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Leadership paper

Topic; What distinguishes high performing teams?

Sky High

What Distinguished the Highest Performing Team of All?

Radar, digital computation, and of course nuclear fission; World War 2 brought in the age of big science with a flurry of revolutionary technologies, each developed in only a few years with the help of government funding. Since then, the super well funded research project has continued to be an important model for scientific development, despite both the warnings of such illustrious figures as Freeman Dyson and Norbert Werner and the visible fact that it has lacked any noteworthy successes for a period exceeding thirty years.

Even the most acclaimed post-war research project, the Apollo Program, was really only a very limited success. (ironically, government funded epidemiology and agricultural research were simultaneously saving hundreds of millions of lives in the third world for a fraction of what the Apollo program cost, but received little acclaim and did not become a model for government programs) We did decisively demonstrate our superior rocketry to the Soviets, and that alone might provide adequate justification for some, but the acclaimed “technological spin-offs” and “scientific findings” are clearly inadequate to justify the massive expenditure of both money and scientific talent. Worst

of all, the anticlimax resulting from the failed Apollo 13 mission, the cancelled Apollo program and the cessation of the US presence in space set a low expectation for future research projects in all fields. The rhetorical statement "If we can put a man on the moon why can't we cure cancer" became the dull forboding idea "We can't send a man to the moon like we once could, and we couldn't cure cancer then, so current projects aimed at "curing cancer" can't possibly succeed. Don't rock the boat with new ideas or expect real results."

Eventually the observation that even huge teams can't accomplish significant results lead to a loss of confidence among most people in the ability of small R&D teams to accomplish anything more than the most trivial innovations. Innovation has predominately been abandoned to shoe-string entrepreneurial dreamers who generally lacked adequate relevant experience. Craig Venter has shown that such dreamers can outperform today's big science projects when they have as much as half the time and one tenth the budget, but the lesson has not yet reached general acceptance. R&D has become primarily a tax loophole rather than the name of a real activity. In the post Apollo atmosphere, it was possible for Xerox to ignore the actual ideas coming out of the now famous "PARC" such as the graphical user interface while spending Apolloesque amounts of money to develop a new copying machine. The worst consequence of this corporate big-science is surely in the medical field, where rapid exponential growth in expenditures yields an ever diminishing stream of actual products, and where the cost of developing one new drug today far exceeds the cost of all medical research in history prior to the 20th century.

Today all of our big science projects have fizzled. The space station is a pointless shell of what was once envisioned, justified only by the need to keep Russian scientists employed and by the pseudo-science of making further observations of the effects of zero g on biological processes, an activity as inane as it is disorganized. We still can't reliably send rockets as far as Mars, yet when our president tries to imagine a scientific accomplishment this echo of Apollo is all that he can come up with. Apparently his advisors are unable to believe in the practical reality of any research and thus see no need to even investigate the feasibility of proposals. Overall, the attitude towards research seems to usually be that it is simply a ritual that one "must" perform without expecting any actual results. Long term projections of statistics like oil consumption, social security, or population bear testimonial to this fact. Any government that considers it worth-while to predict the consequences of today's trends when projected 100 years out is a government that fundamentally disbelieves in the possibility of change. Any government that bases social security estimates on the assumption that life expectancy won't increase by 5 years in the next 70 and which projects 80% of the current cancer death rate in 2050 obviously doesn't believe that it can win the "war on cancer".

When I looked more closely at today's big-science projects, whether the war on cancer, the development of fission and fusion for nuclear energy, hydrogen fuel cells, or the human genome project, I find several major differences between them and my model for successful big science, the Manhattan project. The most important difference is the timeframe. The Human Genome project was conceptualized as an endeavor to span many decades while the war on cancer and the development of nuclear power and hydrogen energy storage are completely open-ended. By contrast, the Manhattan project,

like the other WWII big science projects, was directed at an immediate practical end. The entire project took 27 months, from spring of 1943 to fall of 1945.

Examining its pre-history from beginning to end, there was only a five year gap between Enrico Fermi's pioneering work in neutron bombardment to the discovery of nuclear fission and the concurrent conceptualization of nuclear weaponry by Szilard (inspired by Wells). Szilard took his idea to Einstein, who gave it to Roosevelt's economic advisor Alexander Sachs. Another year passed before Robert Frisch and Rudolf Peierls encountered the stalled proposal, worked out their approximations of how the bomb could work and how much uranium it would take, and promoted it within the government. Trinity took another 5 years.

For things to move this fast, (and it is important to note that they did so in more than just this one instance; radar played a larger role in the war, and computers a larger role in the peace, but all three shared similar origins) one requirement was a very well connected scientific community. This community needed to have connections with people in positions of power, a great deal of freedom to pursue research avenues of their choosing, and global mutual respect. Prior to WWII, the latter was easier to enable, as physics was not a profession. Every physicist was driven by a passion for the subject, not merely be the need for a stable income and for recognition of his or her intelligence. Many physicists were the children of high society rather than middle class workers.

Today it is difficult for a young researcher to win the confidence of his peers to such a degree that they will take the effort to understand a difficult new hypothesis. It is thus much harder for grad students to acquire the recognition that would bring them to the attention of the physics world. Such neophytes who had already proven themselves

made up the bulk of WWII research teams. The average age of a researcher on the Manhattan Project was 29 and this average was heavily influenced by a small number of older scientists. The median age was thus lower still.

Despite the cultural advantages of the day, the Manhattan Project would not have been possible in the absence of the extraordinary collection of individuals who uprooted their lives to work in Los Alamos. The urgency of the war and excitement of one another's presence motivated them to act with a much more intense commitment than commercial motives could have inspired. Oppenheimer endangered his health with the intensity of his efforts, and by the time the project was over he weighed only 115 lbs (he was a tall man, so this implies severe emaciation). Such intensity could not possibly have been continued over the length of a 5 or 10 year endeavor. Neither could any other endeavor, least of all weapons development, have brought together such a collection of talent. 16 of the scientists who worked on the Manhattan Project went on to win Nobel Prizes, 15 in physics and one, Joseph Rotblat, in peace. Rotblat actually did not stay with the project till the end; he left in 1944 upon learning that the Germans had no equivalent program, and devoted the rest of his life to opposing nuclear proliferation.

Despite the situation, Oppenheimer's constant coaching was required to help the majority of the participants to resist their moral qualms. That such an essentially literary man and such a gruff and warlike one as the project's actual director, General Richard Groves, were able to cooperate so fully was the fruit of Nazi horrors. That both were able to master nuclear physics and large project management to the degree necessary to coordinate the necessary efforts is substantially a tribute to the broad humanistic education of the day. However, the biggest secret of their success was humility (a trait

that neither ever displayed again). Knowing perfectly well that much of the success of any given technique depended on scientific variables that were as yet unknown, Grove funded every credible proposal for Uranium separation or Plutonium generation in parallel. Likewise, Oppenheimer allowed his scientists to attempt any approach that they were inspired by when they confronted a problem. Teller was even permitted to blow the whole project off and focus his efforts on the development of the H-Bomb which he saw as the A-bomb's inevitable successor. Removing him from the project when he refused to contribute to the laboratory work would have been an entirely reasonable measure for Oppenheimer to take, but maintaining this level of freedom kept morale high despite wretched living conditions and enabled each scientist to make an optimal contribution by letting the same person's insight search out both problems and solutions. Even if Teller didn't help directly, getting rid of him would have seemed like the removal of a political enemy, and who knew what ideas other scientists might usefully bounce off of him.

Another aspect of Oppenheimer's leadership, one that even the disagreeable Teller praised during the later congressional hearings, was his ability to produce a positive atmosphere. The Los Alamos setting was almost entirely undeveloped. Running water was chronically unreliable and worms frequently crawled out of faucets. Despite this, Oppenheimer managed to maintain a camping or party atmosphere. Frequent parties, with drinks spiked with laboratory alcohol, appear to have added to total productivity. At one point, an administrator from a nearby hospital jokingly asked him to tone it down because the medical facilities couldn't handle any more babies. Creating such a mood among the world's top physicists was probably something that only an excellent physicist with a strongly humanistic bent could have accomplished. It would be

very hard to find such a person either then or today. What may have been more common then was a military person who understood aristocratic attitudes and what they were good for. General Groves' choice of Oppenheimer as scientific director was rather surprising, especially considering Oppenheimer's Communist connections. Such excellent judgment of talent and skill at enabling it to accomplish all that it is capable of, was key to the project's success.

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