

MEMOIR CONCERNING EVENTS AT THE HOOVER INSTITUTION, STANFORD UNIVERSITY IN THE PERIOD 1968 -1974

[RELATING TO SUPPRESSION OF INFORMATION BY WASHINGTON, D.C.]

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BECAUSE THIS MEMOIR CONCERNS "PERCEPTIONS" of the Soviet Union and how this author avoided the conventional (and erroneous) artificial Soviet created propaganda perception, the memoir goes into formation of the author's own perceptions. These early paragraphs can be skipped.

The memoir also includes Appendix II, notes on a post-1974 event which repeated the perceptions problem in another context, and which the author tried to bring to official attention — again, unsuccessfully.

EARLY BACKGROUND OF THE AUTHOR

THE WRITER WAS EDUCATED at Bishopshalt Grammar School, Hillingdon Middlesex (outside London) from 1936 to 1941. This was the traditional English grammar school education with five years of two foreign languages, physics, biology, chemistry, music, history, geography, art, mathematics, English, wood and metal workshop with no elective courses and considerable homework. It was a thorough and no-nonsense basic education, very different from the U.S. high school.



My father was a coach builder for Thrupp & Maberly making Rolls Royce custom wood automobile bodies. My grandfather was former head coachman for the Duke of Beaumont. In the World War One period, he owned a fleet of a dozen London taxis. On my mother's side, the family stems from a line of substantial businessmen in the London furniture trade. My uncle

formed the London Pattern company. In brief, I grew up in a family familiar with hand tools, jigs and fixtures, pattern making and automobile manufacture.

When I left Bishopshalt (in 1941 during World War II), I went to work for Richard Thomas Steel Company head office in London. This company had installed the first hot and cold strip steel mills in England. I worked in the mill scheduling department until called up to the Army in July 1943. Mill scheduling provides an excellent education in the capacity, operation and limits of steel mills and the end uses of various specifications.

In the Army, I was trained as a "driver-wireless operator" with extensive courses in radio communication (#19 and #22 sets), line laying, mine clearing, field maintenance, Morse code, codes and ciphers, all vehicles through personnel carriers. Just before D-Day, we were re-designated as the 42nd Armoured Division (a nominal deception unit with a handful of men and vehicles).

I landed in Normandy on D+10, and went through to Germany. For most of this time, numerous individual assignments gave me freedom for days at a time, and opportunity to learn something of European culture and industry. Seconded for six months to University of Gottingen for de-Nazification work. Demobilized 1947 with rank of sergeant.

In fall of 1947, I started at University of Southampton and obtained Honors Degree in Economics in 1951. The advantage of Southampton then (and now) is that students and faculty were flexible in study, ideas and work. Martin Fleischman and Stanley Pons did their early work on cold fusion at University of Southampton. In the 1940s, study was unregimented and free flowing. No grade point system.

A major plus was presence of an open Communist Party and recommended reading in Marx, Engels and Lenin. This proved to be an excellent foundation to understand the use of dialectical materialism and deception by the Soviets and their foreign Communist Party allies.

In 1951, I spent a few months as a clerk in a plant making gas meters and tinsplate instruments; then returned to Richard Thomas & Co (now part of British Steel). I was appointed to a two years' management training course. This included visits to every major iron and steel plant in England with months spent on the floor of various plants including ore mining, blast furnaces, coke ovens, stripmills, tinsplate and bypass mills, annealing and so on.

This two-year course gave me an understanding of the construction and operation of iron and steel plants and a reasonable grasp of metallurgy and metals testing.

By 1953, I had become disgusted with the laxity of nationalization. I left England and went to Canada via New York. I worked a year in Canadian steel firms, and then joined a mining exploration firm (Desmac Exploration, Ltd.) in charge of exploration logistics and information acquisition. By 1956, we had discovered major deposits of magnetite iron ore in Northern Quebec (Montgol-

fier Township) and had programs in several Canadian provinces. I acquired an introduction to geology and drilling techniques in order to evaluate potential mines. I left because it was too cold (i.e., -40° in winter!).

In 1957, I went to New York and was hired by Hoyland Steel as assistant to the manager of their Los Angeles branch, known as Great Western Steel, Inc. (the manager was an alcoholic, my job was to keep the firm on track, my eye on the manager, and quietly report back to New York). In this job, I visited every operating mine in the Western United States and Northern Mexico, and most of the significant plants, including the L.A. aircraft and electronics industries, i.e., Lockheed, Hughes, North American, and Douglas, as well as lumber mills, rood processing and cement plants, quarries, and shipbuilding in California, to determine the potential for new alloys and specifications.

In November, 1957, I happened to be in the Motorola plant in Phoenix when the Russians sent up Sputnik. This had an enormous impact on the Motorola engineers, and I was struck by their naive acceptance, "The Russians are ahead of us." I knew from my steel experience that the gestation period for a strip mill was 6-8 years under western conditions, and the Russians could not have developed an indigenous strip process in the period 1917-1929 (the start of the first Five Year Plan). From my experience in post-war Germany, when the Soviets dismantled and shipped to Russia what was left of German industrial plants (really, a pile of junk), the "Russians are ahead" theory made no sense at all. Nowhere in my multi-country, multi-plant visits, had I encountered a Soviet process or innovation. My experience was the opposite to the prevailing perception of Soviet technological power.

My assessment was that the USSR in the 1950s and 1960s was a backward, technically dependent economy, without a support infrastructure.

In brief, by mid-1959, I had probably unique training with regard to on-the-floor exposure to industrial processes in five countries. I knew the equipment, who made it, its limits, and who used the equipment. Every technological process has its own "signature." For example, an iron blast furnace has precise shapes and measurements; it requires specific inputs of iron ore, coke, and limestone, of specific grades. You don't design a new blast furnace from scratch — you build on earlier designs with knowledge of limitations with each design change. These can be remarkably precise. Ask any engineer.

With this varied and detailed technological background, in 1959, I quit business and went to UCLA graduate school. My intent was to study the interface between economics and technology. Rather naively (I see today), I thought theoretical economics could teach me something about the efficiency factors behind engineering design. The immediate impact, however, was the rigid doctrinal structure of American graduate education. It turned out fine technicians, but was decidedly not happy with any original ideas or anything that challenged the textbooks.

Economics, I quickly found, treated technology as a constant given factor, not as a developing engineering dynamic. Once again, the assumption "The Russians are ahead" was the prevailing doctrine.

I spent considerable time in the UCLA Main Library and the Engineering Library where I located a wealth of material supporting my idea that everything Soviet had come from the West. I even found the original contract Averell Harriman signed with the Soviets to develop the Georgian manganese deposits. (He was taken! I had negotiated similar contracts in Canada, and could spot the loopholes.)

UCLA libraries also held several series of journals in Russian and English on 1930s construction by Western firms in Russia. These cited the foreign builders never mentioned in the UCLA textbooks on Soviet economic development. At first, I used these findings and ideas in seminars and papers, but soon found they were definitely not welcome, and the idea of challenging the textbooks bordered on treason. I found that prominent faculty such as Armen Alchian and Jack Hirschleifer, who also worked at Rand Corporation on Russian strategy, had naive ideas on Russian economic development and knew nothing — and I mean, nothing— about technology. Products were called, "widgets;" and technology was treated as a constant "given," rather than a dynamic engineering process. I did considerable research for Professor Dudley Pegrum and W.E. Baldwin in transportation and the African copper industry, and wrote an article on the Colombian iron industry, which was later published.

The textbooks and article literature on Soviet economic development had a truck-size omission: there was nothing on the development and origins of the technology used in the USSR. Every text and article had an extremely naive and erroneous description of Soviet development. There were no exceptions to this statement. Even Clark's *The Economics of Soviet Steel*, an otherwise excellent book, had missed the Western origins. Another book was *Economics of Technology*, published in the early 60s, and had no understanding of technology. It was a meaningless mass of mathematics with no relevance to technology in the slightest. Not one technological process was cited. I feel today as I did 30 years ago — the book is a fraud, but accepted in the academic community as an advance of knowledge.

Gradually, I developed a technique in my own research. Assume every Soviet design is indigenous; compare it to similar Western designs known from past experience; locate a transfer mechanism, i.e., installed by Western firms, taken from patent literature, copied within the Soviet Union in a "copying bureau." Then compare design specifications. Always assume the design is Soviet, unless the opposite can be proven.

The UCLA Ph.D. program was not completed. In one topic (macroeconomics), I was told to repeat one question of the Ph.D. finals, but to go ahead with the dissertation (which I did). The Department then gave me a new set of multiple questions. UCLA then denied the Ph.D. I later obtained two D.Sc. degrees elsewhere. Dr. Dudley Pegrum, Chairman of my Committee, then

pushed me as Assistant Professor of Economics at (now) State University of California, Los Angeles.

The same kind of political pressure developed when I came up for tenure at Cal State in 1968. It was approved at the department level. Then tenure was rejected at the Administration level - even though my first book had been accepted at Hoover Institution and no Cal State economics faculty member had a published book at that time.

There is no question my views on Soviet weaknesses were the cause. In 1963, when I gave a lecture on Soviet development to the faculty, I made the blunt statement that Marxist central planning was not viable without outside help. The USSR stagnant, and this was a fatal flaw. When I stated that "Marxism is finished," one could feel the intense disapproval - almost, emotional distress - in the audience. By the early 1960s, it was clear that my perception of the Soviet Union as a technically backward and dependent economy was highly unwelcome to everyone else. The experiences at Motorola, UCLA and Cal State were consistent, in that I found no one agreed with my view of the Soviet Union as a backward, stagnant economy. Soviet propaganda was extraordinarily successful with its progressive image.

WESTERN TECHNOLOGY AND SOVIET ECONOMIC DEVELOPMENT

AS A UCLA GRADUATE STUDENT, I began in 1959 to explore the early technological history of the Soviet Union, and to learn technical Russian (enough to translate technical manuals). I said little about this work (except the one lecture cited above), which continued in my five years at Cal State Los Angeles. At Cal State, I engaged a tutor from the faculty to improve my Russian reading skills.

While at Cal State, I tried to obtain funding to speed up the work. All I could get was \$400 for microfilm of State Department documents from Relm Foundation. In the early 1960s, I read scores of rolls of State documents, including reports from officers in Moscow, Leningrad, and Riga, Latvia. These included interviews with American businessmen returning from Russia, and detailed plans and specifications for plants installed by the West. (Magnitogorsk, Gorki, Uralmash, etc.). These businessmen were not always honest with State officers. They wanted to maintain Soviet good will. A few told State officials they were instructed to paint a rosy picture if they wanted contracts.

The State files supported my theory of technical dependence. However, I noted that State was reluctant to release certain documents on the purport lists. I made a special point to push for declassification, where I found State resistance. In practice, the resistance guided me to the real gold mines of information.

Through Blackwells (Booksellers) in Oxford, England, I bought hundreds of Soviet technical manuals and maintenance charts. It was fascinating to trace the

technology in these manuals to systems I knew in the West. Even the Western language was adopted — for example, "Stilsona" for "Stilson wrench." The maintenance and training charts were invaluable in tracing origins. To this day, I don't understand why the Soviet Union allowed export of these items. For someone who knew the technology, it was like labeling the Western manufacturer. The Soviet copying bureaus were helpful. They duplicated EXACTLY the Western design. In consumer products, however, the Soviets often used military spec materials, yielding an overweight, clumsy product. Sometimes, the Soviets varied the way a part was made, i.e., by casting or stamping instead of forging.

In 1963, the State Department public view was "the Soviets have their own self developed technology" — i.e., Directly contrary to their own files.

By 1966, I had completed the first volume of Western Technology and Soviet Economic Development (1917 to 1930). It took about eight years, using my own funds (except \$400), and with the facts consistently pointing away from academic and official statements and assumptions.

Soviet technology was western technology — period. Soviet attempts at innovation (i.e., the electric tractor) were pitiful.

The manuscript was sent unsolicited to Henry Regnery Company in Chicago. I was surprised to receive a reply letter from Henry Regnery himself, to the effect this was "an extremely important manuscript but not commercial." I wrote Hoover and received a letter back from Alan Belmont (Assistant Director). He had taken early retirement from the FBI, where he was Assistant Director for Domestic Intelligence. In response, I sent the manuscript to Hoover. Belmont suggested I spend the summer of 1966 at Hoover. I accepted. During this summer, I kept a low profile, avoided the social circuit (much to Director Glenn Campbell's disgust), and concentrated on work. Frankly, I didn't want a repeat of the UCLA and Cal State hostility. Only Al Belmont appeared to understand my hermit-like stance, and I will be eternally grateful for his understanding, and the "cover" he provided me, unasked.

I found that Belmont had insisted Roger Freeman, a long-time friend of Director Campbell, a man of many contacts and a former White House Aide, read the manuscript. Belmont then sent it to ex-Russian engineers for evaluation. The Russians were definite: "Sutton is right." Belmont told me this, and added the manuscript was also going to Sovietologists, but not to worry if they made adverse comments. (He once said something to the effect, "we do not accept them.") At some point, Roger Freeman told me that "they" (i.e., White House or CIA) didn't know about the "early period." I remember saying that my work should be repeated and double checked. It made our policy towards the Soviet Union naive and idiotic.

I agreed to return to Cal State for another year, and then go to Hoover as a Research Fellow to complete the two further volumes. Campbell has claimed that Hoover financed the first volume also. They did not. I submitted a complete Volume One, financed by myself. I returned to Hoover about mid-1968 and completed the two volumes by 1971. Volumes One and Two were brought into print fairly quickly. I had only one minor problem with the Editors. They wanted to remove everything on Soviet military use of our technology. I agreed to this.

I completed Volume Three by 1971 and anticipated it would be published promptly, like the first two volumes. In fact, it went quickly to page proof, which I corrected — then I heard nothing for a year. Normally, a book moves quickly from page proof to final book to recoup investment. My inquiries were met with a blank wall. All I could get from Al Belmont was, "It's coming."

We had a war in Vietnam, and I could daily see the product of U.S. and European technology killing our men in Vietnam, while we were still supplying technology for Soviet plants to manufacture weapons and supplies for North Vietnam. I considered this immoral and lunatic behavior. Further, I had personal experience of war in Normandy, in Belgium and the Rhine Crossing in March, 1945. Active war is not pleasant. Its cold, dirty, dangerous. My concern was with men on the ground in Vietnam rather than armchair academics and politicians who had probably never been on the dangerous end of a gun, but who were making technological decisions without knowing technology. In particular, I was horrified at State Department statements directly opposed to materials in their own files.

I decided to do an end-run around whomever was delaying the third volume, which had current application to policy, and was obviously being delayed. I agreed with Arlington House to quietly write another volume on the military use of our technological transfers and so-called, "peaceful trade." My planned, deliberate objective was to spring loose my Volume Three, by publishing the segments Hoover wanted "out" — i.e., the military end-use segments supplied by the West — and so, force their hand. I fully admit this was my intent. I would do the same again, only quicker, and with more publicity.

About this time, Henry Kissinger OK'd export of the Centalign B machines to the Soviet Union. These had only one end use — to machine ball-bearing races to extremely high accuracy for MIRV-ing Soviet missiles. The USSR could then pinpoint-target the United States. The Washington arguments were wrong. (However, DOD had people warning of the dangers.) The Soviets could not manufacture Centalign B machines, and no one else in the world had an equivalent machine.

Kissinger's action confirmed to me that something was badly wrong in Washington. His "detente" strategy was to give the Soviet long-term, needed technological advantage in exchange for short-term political promises which might or might not be kept. Kissinger called this policy "linkages." I called it "lunacy." We were handing over what Soviets really wanted (advanced technology) to receive

empty words. It still baffles me how Kissinger can be regarded as an "expert." Anyone can hand over the keys to the safe and ask nothing in return.

Further, the McNamara "rules of engagement" had tied the hands of the military in Vietnam. In effect, we helped the Soviets build trucks, and then told our pilots not to shoot them up. When one looked at the entire picture, I could only arrive at one conclusion — treason.

I have never formed final conclusion on policy makers like Kissinger, Rusk and McNamara. It could be stupidity, ignorance, ego, or worse. Frankly, I don't know. When foreign diplomats and military people asked me, "do you think Kissinger is a Soviet agent?" I replied, "I don't know. He could be ignorant."

I told no one except Arlington House about National Suicide. It was written in three months, and rushed through production in another three months. It was published as, National Suicide: Military Aid to the Soviet Union (available today as, " The Best Enemy Money Can Buy"). My advance copy is marked, "Received August 27th, 1973." When it was received, Volume Three of Western Technology and Soviet Economic Development was still being held in page proofs, confirming my belief that the delay was deliberate to prevent the information becoming public. (This was 8 months after correction of page proofs.) From rumors, it appeared the pressure was coming from the White House.

Some days after receiving my advance copy of National Suicide, I received a call to go to the Director's office (Glenn Campbell). I found a group waiting, including Alan Belmont, Dick Staar (CIA), Stefan Possony, and perhaps a few others. Campbell immediately launched into an attack, stating that I had plagiarized the text of National Suicide, and that I had no right to produce such a book.

My reply was: (1) I cannot plagiarize my own work; (2) Hoover itself had asked for the military sections to be removed from Hoover volumes, thus releasing the material; (3) We had men being killed in Vietnam, and I had every right and duty to draw public attention to this if our policies were aiding the enemy; and (4) I remember saying, "tell me where I am wrong on a single fact." No one ever did.

I gave some examples, i.e., we were sending machine tools to the Gorki truck plant outside Moscow, and GAZ (Gorki) trucks were on the Ho Chi Minh Trail. The U.S. pilots even commented they looked like Ford trucks (Gorki was built by Ford Motor Company).

Soviet ships on the supply run to North Vietnam were larger, faster vessels, sold by the West under "peaceful trade." I had obtained a list of the vessels from sources that emerged from the woodwork after publication of my first book. From the Soviet Register of Shipping, I located the type, design and origin of every Soviet marine diesel engine. Most were Burmeister and Wain (Denmark), or Soviet copies.

In brief, my information was precise and accurate. Campbell asked me to have National Suicide withdrawn. I refused. End of meeting.

I called Arlington, and they assured me they would not buckle or withdraw the book.

Another meeting followed a few weeks later. Campbell said I had no agreement or contract with Hoover Institution, and I was no longer a Research Fellow. To stall for time, I said I did have a contract (actually, I did not), and I had no intention of changing my position.

I later heard that a few Senators and Congressmen had called Campbell to support me. A week later, Al Belmont came to my office and handed me a contract, signed by himself, with the words, "You are going to need this."

Hoover Institution then removed my name and that of my secretary from the Hoover personnel list. I became a non-person. In subsequent speeches, I found this Hoover Institution action to have significant impact on audiences. Apparently, it convinced people that I was probably right. Hoover also rapidly released Volume Three. They continued to pay me until I left voluntarily a year later. On the way out the door on the last day, Roger Freeman intercepted me and said, "We can work this out; let me talk with Campbell." I said thank you, but no thanks.

My sense was one of absolute disgust. I had, with my own funds and time, undertaken ten years of research of obvious value to the United States, and put up with the naive stupidity of academics at UCLA and Cal State to get the work into print. The response? — insult and harassment!

I concluded Campbell was a weak patsy, and that Washington didn't give a damn about our men in Vietnam. (This is still my assessment today.) The academic world by and large was more interested in preserving its little stock of knowledge. Academic freedom is a sham.

My thanks came from individual citizens. After speeches, scores would come up and say, "God bless you," or, "We are with you." One speech filled the Hollywood Palladium with standees at the back. At the end of the speech, I was called back three times to the podium. The more macabre would say, "it's a wonder you are still alive."

A few gestures were practical. A group of anonymous airline pilots gave me (through a friend) a gift of \$10,000. A Congressman inserted my speeches into the Congressional record. I had a call from Antoine Pinay, former Prime Minister of France. Several VFW posts passed resolutions of commendation. And so on.

Not once did anyone challenge the data or the argument. The press ignored the books (with the exception of the Manchester Union Leader in New Hampshire and the Telegraph in London). However, on a later visit to London, The Times sent a message asking me to meet with the Editor (now Lord Rees-Mogg), who asked a key question: "Why do you think they are so upset?" Roughly, my answer was, "I really don't know. I guess I must be close to the truth."

I made a number of foreign trips, and met senior military or government people from Mexico, Argentina, South Africa, France, Belgium and other countries.

About 1969, Campbell urged me to submit my first volume to the University of London for a Ph.D. This I did. As I had scores of favorable academic reviews, the procedure should have been automatic. Peter Wiles was appointed examiner, and proceeded to raise every conceivable petty point possible. I made two trips to London, and did what I could to answer his points. In the end, I gave up, and figured it was another academic game.

In about 1970, I was contacted by a Colonel Samuel Clabaugh, who at one time had been U.S. military attache in London, a friend of Averell Harriman, and, formerly, with OSS and CIA. He was apparently in routine contact with Eleanor Dulles. Over perhaps a year or 18 months, we exchanged letters and meetings (I went to his apartment in Washington, D.C. for lunch). I was quite clear and definite: our policy is suicidal, developed by the naive or self serving. I remember repeatedly making the point; if I am wrong, someone will challenge me, but all we have is silence. No one dares challenge me. I know the technology, and I am right.

About 1971, Hoover Institution wanted me to give a paper at Ditchley Park in England to a group of businessmen, bankers and academics. This is where I met M. Pinay, former Prime Minister of France, who greeted me, "Tres bien; tres bien." At one point, the Chairman of Dunlop Holdings (who had built several plants in the Soviet Union) stood up and said, "Sutton's information is correct; we have built tire plants in the USSR for many years, and we will continue to build these plants even if it is my own suicide." Even today, I can't fully understand the motivation behind this statement. It's not rational, but that's what the man said. It raises the question of whether some form of brainwashing was used on foreign businessmen. I know there was pressure.

Over the years, my conclusions have firmed up roughly as follows:

1. The U.S. was suckered royally by Soviet propaganda (the "perception problem").

In most cases, the conduits were not agents (like Ames), but individual businessmen, academics, and politicians looking out for their own interests and proposing self-serving policies without challenge.

2. My argument was correct and remains correct today. The Soviet Union was incapable of self-sustaining development and relied on the West for most technical advance. The Russians are very capable people; it was the system that failed, the lack of a mechanism to transform ideas to technical reality in a rational, useful way.

3. My experience has been that the academic world is largely concerned with preserving the status quo and its own perks and prestige. Really new ideas are automatically dismissed. To me, this makes the entire fabric of degrees and honors suspect. These reflect merely an ability to absorb what is already known, not any contribution to future knowledge.

4. My greatest surprise is the sheer lack of moral action. No one asks, what is right?

What is moral? I find this entirely disgusting and shortsighted. The truth will always out, sooner or later. I prefer to be remembered as someone honestly concerned with others, rather than as a sham, self-serving political type. Glenn Campbell even sneered at my moral approach in one well-remembered conversation:

GC: "Tony, you have a problem."

TS: "What is that?"

GC: "You are a moralist."

TS: "So?"

GC: "You can't survive being a moralist. They will break your rice bowl."

On the other hand, no individual, and certainly no country survives without a set of principles. Pragmatism is shortsighted, a crutch for the weak, a cop-out for the greedy.

5. Certain names stand out as key in the Soviet perceptions program and successful operation at the highest levels in Washington:

Armand Hammer. Son of Julius Hammer (founder and financier for the Communist Party U.S.A.). Access to every President from Roosevelt onwards.

Protected at the highest levels in Washington.

Averell Harriman. Received \$1 million gift from Soviets about 1928. (I can provide the State Department file numbers if required.) State Department was told, "hands off on Harriman. Key member of the Georgetown group that had major influence on policy and intelligence.

Henry Kissinger. I need only quote Anatoly Dobrynin (In Confidence, Random House, 1995). "... more than once when Kissinger knew I was going over to the State Department he would ask me to bear in mind that Rogers had not been told about this, that or some other aspect of the issue under discussion and I was not to tell Rogers about it."

Out of the hundreds of examples, I would select the export of Centalign B machines as the key to determine whose side Kissinger was on. I presume there was no mark program on our side (see Appendix), and in fact, it would be difficult because the product has precise measurable characteristics. Kissinger, over numerous DOD protests, provided the USSR with MIRVing capability.

Unless there was some deception program on our side which required Kissinger's action as a cover, then one is driven to the conclusion — unwelcome as it is — that Kissinger was a Soviet agent. Sutton was just one irritant in a larger game, but if the strings provided in the Sutton research were pulled, he had the capability of unraveling the entire Soviet perceptions program.

No one has stepped forward, in 1996 — or even in 2001 — to say, Sutton was right or wrong, because, even today, reputations are at stake. The United States owes me an official apology. When they should have encouraged me to work further to pull on the strings, I was squashed, threatened, harassed, and discarded — for whose benefit?

THE AMES CASE

THE SIGNIFICANCE OF AMES is not that a Soviet agent existed in CIA. This was discussed widely back in the 1970s and before. Even names were floating around. The significance is that Ames surfaces the perceptions program in a way that could not be done by any academic writer. The Soviets paid Ames well — very well, by Soviet standards — to maintain a facade. There were many ways Ames could be used; this was judged to be the most important. And it confirms what I had been saying since the late 1950s. When Lenin wrote about the "useful idiots," and the "deaf, mute blindmen," one wonders if he knew how successful his perceptions program was going to be. Lenin suckered the West royally. Ames is a pitiful pawn, but if you pull on the Ames string, you'll find the rest of the story — and more names.

Be prepared for some shocks.

APPENDIX 1; THE MARK PROGRAM

IN RESEARCH ON TECHNICAL TRANSFERS, I was alert for specification changes in equipment which could affect quality, quantity and product obsolescence. In brief, it is possible by slightly changing specifications of manufacturing equipment, to affect quality and performance of the end product. For example, changing the dimensions and shape of a blast furnace has significant effect on cost, quality and quantity of output. By slight variations in equipment sold to the USSR, one could reduce the benefits. By changing the tensile strength of steel or design parameters, for example, one can influence the quality of the end product. On one or two occasions in State files, I caught reference to a "mark" program (i.e., to sell sub-standard equipment); and elsewhere, statements to the effect that Western businessmen sold outdated designs to the Soviets.

No mention is made in any of my books to this "mark" program. I did once mention it to Alan Belmont, i.e., that I knew of the possibility of such a program; that, if implemented, it was not effective; and I had not mentioned the possibility even in passing.

Possibly, Soviet political intervention into technology made such a program partly unnecessary. A great deal of equipment was ruined because Soviet political rigures insisted on early and fast startups, insufficient maintenance and inadequate training. Russian engineers up to the 70s had about the same hands-on technical experience as a master mechanic in the United States. This has probably changed in some fields, i.e., computers and software.

Another factor is the absence of a secondary infrastructure in the Soviet Union. In the United States, thousands of machine shops, tool and die shops, and independent experts of all kinds give a wealth of backup experience where and when needed. This market-generated structure gives a richness of experience and flexibility to major corporations. The Soviet Union had nothing remotely similar; everything was handled "inhouse."

It is possible that CIA decided a "mark" program was unnecessary, i.e., it was really suckered by the Soviets, and truly believed the Soviet Union had a super-power industrial structure. Or, a mark program may have been inadequately designed. In any event, a mark program was not identified. However, it is difficult to believe that some key technologies (as the Centalign B) were not modified before shipment, if only as insurance. If we did not have such a program for key technologies, one has to ask, why not?

This raises another question. Why did policy debate always focus on the last sale or contract (i.e., the marginal unit) rather than the total of all sales in that technology. If debate was focused on the total industry, rather than single contract, then Soviet dependence is obvious.

In my exploration of the State files, I found that State Department officials filed reports with military information without distribution. The information was killed. To gauge the success of the Soviet perceptions program, one would have to thoroughly explore the hidden recesses of CIA to determine if a mark program was implemented at any point, in any way. If CIA considered such a program and rejected it, then the U.S. really did have a perceptions problem of extraordinary magnitude. For example, if Kissinger allowed the Centalign Bs to go forward without a mark program, then one has to have the most serious suspicions of Kissinger's motives.

I have no way of knowing the truth. However, one can deduce a problem from other technologies allowed to go forward, and where tracking subsequent operation proved that the Soviets had received the most advanced technology (i.e., advanced aluminum rolling equipment, marine diesels, and so on) without modifications. Example: the ships on the Haiphong run were the largest and fastest, and all had marine diesels imported from the West.

In brief, the U.S. should have had a mark program. I do not have enough data to say whether such a program existed or not. However, this would be a fruitful target for exploration of past files. If such a program was not implemented, why not? — and if it was, why did it fail?

APPENDIX TWO: LESSONS NOT LEARNED

THE HOOVER INSTITUTION SAGA starts up again in 1985 with a repeat of the perceptions disaster, and one that may have long-term consequences worse than the Soviet perceptions program. U.S. scientists are running what amounts to a scam to keep their gravy train flowing. This is losing lead time for the United States. A new technological paradigm is emerging and the science establishment is doing its best to deride and ignore the facts.

After leaving Hoover Institution, I continued to work on technological research. I found it relatively easy to earn a modest living as an author (in spite of Campbell's prediction that my rice bowl would be broken), and in the last 20 years, have authored another 20 or so books. I found regular bookstore distribution closed to my books. Campbell was right in this aspect. But apparently others were having their rice bowls broken, and a vast "alternative" book publishing industry is in great part due to the rise of new publishing houses scattered across the United States, handling the work of those who have been persecuted, or whose writing falls outside the narrow New York guidelines.

In any event, I generated enough spare time to work on technology. My exploration went back to ancient technology and science, through alchemy, Goethe, the Steiner movement, and Wilhelm Reich, as well as discarded discoveries of the last 200 years. I found hundreds of ideas and discoveries had been buried for political or financial reasons. In particular, I found that the ancients "knew something we don't today." I found that "underground" researchers were probing modern discoveries inconsistent with and wholly beyond our materialist view of the universe. The energy-matter equations, the basis of physics and the "laws" based on these equations were under assault - partly from researchers in Oriental traditions, and partly from workbench research picking up some of Faraday's observations back in 1831. Other developments were coming from the work of Wilhelm Reich and the anthroposophic tradition. Future 21st century technology will be a paradigm reflecting these discoveries, ignored in the materialist tradition.

Futurists see the future as extrapolation of the heat-pressure/centrifugal-expansion technologies of the industrial age, plus semiconductor technologies. What I see is an entirely new paradigm based on cold vacuum/centripetal vortex technologies, with semiconductors as enabling technology only. Quite different from, and far more powerful than our contemporary technology, and environmentally pure.

About 1985, I wrote to Glenn Campbell (who had been Reagan's Chairman of the Foreign Intelligence Advisory Board, and the logical recipient for any

such ideas), and suggested the new paradigm should be investigated. Campbell probably thought I was looking for some kind of grant (I wasn't), and passed me on to Roger Freeman. I met with Roger Freeman at his house on the Stanford campus and briefly outlined my ideas. I told Freeman, "I am not looking for anything. I am concerned with the future of this country. The science establishment, like the Sovietological establishment, is protecting its turf for self-serving reasons."

Roger Freeman suggested I should see Edward Teller, whom he described as open minded. I found it difficult to get across the point that "open mindedness" is not enough when dealing with a scientific culture that has thoroughly permeated our society. What my analysis suggested was existence of a superior technology to our contemporary technological paradigm, and it would be most unlikely that Teller could grasp its elements, any more than Kissinger had grasped the elements of Soviet technological dependence.

I understand that Teller did agree to talk, but the meeting did not take place. Frankly, I wasn't going to waste my time. I had been insulted and abused trying to help the United States, and I wasn't going to risk any more abuse from "deaf mute blindmen" (Lenin). Time would show whether I was right or wrong. If the gatekeepers won't listen, they have the problem, not me.

Just three years after this Stanford meeting, Pons and Fleischman announced "cold fusion." This was instantly derided as fraudulent by the orthodox physics establishment, and Pons and Fleischman were driven out of the United States to France. Teller's reaction to cold fusion confirmed I was right in not pursuing the Freeman suggestion. Cold fusion is part of the new paradigm. In the last few months of 1995, the U.S. government has confirmed its viability. If Campbell had taken me seriously in 1985, it is quite possible that Pons and Fleischman would have had more favorable reception. In fact, the Japanese now finance their work in the south of France, and the U.S. now admits they are correct - but has lost a decade of lead time.

By extraordinary coincidence, Fleischman taught electrochemistry at University of Southampton (where I studied in 1948) and Pons was his graduate student also at Southampton. (Further, one of the leading researchers in the new paradigm is Harold Aspden, today, at Southampton.)

In January, 1990, I started Future Technology Intelligence Report to record my analysis, and as a device to pick up information. For six years, I have recorded monthly my ongoing work, a matter of record for history to read.

Once again the myopic vision of U.S. technical intelligence is highlighted. Ames is a disaster. Yet the gatekeepers of science and policy are creating another disaster, and will place the U.S. into irreversible technological decline by ignoring the new paradigm.

We have politicized decision making in technology, and U.S. politics is a poor guide to technological decision making. A moral approach would have saved the U.S. yet another embarrassment. If Campbell had taken steps to correct the

injustice done to this writer in 1971 (and earlier), then the cold fusion reaction could have emerged in a different environment. I was already aware of over unity devices, and was looking at over unity N-machines when I approached Campbell in 1985. In fact, if Campbell had backed me in 1987, the U.S. would have arrived at a quicker and more accurate assessment of the emerging paradigm, and would have been ready for the hostile resistance that was forthcoming within the U.S. science establishment.

If anything, the treatment of my research is proof that in the long run, the moral approach is the only practical approach.

Just because Edward Teller designed the H-bomb doesn't ensure he will understand N-machines, cold fusion, or etheric weather control. These are outside the orthodox physicists' framework of understanding, outside the laws of physics, and require study of an entirely new framework of analysis, which is at this time incomplete. The only agreement in cold fusion, for example, is that "something" happens in the lattice structure. The "something" is under discussion and investigation. Much like electricity in the 1850s before Maxwell perfected the theoretical understanding. Etheric weather engineering is even more challenging. I have found that mathematicians (who appreciate golden proportions) are more understanding than meteorologists. In general, electrical engineers appear to grasp the new paradigm more readily. (Kincheloe, de Palma, Constable, Aspden, Tiller, Inomata, Tewari, etc.)

My greatest criticism and sorrow is that decisions on research directions are taken by those with the most to lose by change and who rationalize obstruction by clinging to obsolete ideas and citing obsolete, man-made "laws." That's no way to make progress — or for a country to survive.