focus on exploration to one of exploitation during the latter stages of his career. Unfortunately for him, he never, however, managed to synchronise this vision with a task that the Air Force could legitimately address; in the earliest days of manned spaceflight, he understood the technical challenges posed by exploration, and believed the Air Force was well placed to solve them, but was unable to gain support in the higher levels of government for this mission to be assigned to them. As manned spaceflight matured, he advocated for a comprehensive programme of experimentation to validate his ideas about possible missions, but was then unable to keep the MOL oriented towards them, as it was instead redirected into meeting operational requirements, isolating its original experimental ethos. These, in turn, delayed its development, and by the time Schriever retired, it was already caught in the vicious spiral of delay, increased costs and diminished budgets that would lead to its cancellation. Meanwhile, the manned reconnaissance imperatives disappeared as improved unmanned systems became available.

Finally, as regards administrative span of control and related matters, Schriever moved convincingly to forge effective relationships with NASA, particularly where their cooperation had mutual benefits. He was perhaps fortunate that the services regained operational control of their advanced research and development programmes from ARPA at a critical stage in their evolution. He was

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[414] A brief comparison of MOL and Skylab can be found in the extensive Skylab history by David Hitt, Owen K. Garriott, and Joe Kerwin, *Homesteading space : the Skylab story* (Outward Odyssey Lincoln, NE: University of Nebraska Press, 2008). Hitt *et al's* analysis of MOL (at pp12-13) is hampered by the MOL Reconnaissance Mission still being classified in 2008, but the general principles they outline hold.

unable, however, to recreate the budgetary authority he had enjoyed with respect to ICBM development in order to enable manned spaceflight programmes. The overt reason was a sea-change in the appetite within the Department of Defense for this level of autonomy, as exemplified by Secretary McNamara's centralising tendencies. Underlying this, there was also the fact that concurrency and devolved budgetary authority relied on their objective being assessed as being of critical national importance (described as 'crash' priority in Chapter 3 above). It is very hard to see a programme of laboratory experiments to evaluate what military tasks could be performed in space being accorded such a status.

Taken overall, the military manned spaceflight programmes as pursued by Schriever had the characteristics of a 'pet project'; manageable technical risk with a plausible route to its reduction, an at least adequate administrative span of control, perhaps threatened by budgetary difficulty, but a grave deficiency in a plausible conceptual framework. In the earliest days of manned spaceflight, the conceptual task was simple, but the USAF was excluded from its direct execution by policy. When that stipulation was relaxed, they struggled to define what the evolving manned space mission should be. When they proposed an experimental programme to resolve what it could be, they were unable to protect it from being diverted to meet (transient) operational needs, and by the time those had passed, the programme was too costly and Schriever had passed into retirement. His enduring enthusiasm for the mission was palpable, but he was reduced to lobbying for it from outside the Service.

CHAPTER 6 – DAYDREAMING: THE USAF AND SPACE WEAPONIZATION

The last area of space capability explored by Schriever was the thorny issue of space weaponization. Even today, there is no universally agreed definition of what would constitute a 'space weapon', or what act involving one (construction? deployment? threatened use? actual use?) would constitute 'space weaponization'. In this Chapter, this thesis looks at the systems that at various times have plausibly been considered 'space weapons', and the extent to which the USAF explored their operation or brought them into service. It starts by considering the competing definitions of a space weapon, then looks in turn at the various systems employed, or considered seriously, by Schriever and the USAF. Using the same model as previously, it considers specifically Schriever's understanding of the technical and conceptual problems they posed, and the actions he took to enable their employment.

Space Weapons – Definitions and Scope.

In deciding what might be thought to be a space weapon, an analyst must consider what is their unique or identifying feature. Existing analysis varies, with some commentators analysing putative space weapons by the destructive mechanism envisaged: directed energy (typically laser or microwave derived), various conventional (i.e. non-nuclear) warheads for use in space or against surface targets and use of nuclear warheads, again either against space-borne or terrestrial targets.[415] An alternative view, based simply on the area of operation, provides a simple definition of a space weapon as: "…any system that directly works to defeat space assets from terrestrial- or space-based locations or terrestrial-based targets from space."[416] Generally excluded as 'space weapons' are Ballistic Missiles, on the grounds that though they travel through space, they do not achieve orbital velocity. Views on whether anti-ballistic missiles (ABM) are space weapons or not is less consistent. Without naming specific sources, Paul Stares suggests that they were commonly regarded as a 'space weapon' during

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[415] An example of such analysis can be found in Bob Preston et al., *Space Weapons, Earth Wars* (Santa Monica, CA: The RAND Corporation, 2002) Chapter 3, pp23-50.

[416] David W Zeigler, 'Safe Heavens: Military Strategy and Space Sanctuary', in Bruce M. De Blois (ed.), *Beyond the Paths of Heaven: The Emergence of Space Power Thought* (Maxwell, AL: Air University Press, 1997), 191.

the Eisenhower era.[417] However, contemporary US national policy made a relevant distinction: "… the relation between outer space technology and ballistic missile technology is recognised, US policy on ballistic missiles is not covered in this policy. Anti-missile defense systems also are not covered except to the extent that space vehicles may be used in connection with such systems."[418] While recognising this divergence of opinion, this thesis will, take the contemporary view and include them.

Space Weapons – USAF Experience

Initially, the USAF was excluded from operation of an ABM by military policy; surface-to-air missiles (SAMs - aimed at aircraft) were operated by the US Army, and since the first operational ABM system was a development of the 'Nike' SAM, known as 'Nike-Zeus', the Army retained ownership and responsibility for them. In 1955, the USAF revived a previously abandoned proposal known as 'Wizard' in an attempt to seize the role back from the Army. After a bitter and prolonged dispute over several years, which expanded to incorporate a parallel debate about the competing nature of the USAF 'Thor' and the US Army 'Jupiter' ballistic missiles, the USAF abandoned Wizard for the second time and temporarily withdrew from ABM operations.[419]

At the same time as it failed to secure an ABM role, the USAF was exploring anti-satellite technologies. One justification for this need was the realization that the USA would be as vulnerable to satellite reconnaissance overflight as they hoped the USSR would prove to be. They envisaged thus needing to be able to destroy hostile overflying satellites. Early statements of intent towards this capability are easy to find, for example as stated by General Gavin:

> It is inconceivable to me that we would indefinitely tolerate Soviet reconnaissance of the United States without protest, for clearly such reconnaissance has an association with an ICBM program. It is necessary, therefore, and I believe urgently necessary, that we acquire at least a

[417] Paul B. Stares, *The militarization of space : U.S. policy, 1945-1984* (Cornell studies in security affairs Ithaca, N.Y.: Cornell University Press, 1985) 47-48

[418] From 'General Considerations: Scope of Policy' in 'US Policy on Outer Space', agreed between the National Air and Space Council and the National Security Council on 12 January 1960 and published later that month. Reproduced in John M Logsdon et al. (eds.), *Organizing for Exploration* (NASA SP-4407 - Exploring the Unknown: Selected Documents in the History of the US Civil Space Program, 1; Washington DC: NASA, 1995) 362

[419] For a more detailed analysis of this dispute, see Barry Leonard, *History of Strategic and Ballistic Missile Defense: Volume II: 1956-1972* (Colingdale, PA: Diane Publishing, 2011), and specifically Chapter 4. It is also worth noting that the 'Wizard' proposal of 1955, although touted in some circles as a revival of interest in work begun at Johns Hopkins University in 1947, in fact had almost nothing in common with its predecessor.

capability of denying Soviet overflight – that we develop a satellite interceptor.[420]

Although General Gavin was a US Army officer, similar arguments can found from USAF, and in fact also from USN sources.[421] (It is taken as read, throughout the US discussion at the time, that Soviet over-flight of the USA would be inherently much more destabilising than the reverse case). The USAF, however, struggled to turn this apparent necessity into action. The Eisenhower administration was willing to tolerate experiments and trials to evaluate the practicality of the various destructive mechanisms but was not wiling to see those developed into operational systems, since such a move would be a clear contradiction to the 'space for peace' theme permeating their policy at the time. Paul Stares again provides a comprehensive summary of the status of the various systems considered at the time – some as paper studies, but others that progressed to hardware trials.[422]

A second, and slightly later, driver for interest in developing an ASAT capability came from fear that the USSR was developing a nuclear orbital bombardment system.[423] Public statements such as that made by Premier Khrushchev in August 1961 highlighted these fears: "…we have bombs more powerful than 100 megatons. We placed Gagarin and Titov in space, and we can replace them with other loads that can be directed to any place on Earth".[424] Countering such possible threats was thus a direct analogue to shooting down an incoming sub-orbital ICBM, but since the activity would be directed against an orbiting system, it would more logically be addressed by the USAF.[425]

In 1958, the USA began to explore the effects of nuclear explosions in the upper atmosphere and outer space. These experiments were not directed or conducted specifically by the USAF, though Schriever must have been aware of

[420] James M. Gavin, *War and peace in the space age* (London: Hutchinson, 1959) 215-6
[421] Stares, *The militarization of space* 49
[422] Stares, *The militarization of space* Chapter 6, pp106 et seq
[423] We will consider shortly (p156) the US evaluation of orbital bombardment systems, though none of these achieved significant technical progress. For details of the Soviet system that was eventually introduced in the late-1960s, see also Curtis Peebles and Kenneth William Gatland, *Battle for Space* (Poole: Blandford, 1983) 64-76
[424] Statement by Premier Nikita Khrushchev on 9 August 1961, quoted in Major Richard A Hand, *AU-18 Space Handbook: Vol 1 - A War Fighter's Guide to Space* (Maxwell AFB, AL: Air University Press, 1993) 19.
[425] Unsurprisingly for the time, both the US Army and the US Navy retained ambitions to develop ASATs. Of these, the UN Navy project was considerably the more ambitious. Under the project name 'Early Spring', in 1961 they proposed a submarine-launched ASAT system. It had the advantage of using a conventional, rather than nuclear, warhead, but was eventually cancelled due to concerns about costs and about command and control issues. See Hand, *AU-18 (1993) Space Handbook* 19.

them.[426] Relatively small warheads were used and results were measured both by satellite and by ground installations. Although these earliest tests had specific scientific goals relating to the interaction between the warheads and the Earth's radiation belts, they yielded results later used by ARPA for anti-satellite studies.[427]

At least partly inspired by the 1958 findings, the USAF conducted their first successful demonstration of a potential ASAT missile the following year. Project BOLD ORION had begun as an attempt to develop an air-launched ballistic missile (ALBM), but one test firing was adapted to demonstrate ASAT potential. The unarmed test-shot, launched from a B-47 bomber, was estimated to have passed within a few miles of the nominated 'target', 'Explorer IV'.[428] BOLD ORION did not mature into an operational system, being superseded by the Skybolt ALBM, and its anti-satellite potential ceased with it.

A more ambitious USAF attempt at developing an ASAT system followed, embedded within Project 'SAINT'. Its origins lay in a requirement issued in 1958 as 'General Operational Requirement 170' to be able to inspect a satellite and determine its identity. SAINT was a complicated and ambitious programme, which would initially have provided an on-orbit inspection capability (hence the acronym 'SAINT', from '**SA**tellite **IN**spec**T**or'), but with the potential to grow this into an attack system. The extent to which these roles were emphasised varied during its troubled development period, and there were also policy constraints applied to how much could be said in public.[429] SAINT was eventually cancelled in December 1962 by Secretary of Defense McNamara without any hardware having been deployed.[430] SAINT is described in greater detail in the following section (pp 158-59 below).

The final anti-satellite system touted by the USAF during Schriever's service was also the only one actually delivered. Program 437 began as an alternative to the US Army's Nike-Zeus system, which had been briefly operational during 1963-64. Nike-Zeus had been built as an ABM system, with limited potential

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426 The 1958 *Project Argus* test shots were directed by the Presidential Scientific Advisory Committee and conducted by the Defense Nuclear Agency.
427 Fact Sheet contained in Defense Nuclear Agency Report DNA 6039F - Operation Argus 1958, (Washington DC, 1982). Stares, *The militarization of space* 107-08
428 Rip Bulkeley and Graham Spinardi, *Space Weapons: Deterrence or Delusion* (Cambridge: Polity Press, 1986) 17. The USAF had also explored the potential of another ALBM, 'HIGH VIRGO', in the ASAT role, however the single ASAT shot in the HIGH VIRGO trial was inconclusive in outcome due to telemetry failures. For further details of BOLD ORION and HIGH VIRGO, see Andreas Parsch, 'Directory of US Rockets and Missiles, Appendix 4, Undesignated Vehicles - WS-199', <http://www.designation-systems.net/dusrm/app4/ws-199.html>, accessed 25 November 2015
429 Hand, *AU-18 (1993) Space Handbook* 13-14.
430 Sean N. Kalic, *US presidents and the militarization of space, 1946-1967* (College Station: Texas A&M University Press, 2012) 85

for an ASAT application. Program 437 was overall a more capable system, with the added advantage that it built on existing proven components (the Nike-Zeus was a radical extrapolation of the Nike-Hercules SAM). Program 437…"melded the Thor booster, existing warheads and launch pads, and ADC's worldwide detection, tracking, communications, and command and control infrastructure into an operational ASAT system."[431] It also drew on the 1962 series of high-altitude nuclear tests (Operation FISHBOWL), which had used much larger warheads than the 1958 'Argus' series. The impact that these explosions had on the then orbiting population of satellites demonstrated (in a rather indiscriminate way) the potential delivery of a working ASAT; work on Project 437 was authorised on 11 January 1963, and the system became operational in June 1964. It remained in existence, albeit latterly with much reduced reliability and effectiveness, until April 1975.[432]

Finally, there are the most controversial of space weapons – those designed to threaten targets on the Earth's surface from space. These are collectively referred to as 'orbital bombardment systems'. Available information on these systems is sketchy at best, but two identifiable attempts to procure and operate them can be found. The first originated in the earliest days of the US military space programme, and the second became a component of the manned space programme.

The German rocket scientists who came to the USA after the end of World War 2 brought with them ideas about the potential of orbital bombardment systems. Wernher von Braun was undoubtedly one of their proponents; Michael Neufeld's biography of him cites interviews he gave to Associated Press in 1957-58.[433] In it, Neufeld asserts that without mentioning it explicitly, von Braun was plainly envisaging an orbital bombardment system. Von Braun then removed any doubt in an article he wrote for the mass media, eventually published in June 1958. In this, he pointed out that a manned space-station would make a good launching base for nuclear warheads to threaten terrestrial targets.[434] Von Braun's views can thus be referenced with confidence. However, further analysis of these early systems becomes difficult, due to a paucity of impartial reporting.

[431] Clayton Chun, *CADRE Paper Number 6 - Shooting Down a "Star": Program 437, the US Nuclear ASAT System and Present-Day Copycat Killers* (Maxwell AFB, AL: Air University Press, 2000) 10.
[432] Chun, *Shooting Down a "Star"* 17, 18, 31.
[433] Neufeld, *Von Braun* 316.
[434] Wernher Von Braun, 'How Satellites will change your life', *'This Week' Magazine,* 8 June 1958. Von Braun envisaged small nuclear warheads re-entered from the orbiting platform, with terminal guidance provided by the space station crew using optical systems.

It seems likely that Walter Dornberger shared von Braun's views, for example, but it is hard to detail his understanding or opinions. Dornberger had worked with von Braun in Germany during World War 2, and like him, had come to the USA after the war. In 1947, while von Braun was embedding himself in the emerging US Army space programme, Dornberger wrote a report for the USAF on the potential of bombardment systems. No copy of this report can be located but its content is described in several publications. Writing in 2001, Karl Grossman states that the USAF began early work on implementing such a system under the acronym NABS ('Nuclear Armed Bombardment System'). Unfortunately, Grossman relies on earlier reporting by another author, Jack Manno, and does not cite any primary sources for his work.[435] Yet another journalist who reported at the time on bombardment systems was Philip Klass, who was then on the staff of *Aviation Week* magazine; Klass's reporting appears consistent with Manno and Grossman. Klass's work is also cited by others, most notably by Michael Golovine.[436] From these brief references, all that can be stated with confidence is that the USAF probably did study a NABS system during the late 1940s or early 1950s, drawing on the opinions and ideas of the German scientists and engineers then working for them, but only took those studies to the conceptual stage. By 1960, a slightly less ambitious scheme had also reached the concept stage. The 'Positive Control Bombardment System' (PCBS) was the outcome of an Aerospace Corporation study conducted for AFBMD. Rather than proposing a long-endurance orbiting system holding an enemy target at risk continuously, it addressed the problem of determining the need for a retaliatory strike in the presence of ambiguous warnings. It proposed a short-life orbiting system which could be launched immediately 'on warning' to enter a low-earth orbit for a brief period. It would then be commanded to attack if a warning was confirmed, or alternatively, it was envisaged to re-enter quickly in a safe area, with its warhead disarmed. An alternative profile would be for it to launch into a barely sub-orbital flight path 'the long way round' to its target, again in anticipation of commands either to attack or

[435] Karl Grossman, *Weapons in Space* (New York, NY: Seven Stories Press, 2001) 65. Grossman is an investigative journalist, writing in a journalistic style. Jack Manno, whose work he relies on, has also written works on space weaponization, but they, in turn, have been criticised for their overtly polemical style, for example by Professor Asif Siddiqui – see Asif Siddiqi, 'American Space History: Legacies, Questions...', in Steven J Dick and Roger D Launius (eds.), *SP-2006-4702 Critical Issues in the History of Spaceflight* (Washington DC: NASA, 2006), 450, fn54.

[436] Golovine was a Russian-born naturalised Briton who served in the Technical Intelligence branch of the RAFVR during World War 2 (including evaluation of German 'V' weapons), and afterwards worked for Armstrong-Siddeley and Hawker-Siddeley in operational research appointments. See M N Golovine, *Conflict in Space: A Pattern of War in a New Dimension* (London: Temple Press, 1962) 93-95. Klass had a long career, but is most widely remembered for his later (extensive) publications debunking 'UFO' stories.

to achieve 'safe' disposal.[437] The context of the brief reporting available on the PCBS tells its own story: the covering letter is a response from HQ USAF to the legislative liaison office of the Secretary of the Air Force. The House Science and Astronautics Committee had apparently discovered that AFBMD had held a briefing session with industry on the proposed USAF space programme, and has asked for a copy of the brief. A copy was duly supplied, with explanatory notes indicating which of the concepts or projects briefed were already in service or development. The PCBS is noted in the covering letter as being limited to a "study to determine feasibility"; in the main brief, it is noted that "BMD has requested industry to look further into this type of system under Study Requirement 199A".[438] Study Requirement (SR) 199 (without the 'A') is noted in a contemporary guide to space programmes as being a "third generation ICBM to be operational by 1970. To counteract antimissile missiles".[439] With that rather cryptic description, PCBS disappears from public record.

Walter Dornberger had one more opportunity for involvement in a space weapons system, via his work for the Bell Aircraft Company, whose staff he joined after his early work for the USAF. Dornberger played an active role in developing the X-15 experimental aircraft and then served as a consultant on the X-20 Dyna-Soar project described above. Among the various roles envisaged for the X-20 was bombardment. This arose from one of the three programmes – 'Brass Bell', 'Hywards' and 'BOMI' which were merged to become the X-20 programme. BOMI was an acronym for 'Bomber Missile' – a proposal that merged experimental and operational aspects of ICBM and manned spaceflight technology in a two-stage boost-glide vehicle intended to deliver a nuclear payload against a distant target. Although the X-20 always remained a research programme, it was envisaged as leading to operational applications. At the time of Dyna-Soar's cancellation in 1963, it had failed to demonstrate sufficient range or payload potential to enable the bombardment mission, and as policy had turned against such uses. It is hard, however, to escape the view that its proponents still saw weapons delivery as one of its eventual roles.

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[437] *'Summary of AFBMD Presentation to Industry'* on 14 September 1960, with undated covering letter from HQ AF to Sec AF Legislative Liaison office. *NRO Corona Archive Document 573,* pp 18-19. The 'long way round' concept was in essence a Fractional Orbital Bombardment System such as was briefly introduced into Soviet service and which served as a spur for US ABM activity in the Pacific.
[438] *ibid.*
[439] Horace Jacobs and Eunice E Whitney, *Missile and Space Projects Guide 1962* (New York, NY: Plenum Press, 1962) 182 (Springer reprint 2009)

Space Weaponization – Schriever and Technical Progress

Given that so few of the USAF's space weapons programmes came to fruition, it is sometimes difficult to establish the extent to which Schriever understood the detail of their potential and their problems. Nevertheless, some insight can be found in his remarks and activities, which must serve to establish the extent and nature of his involvement in them.

It was stated above that Schriever must have been aware of the early US trials of nuclear detonations in space that informed early ABM and ASAT work. This is because the rocket used to launch the warheads detonated in the trial were originally produced to test the re-entry characteristics of the Atlas ICBM warhead; the Lockheed X-17 was a solid-fuelled rocket designed to lift the various shapes proposed for the Atlas warhead to the edge of space and accelerate them to representative speeds for re-entry trials. The X-17 was produced specifically to support the Atlas programme – Schriever is quoted by John Chapman as noting "[the X-17] proved to be a quick and accurate way to gain reliable data without flying a full-scale missile".[440] At the end of the X-17 programme, which comprised 26 flights, the ARGUS programme constructed a further 7 X-17s from spare parts, which were then used to loft the ARGUS payloads.[441] Although there is no detectable mention of Argus in Schriever's diaries for the period of the Argus trials, given the importance that Schriever attached to shepherding assets and resources related to the Atlas programme, seen most notably in his concerns to prevent Thor and WS-117L activity from impacting on Atlas delivery schedules, it seems inconceivable that he would not have been aware of the fate and disposition of X-17 hardware.

Schriever had also almost certainly begun thinking about ASAT systems more generally, perhaps as early as 1956. This was when Air Research and Development Command (ARDC) – the parent organization of the Western Development Division – first began a formal study of ASAT technologies. In 1959, Schriever himself commissioned a technical and operational concept from the Space Technology Laboratories for the SAINT system. Project SAINT grew to incorporate a number of proposed developmental stages, leading eventually to an armed vehicle capable of destroying a target satellite. A snapshot of the proposed programme in 1959 outlined the various stages:

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440 Chapman, *Atlas* 81. Chapman does not cite the context of the remark.

441 Dennis R. Jenkins, Tony Landis, and J Miller, *AMERICAN X-VEHICLES: An Inventory—X-1 to X-50 (NASA SP-2003-4531)* (Washington DC: NASA - Office of External Relations, 2003) 25. Defense Nuclear Agency, Short Report DNA 6039F - Operation Argus 1958 18.

> The proposed Saint program divided into four parts: (a) Green Saint - early demonstration of ground launched rendezvous and inspection technique against a cooperative satellite, consisting of four vehicles beginning in 1961…(b) White Saint - an outgrowth of Green Saint to demonstrate an operational unmanned interception and inspection system consisting of five launches beginning in January 1963…(c) Silver Saint - demonstration of an air-launched interception and inspection technique…(d) Blue Saint – a manned operational and interception and Inspection system with the first R&D launching in July 1965, and with the first operational launch possibly in 1967.[442]

By 1961, when development was underway in earnest, Schriever had risen to the rank of General and was commanding Air Force Systems Command. SAINT development was then supervised by Major General Ozzie Ritland, the Commander of AFBMD. It is thus difficult to gauge how much routine understanding Schriever had of the progress being made. Ritland selected proven components from the USAF inventory to make up the SAINT booster – he planned to utilize an Atlas first stage, coupled with an Agena second stage to lift the SAINT payload alongside its target; both would have been familiar systems for Schriever, the Atlas because of its ICBM pedigree, and Agena because of its multiple uses within the CORONA reconnaissance programme. Schriever also maintained overall responsibility for the proposed test programme. He is cited as having approved 4 test flights initially, though increasing concerns about development progress led technicians to propose increasing this to 8.[443] Eventually, however, the complexity of the programme, its limited capacity (SAINT relied heavily on 'SPADATS' the space tracking systems which at the time lacked the sophistication to support it properly) and its vulnerability to overload or swamping by multiple targets led to its cancellation.

Schriever noted the end of the programme; McNamara cancelled SAINT on 3 December 1962, and in his diary for 18 December 1962 Schriever notes:

> Earliest capability (Defense) – Air Launched Satellite Interceptor.
>
> - RAND brief ESD (SPADATS Accuracy).
> - Determine whether all resources are being brought to bear on this subject.

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[442] SAINT Chronology entry for 10 August 1959. USAF Historical Division Liaison Office, *USAF Space Programs 1945-1962* (Washington DC: Headquarters USAF, 1962) K-2. The full chronology details the various revisions to this basic outline, but this summary is representative.
[443] Clayton Chun, 'A Falling Star: SAINT, America's First Antisatellite System', *Quest,* Vol 6 No 2 1998)

- Conceptual Paper on need for defense against satellites.[444]

Plainly he recognised that although SAINT had been cancelled, the need for the capability remained, and he understood that the limitations of the Space Tracking System had been fundamental to the failure and that an air-launched ASAT might show more potential. Whether at that time he had in mind a system such as BOLD ORION, which had been cancelled in the previous year, or whether he envisaged a system based on a smaller missile (such as would eventually become the ASM-135 system carried by F-15 air defence aircraft in the 1980s) is unclear. Furthermore, It is not clear from the diary entry whether Schriever envisaged commissioning a new conceptual paper on the need for ASAT systems, or was referring back to earlier policy papers on this topic. What was clear was that Schriever remained wedded to the principle of space weaponization. Earlier in 1962, he had contributed a 'headline-summary' of USAF aspirations for military space systems to a trade-journal review article. In it, he noted that:

> …the United States must give every encouragement to the rapid advance of space technology and its adaption into operational space systems, both manned and unmanned…
>
> The conduct of future military operations in space depends on developments in a number of specific technological areas. These include launch vehicles; propulsion in space…sensors…**weapons** and command and control.[445] [emphasis added]

As events transpired, Schriever's last foray into ASAT systems would not be air-launched. Instead, he would be instrumental in delivering Program 437, based on the "Thor" IRBM. The shift to ground-based systems had been endorsed by the Department of Defense; Dr Harold Brown, the Director of Defense Research and Engineering had cited both technical and financial reasons in a prominent address while Program 437 was under consideration.[446] Thor had always been intended to be a 'stop-gap' missile until Atlas and later systems had entered service, and by late 1962 was in the process of being withdrawn from use, so finding a new use for the hardware certainly addressed cost issues, as well as driving significant risk out of the programme. Schriever began by issuing 'Advance Development Objective 40'. This outlined the requirement for an ASAT, and was issued on behalf of Aerospace Defense Command (ADC).[447] The technical

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[444] *Schriever Archive,* Box 6, Folder 13. Bulleting in original.
[445] Bernard Schriever, 'The Air Force Space Mission', *Missiles and Rockets,* 10/13.).
[446] For coverage of Dr Brown's remarks, see Michael Getler, 'Accuracy, Payload, Cost Favor Ground-Based Anti-Satellite Systems', ibid.13/18.
[447] Clayton Chun, 'Program 437; Thor's Hammer Strikes', *Quest,* Vol 7 No 3 1999). Chun cites ADO 40, and implies that it was issued before the SAINT programme was terminated, albeit when it appeared likely that this would happen. He does not provide a date for this event.

challenges envisaged then lay largely outside Schriever's control.[448] Thor was in service, and its warhead would be suitable for the ASAT role. A Thor launch facility existed at Johnston Island in the Pacific, one of the intended operational locations (it had been installed there to support the 1962 high-altitude nuclear explosion 'Fishbowl' tests), although it would require augmentation for this new role. ADC had already outlined their own ideas for developing an ASAT system, and this work would also be subsumed into the Thor ASAT project, which became 'Program 437'.[449] As was noted above, development work was broadly successful, and the system was declared operational on 10 June 1964. Although it suffered from declining effectiveness due to the creeping obsolescence of Thor, and environmental factors including hurricane damage incurred in 1972 on Johnston Island, it remained at least notionally operational until 1975, long after Schriever had retired from the USAF.

The final category of systems where Schriever's showed technical insight is bombardment systems. He was an active proponent for such systems, and had comprehensive strategic reasons for believing them to be useful and desirable. However, beyond the studies outlined above, and to the best of the author's knowledge, no serious attempt was ever made to construct or introduce to US service such a system; there is no evidence that 'NABS' or 'PCBS' ever progressed beyond the conceptual stage, and although an armed role was envisaged on paper for Dyna-Soar, that programme was cancelled long before the engineering challenges would have become apparent. Thus, there is no programme with which to gauge Schriever's technical understanding on practical grounds. Schriever did, however, consider some of the technical issues posed by bombardment systems in the first text he co-authored with Sam Cohen; the bulk of the technical analysis is found in the first part of Chapter 10.[450] He described the components of a bombardment satellite as he envisaged it, including a warhead within a re-entry vehicle containing a guidance system, a launch mechanism to eject the warhead when required, and an associated control, communication and power generation system.[451] He recognised that there were several technical challenges that would be faced in its design and highlighted three related problems he felt especially important: guidance challenges inherent in releasing the warhead from orbit,

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[448] Peebles and Gatland, *Battle* 86. Peebles confirms Schriever's enthusiasm for developing the capability, and notes his 'call[ed] on ADC to render the system operational".
[449] Peebles and Gatland, *Battle* 87.
[450] *Schriever and Cohen, ICBM* . The provenance of this document was described in Chapter 3, p80 *et seq*.
[451] Schriever and Cohen, *ICBM* Chapter 10, Page 12.

associated orientation requirements for the carrying satellite, and the precision required from the ejection mechanism.[452]

He saw the guidance issue as being a logical, but more complex extrapolation of the problems faced by a manned bomber aircraft, where the launch platform crew could see the target, and the ICBM where guidance was automatic, but began from a fixed location (the launch pad or silo) against a pre-determined target. He implicitly believed the problem to be soluble (without spelling out how it might be done) and conflated it with the constraints that would arise from targets only being reachable from a given satellite during part of its orbit. In fact, this issue would probably have been soluble relatively easily with the passage of time; ICBM accuracy improved as guidance systems matured, and so long as the warhead knew its position at launch, guiding to a given target would probably not have posed major problems. However, it seems that Schriever did not envisage that possibility. The ballistics of a warhead would have been known, however, and from that, calculation of practical launch positions could have been derived.

Satellite orientation seemed a lesser problem to Schriever, and here he identified a practical solution. He suggested that since the satellite would have a clear view of the heavens from outside the Earth's atmosphere, precise attitude information could be derived by celestial means. This was both practical, and achievable at the time: CORONA reconnaissance satellites used similar systems for observation camera pointing and referencing.

Finally, Schriever was concerned with a high degree of precision and repeatability in the release mechanism for the warhead. Here, he may again have been thinking of CORONA parallels; the CORONA satellite ejected exposed film in capsules for return to Earth. These were ejected from the 'parent' satellite, but then decelerated by a built-in rocket to achieve re-entry.[453] This occurred about a quarter of an orbit before the capsule reached the Earth's surface, and the projected impact area was measured in hundreds of miles.[454] It is unclear whether Schriever really thought through the implications of the issue here – he was seeking to achieve accuracy comparable to contemporary ICBMs, which were constrained in accuracy required by the yield of their warheads. Several orders of

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452 Schriever and Cohen, *ICBM* Chapter 10 Page 14.

453 The similarities between CORONA and a bombardment system had occurred to another key player, albeit in this instance in the opposite sense, beginning with a bombardment system and extrapolating it to a re-entry system of some sort.

454 Albert D Wheelon, 'CORONA - a triumph of American technology', in Dwayne A Day, John M Logsdon, and Brian Latell (eds.), *Eye in the Sky: the Story of the CORONA Spy Satellites* (Washington DC: Smithsonian Institution Press, 1998), 36.

magnitude improvement in accuracy were plainly needed to adapt this technology to warhead delivery, but precision in the release mechanism (as opposed to accuracy and consistency in the re-entry rocket motor) would have been but one of the issues. Implicitly, Schriever did not envisage guiding the re-entry vehicle during its flight, since that would surely have ironed out any small fluctuation in the release mechanism.

Another technical speculation that undercut Schriever's analysis of the practicality of orbital bombardment was that he recognised the demands of launching multiple satellites. He recognised that the costs of orbiting a significant number of satellites would be large; much greater, for example, than the cost of maintaining missiles ready for launch on the ground. He believed, however, that he had a solution for this issue:

> Since we would plan on placing these payloads in orbit during times of peace, we can contemplate using a delivery vehicle whose major cost components are recoverable so that we do not expend a highly expensive rocket for each payload in orbit. The concept of using recoverable boosters is now under investigation and the feasibility of such a scheme seems well established.[455]

Schriever thus confirms the existence of studies into recoverable launch systems even at this early date, but it is a matter of record that although various re-usable systems were posited over the years, most notably the Space Transportation System (STS – the Space Shuttle) as achieving cheap and reliable access to orbit, none has yet fulfilled its promise or demonstrated anything approaching the capability envisaged by Schriever.

There were other technical issues that appear not to have been considered by Schriever. They include launch reliability, on-orbit 'shelf life', and the relative costs of such systems compared to the practical alternatives. We will summarise each quickly to outline problems that could and should have been apparent to Schriever even as he proposed such systems.

Firstly, any system designed to ensure the safe placement of nuclear warheads in orbit would need to demonstrate extreme reliability. But launch reliability, or rather launch un-reliability, had plagued early space-flight efforts. Statistical reliability for Atlas, Thor and Titan orbital launches during the 1950s and 1960s was never better than 90%, and on occasions dipped below 50%.[456] Any

[455] Schriever and Cohen, *ICBM* Chapter 10, Page 13.
[456] Statistics taken from www.spacelaunchreport.com, which maintains a database by nationality and launcher type from 1957 onwards, suggest that the Thor failure rate in orbital attempts was 7/13

attempt to orbit a bombardment system exposed the warhead(s) to this risk.[457] The implications of loss of nuclear warheads were understood at the time. Evidence can be found from the various nuclear accidents that had affected the deterrent force from its earliest days, but if anyone was in any doubt whether accidents of this kind could apply to missiles, the evidence was also in plain sight. On 25 July 1962, one of the intended high-altitude nuclear tests at Johnston Island, using a Thor missile, failed at launch. The Range Safety Officer destroyed the warhead by remote control and the Thor missile exploded. A large amount of contamination occurred, and the subsequent clean-up operation took several weeks. Contemporary reports assert that no personnel received significant radiation dosage at the time, but the contamination is still detectable at the site today.[458] All this should surely have given pause for thought to anyone proposing the planned launch in peacetime (as opposed to responsive missile launch under attack) of large numbers of nuclear warheads.

Assuming that the risks of launch were deemed acceptable, any proponent of nuclear bombardment systems would then have had to cope with the implications of maintaining a significant number of complex satellites and the associated weapons payloads on orbit. Again, evidence of contemporary failures was legion – Schriever would have had to account for (for example) the twelve consecutive failures of the early 'Discoverer' satellites before one functioned as intended on-orbit, the re-entry failures that continued to affect the programme (planned re-entry of a capsule for recovery in a designated area was not dissimilar to the intended re-entry of a warhead onto a target), and finally with the need to dispose safely of the bombardment satellites if they reached the end of their intended life without being used (in the case of a bombardment satellite, the safe life of the warhead without maintenance might well have been less than that of the carrying vehicle). The risks either of not being able to de-orbit a failed satellite, or

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(54%) in the 1950s and 33/271 (12%) in the 1960s, Atlas recorded 50% and 19% respectively in the same periods and Titan had a 10% failure rate in the 1960s. Reliability did improve over these periods, and where launchers were man-rated to support manned spaceflight launches, reliability was 100% (until the Space Shuttle failure in 1986), but this was only achieved at significant cost.

[457] Later debates about orbital bombardment systems have included consideration of non-nuclear payloads, often including kinetic-energy penetrators but even these pose significant problems. For an analysis of some of these issues, see Major William L Spacy, *CADRE Paper Number 4 - Does the United States need Space-based Weapons?* (Maxwell AFB, AL: Air University Press, 1999) 26-29

[458] See Defense Nuclear Agency, *Report DNA 6040F - Operation DOMINIC 1 1962* (Washington DC: Defense Nuclear Agency, 1983) 232 for details of the incident. Although the report cited was written in 1983, it plainly relied on contemporary accounts for details. The assertion that nobody received significant radiological exposure during the incident is contested and various veterans of nuclear testing in this era have received compensation for injuries and illness consequent upon the tests.

alternatively proposing where and how to send the bombardment satellite and its payload at the end of its life would have been substantial.

Finally, with his background in managing costly programmes, Schriever would have had to have given thought to the costs of what he was proposing. Notwithstanding his faith in the savings achievable from re-useable launchers, each nuclear warhead launched on a bombardment satellite was essentially expended at launch, and over time the costs of such activity would have mounted alarmingly. Each warhead would carry a quantity of fissile material, which would probably not be recovered if unused. The reliability of launchers was improving in Schriever's time, but the costs of 'man-rating' the missiles to serve as boosters had proved substantial, and essentially a bombardment system would have needed similar reliability (at similar cost).

There is no evidence that Schriever considered these issues in any depth, if at all. We will look shortly at his motivation for proposing orbital bombardment, but none of it addressed issues like these.

Space Weaponization – Schriever, Policy and Strategy

Whatever the practicalities of Schriever's involvement in space weaponization, there can be little doubt that he not only regarded it as inevitable, but also as desirable. In one of his earliest public speeches after Sputnik had orbited the Earth, he noted:

> In the long haul, our safety as a nation may depend upon achieving "space superiority". Several decades from now the important battles may not be sea battles or air battles, but space battles, and we should be spending a certain fraction of our national resources to ensure that we do not lag in obtaining space superiority.[459]

By 1959, views among Schriever's staff on the desirability of implementing this aspiration had become clearer. In a draft paper entitled 'Air Force Space Mission', a clear intent was stated: "At the present time we have requirements and are planning satellites for early warning, inspection and interception, attack alarm, retaliatory attack and logistic support".[460] The larger paper is firmly rooted in 'aerospace' terminology, laying claim to operations in the aerospace continuum as the natural function of the Air Force, though at the time, Schriever was plainly

[459] Address by Schriever to the Astronautics Symposium at San Diego, CA, 19 February 1957. Extract retained in *Schriever Archive, Box 164, Folder 3.*
[460] *Schriever Archive, Box 6, Folder* 3 ('1959 undated'). This is the same paper as was referred to in the preceding Chapter. Its context is unclear – it might have formed the basis for a speech or testimony, but its classification ('Secret' at the time) would appear to rule out public use.

grappling with the technical problems of operations in ‘space’ as opposed to ‘airspace’; as was noted in Chapter 5, this exposed difference of opinion between Schriever and General White, the Chief of Staff of the Air Force. Nonetheless, for the applications of satellite identification and interdiction, both the need, and the Air Force claim to primacy in this area are almost assumed.

The sphere of operations where even Schriever recognised that he was moving into contentious strategic ground was orbital bombardment – the ‘retaliatory attack’ satellite alluded to immediately above, but here at least, his motivation and rationale can be assessed with some confidence. Firstly, he made clear in later life, that he was influenced by the thinking of a colleague, Major General ‘Harry’ Evans. Evans had, like Schriever, served in the Pacific during World War 2, and after the war had pursued a career as a reconnaissance pilot, gaining early experience in the RB-36 reconnaissance version of the B-36 manned bomber. He attended the University of Michigan for two years during 1950-52, and then worked in nuclear weapons development prior to attending the Air War College at Maxwell AFB during 1956-57.[461] While there he wrote a thesis on ‘The Weapon System to Follow the ICBM’, in which he apparently advocated for a nuclear bombardment systems, which he entitled ‘Floater One’.[462] On graduation from AWC, Evans was assigned to work for Schriever at AFBMD. He recounted the details of this at some length to Neil Sheehan in an interview in support of Sheehan’s biography of Schriever described earlier. Since it contains Evans own account of his paper, the relevant portion is quoted in full:

> M.G. Evans: 'And then I went to Air War College for one year at Maxwell, and then, after graduation from Maxwell, I was assigned to the Air Force Ballistic Missile Division under Benny Schriever.
>
> Mr. Sheehan: Oh, in 57.
>
> M.G. Evans: Yes. It was BMD. I might add, again this is the influence of a friend, I have no idea how I got out there, how come I ended up out there since I hadn't been in the atomic weapons business. But, one of the things I did for a thesis was to write at the War College. I wrote a thesis on the weapon system to follow the ICBM, and the weapon system was called Floater One, and it was a bomb in orbit.
>
> Mr. Sheehan: And it would be launched from a satellite, you mean?
>
> M.G. Evans: No, it was launched from the ground and put into a satellite orbit, and you would call it back down and retro it back down whenever you wanted to use it.

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[461] Major General Harry L Evans, USAF Official Biography.

[462] Evans’ thesis is in the custody of the Air University Library at Maxwell AFB to this day, but also remains classified. It has resisted all attempts to gain access to its content through content review for declassification and FOIA requests, even as it approaches the 60th anniversary of its submission. All inferences about its content arise from mention in unclassified interviews.

Mr. Sheehan: Oh, I see, you'd bring it out of orbit.

M.G. Evans: Yes, out of orbit -- you started in orbit.

Mr. Sheehan: And you had rockets on it and you would fire the rockets to bring it down.

M.G. Evans: That's right, fire the rockets to bring it down. I've often wondered if writing that thesis had anything to do with my ending up at the ballistic missile division.

Mr. Sheehan: You mean, somebody saw it perhaps.

M.G. Evans: Somebody might have seen it, I don't know. It wasn't a very well written document, but it was kind of interesting to explore what it took to put a bomb in orbit and stabilize it and keep it there, and to retro rocket it down. And, it turned out, when I think about it in retrospect, there was an awful lot of stuff I learned about writing the thesis at the War College that I learned later that I had to put to practice in the Discoverer program.

Mr. Sheehan: I see, because what you were doing at the War College was like dual of learn, and the fact that you were theorizing it out.

M.G. Evans: Substitute a camera for the warhead, and deorbit the camera with the film, and it was very much the same mission that I had written the thesis on.[463]

Schriever's own views on the need for and advantages of space weapons are found principally in the first Schriever-Cohen manuscript, Chapter 10. About the time this was being written, nuclear strategists were pondering what impact ICBMs would have on deterrence theory; specifically, they were analysing how strategy would be impacted by the hardening of the launch installations for second and third generation ICBMs. First generation ICBMs like Atlas-D operated from so-called 'soft sites'; geographically fixed bases where the missiles stood on exposed launch-pads. Later Atlas-D and Atlas-E models allowed the storage of on-alert missiles in concrete 'coffins', which provided rudimentary protection, but it was only with the introduction of Atlas-F, and associated silos for on-alert missiles, that protection from a pre-emptive nuclear strike became possible.[464] Titan missiles were envisaged to be silo-based from introduction to service (albeit brought to the surface for launch in the case of Titan I), and Minuteman missiles would be silo-based from the outset with the added possibility of mobile basing in purpose-designed rail cars.[465] The implications of all this for Schriever was that as the

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[463] *Sheehan Archive, Box 15.* The author has seen a transcript of a Schriever oral history interview where Schriever acknowledges the influence, but was unable to copy it due to library restrictions at the time.
[464] Walker and Powell, *Atlas* 153-54. The protection offered by silo-basing for Atlas was compromised by the need to bring it to the surface for fuelling immediately prior to launch.
[465] Stumpf, *Titan II : a history of a Cold War missile program* 29, 33. Roy Neal, *Ace in the Hole: The Story of the Minuteman Missile* (Garden City, NY: Doubleday Inc, 1962) 139-42, 68. The mobile Minuteman concept was cancelled by the Kennedy Administration in 1962, for a variety of technical

(hardened) silos were being placed across the countryside, from the Soviet standpoint, they were moving from being counterforce targets, that could be attacked by the Soviets at (relatively) less risk to the US civilian population, to becoming countervalue targets which if attacked would expose civilians to substantially enhanced levels of risk. His own concerns about this were clear:

> Should the Soviets attempt to destroy our present SAC-forces (both bomber and missile bases) in this country, in an effort to wipe out or retaliatory power, the civilian casualties would run quite high: From two to five million people might be killed outright by the blast effects and some twenty to forty million might die due to radioactive fallout effects…
>
> …Several years from now, with the establishment of the Minuteman missile system, we will pose the Soviets with a United States-based strategic force, whose vulnerability to surprise attack is but a small fraction of our present SAC vulnerability…Missiles will either be hardened (in underground silos) or made mobile by mounting them in railroad cars…
>
> …What must be taken into account, when we look at our programmed deterrent posture of some years hence, is that the vulnerability of our home-based forces and the vulnerability of the Nation itself will go hand-in-hand. If we consider the size of force that must be launched against this future strategic complex, the grim fact that emerges is that even if the attack is unsuccessful the Nation will suffer a terrible degree of physical devastation and radiological contamination…
>
> …What we will have done is to so thoroughly implicate the total populace with an attack upon the purely military elements that the issue of peace or war will truly become one of life or death.[466]

Schriever then posited orbital bombardment systems as a response to this guilt-provoking realization:

> With the foregoing discussion in mind, let us now turn upward toward space and investigate the possibilities of ultimately removing our strategic forces from the confines of our country and placing them at some distance from the Earth. More specifically, we shall consider the application of a bombardment satellite in an attempt to mitigate the political and social problems which may arise from a prolonged continuation of basing strategic forces in the United States.
>
> Suppose that we decided to place a large number, many hundreds or even thousands of unmanned bomb-carrying satellites in orbits at altitudes in the range 300-500 miles…[467]

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and economic reasons. For a discussion of the ultimate limitations of mobile Minuteman, see Chapter 4 of Steven A Pomeroy, *An untaken road: strategy, technology, and the hidden history of America's Mobile ICBMs* (Annapolis, MD: Naval Institute Press, 2016) (pp 67-90)

[466] Schriever and Cohen, *ICBM* Chapter 10, from pp 9-11. There is no clue in the mss where Schriever and Cohen derived their casualty forecasts from; it is the author's opinion that if the estimates were based on intelligence assessments, Schriever and Cohen would have faced difficulty getting this passage cleared for publication. The point that they are making is, however, independent of the detail.

Schriever next embarks on the lengthy exposition of the technical features and advantages of a bombardment system that we touched on earlier in this chapter, including his previously mentioned hope that a reusable booster would reduce the launch costs to the point where the system itself would be affordable. He uses the features of his proposed bombardment system to argue that it would be exceptionally difficult to attack, citing the possibility of adding multiple decoys to the satellite constellation, and his belief that hardening the bombardment satellites to resist the harmful radiation effects from nuclear explosions aimed against them would result in a well-nigh impregnable system. This latter assumption, although strategically desirable, is another instance of Schriever's technical optimism; given his correct identification of a long duration and reliable power supply as an essential feature of the bombardment satellite, either it would have required solar panels, which would be vulnerable to attack, or the satellite would have had to carry an internal nuclear power source in addition to its nuclear warhead payload. Even then, Schriever's trust in the ability to harden a bombardment satellite to this extent is perhaps optimistic. But he saw the strategic advantages as overwhelmingly worth the effort:

> If we could remove our strategic arsenal from the face of the Earth and place it in orbit, a unique and invaluable degree of flexibility could be achieved…we may have found a way to cope with a terrifying problem to the extent that non-military forces…will have a more extended period to effect solutions of international disputes. …an aggressor might still threaten to bomb our country, but he would have to divest himself of every thread of logic to do so.
>
> As long as we have our deterrent force stationed within the country, we cannot dismiss his threats as insane. (They may be immoral, but by no means necessarily insane). But if we have removed the deterrent into space, we can dismiss this threat as sheer bluff, since the attack of the country itself, assuming that we possess an invulnerable deterrent force, would only ensure his own destruction.[468]

Schriever's wish to 'de-militarize' the homeland by moving away from fixed-silo installations for ICBMs was logical in itself. However, moving the deterrent force into orbit was not the only way of achieving this, and it is clear that Schriever was aware of this. The first major strand of his efforts arose from the development of the Minuteman solid-fuel ICBM. Minuteman had been promoted vigorously by one of Schriever's subordinates, Col Edward Hall. Hall had worked within WDD

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[467] Schriever and Cohen, *ICBM* Chapter 10, page 12.
[468] Schriever and Cohen, *ICBM* Chapter 10, pp24-25.

since its formation, specialising in propulsion development. The initial adoption of liquid-fuelled rockets had been a technical necessity, as well as a logical progression from exploitation of the V2 after World War 2; although their simplicity was attractive, nobody in the world knew how to make large enough solid-rocket motors to achieve intercontinental range during the early days of the ICBM programme. Thus Atlas and Titan (and the Thor IRBM, and in fact their Soviet analogues too) were predicated on liquid fuel. But Hall was convinced that such a solid-fuel motor would be possible, and led early development work to achieve it under Schriever's supervision.[469] Two further spurs to the work emerged at the same time.

The first was awareness that the US Navy were working to develop a solid-fuelled IRBM suitable for submarine launch. The Navy had developed a very early maritime nuclear strike capability via the 'Regulus' cruise missile. 'Regulus I' had entered service as a component of the US deterrent, capable of launch from both surface vessels and submarines, but it was a subsonic (i.e. vulnerable) missile of limited range and questionable accuracy. Regulus II would have addressed many of these concerns – it was a supersonic missile of medium range with a much improved guidance system. But these improvements would have come at significant cost, and the Navy turned to ballistic missiles. They discontinued development of Regulus II, and having briefly involved themselves in a joint project with the US Army to develop an IRBM that could be launched from land or sea, they then set to work designing the Polaris.[470] In an early historical work about the Polaris programme, Harvey Sapolsky summarises some of the challenges the programme faced, both from within the Navy (resistance to innovations such as dual-crewing of ballistic missile submarines to increase utilization, loss of the coveted 'general-purpose' or 'multi-role' tag applied to other naval combatants) and from without. Nonetheless, there was sufficient enthusiasm from senior Naval officers to ensure progress. Admiral Arleigh Burke, the Chief of Naval Operations, had concluded that: "only the service which first developed a satisfactory launching

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[469] George A Reed, 'U.S. defense policy, U.S. Air Force doctrine and strategic nuclear weapon systems, 1958-1964: The case of the Minuteman ICBM', (Duke University, 1986 PhD), 51-52. Hall was known for his abrasive personality, and the relationship between Schriever and Hall was fractious, though it appears that Schriever respected his technical competence.

[470] For an account of Regulus I and II, see Kenneth P Werrell, *The Evolution of the Cruise Missile* (Maxwell AFB, AL: Air University Press, 1985) 113-19. The Joint Army/Navy missile was the 'Jupiter'; eventually developed to success by the Army, although it was then, due to its range, allocated to the US Air Force for its brief operational career; the Air Force saw little need for it as its capabilities overlapped with their own 'Thor' IRBM. Jupiter was liquid-fuelled, and would have been challenging to adapt to submarine launch. See Wernher Von Braun, 'The Redstone, Jupiter and Juno', *Technology and Culture,* Vol 4 No 4 1963)

system would be able to count on having a long-range missile capability".[471] The external threats emerged largely from the Air Force, and centred on their concerns about the immaturity of solid-rocket propulsion, the problems of communicating with submerged submarines, potential navigation inaccuracy, and the constraints of the smaller warheads that Polaris was restricted to carrying.[472]

The second spur to Air Force interest in solid-fuel missiles came from one of several studies instigated by Schriever around this time. The 'Bacher' panel (named after Professor Robert Bacher, its Chairman) reviewed the state of the Air Force missile programme and its possible future developments, and in August 1957 reported that there was an:

> …immediate need to construct a missile force capable of surviving a surprise attack, and noted that survivability might depend on a small, simple and reliable missile, possibly one compatible with mobile or semi-mobile bases. Such a missile would most likely be fuelled by solid propellants, rather than the liquid fuels used by the missiles then being developed.[473]

The Bacher panel report appears to have had a significant effect on Schriever's views on solid-fuel rockets. Previously sceptical, over the course of the summer and autumn of 1957, he became if not utterly convinced, at least persuaded that they potentially had a role to play.[474]

From all these factors, came the origins and goals of the Minuteman ICBM. A succinct summary of these is found in a volume actually devoted to the history of guidance systems within the overall missile programme. Donald Mackenzie notes:

> The use of solid fuel alone would not have been sufficient to justify yet another ICBM program. The risks of liquid fuel were not as pressing an issue for the Air Force as for the Navy…But Hall integrated his preferred fuel into a distinctive picture of a new ICBM. More radical…than the proposal to use solid fuel was the central criterion Hall applied to his missile: it should be cheap.

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[471] Internal memorandum by Admiral Burke, following a National Security Council meeting in 1955. Cited in Richard G Hewlett and Francis Duncan, *Nuclear Navy 1946-1962* (Chicago, Il: The University of Chicago Press, 1974) 308

[472] Harvey M Sapolsky, *The Polaris System Development: Bureaucratic and Programmatic Success in Government* (Cambridge, MA: Harvard University Press, 1972). For discussion of the internal USN debates, see 34-37. For an outline of USAF objections see 38-40. Of note, Sapolsky assesses that countering the USAF objections served as a spur to improving the overall performance of the Polaris system when it entered service.

[473] Bacher Panel final report, summarised in Reed, 'The case of the Minuteman', 50

[474] Reed, 'The case of the Minuteman', 53-54. There are three passing reference in Schriever's diary relevant to these topics: on 22 July 1957 he met with the 'Ex Ctte' of the Bacher panel, on 13 August, he was noting calculations about the implications of hardening facilities, and on 14 August, he directed Dr Simon Ramo to forward the Bacher panel report to Dr Jerry Wiesner. Dr Wiesner was at the time the director of the Electronic Research Laboratory at the Massachusetts Institute of Technology, but he has worked at Los Alamos National Laboratory at the end of World War 2, and would be appointed to head the President's Scientific Advisory Committee under the Kennedy administration in 1961. *Schriever Archive, Box 5, Folder 2.*

> Hall envisaged…a single site where as many as 1000 to 1500 missiles would be assembled, deployed in dispersed silos in constant readiness for firing, and their parts recycled if computers…indicated failure. The chosen name of Minuteman conveyed instant launch as well as revolutionary pedigree.[475]

Mackenzie also notes that Hall 'was not a sophisticated strategic thinker'.[476] But whether by accident or design, he had proposed an affordable first-strike capability for the USA. Minuteman I was not an inherently accurate weapon (although accuracy of subsequent models improved notably). But against 'city-sized' countervalue targets, it posed a significant threat. Colonel Hall left the Minuteman programme in 1959, and some of his more ambitious design objectives (such as the 'super-site' facility) did not survive his departure. But a large, affordable arsenal of countervalue missiles would be embraced by the Kennedy/McNamara Department of Defense in the early 1960s and silo-based Minuteman's future would be assured.

During October 1957, Schriever, Hall and Colonel Charles Terhune 'took their show on the road' in a series of briefings to Senior Commanders, culminating in briefs that persuaded Generals Curtis Le May and Tommy Power (Vice-chief of Staff of the Air Force and Commander of Strategic Air Command respectively) of the proposal's merits. By April 1958, Minuteman (as a silo-based system, integrated with the existing ICBM work being directed by Schriever to deliver Atlas and complete development of Titan) had been approved.[477] But during the explanatory briefings, General Power had also been intrigued by the options offered by mobility. Hall had not envisaged mobility as a key feature of Minuteman, believing it compromised his faith in a low-cost system, but Power favoured the possibilities offered by deception implicit in mobility, and in September 1958 directed AFBMD (under Schriever) to study mobility across the ICBM programme. Schriever complied, though it was very quickly discounted as an element of the Atlas and Titan programmes. Road and river/barge options for Minuteman were considered, but rail quickly emerged as the preferred solution; along with the limitations of road (expense and physical constraints) and water (too limited in extent and too close to population centres), Pomeroy notes that Schriever was sensitive to the pressures being felt within the rail industry by the advance of the automobile and was confident that they would be amenable to the proposal to base

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475 Donald Mackenzie, *Inventing accuracy: a historical sociology of nuclear missile guidance* (Cambridge, MA: The MIT Press, 1990) 152-53.
476 Mackenzie, *Inventing accuracy* 154.
477 Neal, *Ace in the hole* 88-97.

missiles on the system.[478] It also seems reasonable, however, that Schriever was forming his ideas about the desirability of getting the deterrent force off the homeland altogether.

Mobile Minuteman would ultimately fail to meet its early promise, but meanwhile another avenue explored was the development of an air-launched ICBM or 'ALBM'.[479] Such a system would, its protagonists believed, combine desirable features of the ICBM and the manned bomber; the most notable of these programmes was the Douglas GAM-87 Skybolt. The bases where the launch aircraft originated would, of course, remain obvious military targets for an opponent, but this was already the case for the nuclear bomber force. An ALBM force would be dispersed to airborne alert in time of crisis without attracting incoming fire to large swathes of rural America. Schriever was certainly aware of the programme's existence – there are periodic references to it in his diary, although in most cases, the context is unclear.[480] But Skybolt encountered technical problems and cost overruns that eventually led to its cancellation by Secretary McNamara in 1962.[481]

Both mobile Minuteman and Skybolt would have remained USAF-operated systems, but the most significant attempt to address the vulnerability of silo-based ICBMs came from another domain. While all this development was underway, the US Navy was developing the Polaris intermediate-range ballistic missile and its associated submarine launch platform.

In his history of the Polaris programme, Harvey Sapolsky makes clear Schriever's early awareness of the Polaris programme and the Air Force attitude to it; regarding some early adverse publicity being leaked to the media by Air Force sources, he notes that Schriever was asked by Admiral Raborn, then Director of the Naval Special Projects Office, to intervene and prevent the spread of

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478 Pomeroy, *Untaken road* 67-70.

479 The overall technical concept for the rail-carried Minuteman appeared sound, culminating in trials of a representative rail-car, but drawbacks included wear and tear on the missiles (the vibration environment of rail haulage damaged the delicate guidance system), some tactical doubt about the implications of needing to be at a surveyed launch location to use the missile with acceptable accuracy, and the manpower bill attendant on crewing multiple missile trains.

480 For example see Schriever Archive, *Box 6 Folders 9 & 10.* Typically, the occurrences simply list 'GAM-87' as a topic for discussion within a conversation, meeting or telephone call without further detail, although there is one slightly more substantive reference to the suitability of various launch aircraft options.

481 Pomeroy lists the various technical shortcomings of Skybolt in his brief mention of the cancellation. An alternative view is that following the UK Government decision to purchase Skybolt to deliver the independent UK deterrent, factions within the USA saw in its cancellation a means of curtailing that independence. Pomeroy, *Untaken road* 117, Nigel Ashton, *Kennedy, Macmillan and the Cold War: the Irony of Interdependence* (Basingstoke: Palgrave, 2002) 166-67

misinformation.[482] Coverage of the subsequent sparring between the single services is patchy, but an interesting insight to the positions being taken is provided by an internal US Navy memorandum of April 1958. In it, the Chief of Naval Operations is providing an overview of the latest round of budgetary negotiations to his brother Naval officers. In this document, he describes both the Navy's attitude to the budget bids of the other two services, and their reciprocal comments on the Navy requests. Relating to the missile programmes of each, it records:

> In commenting to SECDEF on the Navy's submissions, the Army and the Air Force seized the opportunity to expound upon their pet peeves against the Navy….
>
> > a. On POLARIS the Army stated that the feasibility of the system is still unproved, the missile is too inaccurate for effective use against priority targets, and therefore the system will be effective only against urban and industrial targets rather than specific targets such as submarine pens, air fields, and missile sites. This places POLARIS in the category of an over-all deterrent system and raises the question of how much and what proportions of ICBM's, IRBM's and atomic-capable aircraft are required.
>
> The Air Force stated that POLARIS is open to question on:
>
> > (1) The ability of the system to maintain an alert on station and thus its ability to fulfill specific target commitments.
> > (2) Its accuracy except when located at or very near, previously surveyed geographical or oceanographic points.
> > (3) Its ability to transmit and receive communications while submerged, and thus its ability to respond to centralized control

And later, it noted:

> …the Air Force requested $259 million for development of an ICBM MINUTEMAN Weapon system. The Air Force has a long ways to go before they will even have the ATLAS, and its successor, the TITAN.
>
> The MINUTEMAN, which is to be a solid propellant missile, would conceivably be a close successor of the first two, but according to the Air Force would have shore-based mobility. It appears that by means of the MINUTEMAN project the Air Force hopes to kill our POLARIS program. They have made drastic claims about the minimum costs and time scale of completion of MINUTEMAN in comparison with POLARIS.
>
> …We bought only a small part of what the Air Force tried to sell SECDEF…We allowed funds primarily for R&D… and to increase the number of SNARK squadrons and SNARK missiles which the Air Force doesn't want much for some reason we don't understand, since it is a good

[482] The date of the incident(s) is not clear, but Sapolsky cites subsequent discussion at a Navy meeting in February 1957. Sapolsky, *Polaris System Development* 40. There is no obvious corroboration around that date in Schriever's diary, nor record of any specific action taken.

missile.[483]

This last comment would surely have caused some comment had it been released to the USAF, since SNARK was most definitely not a ‘good missile’.[484]

Schriever would also probably have taken exception to the line about the USAF using MINUTEMAN to ‘kill’ the Polaris programme. Although there was clearly competition between the services for funding for their programme, there was also an awareness of the need to cooperate, which had been formalised in the previous year. The driving force on the USAF side towards cooperation was General Donald Putt – Schriever’s predecessor as commander of ARDC (and immediate superior while he was commanding WDD). The backdrop to the tension was the original design intent for Minuteman – small, solid-fuelled for ease of storage, safety and reliability, (relatively) cheap, , but with true inter-continental range. Polaris, meanwhile, had emerged as a more promising means of expanding naval nuclear launch-options than trying to deliver the Regulus II cruise missile, or use of a liquid-fuelled missile afloat. Substantially smaller than Minuteman (constrained by practical dimensions for a submarine) and much shorter range, Polaris was a two-stage as opposed to three-stage missile. But the fact that they (Polaris and Minuteman) were both ‘second-generation’ solid-fuel missiles gave the impression that they were similar. Steven Pomeroy relates the implications of this:

> …the Air Force and Navy warily eyed cooperation. A March 1956 letter from Richard E. Horner, the DoD's Acting Assistant Secretary, Research and Development, to Clifford C. Furnas, the Assistant Secretary of Defense for Research and Development, oversimplified the joint-service work on solid propellants by asserting that "there are no significant differences between solid rocket engine requirements for land or sea-based use. That was reasonable for individual components but overlooked the contextual differences driving each service’s missile design needs. Regardless, Air Force General Donald Putt, Deputy Chief of Staff for Development, and Navy Admiral William F. Raborn, head of the Polaris program, agreed not to block each other's programs, probably because they were complementary weapons – the Navy pursued an intermediate-range missile for use in submarines, while the Air Force sought an ICBM. Horner directed the services "to keep the two programs complementary, the air force will avoid duplicating the navy effort to bring forth a solid propellant IRBM [intermediate-range ballistic missile] engine using presently available

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483 Chief of Naval Operations 'Dope' - CNO Personal No 36 to Flag and General Officers, (Washington DC, 1 April 1958) 12-13

484 The Northrop SNARK was an air-breathing, supersonic cruise missile, not dissimilar to the US Navy ‘Regulus II’ programme. Based on a concept dating back to 1946, it suffered numerous delays due to budget constraints, re-prioritizations (not least to accommodate early ICBM development) but most notably technical issues. It was eventually declared operational in USAF service, at one base only, for 11 months during 1960-61, but even then was regarded as barely credible, due to its demonstrated unreliability and inaccuracy. For a short history of the programme and its travails, see Werrell, *Cruise Missile* 82-97.

technology. On the other hand, the navy must avoid duplicating the air force program." [485]

Schriever appears to have received and understood this guidance, and there is certainly little in his papers to indicate dissatisfaction with Navy activity while Minuteman and Polaris were developed in parallel. There is just one apparent instance of him pondering whether the two systems were complementary or not. In the '1961 undated' collection of diary entries are two pages entitled 'Geo-Strategic Positions'.[486] Their contents bear stating in full:

1. Polaris v M^2
   - Need for a force that has a credible first-strike capability.
   - Without such a capability, our negotiating position will continue to deteriorate.
   - Intelligence will improve.
   - Only M^2 has the reaction time and falls into practical cost range to permit adequate numbers.

a) Credible first strike –

> Do not telegraph our punch, i.e. provocation level to pull trigger

Requirement – intelligence, command and control, numbers.

Soviets only respect power.

2. Limited War Response –

   a) We do not have a credible first-strike capability today – it may be hard to achieve.

   b) Our strat forces are only adequate to deter total war. Suicidal to launch in response to a Laos, Cuba etc.

   c) Selective response cap inadequate and inflexible – relative to today's technology. [487]

The inference from this is that Schriever is looking to Minuteman to provide a first-strike capability for the US, though he plainly appreciates the size of that task, and that he feels that the Polaris programme would struggle to deliver it both for technical and financial reasons. In his second paragraph, he appears to be referring to the need for a 'limited war' first strike, which he sees as separate from a

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[485] Pomeroy, *Untaken road* 53. There appears to be a small typographic error in the source; Richard E Horner was the Air Force Assistant Secretary for R&D at the same time as Clifford Furnas held the equivalent DoD appointment.

[486] The pages referred to are completely undated, and it is unclear what criteria the curator applied to dating them. See footnote following for discussion of possible alternative dates.

[487] *Schriever Archive, Box 6, Folder 11.* Bulleting and formatting copied from original as far as possible. The second page continues with more notes about weapon systems, but may have been a later addition, unrelated to this (though it does include a mention of a lightweight fighter aircraft and of the 'Neutron Bomb' – emphasising that it is/will be a small weapon). Although included in a '1961' folder in the archive, the pages are undated, and the sense of the content would support both earlier (reference to 'Polaris v M2') and later (reference to Cuba, if that implied the Missile Crisis of 1962) dates.

‘total war’ first strike capability. What is less clear is what he saw as possible solutions to this. Contemplating limited or flexible nuclear responses in 1961 would not have sat easily with the doctrine of massive retaliation via the (US) Single Integrated Operations Plan. Assuming the archive dating of 1961 is accurate, Mobile Minuteman had either just been cancelled or was about to be, so probably was not seen as part of the solution to this. Possibly, Enhanced Radiation Weapons (the ‘Neutron Bomb’ reference footnoted above) were seen as an alternative.[488] But looking back to his exposition of the advantages of an orbital bombardment system described above, it is hard to avoid the conclusion that Schriever saw such a system as providing an immediate first-strike capability, and potentially a second-strike advantage for the USA, perhaps across multiple theatres of conflict, if the technical challenges could be overcome.

Ultimately, despite Schriever’s enthusiasm, the United States did not pursue an orbital weapon system along the lines he envisaged. Schriever’s technical optimism was in the main, untested. The reasons why this was so were not, however, technical. Instead, the US voluntarily constrained itself not to develop them, via a series of multi-lateral agreements culminating in the ‘Outer Space Treaty’ of 1967.

The chronology of these agreements can be quickly summarised. The Eisenhower administration had considered limitations on activities in space from the earliest days of the space age, but discussion began from an overall consideration of nuclear disarmament and associated limitations on nuclear tests, with constraints and accommodations of space activity as a means of verifying this. Serious consideration of constraints on orbital bombardment systems came slightly later, in the very last days of the Eisenhower administration. In September 1960, the President proposed a ban on orbiting weapons analogous to the agreement recently concluded prohibiting weapons in the Antarctic, but subject to agreement on an inspection and verification regime. This proposal foundered in the face of Soviet indifference.[489] Beginning in 1961, President Kennedy worked in the first instance towards the establishment and signing of the Limited Test Ban Treaty, which among other things, outlawed nuclear testing in space; this would thus have had immediate impact on any proposed improvements to ASAT or Ballistic Missile Defence applications that relied on exo-atmospheric nuclear explosions for their

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488 Options for tactical nuclear campaigns are examined in more detail in the ‘second Schriever-Cohen manuscript’ tentatively identified earlier. See Chaper 3 pp 83-84

489 Raymond L Garthoff, 'Banning the Bomb in Outer Space', *International Security,* Vol 5 No 3 (Winter 1980-81

effect. It would not, in and of itself, have prevented the launching of an orbital bombardment system. However, Kennedy had been equally enthusiastic about restraining nuclear weapons deployment in space; addressing the United Nations General Assembly in 1961, he had spoken in favour of:

> …keeping nuclear weapons from seeding new battlegrounds in outer space… reserving outer space for peaceful use, prohibiting weapons of mass destruction in space or on celestial bodies…[490]

In 1962, the United States proposed a draft treaty on 'General and Complete Disarmament' which included the provision: "The Parties to the Treaty would agree not to place in orbit weapons capable of producing mass destruction'.[491] Although this treaty proposal did not in the end mature (indeed it was described as 'pie in the sky' by Garthoff),[492] it formed the basis of further treaty proposals and reflected the Administration's position until binding agreements could be entered into.[493] These were approached by other non-binding declarations, most notably that which immediately preceded the unanimous adoption of UN General Assembly Resolution 1884 in October 1963. The resolution:

> 1. *Welcomes* the expressions by the Union of Soviet Socialist Republics and the United States of America of their intention not to station in outer space any objects carrying nuclear weapons or other kinds of weapons of mass destruction;
>
> 2. *Solemnly calls upon* all States:
>
> • To refrain from placing in orbit around the earth any objects carrying nuclear weapons or any other kinds of weapons of mass destruction…[494]

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490 'Address by President Kennedy to the [United Nations] General Assembly, September 25, 1961', reproduced in Robert W Lambert (ed.), *Documents on Disarmament 1961* (Washington DC: United States Arms Control and Disarmament Agency, 1962) 470

491 'United States Proposal Submitted to the Eighteen Nation Disarmament Committee: Outline of Basic Provisions of a Treaty on General and Complete Disarmament in a Peaceful World, April 18, 1962', reproduced in Robert W Lambert (ed.), *Documents on Disarmament 1962 - Volume 1, January-June* (Washington DC: United States Arms Control and Disarmament Agency, 1963). Quoted phrase on page 360.

492 Garthoff, 'Banning the Bomb in Outer Space', (*International Security)*

493 For further details of the Kennedy Administration's development of Space Weapons Policy, see Document 226 'Editorial Note' in David W Mabon and David S Patterson (eds.), *Foreign Relations of the United States 1961-1963 - Volume VII, Arms Control and Disarmament* (Washington DC: US Department of State, Office of the Historian, Bureau of Public Affairs, 1995). Raymond Gartoff's article cited above also provides much detail of the policy implementation process (Gartoff was a senior State Department official at the time of treaty negotiations and served as Executive Secretary of the 'NSAM 156' Committee).

494 Resolution adopted by the General Assembly1884 (XVIII). "*Question of general and complete disarmament".* 1244th Plenary Meeting, 17 October 1963.

Although this resolution was non-binding, the declarations that preceded it, and the clear signal that it sent about US intentions ended any interest in developing an orbital bombardment system.

In the face of this coordinated policy development, the role of the US Armed Forces was limited. After the rejection of the 'General and Complete Disarmament' proposal early in 1962, the President issued 'National Security Action Memorandum 156 (NSAM 156) which established a Committee to consider the ongoing negotiations (relating both to disarmament and to satellite reconnaissance), and to:

> …carefully review these negotiations with a view to formulating a position which avoids the dangers of restricting ourselves, compromising highly classified programs, or providing assistance of significant military value to the Soviet Union and which at the same time permits us to continue to work for disarmament and international cooperation in space[495]

The Department of Defense was represented on this committee by the Hon Paul Nitze, at that time Assistant Secretary for International Security Affairs. It was charged with reporting quickly (it was established on 26 May to report by 1 July). Its report was rendered in time, and made 19 separate recommendations. Of these, the first 18 concentrated on reconnaissance satellite operations, with a very brief mention of anti-satellite systems (the report recommended not drawing attention to them). The final recommendation revealed a split in the committee:

> It is recommended that a decision be made now as to whether to propose a separate arms control agreement banning weapons of mass destruction from being carried in satellites, with appropriate verification controls. The US has proposed discussion of such an agreement along the general lines of the provisions contained in the April 18 Treaty Outline. The members of this Committee are not agreed on the net advisability of making such a proposal, which of course depends on political considerations apart from its effect on the reconnaissance satellite program. (See Tab C for a summary of the considerations pro and con.) They are agreed that no such proposal should be tabled until the question has been reviewed with you.[496]

The NSC concurred in all the reconnaissance recommendations and referred the weapons question for further study. Further discussions ensued, and the President's final word on the subject was promulgated on 2 October 1962, by

[495] NSAM 156 text reproduced in Paul Claussen, Evan M Duncan, and Jeffrey A Soukup (eds.), *Foreign Relations of the United States 1961-63 (FRUS 61-63) , Volume XXV Organization of Foreign Policy; Information Policy; United Nations; Scientific Matters.* (Washington DC: United States Government Printing Office, US Department of State, Office of the Historian, Bureau of Public Affairs, 2001). Document 420.

[496] NSAM 156 Committee Report, reproduced in Claussen, Duncan, and Soukup (eds.), *FRUS 61-63 Vol XXV*. Document 421. Regrettably, the summary of the *pro v con* arguments described as 'Tab C' above are reported to have been lost and are not included in this official record of the discussion.

issue of NSAM 192. This noted that the Joint Chiefs of Staff had been consulted within the discussions, and that all the recommendations on the original report were endorsed, except that the President wanted advance notice before any comprehensive disarmament proposals (including deployment of orbital missiles) were tabled at the UN or elsewhere.[497] The clearest outline of the Joint Chief's thinking is in a record of discussion at the "Committee of Principals" in October 1963; plainly this is a year after some of the detail had been agreed, but it appears to be the best publically available minuted meeting where the Joint Chiefs were represented in addition to DoD senior leadership.[498] The bulk of the meeting content (conducted by representatives from the State Department, the Arms Control and Disarmament Agency (ACDA), the White House and the Department of Defense) concerned the form and timing of declarations leading up to the proposed General Assembly resolution, the possibility of the resolution being presented formally by proxy states and the dangers of the resolution being amended while under discussion. Of the three Joint Chiefs representatives present, only one – General Hamlett – spoke.[499] He made it clear that the Joint Chiefs were happy to forego orbiting bombardment systems, and that their possible reservation hinged solely on the term 'nuclear weapons' in orbit. "The Joint Chiefs had wished to reserve the right to place small nuclear weapons in orbit."[500] They had hoped by substituting 'weapon of mass destruction' in any agreement to preserve this freedom, thus allowing possible small warheads as ASAT systems; General Hamlett also wished to protect prospective nuclear rocket propulsion systems from prohibition.

In all this, Schriever's voice is absent. He had written down, but not published, his views on such systems in his first manuscript with Cohen and he plainly saw them as both useful and achievable. But he failed completely in any effort to gain support for this view. In 1962, he considered a further appeal directly to the public on the topic. In the Schriever Archive at the Library of Congress is a short (9 pages) essay on the advantages of orbital bombardment systems. It is essentially an advocacy piece, positing Soviet development of an orbital

[497] NSAM 192 text available online at the JFK Presidential Library website. http://www.jfklibrary.org accessed 19 December 2015.
[498] Meeting of Committee of Principals to Discuss 'Bombs in Orbit' at ACDA on 8 October 1963, Record of Conversation reproduced in Evans Gerakas et al. (eds.), *Foreign Relations of the United States, 1961–1963 Volumes VII/VIII/IX, Microfiche Supplement* (Washington DC: Bureau of Public Affairs, Office of the Historian, Department of State, 1997). Document 222.
[499] General Hamlett was at the time Vice Chief of Staff of the Army. He was accompanied at the meeting by Major General Power USAF – then Commander of Strategic Air Command – and a Colonel Sykes (affiliation and appointment unknown).
[500] General Hamlett remarks at the meeting, *ibid.*

bombardment system (without confirming whether this was known, assumed or merely hypothetical). The original is marked 'DRAFT 23 May 62 (Col Cella)'; whether this indicates Col Cella as the originator, or whether it was a Schriever draft that Col Cella had collaborated on or commented on is immaterial. What is of interest is that the front page is annotated in Schriever's handwriting with what appears to be a range of intended readers or recipients. One column of surnames include a few that can be attributed with confidence: 'Vinson' is presumably The Hon Carl Vinson – at the time Chairman of the House Armed Services Committee, 'Goldwater' is likewise presumably Senator Barry Goldwater, an Air Corps and Air Forces veteran and longstanding member and sometime chairman of the Senate Armed Services Committee; 'Stennis' is probably Senator John C Stennis, another senator with defence interests. In a separate column are three more: 'Chas Murphy' may have been Mr Charles S Murphy – adviser to Presidents Truman, Kennedy and Johnston on an eclectic range of topics. 'De Seversky' was surely Alexander P de Seversky, the Russian émigré aviator, inventor and strategic air power advocate, while 'Drake – Readers' Digest' probably refers to Francis Vivian Drake, the Defense Editor at that magazine at the time.[501] The implications are that by 1962, Schriever had been isolated from the higher levels of strategy and policy formulation and was considering resorting to 'open letters' and advocacy pieces in popular journals to achieve his policy goals. He recognised the problem(s) he faced and in fact analysed them succinctly.

Additionally, in the classified section of the Schriever archive is a short 'note to self' in Schriever's own handwriting. It is incomplete (it stops in mid-sentence) and undated, but pagination suggests nothing is missing and filing and sense suggest it is roughly contemporaneous with the weaponization debate. The handwriting is clear and legible; it states:

> <u>DoD deficiencies</u>
>
> 1 <u>US Mil strategy</u> – <u>Natl Policy and Technology</u> are <u>interdependent</u> to the highest degree – this of course also applies to mil tactics.
>
> 2. Major decisions must be made at the <u>highest level</u> of Gov't (Departmental (<u>inter</u>)
>
> 3. <u>Mil</u> operational and logistics factors are fed in thru a direct channel – joint and specified commands to the SecDef (JCS). Major decisions are not

[501] *Schriever Archive – Recently Declassified Documents folder*, originally in 'Military Papers – Satellites and other space issues – papers of interest to Schriever 1958-1963' (classified folder). Francis Drake co-authored some of his Reader's Digest articles with Catherine Drake (relationship unknown) and it is possible that Schriever is referring to her rather than him.

or at least need not be service party line influenced – nor compromised by layers of review and approval and compartmented org elements

4. Aside from current operating forces, the most important DoD decisions involve the nature of the next generation of forces structure.

5. Decisions in this area should be based on Natl Policy – Mil Strategy – operational and logistic factors = enemy cap – tech feasibility – cost effectiveness etc

6. The Service R&D org having the greatest cap for systematic analysis of those factors and in control of the vital technological factors:

1. do not have direct access to the top
2. are inhibited – influenced - coerced by Mil and Sec layers above
3. Service body English is applied for party line reasons on all proposals reaching DoD level.
4. Proposals are suspect.
5. Large adjudicating staffs are justified – generally mediocre and bureaucratic
6. Decision makers finally get a thoroughly screened massaged and slanted version from people far removed from the doing – and with no direct responsibility. The responsible individual seldom if ever is given a hearing

7 This applies also to PSAC [*the Presidential Science Advisory Committee*] which has become extremely influential but which also has no responsibility.

8 The services of course have made R&D their battleground since copping development prizes establishes the further positions of the service and also [502]

Schriever has here identified why he had failed to gain traction for his ideas; major decisions had to be made at the highest level (paragraph 2), national policy has a dominant role in this decision-making (paragraph 5), and the service R&D organisations are isolated from the debate, largely through interposition of multiple layers of bureaucracy (paragraph 6, and especially 6.6). Schriever had no counter to this procedural isolation, and it was his failure to come up with one, as much as the technical problems he would have faced, which prevented him advocating successfully for space weaponization.

In conclusion, there are two other aspects of this memo that have lasting resonance. Firstly, the implicit criticism of excessive layers of supervision and

[502] *Schriever Archive, Recently Declassified Documents folder,* originally in 'Military Papers – Satellites and other space issues – papers of interest to Schriever 1958-1963' (classified folder). Paragraphing and underlining in original, abrupt ending also preserved. As is typical, there is no clue as to the context of this memo – it might have been the foundation of an article or essay, notes for an impromptu address or the basis of a more carefully planned one, or even just Schriever clarifying his thoughts in isolation. But it is plainly Schriever's own work, drafted personally and capturing his thoughts at the time.

review can be read as a critique of the fractious relationship between Schriever and Robert McNamara, then Secretary of Defense. A good summary of its implications in this area is captured by Steven Johnson:

> …but a more serious problem was a tendency toward "creeping centralization" at the secretary of defense level. … officers perceived a "trend toward imposition of super-management organization at the top of current review and approval channels." The problem was subtle, "the more intangible consequence resulting from more detailed time-consuming control by the higher echelons outside of the Air Force." External controls removed "program control flexibilities" from "responsible operating levels in the field." Schriever's management reforms, encapsulated in the 375-series regulations, sought to decentralize decision-making processes to the project manager to reduce weapon system lead times. He lamented the increasing delays and the volume of work generated by McNamara's requests for information, which subverted AFSC's procedures.[503]

The second is the prioritization of 'next generation of forces structure' in paragraph 4, which can be seen as presaging Project FORECAST, the wide-ranging re-run of the 'Towards New Horizons' survey from the 1950s that Schriever would instigate and lead during 1963-64. Schriever came to a grudging acceptance of the primacy of policy considerations, as witnessed in a post-retirement address he gave at the Air University in 1969 where in the main he outlined how Project FORECAST had been conducted and the areas of technology it had explored, but which he opened with a review of the relationship between technology and strategy in the recent past:

> During the past several years, we have had a policy which has inhibited technology. For the past eight years, we have had a very active policy leading toward a political détente with the Soviet Union. I am sure no one quarrels with the basic idea of a detente between the United States and the Soviet Union. What is debatable is whether a detente is actually possible and how we should go about achieving it.
>
> There were people who expressed the belief as far back as the early 1960s that the Soviet Union desired an accommodation with the United States. They felt that if we could only reduce cold war tensions by avoiding provocations with the Soviet Union we could in fact achieve a detente. Among the things which these people considered provocative was our strategic superiority, which they thought would induce or initiate action leading to new weapon systems. The theory was that these new systems would tend to escalate the arms race, especially nuclear weapon systems. I will not argue whether these beliefs and the actions taken in recent years on the basis of these beliefs were right or wrong. I will leave it to your judgment to determine whether we have made any real progress toward a meaningful political detente. But the fact remains that in recent years we have slowed down our progress in military technology as a result of the

[503] Stephen B. Johnson, *The United States Air Force and the culture of innovation, 1945-1965* (Washington DC: Air Force History and Museums Program, 2002) 207-08

detente theory.[504]

Schriever thus acknowledges the priority, but plainly rues its effect on the delivery of advanced technology for operational use.

Space Weaponization – Schriever and Administrative Action

Given the very limited extent of the USAF development of space weapons, there was correspondingly little that Schriever could do to smooth their introduction administratively. Two aspects only seem worthy of note. The first relates to his involvement in ASAT development, where he quickly identified the need to collaborate with Aerospace Defense Command in order to exploit their expertise in the rudimentary space-tracking system then in place. In doing this, he was not only making best use of the available resources for the programme, he was also re-emphasising Air Force Systems Command (an R&D focussed organisation) primacy in weapons systems development over front-line operational organisations such as ADC.

The second administrative detail of note relating to weaponization was recounted above – the Secretary of Defense's antipathy (shared by other DoD seniors) to Schriever's employment of concurrency. This, coupled with increasing budgetary pressures on defence as the US's involvement in SE Asia deepened (recall also the analysis in Chapter 3 above; concurrency was rarely claimed by anyone (apart from Schriever) to save money) fuelled the pressure on any system that could not be shown to be economical. Walter J Boyne captures the dilemma succinctly:

> The very complexity of AFSC, the urgency of its mission and the success Schriever had experienced with the technique in creating the ICBM fleet made him push for concurrent rather than sequential development of the various projects. Strong views against this were voiced at the top, including those of the Director of the Directorate of Defense Research and Engineering (DDR&E), Harold Brown, then the third most powerful man in DoD. Schriever prevailed, and for the most part, concurrency worked…
> …The war in Vietnam put a budget ceiling on AFSC's activities for more than a decade and placed a more critical focus on immediate operational problems that had emerged in conflict…
> With peacetime budget restrictions, risk in weapons development was feared more than delay. The concurrency concept fell into disfavour so that

[504] Bernard Schriever, 'Technology and Aerospace Power in the 1970s', *Air University Quarterly,* Vol 20 No 6 (September-October 1969). The journal article is an adaptation of the 1969 'Thomas D White' Lecture delivered by Schriever on 19 February 1969.

> development and production were severed and a return made to traditional "buy-before-fly" methods.[505]

It is possible that Boyne is conflating a systemic loss of confidence in concurrency with McNamara's innate centralising tendencies, but in either case, the outcomes were not favourable for Schriever.

Space Weaponization – Summary

How then, should we evaluate Schriever's activity relating to space weaponization against our technical, conceptual and administrative yardsticks? Partly because of its fitful and fractured nature, his involvement in space weaponization is harder to trace coherently than in more successful programmes. In its limited success, most notably in the introduction to service of a rudimentary ASAT system, Schriever identified the strategic need, and moved to redeploy and recycle systems (specifically converted Thor IRBMs) that he had previously helped develop into a new role. Recycling an existing system 'bought out' a significant portion of the technical risk. The strategic need was matched by a credible concept that could be developed and deployed quickly, and Schriever's administrative acumen was equal to the task. In particular, he recognised the need to collaborate with Air Defense Command for operational and technical reasons, and took the requisite administrative steps to do so. Thus once again, we see the pattern repeated from the ICBM and reconnaissance satellites – technical risk reduction, conceptual maturity and appropriate administrative authority combining to deliver a successful programme.

If Schriever had been held to account for his wider campaign for space weaponization, and particularly his interest in and enthusiasm for orbital bombardment systems, he would probably have had to admit that it had all the characteristics of a 'daydream'. He certainly believed that he had a rational explanation of such a system that was technically sound. All that he needed was a live development programme that would allow him to drive the technical risk out of it and the administrative authority to manage it; insofar as he was simultaneously working to develop an orbital reconnaissance system that overflew the Soviet Union and then returned a capsule to a desired surface location, he probably felt that he had a reason to be optimistic about the some aspects of the problem. But in truth, his ideas about space weaponization were far less mature than he

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[505] Walter J Boyne, *Beyond the Wild Blue: A History of the US Air Force 1947-2007 (Second Edition)* (New York: Thomas Dunne Books - St Martin's Press, 2007) 210-11

believed. It is the author's contention that he would have faced excessive costs and insuperable technical problems in delivering an operational system. The (un) reliability rate from contemporary satellite launches in Schriever's time would have been a terrifying prospect when each launch carried multiple thermonuclear payloads, there is no sign that he seriously addressed the 'end of life' issues relating to such a system, and it is only today, in 2016, that we are seeing first signs of the development of reusable launchers that might have made Schriever's plan affordable and technically plausible.[506]

With all these technical challenges outstanding, orbital bombardment systems meet the definition of a 'daydream' in this thesis' analytic method; something that its proponents held to be conceptually desirable, despite lack of technical plausibility. The only qualification to this is that Schriever's views on weaponization also faced serious conceptual challenges. A key part of conceptual maturity is a higher command authority that believes in the system being proposed and is willing to shape doctrine for its use. In the context of innovation, this may require the protagonist to lobby for such acceptance. But although Schriever railed against the 'space for peaceful purposes' mantra then current, he did so in an ineffective manner, and thus he never succeeded in developing a credible space weaponization concept. On the contrary, the political tide at the time was flowing in the opposite direction, and subsequent events would have undercut his proposals fatally.

Finally the deepening involvement of the USA in the conflict in South-East Asia at the time of these proposals and the consequent pressures on the defense budget would almost certainly have eradicated any chance of funding, even if the technical and conceptual challenges had been successfully addressed. All that can be said in summary is that, thankfully, circumstances conspired to render both the technical and the policy debates surrounding such systems hypothetical.

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[506] Echoes of the concern about safety relating to nuclear payloads at launch continue to this day, for example surrounding proposals for launch of deep-space missions powered by radio-thermal generators, typically using plutonium fuel. For an accessible summary of the safety concerns, including some historical examples stretching back to Schriever's time, see Andrea Gini, 'Safety of Nuclear Powered Missions', *Space Safety Magazine,* Vol 1 No 1 (Fall 2011)

CHAPTER 7 – CONCLUSIONS

Introduction

In the preceding three chapters, this thesis has dissected Schriever's successes and failures in exploiting and developing military spaceflight capability during the 1950s and 1960s, using the analytic categories – technical risk reduction, system risk reduction and administrative control – outlined in Chapter 1. In this concluding chapter, it will revisit and address the research question originally posed, compare the results received across Schriever's activities and consider the extent to which they validate the analytic method. In closing it will then appraise Schriever's character and comment on the various insights gleaned during its preparation.

An Analytic Method for Technically Advanced Systems

This thesis asserted that in order to introduce technically advanced systems into military service, it is necessary to make progress along three separate avenues. Firstly, technical risk must be reduced by innovation, leading to development of the equipment concerned. This kind of progress is measured today via 'Technology Risk Levels' (TRLs), though these had not been formalised in Schriever's time. Next, conceptual risk must be reduced, by attention to the non-equipment aspects of the innovation, such as personnel and training issues and assurance that it conforms to doctrinal prescripts. Issues of legality and policy-compliance are best considered as 'non-equipment factors' too. Measurement of progress against non-equipment factors is of necessity less formal than the TRL equivalent, but a 'traffic light' assessment scheme is proposed to indicate good, marginal or inadequate progress. Finally, it is suggested that having (or gaining) an appropriate span of control and authority to achieve progress against these two demands is a further necessary step towards ultimate success.

It is asserted that Schriever validated this schema in both positive and negative cases during the course of his work. It must immediately be admitted that this can only ever be a demonstration, albeit a strong one, rather than a rigorous proof. The component parts might be necessary but not sufficient conditions, or might even be coincidences, though correlation in both positive and negative directions makes the latter less likely. It is also not claimed that Schriever himself advanced this thesis. Where he complied with it, at best it can be stated that he did so unconsciously. There is direct evidence, particularly relating to ICBMs, but to a lesser extent in other areas, of where he explicitly acknowledged the importance of non-material factors alongside material ones; the importance of technical issues can be taken as read given the nature of Schriever's appointments and the acknowledgement of his technical competence, dating all the way back to his first appointments following World War 2. Lastly, Schriever's actions in achieving adequate span of control to drive programmes to success can also be clearly demonstrated in several instances.

Next, it must also be stated that although this thesis established the existence of mature numerical scoring systems for technical readiness, and posited an analogous, though less rigorously defined, assessment method for conceptual and 'systems' factors, no attempt has been made to apply these retrospectively to Schriever's work:[507] In the case of technology readiness levels, assigning their achievement to his various programmes across time would require unachievable access to records in order to establish the various dates where different stages of technical progress were first demonstrated. Suffice it to say that for systems that entered productive service, achievement of a high TRL has been demonstrated.

For conceptual and systems factors, the purpose of outlining a scoring schema was simply to demonstrate that there was some sort of graduation in the state of development reached and that progression along the axis could thus be demonstrated. Defining the values reached at given points would again require comprehensive access to records and would additionally involve a greater degree of subjectivity. Nonetheless, success in a programme suggests that these problems had been overcome, and where failure can be attributed to them, it again validates them as meaningful indicators.

Validation of the Analytic Method

The initial research question posed by this thesis was "What did Bernard Schriever believe to be the utility of military spaceflight (manned and unmanned), how did he act to realize those beliefs, and to what extent were his views justified?" In order to evaluate Schriever's progress It is the author's contention that these questions have been answered as follows.

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[507] Note specifically, as stated above and in Chapter 1, that Technology Readiness Levels were not formally codified until the 1980s.

Firstly, he believed that development of ballistic missiles had equipped the USAF to exploit the emerging space age in all its dimensions. Next, he had already identified the importance of gleaning strategic reconnaissance, principally about the intentions and activities of the Soviet Union, and had clear ideas about how satellites could contribute the task.[508] Additionally, he held the view that translation of the strategic deterrent mounted on ICBMs to orbital platforms was both possible and desirable, principally driven by concerns that silo-deployed missiles would turn large parts of the continental USA into valid military counter-force targets. His views on the utility of military manned spaceflight were less mature and well developed; he had a sincere belief that after the initial bout of experimental and exploratory missions into and beyond Earth orbit, valid military missions requiring a manned vehicle would emerge, though he was less clear about what those missions would be. One idea he clung to, probably based on early experiences with reconnaissance satellites, was that the problems of automation of payload functions on a satellite and general reliability concerns about payloads would make crew control and intervention necessary. The possibility of direct combat in space could not be ruled out either, in his opinion, though in the time-frames he was directly interested in, he initially proposed experiments to establish what a crew member could or could not do, rather than rapid advancement of operational systems.

Regarding how Schriever acted to achieve these aspirations, his direction of the ICBM programme provides the clearest and best documented example of his management techniques. He acted as a manager and system integrator, rather than a working development engineer, harnessing effective partnerships with contractors and placing a premium on comprehensive information flows between parties. He adopted a holistic approach to simultaneous ('concurrent') development of the operational system, involving both hardware and non-hardware factors such as personnel management and training. He lobbied effectively at the highest levels of government to ensure approvals for his plans were forthcoming and he acted to secure effective oversight and budgetary authorities to deliver an operational system within challenging time-constraints. The analytic method proposed in this thesis assigns these multiple activities to one of three categories

[508] Although he initially championed the use of satellites for strategic reconnaissance, Schriever was undoubtedly aware of other satellite applications in support of terrestrial forces, most obviously satellite communications, since that was an (Army-sponsored) application that made use of an Atlas launcher as early as December 1958, predating practical reconnaissance efforts. However he plainly saw reconnaissance, and allied surveillance activities such as missile launch warning, as having the greatest priority, at least for the USAF.

and considers how Schriever used them both in the ICBM and in his other initiatives. This analysis will be reviewed in turn for each of them.

Finally, regarding the extent to which Schriever's hopes were justified for each of his goals, the analytic method proposed has been used to examine the degree of success or reason for failure for each of his major initiatives. The ICBM programme is already widely acknowledged as successful; an early operational capability was developed, and the limitations of the earliest platforms were quickly addressed by follow-on systems. Satellite reconnaissance similarly delivered rapid early results which further improved over time, and the ICBM fleets provided the basis for families of launchers which although much developed and modified, served as workhorses for military and civil space programmes for many years.

Schriever's faith in space weaponization was far more problematic and accomplished much less. Apart from delivery of a limited anti-satellite and anti-missile capability, which yielded limited operational capability for about 10 years, most of Schriever's other ideas were broadly misplaced. Deeper analysis follows below, but orbital bombardment systems as envisaged by Schriever proved both contrary to emerging national and international strictures, and would arguably have faced severe practical difficulties. Schriever also never succeeded in delivering a military manned spaceflight programme, though for more subtle reasons. Initial proposals foundered essentially over policy – specifically because of an American wish to establish an identifiably civilian space exploration programme. Later efforts failed (among other reasons) because Schriever was unable to preserve the experimental ethos of the Manned Orbiting Laboratory against operational imperatives. Subsequent delays, concomitant cost increases and the budgetary strictures of American involvement in conflict in South East Asia led to ultimate cancellation after Schriever's retirement, but it is hard to see how he would have addressed these problems had he remained in the service.

Schriever's successes

As was stated above, Schriever's management of the ICBM programme is arguably the best example of him achieving progress along the three axes of our model; it is certainly the best documented. Within it, he simultaneously maintained technical leadership and management of progress, which drove technical risk out of the programme while managing systems progress. He also lobbied and manoeuvred consistently to establish and maintain the overall programme administrative control and oversight he needed. The steady path of technical risk reduction can be traced relatively easily; short-range ballistic missiles were demonstrated in Europe at the end of World War 2, and studied and improved upon during the late 1940s in the USA. Inspired by his mentor 'Hap' Arnold's faith in advanced technologies, and almost certainly informed by exposure to RAND Corporation studies as part of his Scientific Liaison duties, Schriever gained a quick appreciation of their potential. He was also, however, acutely aware of the problems of accuracy (or otherwise) when compared to the yield of early nuclear warheads and range when compared to their weight. It was 1953 before this conundrum was solved by the demonstration of the possibility of a lightweight thermonuclear warhead, which was simultaneously light enough to be carried the required distance while having sufficient yield to be effective within the accuracy achievable at the time. That Schriever understood all this is demonstrated by the presentations he was giving at the time, most obviously demonstrated by the 'Rome' presentation described in Chapter 4. Between the establishment of WDD under Schriever's command in 1954 and the delivery of the operational Atlas system in 1959, he showed a comprehensive understanding of the technical challenges he faced. These included refinement of the Atlas booster design, selection and development of the guidance system and proving the design of the re-entry vehicle, as well as integrating these sub-systems together to form the complete missile. Again, there is ample evidence in Schriever's diaries and the briefings that he gave, particularly during 1956 and 1957, which he remained fully aware of the challenges he faced.

Regarding system risk and its reduction, it is equally clear, particularly from the direction transmitted to ARDC and then to WDD from Headquarters Air Force in 1955 that the Atlas programme had to address all aspects of delivering an operational system, and that Schriever rose to the challenge. The clearest demonstration of this is probably in the monitoring of a personnel and training scheme within the WDD performance management system alongside the tracking of technical progress, and significant effort to develop suitable infrastructure at the development base (Vandenberg Air Force Base) to deliver an early operational capability. Schriever's own remarks from the time confirm the importance he attached to filling in gaps where they had occurred. Doctrinally, ICBMs fitted logically into Air Force capabilities, and there were clearly agreed national policy goals to be addressed by the delivery of a credible deterrent system at the earliest opportunities, so here, less work was required. Nonetheless, we also see from Schriever's briefing and advocacy activity that he identified the importance of reassuring the country's senior leadership that his programme was capable of fulfilling these goals.[509]

Finally, in the ICBM programme we have Schriever's clearest adoption of concurrency as a management tool. Even though, as was shown, he was not entirely consistent in his application of the term, the ICBM programme met its fundamental requirement to be managed in this way; a critically important effort afforded 'crash' priority. Additionally, Schriever recognised the sclerotic nature of the budgetary processes he inherited, and in advocating for and shaping the Gillette regulations to supplant them, took effective steps to circumvent the problem. Lastly, note the multiple technical strands (booster, warhead integration, re-entry vehicle and guidance system) that Schriever developed simultaneously, and close cooperation between the Programme Office and the contractors engaged in production.

All this allowed Schriever to pursue progress towards technical and systems risk reduction, enabled by effective management and appropriate authorities in the case of the ICBM programme, and that the programme then delivered a useful capability on time.

Turning now to satellite reconnaissance, the Schriever Archive yields clear evidence of his grasp of the technical challenges inherent in developing reconnaissance satellites; on one hand, as he explained to General Power in 1955, he recognised the small increase in velocity required to turn a ballistic payload into an orbiting one, but he also recognised that while that would indeed put the payload into an orbit it could sustain without thrust, the challenges of the mission were only beginning at that point, and constraints such as the need to stabilise and orient the satellite correctly and to command it to perform its mission would pose significant challenges.

It was unclear at the start of development efforts what the best method of retrieving information from the satellite would be; ejecting undeveloped film from the satellite back to Earth solved some technical challenges by circumventing them, but also imposed an inevitable delay and additional risks (lost capsules, malfunctions during re-entry) to recovering useful information. The principal alternative of developing the film aboard the satellite and encoding and

[509] It is worth highlighting that despite its deficiencies as a reference document, Vince Ford's description of the personal dynamics of Schriever's lobbying, and the corroborative detail such as the Capitol Building meeting between Schriever and Vice-President Nixon provide a unique resource for historians of this period.

broadcasting the information contained on the developed film to a ground receiver gained much in timeliness but at the cost of complexity, and of other tactical and operational disadvantages, for example in the limited time a satellite would be in sight of a ground station, and consequent inability to return information fast enough. Schriever's early preference for 'develop and broadcast' methods was ahead of the technology of the day, and it was only when he switched his attention to film-return that success became possible.

Schriever's greatest contribution to the development of satellite reconnaissance may have been in the area of system risk reduction. His very early recognition of the strategic imperatives relating to cold-war reconnaissance requirements, as evidenced by his conversation and interaction with Dick Leghorn in 1946 and 1947 shows a doctrinal and conceptual foresight that was truly remarkable. It is deeply regrettable that Schriever's Development Planning Objective for Intelligence and Reconnaissance appears to be lost. The inferences that can be drawn from its reported content provide substantiation of Schriever's vision, and barring recovery of a copy from an unforeseen source is probably all that scholars will be left with.[510] Schriever's enduring faith in the importance of strategic reconnaissance throughout the late 1940s and through the 1950s until he gained control of the WS-117L programme outlined in Chapter 4 above was plainly influential. His efforts to harness insights arising out of the work of the RAND Corporation was also plainly important in terms of conceptual development and informing national policy formulation.

Finally, Schriever faced significant challenges in securing appropriate administrative control to manage the reconnaissance satellite programme successfully. Chapter 4 described the need both to manage the relationship between the USAF and ARPA and to maintain the programme during a time of financial stringency. The fact that it is harder to identify the key breakthroughs that Schriever made reflect the problem outlined in Chapter 1; strict metrics for administrative action and freedom of manoeuvre are probably not definable. All that can be noted is that Schriever was aware of the problems he faced. He benefited from the Soviet decision to launch Sputnik at a critical moment, which plainly demonstrated the feasibility of orbital systems and managed to negotiate

[510] As was noted in Chapter 4, Schriever commented directly on the fact that the 'I&R DPO' was both highly classified and circulated on a very limited basis. It is possible that a copy exists within the National Reconnaissance Office archives, and that it might become available through their ongoing declassification work. It is also possible that a copy exists within an archive of personal papers, such as that of Colonel Dick Leghorn, currently under conservation at Boston University. Only the passage of time and further archival exploitation will tell in either case.

the pitfalls of shared responsibility with ARPA, the establishment of the NRO and the concerns of the CIA to his ultimate advantage.[511]

This again shows that appropriate attention was paid to the technical, system and administrative aspects of the reconnaissance satellite programme, and that Schriever was instrumental in each instance. Does this validate the model? In the strictest sense, 'no'; in most cases we cannot impute motives to Schriever retrospectively. It can be shown that his decisions were objectively sensible and contributed to programme success in several cases, but we can only presume that he took them for the reasons implied. Nonetheless, using it to analyse the instances where Schriever failed to achieve his goals can further validate the model.

Failure cases

It is impossible to determine when Schriever first thought in detail about the potential of military manned spaceflight. On one hand, he must have been aware of the RAND Corporation work on spaceflight, which from its earliest days included studies on the practicalities and utility of a manned spacecraft.[512] It would also seem reasonable to infer that, given his intimate involvement with management of Research and Development at Headquarters Air Force, he would at least be aware of the bio-medical research interest being shown by the Air Force medical community from its outset. On the other, there is no obvious documentary record of him speaking about manned spaceflight until his famous speech in 1957, and the topic does not feature significantly in his diary until 1958, when references to the MISS programme begin to appear. Yet his 1957 remarks at San Diego, and those that followed them, showed considerable foresight and preparation, suggesting previous consideration of the issues. For now, all that seems reasonable to surmise is that he had given manned spaceflight issues some attention before 1957, but that the demands of managing the ICBM programme, and then the early reconnaissance effort, had occupied his time to a substantial extent. Once military manned spaceflight seemed like an achievable goal, his engagement increased substantially.

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[511] The author acknowledges that the complex relationship between the USAF, the CIA and the National Reconnaissance Office is not explored in any depth in this thesis – it would form a valid subject for a thesis in its own right – but many of the ramifications played out after Schriever had retired from the USAF and as such they would not inform the initial research question.

[512] The challenges of manned spaceflight were explicitly explored, to a greater extent than unmanned military applications, within the initial RAND report of 1946, although subsequent RAND studies quickly began to explore unmanned applications too.

The earliest MISS proposals were a sincere effort to ensure that the USA, having lost the race to launch an artificial Earth satellite stood a chance of being first to undertake manned spaceflight, but they would also have secured a leading role for the USAF within that effort. However, it would have been contrary to the 'space for peace' policy of the Eisenhower administration, and the proposals died with the formation of NASA. Schriever's response may have been to nurture the development of the manned SAMOS E-5 capsule, ostensibly as a back-up should the NASA Mercury capsule encounter difficulties or delay; a slightly conjectural account from Day tells the story plausibly, but not irrefutably for our purposes.[513] What does appears unarguable is that Schriever advocated strongly to preserve the E-5 capsule, and to promote its use for biomedical research at the end of its life; meanwhile, the original reconnaissance payload specialists were puzzled by the modifications being made to the capsule. The point became moot when SAMOS E-5 was cancelled for the second time, but once again Schriever's involvement in the BOSS proposal to fly a primate in the SAMOS capsule substantiates his interest in the medical aspects of spaceflight and his wish to keep the USAF engaged in its exploitation.

Schriever's major attempt to secure a manned role in space for the USAF was his direction of the Manned Orbiting Laboratory programme. Here he had a funded military spaceflight proposal to direct, but which carried from its early days an internal conflict; was it, as its name suggested, a laboratory to determine what roles a military astronaut could usefully perform, or was it an operational system to deliver reconnaissance and surveillance of potentially hostile territory? The two requirements were not in themselves totally irreconcilable, but nor were they completely compatible.[514] A measure of the shift in balance between them can be gauged from the decision to move the launch site for MOL operations from Cape Canaveral in Florida to Vandenberg AFB in California. The move was undertaken in order to enable a polar orbit for the MOL by allowing a southwards launch over the Pacific Ocean; such a launch would not be possible either northwards or southwards from Cape Canaveral. The implications for reconnaissance is that the MOL would in a polar orbit overfly the entire Earth's surface in the course of a

[513] The 1965 Memorandum by Mr Wheelon mentioned in Chapter 5 substantiates the existence of a manned SAMOS E-5 proposal, but offers no insight on Schriever's part in the programme.
[514] This thesis relies heavily on Carl Berger's account of the MOL programme, as this was the most comprehensive account of the programme released in the initial tranche of declassified MOL documentation. It is possible that a more balanced view of the programme, and perhaps resolution of questions relating to the true MOL objective – laboratory or operational reconnaissance asset? – will become possible as scholars work through the voluminous collection of primary material accompanying the declassification decision. The author is also aware of extant oral history interviews not yet released, which might shed further light on this interesting question.

mission, but at the (not inconsiderable) cost of developing launch facilities at Vandenberg AFB; the laboratory mission would have been entirely possible from Cape Canaveral.

Schriever appears to have had a sound technical grasp of the nature and challenges of the MOL mission, and in retrospect those challenges appear soluble. The MOL employed several already mature systems – a modified Gemini capsule for crew transport and a developed Titan booster (derived from the Titan ICBM that Schriever had introduced to service) for launch. The one test flight of the entire programme was a successful sub-orbital trial of the developed booster leading to a successful test of the modified Gemini capsule at re-entry. Only the core MOL vehicle was new; regarding its technical plausibility, all we can note is that within 10 years a larger, but conceptually similar, vehicle served as the NASA Skylab laboratory. That programme in turn conducted three successful missions comparable or longer in duration than the intended MOL flights and in fact demonstrating capabilities such as crew repairs that the MOL had been intended to explore.

On the conceptual side, Schriever again oversaw some of the necessary lines of development; most notably in recruitment of an astronaut cadre for the MOL. However, in the author's opinion, the fatal conceptual flaw in the MOL programme was Schriever's inability to retain the experimental or laboratory nature of its mission. Once the MOL became inextricably linked to reconnaissance requirements, the intelligence community and their operational imperatives, its fate was taken out of Schriever's hands and became hostage to delays, cost increases and budgetary vicissitudes beyond his control. In fact, Schriever had retired from the USAF before the programme was cancelled, but this in itself emphasises the delays the programme had endured.

Schriever's other conceptual and policy initiatives relating to manned military spaceflight, such as his promotion of the 1961 Air Force space policy and the 'Lunar Expeditionary Force', also suffered from critical conceptual flaws, most notably the impact of the emerging consensus about peaceful uses of space that would be enshrined in the 1967 Outer Space Treaty. They were never tested at the programme level, and it seems fair to assess them as technically impractical too, certainly as over-ambitious; consider, for example, the size and scope of the Apollo programme, which over 5 years of flight operations managed to land a total of 12 astronauts on the lunar surface for no more than 3 days at a time, with the

USAF's 1961 aspiration to have a permanently manned base on the Moon with a population of about 20 astronauts, and with occupation commencing early in 1968.

In summary, then, the USAF's Manned Spaceflight aspirations had the characteristics of a 'pet project' according to our analytic scheme. Much (though not all) of it was technically plausible, and in most cases Schriever seems to have had a clear idea about where the remaining challenges lay and how they might be addressed. Conceptually, however, he struggled to describe the missions that military astronauts might perform, and to translate that vision into practical action. Additionally, his plans fell foul of emerging legal norms and diverged from emerging US government policy. By tying the development of the MOL to an operational requirement, he also exposed the programme to budgetary pressures that ultimately led to its cancellation.

Finally, in the field of space weaponization, Schriever pursued ideas that seem radical and forward leaning even today. In a very limited case – that of delivering an operational anti-satellite system – he led the introduction into service of a credible system. The development of System 437 followed the model for successful programs outlined – technical success, in this case largely driven by the adaptation of systems already in service and careful development of infrastructure, coupled with collaboration to develop the radar required for an effective system and advocacy leading to a policy-compliant system that satisfied legal norms. In the case of orbital bombardment systems, however, Schriever again failed to make adequate progress both in the technical and conceptual arenas.[515] The hardest part of the analysis is to work out which was the less practical: the technical feasibility or otherwise of his ideas or the conceptual and policy difficulties he would have faced in implementing them.[516] Taking the latter first, Schriever had clear ideas why orbital bombardment systems were desirable, coherently stated in the first Schriever-Cohen manuscript. But he failed to persuade others of their merits and in fact was fighting an emerging consensus to the contrary across the US

515 It would be instructive to gain access to the source(s) of Schriever's conceptual underpinnings for his ideas here. A useful start would be to gain sight of Harry Evans' Air War College paper of 1956-57, which almost certainly had some influence on Schriever's thinking. However, despite ongoing FOIA requests by the author, it remains classified and un-releasable in 2016, the 60th anniversary of its writing.

516 In this respect, uncovering a copy of Dornberger's original report to the USAF from 1947, and any contemporary accounts of the 'NABS' programme that followed it, might shed light on how and why so many people at the time had faith in bombardment systems and how their technical challenges were viewed at the time. Further amplification of the PCBS proposal of 1960 would also be useful; all that can be found at present is the correspondence between the USAF and Congress which acknowledges the discussions on such a programme had taken place with industry. However, no clues have been uncovered at all about where such records might exist, and in the case of NABS/PCBS documentation it is likely that they would still be classified.

administration. There is no clue anywhere in the Schriever Archive as to why his summary of this rationale in the Schriever-Cohen manuscript was not published, but it is clear that what advocacy he did attempt was unsuccessful. Simultaneously, he demonstrated some understanding of the technical challenges he would have faced, and in some cases he had plausible means of overcoming them, but he severely underestimated their magnitude. As such, Schriever's enthusiasm for orbital bombardment systems provides evidence of failure to make progress on either technical or policy development. Policy might conceivably have changed with the passage of time or the arrival of a new administration, but it appears to the author that the technical challenges could not have been overcome with the technology of the day (or even with any plausible extrapolation of it). As such, orbital bombardment systems fell, marginally, into the category of a 'daydream' – a conceptually desirable outcome (in Schriever's eyes) that exceeded the technical competence of the time.

The analytic model – summary

This thesis has thus shown the plausibility of the analytic model in both positive and negative senses. Where its prescriptions were followed, programmes were broadly successful, and where failure occurred, there were clear problems on at least one of its lines of activity.   It should be noted however that Schriever, who was keen in his earlier days to outline his management methods, most notably in the case of concurrency, does not appear to have analysed his own work in these terms at any point. Additionally, the rationale behind decisions must often remain speculative; in the absence of clear evidence, a sound decision leading to a successful outcome might still have been made for erroneous, speculative or even lucky reasons. Nonetheless, the model utilised would form the basis for a plausible analytic tool for any introduction of a technically advanced system into defence or military service, as demonstrated here by its use against multiple test cases from Schriever's career.

So how could the model be adapted or further validated? Any future test case should be chosen with an eye to the quality of data likely to be available; on one hand this favours further historical studies rather than any attempt to analyse ongoing projects, if only to avoid issues of classification. On the other, it probably constrains suitable cases to the recent past, if only to ensure sufficient data and perhaps access to participants and their recollections.[517] A possible improvement would be to develop a more systematic and discriminating schema for analysing 'concept space'; this might, however, be purely of use to retrospective analysis. TRLs as a metric are currently used by project practitioners and managers as working tools to measure progress, enforce contracts, judge research proposals and award funding, to name but some uses. Whether there would be the same appetite for a conceptual framework is less clear, but its merits as an analytic tool seems established.

Closing thoughts on Schriever

Along the course of this analysis, several new insights about Schriever have emerged, which may be of use to other scholars working on his life. One of the original spurs to the author when embarking on research for this thesis was a casual remark made about Schriever by a retired historian who has worked on this period extensively. He noted in personal correspondence with the author: "After his retirement, Schriever got carried away pushing weapons in space with which to attack the earth and shoot down missiles. Sad ending." The prospect of exploring views on space weaponization was intriguing, although in the end, this thesis says almost nothing about Schriever's post-retirement interests.[518]

What emerged, however, was a portrait of Schriever's achievements and failures that went beyond the reasonably well studied aspects of ICBM development, and showed considerable conceptual subtlety in Schriever's analysis. In most cases where his aspirations were either impractical, or with hindsight undesirable, he nonetheless had deeply held views which he had thought about carefully to justify them. To point out the flaws in his thinking, or to explain why those outcomes are now held to be undesirable does nothing to diminish the sincerity with which he held them. Previously unknown Schriever writings emerged during exploration of the Schriever Archive at the Library of Congress.

Within that archive, on one hand, Schriever's diaries proved less illuminating than the author initially hoped; much of the content is mundane, some is obscure due to abbreviation, and questions must persist about its completeness.

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[517] One possible topic might be an appraisal of the Manhattan Project during World War 2; it meets the criteria of technical complexity, novelty at the time and coincident technical, managerial and conceptual challenges. Others might be to look at the introduction of nuclear propulsion to maritime strategy, or the more recent move to precision-guided munitions in terrestrial combat.

[518] The originator is almost certainly referring to Schriever's post-USAF employment as a consultant to President-elect Reagan during 1980-81 as a member of the Transition Team preparing him for his assumption of office, and prior to the announcement and development of the 'Strategic Defense Initiative' (SDI) from 1983 onwards.

On the other, the discovery of two co-authored but unpublished book manuscripts, one certainly and one probably the joint work of Schriever and Samuel Cohen was an unexpected surprise; the earlier of the two provides Schriever's otherwise unknown rationale for orbital bombardment. Some gaps in the Archive record, probably attributable to enduring classification, were addressed by NRO declassification activity, shedding additional light on Schriever's contribution to space-based reconnaissance, and particularly the MOL programme.

Finally, the Ford manuscript provided context for some of the personal interactions around the core of the ICBM programme, but to the extent that they illustrate Schriever's enduring care and regard for his old colleague in attempting to bring the manuscript to market, even after his own retirement, does him credit. In the end, like some of his other space aspirations, this attempt failed, though sections of Ford's thinking survive in Neil Sheehan's biography of Schriever.

In summary, the high regard in which the USAF held Schriever (evidenced by the tribute of renaming Schriever AFB in his honour) shone through, and the reasoning behind this esteem became clearer. It is the author's hope that other scholars will return to Schriever as a topic for research; there are still gaps in the accounts of his life that could be covered, and this conclusion has outlined sources, and potential lines of enquiry that might profitably be explored to fill them.

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