

**Final Report on:**  
**“Science, Technological Progress, and the Role of the Government”**

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The argument for government involvement in science goes back at least to Francis Bacon. Bacon’s argument has been boiled down by Kealey and Niskanen to the following claims: Economic growth requires advanced technology. Advanced technology requires basic research in science. Basic research in science requires government financing. Even classical liberals, such as Thomas Jefferson, who are generally convinced that the value of individual liberty implies a minimal government, have often grudgingly accepted that the case of science is different. Sometimes, as in a famous article by Kenneth Arrow, the basic argument is captured in the claim that science is, like national defense, a crucial “public good.” Bacon’s viewpoint has been dominant in the West in the last half century and has been supported and elaborated in famous essays by Vannevar Bush and by Michael Polanyi.

Recently Martino, Kealey, Niskanen, and others have re-opened the discussion by questioning one or more of the links in “Bacon’s Chain.” Among the questions that can be raised are: How much does technological advance depend on basic research in science, and how much (as Smith, de Tocqueville, Hamilton and maybe Jefferson thought) on tinkering, learning by doing, or on more applied research and development? To what extent is the “linear” view correct that basic research in science leads to technological progress? Or alternatively, to what extent are Rosenberg, Mowery, Kealey and others correct that the interaction between science and technology is more complicated, with a nuanced mutual interaction between technology and science? And if the nuanced view is more correct, then does that undermine the case for government support for science, or leave it intact?

Are scientific peer-review evaluation processes effective at advancing science? Or, as Harry Truman thought, is some more customer-oriented approach more likely to advance science? And if a customer-oriented approach is better, should it be implemented, as Truman thought, through active federal oversight of scientific priorities, or as Kealey and others believe, through more funding from industry, philanthropy and from the scientists’ self-funding? Ben Rogge used to remind us that “he who pays the piper, calls the tune.” So if the government pays for science, will the tune called be one that is best for the advance of science, and best for the taxpayers?

To what extent are the motives of scientists different from others? If, as Smith believed and

as scientists like to believe, the motives of scientists involve the pure pursuit of knowledge, then does this imply that science would be self-funding, even in the absence of government funding? Or would the lesson be, as Polanyi might argue, that the government should pay the piper (scientist) and trust the piper (scientist) to call his own tune? Alternatively, if Bartley is right that academics generally have no more noble motives than other mortals, what does this imply about the institutional structure for encouraging the growth of knowledge?

Bartley endorses the “marketplace of ideas” analogy and advocates attention toward the “economics of knowledge.” But although Bartley is very critical of universities and other traditional academic institutions, he only vaguely points toward existing, or possible, alternative institutions that could better promote a free and efficient marketplace of ideas. The issue is taken up by Kealey, however, who argues that private industry in a free market will have substantial incentives to invest in the growth of scientific knowledge.

The colloquium focused on readings that articulately address many of these issues. The issues are not only directly relevant to long-standing and current policy disputes, but more importantly speak directly to fundamental issues of the nature of knowledge, progress and responsibility in a free society. The fundamental question is: can government support of science sufficiently advance the quality of life to justify to loss of liberty that the expansion of government entails?

The rest of this report will summarize the content of each of the six sessions in a fairly comprehensive manner. Material presented in brackets { } was not spoken by the participants, but is added to provide context for possibly elliptical remarks. Most of what is attributed to participants below does not consist of exact quotes, but rather consists of paraphrases filtered through my notes and memory.

## **“More Details on the Sessions, Themes and Readings”**

### **Session 1:**

Francis Bacon. The Advancement of Learning; Novum Organum; New Atlantis. (Vol. 30 in “Great Books” series) Encyclopedia Britannica, Inc., 1952, pp. 29-32. [ISBN not in book]

Vannevar Bush. Science: The Endless Frontier. Washington, D.C.: National Science Foundation, July 1945 (reprinted July 1960), pp. 1-40. [ISBN not in book]

*Bacon was one of the earliest and most influential scholars writing on the method and institutions of science. He made the case for the positive impact of science, and for the desirability of government support of it. Barfield has called Bush’s report “the single most influential document shaping U.S. science and technology policy over the past half-century.” In the Bacon tradition, it makes the case for expanding the government financing of science into the post-war era, by expressing optimism about the importance of science, and the ability of government to speed the progress of science.*

John Danford proposed we spend the session discussing the enterprise of science. One question is

how much hierarchy, coordination and centralized planning promotes science. Descartes was a promoter of hierarchy and coordination.

Scott Ostro says that scientific progress sometimes results from planning and sometimes is more haphazard. Joe Martino mentions that cities end up fitting together well, though not well-planned. Gonzalo Munevar says that V. Bush does not agree with Descartes. Gonzalo reads Bush as mainly supporting the unplanned progress of science.

David Levy points out that the Bush document is clearly a reflection of the American experience during the war—the goal was to turn big rocks into rubble. Levy likes Bacon.

Gonzalo says that scientific progress results from the free play of free intellects. Joel Mokyr read Bush in the perspective of Bush's time, and in Mokyr's role as an historian. Most of the allied victory was not due to any major scientific discovery, but to applications of previous discoveries. Steve Ostro mentions that he thought radar *was* a scientific discovery that was essential to the outcome of the war. John Moore agrees that science did make a contribution.

Terence Kealey says he is warming to Bacon. He believes that only 1 or 2% of scientists really matter. Ed Larson supports hierarchy in a sense. He believes Bush and Bacon go together---they both wanted scientists mainly back in universities. Bush thought the U.S. scientific elite was in a competition with the Soviet scientific elite.

Steve Postrel says that the Bush document is part of the unwinding of the war effort and a continuation of the New Deal. Another notion in the document is that basic research comes before applied research. This is the most objectionable part; nobody really believes it.

John Moore thinks that the wartime effort justified Bush's views. Bush saw the end of the war as a good time to garner public support of science.

Ed Larson claims that the whole document is a shill for *university* science. Susan Feigenbaum says that the Bush document speaks to Bush's view of the optimal structure of academic science. The primary place for medical research is the university. The genome project has shown that competition in science speeds the rate of scientific progress---the 20 years projected for completion of the genome project turned into 2 years as a result of competition.

Virginia Postrel is struck that the Bush document was not very well-considered. She thinks it *is* well-considered as a play for patronage. With this goal in mind, Bush's claim that support for science would result in full-employment, had to be in there whether Bush really believed it or not.

David Levy emphasizes that one of Bush's important points is to argue for a post-war openness for scientific results. David suggests comparing U.S. to British post-war policies. Turing's machine remained classified in Britain until 1970. Bush's document is pushing for openness---science is providing honest-to-goodness public goods.

Joel Mokyr suggests that Virginia has opened a big door---the Bush document is really just rent-seeking. It is just scientists asking for a slice of the pie; just cynical manipulation. John Moore

points out that the title of an official history of the NSF is: “A Patron for Pure Science.” Virginia Postrel says it is important to look at the *other* funding options that were available at the time.

Stan Liebowitz suggests that rent seeking often means rent dissipation. Stan believes that Bush really believes most of what he says, though he is trying to push as hard as he can (as in the full employment claim). Virginia Postrel agrees that Bush may believe *most* of what he’s saying.

Robin Hanson says the essence of Bush’s argument is: we were successful, so we should get more. The medicine part is very weak---there has been very little effect of science in reducing mortality.

Gonzalo Munevar mentions that Bush has to be seen as responding to Roosevelt. He also asserts that radar was *basic* to winning the Battle of Britain.

Responding to Hanson, Art Diamond mentions Bush’s claim of the importance of penicillin in reducing war-time mortality. Terence Kealey gleefully points out that penicillin research at Oxford was funded by the Rockefeller Foundation.

Joe Martino says that when he was in the military, he often drafted letters for his superior to send to Martino. He also mentions that after the war, the multiple sources of funding was a great thing.

Susan Feigenbaum says that government funding of science crowds out private funding, like that given by the Rockefeller Foundation.

David Levy and Joel Mokyr have an exchange the gist of which is that World War I was the first major war in which deaths from combat exceeded deaths from disease.

Terence Kealey says that 99% of people believe in the linear model of advances in science leading to advances in technology, which in turn lead to economic growth. Bacon, Bush, and Jefferson all believe that science is a public good. However much you believe in the market, most folk believe that science is an exception. Gonzalo suggests that most folk think about *applied* science as a public good. Stan Liebowitz warns that we should be more precise on what “public good” means. According to Samuelson, it means a good that cannot be used up and a good where the creator or owner cannot keep other people from using it. Stan claims that the definition matters for policy.

John Moore suggests that scientific discoveries differ in their appropriability, e.g., the Higgs Boson vs. the genome.

Steve Ostro says that as a scientist he has some insight into the scientific enterprise. He says that there are some individual scientists who need nothing to do their science; some science needs teams; some needs teams of teams; some needs millions and millions of dollars. Some scientists are motivated by a drive to use nature, to control it, to be rich. He has encountered the whole distribution of motives. A key question is who decides which directions will be taken?

On motives, Steve tells an anecdote. When he was a graduate student, the door of his advisor had a note with the title “Read This.” The note said that there was a nil chance of getting a job. Terence Kealey interjects that the note was wrong. Steve responds: “but I proceeded even though I believed

it.” “Science in spite of everything” is our motto.

Ed Larson suggests that even *before* the Bush document, most science was funded by government--*state* government. Bush was trying to change the university from a teaching institution to a research institution. Cold Springs Harbor, Bell Labs, and Edison, existed before this, but universities were primarily about teaching. The Bush document primarily aims to transform the university. He won and we all lost.

Robin Hanson claims that Bush assumes the transformation of the university, but doesn't argue for it.

John Moore cites the statistic that in 1953, 60% of science support was from government and 30% was from industry.

David Levy and Virginia Postrel have a brief exchange over whether V. Bush was familiar with Keynes' work.

Joel Mokyr says the biggest expansion in government support for science came in 1957 after the Russians launched Sputnik. He says there is a “Willy Sutton” effect. Why do the scientists go to the government for funding? Because that's where the money is. The letters requesting funding are the same whether they are sent to private foundations or to the government.

Susan Feigenbaum emphasizes the differences between private and government funding. For example, one goal of government funding has been to build up weaker institutions.

David Levy reports that there are a huge number of R&D institutes in government. The government created DARPA so that Sputnik would never happen again. DARPA didn't even use peer review. Things are complicated. {DARPA was involved in some of the initial development of the internet.}

John Moore agrees with the Willy Sutton theory. The NSF comes from taxes, while foundations come from voluntary money. So the long term future looks brighter with taxes. As an aside to David Levy, John mentions that there are about 700 R&D institutes in the government.

Terence Kealey notes that Bush wanted to create a particular form for the NSF. Truman vetoed it, because he believed that the NSF should be democratically accountable. As recently as 1940, private funding of science in the U.S. was much greater than public funding.

Virginia Postrel says that in these readings the central question is that we inside the scientific world know what it takes. It takes colleagues, libraries, etc. The question is how do you get it? Can you do that without being a landed aristocrat? How can you do science in a bourgeois republic, especially capital intensive science?

Gonzalo Munevar was captivated by Bush, but kept looking for the role of serendipity. The question still remains for Bush: who do we fund?

Joe Martino says that he spent most of his military career buying research. It is important to

distinguish between buying research for the military's own needs, versus funding research for other people to use. Virginia Postrel elaborates that this distinction is critical to the argument that the government is essential to the development of the internet.

Steve Postrel suggests that a context for U.S. science is the heritage of Puritanism. Many scientists were sons of clergy. They had the same norms---they wanted to do pure things, not pursue wealth. There is a threat in the military control of science, but also a private threat expressed elsewhere as patent-seeking.

Ed Larson says that Bush plays on a theme of Frederick Jackson Turner {Turner expressed his "frontier thesis" in 1893}. Space science has had a lot of applied science come out of it. The government has to fund it because nobody else will. Ed is not sure this argument works.

Gonzalo Munevar mentions that Bell scientists needed Princeton scientists to explain their results.

Joel Mokyr suggests that the best thing that happened to science at the time of the Bush document was the openness to overseas scientific talent moving here. David Levy says that the U.S. was free-riding on Europe, since these scientists were educated in Europe. Virginia Postrel suggests that Bush is not talking about the influx of immigrant scientists. Art Diamond suggested that mentioning the immigrants did not serve Bush's main argument that the war had resulted in too few scientists in the U.S.

Joel Mokyr and Joe Martino identify Bush's argument as more scientific rent-seeking. Robin Hanson suggests that almost all academics would say that Bush was about right.

Virginia Postrel says that the scientific establishment *does* want to brain drain the world because they like the idea of being with other interesting people. Ed Larson adds that the scientific establishment doesn't want to train foreign students who go home. Joel Mokyr indicates that most stay.

Susan Feigenbaum suggests to Robin, that we need to survey the opinions of scientists who have left academics, not just those who have stayed at the university.

Gonzalo points out that by now most of the brain drain is over.

Steve Postrel says that when hardware folk were asked about government funding, they liked the government funding of universities, and did not like the government funding of national labs. The universities were seen as more productive.

Susan Feigenbaum says she's more focused on medicine than hardware development. John Moore says that many companies were more supportive of the government's genome project rather than Celera's efforts.

Terence Kealey says that the Prussian university was designed to help Prussia catch up with Britain. We're copying people who are copying us. And the university does little confronting of the government. Ed Larson adds that the university also does little teaching.

David Levy suggests that just as there is an exchange theory of government, we should ask what would be an exchange theory of science. Such a theory would tell us how to maximize the value of complementary inputs. Whatever is produced is openly appropriable.

Joe Martino closes the first session by questioning the assumption that private firms would do not research in the absence of government funding of science. But scientists did Nobel-Prize-winning and Maxwell-prize-winning research while working for private industry.

## Session 2:

Adam Smith. "The History of Astronomy." In Essays on Philosophical Subjects. LibertyClassics, 1982, pp. 33-53. [ISBN 0-86597-023-8]

Adam Smith. The Theory of Moral Sentiments. LibertyClassics, 1982, pp. 124-125. [ISBN 0-86597-012-2]

Alexander I. Solzhenitsyn. "At the Fountainhead of Science." Ch. 80 in: The First Circle. New York: Harper & Row, 1968, pp. 501-509. [ISBN not in book]

*Smith's essay has been identified as pre-Kuhnian Kuhn, and by Schumpeter as better than the Wealth of Nations. The essay expresses Smith's views of the nobility of the motives of scientists. (A brief selection will also be included from The Theory of Moral Sentiments where Smith comments on Isaac Newton's noble motives.) Solzhenitsyn's The First Circle could be interpreted as arguing that the internal values of scientists are sufficiently strong that science can survive even under the most authoritarian of regimes.*

John Danford begins the session by suggesting that we focus on the question of why scientists do science?

Steve Ostro {who is an asteroid scientist} says that science excites wonder. He gives an extended example from his own work in the last few days. He has discovered an object in an orbit that has never been seen before. Only four folk in the world, before those in the room, know of this. It is within 1,200 miles of the moon and will be within the orbit of the earth until later this year, from data through last night. An object like this, it has been argued, cannot be natural. It is the size of the room we are in.

Joe Martino says that science is fun and exciting. When the dots fell on his theoretical curve, it made his day. Robin suggests that the answer is more complicated. He says that we like to show off; to show we're smart.

Joel Mokyr wishes his economist colleagues were present--those who work on the endogenous growth theory {e.g., Paul Romer}. They treat science as a big black-box: money goes in, money

comes out. But the model ignores the role of curiosity. No straight profit-maximizing motive explains it. There *will never be* a good economics of science.

Susan Feigenbaum is disturbed that creativity, etc., is being uniquely attributed to scientists. She points out that entrepreneurship has exciting problems too.

Ed Larson suggests that doing science has survival value. Terence Kealey suggests that scientists act out of pure ego, as derived from sexual selection. Chairman Mao could not destroy science, even though he threw scientists out of windows. Mao was most effective at destroying science when he made all papers anonymous. Terence thinks Steve Ostro is in it for his ego.

Steve Ostro rejoins by noting that he took his name off a circular so that he wouldn't be bothered by the barrage of email.

Joel Mokyr notes that in the quote from The Theory of Moral Sentiments, Smith was wrong about Newton, but right about many others.

David Levy suggests that what mainly motivates scientists is the desire for honor and approbation. George Stigler had David read Robert Merton on approbation as measured partly by citations in the literature. One important question is how do you get an increasing returns model going? Even Schumpeter, who can't abide Smith, adores Smith's "History of Astronomy." Smith sees something about the history of astronomy that resonates.

Steve Postrel notes that generally economists don't explain preferences. Business managers are assumed to want money; we don't integrate well the consumption value of any activity. Some people will pay for the chance to do science. Joel Mokyr says that in The Theory of Moral Sentiments, not the passage in our readings, Smith says this.

Gonzalo Munevar says that according to Kuhn scientists are raised to solve puzzles and show off what they can do. Konrad Lorenz found that rats have curiosity which came about through evolution. Curiosity is a form of play. Virginia Postrel says "this is the perfect set up," to which Gonzalo responds "I'm just your straight man?"

Virginia Postrel argues that intrinsic satisfaction is an important part of why folk do science. Still, they come back to: please send me money so I can have fun. They still need some minimal level of support.

Stan Liebowitz is not sure what the payoff is in trying to learn what makes scientists scientists. He thinks the important question is: if we give more money, will we get more science?

Steve Postrel says the fact that zillions of folk want to be actors has something to do with how little is paid. Stan Liebowitz adds that science is not all that unique among higher level creative professions. Gonzalo disagrees, saying that it *is* different because science fulfills something that being an architect would not.

Virginia Postrel says that law is mentioned as an alternative to science. You need to pay money to

lawyers.

Ed Larson says people desire many things. Some go into science for religious reasons; you want to do it to understand God.

Robin disagrees, saying that people in science don't want the usual human motives attributed to them.

Art Diamond asks if Steve Ostro's case is usual?

Joel Mokyr said his physician wife found that lower doses of a medicine worked better against a cancer than higher doses, and then spent the next 25 years trying to find out why. It's an anomaly in the data, like the kind of thing Adam Smith is talking about.

David Levy says some of reputation is in funding or fame. You can think of 12 great names in Greek philosophy. Can you think of 10 olive merchants from the same time? Are scientists more famous than lawyers? Joel Mokyr answers that Michael Jordan and the Pope are the best known people of the present time. Gonzalo responds that now Frank Sinatra is more known than Niels Bohr, but 1,000 years from now no one will know who Sinatra was, but there will be a few who will know who Bohr was.

Terence Kealey disagrees with Virginia on lawyers. Virginia says back at the time of Bacon, lawyers were the alternative profession to becoming a scientist.

Gonzalo asserts that there is an evolutionary advantage to curiosity; curiosity has survival value. Most of us live in cold environments where humans couldn't live without the fruits of curiosity.

Steve Ostro feels as though he's on the psychiatrist's couch. Steve Postrel responds that at least he is not being dissected.

Steve Ostro says that fame is fleeting; we are all only a blip on the cosmic scale. He's not much good at anything else but science. If you see something that needs doing, and you can do it, you do.

As a scientist, you never lie. By agreement, you set yourself up to be attacked. If you make mistakes, you undermine your credibility. He is extremely lucky to do something that is valued by the public.

Susan Feigenbaum reads with sarcasm the Smith passage unrealistically praising the purity of Newton's motives. She asks if there have been any changes in the virtue of academics? Ed Larson asks why we should assume that things have changed? Maybe Smith was wrong. Gonzalo Munevar responds that Smith was wrong.

Virginia Postrel says that everyone agrees that scientists have pleasure as a motive in what they do. She agrees with Susan that other occupations do also. Another view out there is that you buckle down and do your duty in a very hierarchical system where someone directs you to what unpleasant work you do.

Gonzalo Munevar says that at his school one of the new hires is doing all of the work, but getting no research done.

John Moore says many of those in science start with low pay. A lot of time is spent as the slave of a senior professor with the hope of a post-doctoral fellowship. Then the rest of the career is spent scrambling for grants. Opportunities in industry are greater. Why do they stay? It must be because of significant non-pecuniary motives. More money will bring in those more marginal in their commitment to science.

Joe Martino says most scientists want fame *among their peers*. Joel Mokyr says that the great bulk of the people doing science have no hope of being among the greats, but they value recognition from their community of scholars. Someone says that they read your paper; it's the thrill. Very few think about posterity.

Robin Hanson says that people fight to converse, showing that curiosity isn't everything.

Virginia Postrel recommends *The Mating Mind* by Geoffery F. Miller.

David Levy asks if scientists are amateurs rather than professionals, will they be more or less careful? How about with or without team production of science? David adds that he bets everyone knows their "Erdos number." Robin Hanson responds that "I'll take that bet." {Paul Erdos was a famous, prolific, eccentric mathematician---the Erdos number tells how close you are to Erdos. Erdos has a 0, co-authors have a 1, etc.}

Terence Kealey thinks he has an answer to why scientist's motives changes. Two overlapping societies in Britain did the same kind of science, one of professionals and the other of amateurs. The professionals did it better, but the amateurs were more polite.

Stan Liebowitz is surprised at the pejorative attitude toward those who go into science for the money. John Moore responds that he hadn't said that those who go into science for the money would do a worse job.

Art Diamond asks if the institutions of science work if a critical mass of scientists (at least many of the gate-keepers) do not have "pure" motives? John Danford responds that we'll look at that question tomorrow.

Steve Postrel says that there is a tipping effect as the profession gets bigger and bigger.

Joel Mokyr says that a scientist cares about her own little enclave.

Virginia Postrel makes a comment on gentlemen amateurs. There is a horrible competitiveness of writers, e.g., Samuel Johnson's 'nobody but a blockhead ever wrote for money'. Writers for money, may have been great writers.

Gonzalo Munevar responding to John Moore, says that for anyone to be a scientist, you need to be in a society where it is possible; in Columbia you could not be pursuing heavy leptons. {heavy

leptons are one of the particles discussed in physics} There have to be conditions in place for you to even *aspire* to be a scientist.

Ed Larson says that he taught an honors course on what it means to be a scientist. He mentions Paul Ehrlich's The Magic Bullet. Most honors students said they didn't want to be scientists because it is hard. You have to be *absolutely consumed*. Madame Curie visited women's colleges in the U.S. They asked the students if they wanted to be like her? Only 2% said they did; the rest said it is too hard; it's not fun.

Susan Feigenbaum says that any entrepreneur is more consumed than a scientist. It's not good to discriminate between scientists and others.

Steve Ostro says his son said Steve's crazy. Steve spoke to Cal Tech undergrad students. No one wants to be a scientist; only 2 or 3 wants to be an engineer. The rest want to be lawyers or to go into business.

Stan Liebowitz says that Nobel laureates are different.

Gonzalo Munevar says that people become driven *later*, after they enter the job market.

Steve Postrel recommends The Stars are Not Enough, though he calls it an obscure book dealing with high end institutes.

Joe Martino suggests that there is a distribution of talents. If you expand science too greatly, you get too many incompetents. He tells a joke about asking an accountant, a lawyer and an engineer whether it's better to have a wife or a mistress. The engineer says its better to have both. The wife will assume you're with the mistress. The mistress will assume your with the wife. And you can actually get some work done.

Robin Hanson comments on the Solzhenitsyn story that scientists often provide legitimization, which is often a criticism of scientists.

John Danford asks what is the connection between science and tyranny?

Joel Mokyr says that there is something misleading in looking at the top 1% of anything. The vast bulk of people live life very much like the rest of us. The top 1% work harder than the rest in *any* profession.

Ed Larson says that there's more a sense that students think it's harder to be a scientist. John Moore says one reason is *math*. Gonzalo Munevar adds that you need a Ph.D., then a post-doc, and then you see the note on the door {that Ostro told about above}.

John Danford suggests that it is an exaggeration to say that it takes 17 years of training and apprenticeship before a scientist can do science. He says that full professors are doing the same thing as assistant professors.

Virginia Postrel focuses on the psychology of the career decision. She says that there is a sweet spot between boredom and anxiety. Most students have courses in science that put them more in the anxiety range.

Gonzalo Munevar suggests that in other fields, you get to do it right away.

David Levy says it is revealing to ask how scientists work when they become expert witnesses. They behave differently.

### Session 3:

Adam Smith. An Inquiry Into the Nature and Causes of the Wealth of Nations. Vol. I, LibertyClassics, 1981, pp. 13-24. [ISBN 0-86597-006-8] *Selections on the importance of technology and the sources of technological improvement.*

Thomas Jefferson. "Manufactures." In Notes on the State of Virginia. New York: W.W. Norton & Co., 1964, pp. 164-165. [ISBN in book]

Thomas Jefferson. "A Letter to Benjamin Austin." In Merrill D. Peterson. The Portable Thomas Jefferson. Penguin Books, 1985, pp. 547-550. [ISBN 0-14-015-080-3]

Alexander Hamilton. Selection from "Report on the Subject of Manufactures." In The Papers of Alexander Hamilton, December 1791-January 1792. New York: Columbia University Press, 1966, pp. 250-269. [ISBN not in book]

Alexis de Tocqueville. "Why the Americans are More Addicted to Practical than to Theoretical Science," Ch. 10 of Democracy in America. Chicago: University of Chicago Press, 2000, pp. 433-439. [ISBN 0-226-80532-8]

*Smith viewed technological improvement as an important contributor to economic growth. He also viewed technological improvement as arising more from the practice and attention of technological laborers, than from science. To the extent that Smith is right, the "Bacon chain" for government support of science is weakened. Jefferson and Hamilton both seem to agree that science is not necessary for technological growth, but disagree on whether technological innovation is necessary for economic growth. De Tocqueville supports Smith's view, by recounting how in America theoretical science was not a driver of practical progress.*

John Danford starts the discussion by asking how much does technological advance depend on basic research in science, and how much on tinkering, learning by doing, or on more applied R&D?

Steve Postrel says that Smith has it both ways; the division of labor is good in one place and bad in another.

Joel Mokyr says the division of labor allows a manager to assign workers to tasks for which they are

qualified. He refers us to the paper by Gary Becker and Murphy in the Quarterly Journal of Economics that shows that the division of labor makes management harder.

David Levy says that Smith thinks that everyone is the same initially. Terence Kealey responds that Smith never had children of his own.

Gonzalo Munevar says that in logic, Smith is not to be faulted. The pin example is impressive, showing that specialization leads to greater productivity, but is boring for the workers.

Ed Larson is not persuaded that a person with a tiny task would be innovative.

John Danford reports that when he worked on an assembly line, he figured out a way to bag ice better. Robin Hanson reports that when he worked on an assembly line, *he* didn't innovate.

Joe Martino adds an anecdote. He had a job laying bricks in which the foreman held a dozen patents of new ways to handle bricks. The shop steward told the workers that the foreman was killing jobs.

Gonzalo Munevar mentions that many defects result from workers being bored. He says that production by teams may help address the problem.

Art Diamond suggests that Smith's example of the small boy innovating was made up. He mentions that differences in productivity between the U.S. and Japan have been attributed to Japan's openness to worker innovations.

Ed Larson asks what's the incentive for the worker to come up with new ideas? Who came up with the automatic starter on cars?

Steve Postrel reports that in Japan there has been a decrease in specialization. The Toyota system involves workers doing multiple tasks. In Japan, pay varies based on the performance of the factory. *Process* innovations involve more learning by doing, as compared with product innovations.

Steve Ostro asks what this has to do with government support of science?

Virginia Postrel says that people who spend all of their time on a task may be able to make *some* kinds of improvements.

Art Diamond responds to Steve Ostro, by suggesting that the case for government support of science is stronger to the extent that technological innovation depends on the advance of basic science. Joel Mokyr supports Art, saying that if the process goes on its own, then the government does not need to intervene. Mokyr thinks that eventually you run into diminishing returns from gains simply from learning by doing. Learning by doing in milking cows would never lead one to invent a cow milking machine.

Robin Hanson argues that the economy is growing faster recently. Learning by doing has been

around for thousands of years, so what is different that has resulted in the faster economic growth?

Joe Martino says that there is a limit to how much you can improve a process by learning by doing.

Terence Kealey wants to offer an alternative to science as a reason for technological growth. It is a myth that the 17<sup>th</sup> and 18<sup>th</sup> centuries saw new technology. Legumes were known thousands of years earlier. Everyone lived at a subsistence level prior to the market economy.

Joel Mokyr disagrees, saying that turnips were new. They fed cattle and produced manures. Terence is unconvinced, saying that legumes were the key.

Gonzalo Munevar says that there is a second way to consider the relevance of this material. He suggests that Art Diamond's rendering may involve a mistake like Kuhn's distinction between normal and extraordinary science.

In answer to Robin Hanson, Art Diamond mentions the hypothesis that the speed of adoption of technologies, rather than science, is responsible for the faster economic growth. Robin Hanson responds that he actually agrees that the speed of adoption is what matters.

David Levy says that technology diffused through machinery. The endogenous growth discussion starts with Smith; the question of funding is still there.

Steve Postrel says that Smith says philosophers are *not* specialists. David responds that Steve should read the next sentence where Smith says there *is* specialization.

Virginia Postrel says that one of the arguments in favor of the funding of science is to allow more people to specialize in being scientists.

Joe Martino comments on Robin's issue of what's changed. He says that during Paleolithic times, the rate of growth of flint was equal to the rates of growth today. Joel Mokyr and Robin Hanson both say that can't be true.

Virginia Postrel says that it took less rock to get the same flint.

Robin Hanson says his point was to offer an hypothesis on what is different.

Joel Mokyr says there are examples of specialists talking to each other. You can also specialize in the invention of new things. He suggests turning to Tocqueville. Nations may specialize in different kinds of knowledge.

John Danford wonders about our reaction to Tocqueville's claim that there has never been a great theoretical physicist in America.

Ed Larson mentions Franklin and Steve Postrel mentions Feynman. Gonzalo Munevar mentions Feynman and James Watson, co-discoverer of the structure of DNA.

Terence Kealey says that a democracy has to be practical. He says that science will do find with support from industry, citing the many scientists employed by industry.

Art Diamond replies that one reason firms hire scientists is to interpret for the firm the work of government-supported university scientists. Susan Feigenbaum says there is a role for government.

Ed Larson says that part of the division of labor is to have inventors; his grandfather was an inventor. In the days of Williamsburg, there was no place to sell barrels, so there was no incentive to invent a better barrel. John Danford adds, that in that case, you may as well saunter.

David Levy comments on Tocqueville's claim, that there was a huge shock called Hitler. So American students got contact with great European scientists. Smith starts with a problem: how do I get out of work? With almost free computer time, ideas that were talked about 150 years ago and dropped, are now alive and well. This is different than the basic linear model.

Stan Liebowitz says that you are going to get *some* basic research without government. The question is whether you get the right amount? This is a very hard and empirical question. The most important thing is the division of labor.

John Danford says there is no such thing as government money, it is all our money.

Gonzalo Munevar says that there are two arguments for the government coming in. Tocqueville discusses that the star gazer is criticized in a democracy. If Tocqueville is right, then funding the star gazer is a reason for government coming in, in a democracy.

Virginia Postrel thinks that Tocqueville is right on this, even today. But then, the question still remains of how such a democracy would be expected to vote support for the star gazer?

Steve Postrel answers that you need to sell people on the linear model.

Gonzalo Munevar believes that people in a democracy want lives with some excellence. John Danford puts it more strongly, saying that people do not just want to be satisfied pigs.

Ed Larson says that the form of the Bush argument was: you have got to have government involvement because otherwise you will not get these good results.

Stan Liebowitz claims that an important current debate is whether competitive or monopolistic market structures result in more innovation. There is no room in the standard economic model of competition for innovation---it is too simple.

John Danford says that one good thing about aristocracy is that people think about God and higher things. John says he's not much of a fan of democracy, he's a fan of rule of law.

Virginia Postrel says that Tocqueville would be surprised that in a democracy people vote support for science. Gonzalo Munevar adds that scientists from other countries could only do their great work *here*.

Joel Mokyr asks if Tocqueville is relevant? Stuff moves quickly across the ocean, though many don't have access to this information. Britain makes big contributions to science. France made contributions to theory *after* they became a democracy.

Joe Martino returns to Stan Liebowitz's issue of whether there is underinvestment in basic research. Joe argues against this, saying that there is no way to tell what the marginal returns are. John Danford asks, shall we vote?

John Moore believes that a case can be made that without the government, too little would be invested in basic research. But it is not realistic to think that the government could decide the optimal level.

Gonzalo Munevar wants to argue for big bucks from government in a big way for experiments of some colleagues of Steve Ostro. No industry would provide support for massive space research.

David Levy points out that you don't have to be able to calculate the social optimum in order to know you should be doing more of something.

Virginia Postrel points out that in China the government had a complete monopoly. She quotes from Mokyr's Lever of Riches. The state lost interest in science, thinking that it had nothing to do with Chinese culture.

Joe Martino asks if it is necessary for space science to be so expensive? He suggests that maybe we should wait until the costs go down. He likes the "X prize" for the first reusable space launch vehicle. Joe's on one of the teams competing for the prize. Gonzalo agrees with Joe---the shuttle has been a complete disaster.

Gonzalo Munevar gives an epistemological defense of space research, based on the desirability of exposing ourselves to the serendipitous and unexpected phenomena that are likely to be encountered in space.

Terence Kealey grants that without government, we would never have a space program, though we would have satellites.

Ed Larson suggests that the government got involved for military reasons.

David Levy asks if people could allocate their tax dollars, how would people choose? He thinks science funding would go up.

Steve Ostro returns to the question of whether Tocqueville is right about the absence of scientific theory in America. Steve says that every American university has pockets of theory. Steve Postrel responds that Tocqueville wrote over 100 years ago---give the man a break.

Steve Ostro says that people's opinion of NASA is higher than their opinion of any other agency. On the division of labor, having lived in New York and New Jersey, he supports the government's

support for agricultural research.

Robin Hanson suggests that it is not at all obvious that you get too little basic science; you might get too much. Joel Mokyr says that if there are high rates of return, then maybe we're not doing enough. The rates of return might be as high as 40 or 50%.

Joe Martino reports on a working paper he has read comparing government and private expeditions to the arctic. The paper finds that more people died on the public than on the private expeditions; and the private expeditions took most of the prizes.

#### Session 4:

Charles Babbage. "On the Inducements of Individuals to Cultivate Science." Ch. 2 in: Martin Campbell-Kelly, ed., The Works of Charles Babbage. Vol. 7, New York University Press, 1989, pp. 5-20. [ISBN 0-8147-1113-8]

Michael Polanyi. The Republic of Science. Chicago: Roosevelt University, 1962, pp. 5-27 [ISBN not in book/pamphlet]

*Charles Babbage gives a thoughtful argument on the motives of scientists and how the government can effectively encourage science. Michael Polanyi's "The Republic of Science" has been one of the most powerful arguments that scientists have developed effective institutional processes for the advance of science. In an argument quite complementary to scientists, he argues, in part through market analogies, that society is best served if scientists are left free to evaluate themselves through enlightened institutions of peer review.*

John Danford mentions that Michael Polanyi was the brother of Karl Polanyi. Babbage was a fierce enemy of organ grinders. Danford has trouble making out the main point of the Babbage paper.

Virginia Postrel read the Babbage paper as a venting of status frustration. Virginia says that Terence had suggested that maybe this was patronage-seeking; she doesn't see it that way. Babbage *wants* this to be an aristocratic society; he wants the status.

Terence Kealey says that the Babbage piece made Terence angry. Babbage went on to succeed. He wrote in 1831; the next year Faraday discovered electromagnetic induction; the next year Darwin went on the Beagle. Babbage argued for the decline of British science just before its greatest moment. {Actually, one account states that Faraday discovered electromagnetic induction in 1831.}

Gonzalo Munevar says that the Babbage reading is similar to the Bush reading. Abstract truths yield fruit, but often a long time hence, so the market will not step in because it is so long.

John Danford asks whether government is the only source? Gonzalo answers that Pierre Goodrich

wasn't around.

Steve Ostro says that even without prestige, science is done because scientists are driven.

Robin Hanson says that very few are capable of doing the abstract stuff. Usually there is an argument that the patron is special in being able to appreciate it.

Ed Larson says that at Babbage's time Dalton had been turning stuff out privately and Lyall would soon. In Lyall's case, a tolerable lawyer was exchanged for a great scientist.

Steve Postrel says that nationalistic competitiveness was a concern.

Virginia Postrel says that compared with Bacon, Babbage does not present a case for government funding of science. In abstract math, it's about you and a piece of paper. Babbage is concerned with prestige more than money; government is where the prestige comes from.

Gonzalo Munevar responds that you still have to eat. If you have to do something else to eat, you can't do science.

Steve Ostro asks Virginia if the only source of prestige is government? Virginia says 'yes', according to Babbage.

John Moore says that Babbage is concerned with practical issues of efficient operation of scientific institutions.

John Danford mentions that Einstein and Mill did not receive government support. Art Diamond responds that they were working for cushy, quasi-governmental bureaucracies.

Virginia Postrel mentions that the U.S.S.R produced Solzhenitsyn. So does that imply that we should advocate totalitarian regimes?

Joel Mokyr says that higher classes don't much care about science. If a profession is highly regarded, it will be highly remunerated. Babbage's piece is more of a plea to society than to government.

Joe Martino says abstract work doesn't need much money to support it. Einstein may have done his work in the evening--he doesn't know. Gonzalo Munevar responds that in the patent office Einstein had time, but that he did most of his work in cafes.

Joel Mokyr ironically suggests that it was a good thing that there were so few Swiss patents. Terence Kealey mentions that Einstein handled the patenting of chocolate Toblerone.

Terence Kealey claims that in his list on p. 132 {Diamond's numbering}, Babbage omitted Rumford---evidence that this is a profoundly dishonest document. Babbage wanted honor from the crown.

David Levy defends Babbage by noting his involvement in the construction of the log tables. Constructing the log tables was expensive. Babbage was successful in getting funding for his computer stuff. *His* abstract stuff was funded.

John Danford interjected that “abstract” seems to mean “no use now.”

Robin Hanson says that Babbage’s position is different than Polanyi’s on the question of whether the science profession is self-policing. For Babbage, the government must police.

Ed Larson says that Babbage and Tocqueville were over-making their case---they underestimate the aristocratic support for science.

Susan Feigenbaum is struck by a lack of prescription in Babbage for optimal institutions. It is *not* the university where scientists are required to teach; and it is *not* the government. What institutional form is he supporting?

Joel Mokyr says that he hates to disagree with Terence and others, but that science is ebbing in Britain at the time of Babbage. The chemical revolution passes them by. In the 1830’s and 1840’s there is nothing in organic chemistry. None of the important work in chemistry during the period was done in Britain.

Terence Kealey says “false.” Ed Larson is with Terence.

Virginia Postrel says that her comment is relatively trivial. She thinks that Babbage was advocating institutions like the Institute for Advanced Study. Gonzalo Munevar agrees, and adds that he is advocating the sort of support and status that Galileo received.

David Levy thinks that Babbage had problems with all institutions, referring us to Babbage’s “Nineth Bridgewater Treatise.”

John Danford turns our attention to Polanyi, whose piece he likes, though he thinks it is difficult. He likes the treatment of spontaneous order.

Joel Mokyr notes that the piece was written in 1962 before Donald T. Campbell’s influence on self-organization. Many of the ideas in that literature are here---it is a foreshadowing of an important literature.

Steve Ostro begins by noting that it would have been useful to have had the original date appear in the readings list for the Polanyi piece. Steve goes on to say that it is impossible to explain the process of peer review to someone out of science. Polanyi has a great analogy; then he loses touch with things.

Robin Hanson likes the jigsaw analogy. He is not sure it would work---motives might matter. John Danford raises the motives of Watson and Crick.

Steve Ostro says the jigsaw analogy is a bad analogy---too simplistic.

John Moore says that to Polanyi, spontaneous order is good and direction from the center is bad. Polanyi's point is a good one. Should government direct? If so, how?

Robin Hanson suggests that Polanyi's procedure might work for an actual jigsaw puzzle.

David Levy notes that Polanyi worked on self-organizing crystals in his own scientific work. David wonders if there are any Darwinian moves in Polanyi. Polanyi has an analogy between self-organizing in markets and self-organizing in science. You need motivational homogeneity for this argument to go through. If everyone is truth-seeking, then this goes through. What if *not* everyone is truth-seeking?

John Danford interjects to ask what if what they really want is to win the Nobel Prize? David adds the questions, what errors will that add? Do you believe something because the experts believe it? That could result in an information cascade.

John Moore points out the Lysenko was directed by government. In Polanyi's institution, you have mutual criticism.

Ed Larson says that the value of freedom is to make science work. The public wants more than wealth and power. Ed also wants to note that scientists cannot agree on scientific method.

Steve Postrel reminds us that Polanyi is concerned with the issue of authority. He is concerned with an excessively objective approach to knowledge. His point on authority is absolutely correct; not every result will get a hearing. You know a species when you see it; there is a network of authority. There is a middle ground between a static society and revolution---a liberal society. Self-organization stories are part of it.

Gonzalo says that Polanyi has a very British view of a free society---like the view of Terence Kealey and Mill. Tradition determines what are facts. Campbell and Toulmin are only by analogy Darwinian. Polanyi was not saying that he was showing that science was rational.

Joel Mokyr doesn't think the literature wants to equate "Darwinian" with "rational". The Darwinian approach focuses on selection; each society has different selection criteria. E.g., consistent with Darwin, you could have a Markovian chain that is constrained at each moment. You could be a Latourian, or Panglosian, depending on the selection criteria.

Gonzalo Munevar responds to Mokyr that not all selection mechanisms are Darwinian. To which Mokyr responds to Gonzalo that he will send Gonzalo the first chapter of his new book.

Joe Martino says that Polanyi has scientists immersed in a network of scientists. A scientist selects a project worth doing and capable of being done at the time. Suppose you do it, and it's wrong? In most cases, other scientists don't check it until we use it, and it doesn't agree with what we find. E.g., Piltdown man.

Steve Postrel says that we learned from the Double Helix that the textbooks were all wrong.

John Moore says that economics is the only profession where you can be wrong your whole life and never pay a price. John Danford says that's true of political theorists, too.

Virginia Postrel says that in Polanyi you have order without design, through variation and selection. We are striving towards a hidden reality. Steve Ostro says "it's an onion." Joel Mokyr asks if we are working our way in? Steve Ostro says we are working our way out. Steve Postrel asks, but does it make you cry?

Robin Hanson asks where in the puzzle should we work? John Moore asks the related question, when you think of public support, how do you decide which areas receive how much support?

Terence Kealey raises the question of authority. Can government fund satisfactorily? Can you put in a grant application for painting Guernica? {Picasso's disturbing anti-war mural} Early scientists were fighting authority. Now, there are not so many great scientists.

Ed Larson says that Polanyi argues from experience that society must cultivate science on its own terms, if science is to make any progress. Does Polanyi overstate his case?

Stan Liebowitz is having trouble with the analogy of the invisible hand. Polanyi's story is incomplete. What are scientists maximizing? Truth? This is different from the Smith thesis, because in Smith, the invisible hand is maximizing the well-being of consumers.

Steve Ostro asks if the republic of science is ruled from top to bottom? And he asks how can science be undermined? By an authoritarian regime? By bad peer-reviewing? But these problems are self-correcting. What if we have science by press-release, or by internet? In either case, scientists will be pandering to public opinion. But science will be self-correcting on some time scale that may be long.

Joe Martino says that articles in Science have been identified with significant error within a couple of weeks.

Susan Feigenbaum asks what is the time scale for self-selection? Incentives are affected by the degree of competition.

Steve Postrel says that at the end of their careers, many scientists criticize peer review. Peer review could have type one and type two error. {One of these is to accept a falsehood as being true; and the other is to reject a truth as being false.}

Ed Larson reports that a typo was added by the journal Nature, was pointed out to them, and was never corrected.

Joel Mokyr says that we are struggling with the idea of self-selecting systems. Are they stable? What will the results be like? Darwin owed more to Smith than to Malthus. To what extent is science a self-equilibrating mechanism? The answer is unknowable, because we don't know what science would come out of a different system. We only get one observation. Though maybe China is another observation.

David Levy responds that we can learn by trying experiments, e.g., require the publication of data to encourage replication. Make small changes and see if it makes a difference.

Susan Feigenbaum says that you could look at work that is supported by foundations versus work that is supported by the government. Mokyr responds that these are not independent of each other; they feed into each other. Gonzalo Munevar suggests that we could make inter-country comparisons, e.g., French science collapsed due to central authority.

Joel Mokyr claims that for the last 100 years there has been only Western science, not French, British, etc. Gonzalo replies that there have been fewer great French scientists.

John Moore goes back to Polanyi's support for decentralization. What is the source of authority over scientists? Should the National Science Board be independent of government control? Polanyi makes the argument from tradition, but this point must be continually argued.

Virginia Postrel is concerned that toward the very end of the piece, that in advocating a liberal order, Polanyi has added to the confusion between positive and negative liberties. You need freedom of speech to be able to criticize. On the other hand, you don't want to be in a Tom Paine world; you don't want hippies.

John Moore is reminded of Schumpeter's question: can capitalism survive?

Gonzalo worries that we need to take the word of experts all of the time, and citizens have no way to judge. The consensus chains that are Robin's worry, sound to Gonzalo like gossip. This is worrisome.

David Levy asks how long it takes for science to self-correct? There has been a hellacious debate on C.P. Snow's two cultures. What if someone claims that some discipline is *not* a science? What would be our reaction? Disciplines seem self-perpetuating. You have to replicate; check it out.

Joe Martino is dubious about Polanyi's analogy. Science is *not* self-equilibrating. It may be self-correcting. How can you ever tell what the optimum is? On different standards: different scientists from different disciplines have different standards.

Joel Mokyr suggests that the historical record has been one where in the past there have been non-optimal equilibria, e.g., phlogiston. It takes a *very* long time for self-correction. There is no evidence that *we're* any smarter.

Ed Larson asks how much we scientists are looking for an underlying reality? Science is self-correcting because it is seeking an underlying reality. Gonzalo Munevar says that that is not in Polanyi. Gonzalo also suggest that scientists have a monopoly on practice too, as in the past restrictions on the practice of acupuncture. John Danford asks Gonzalo: is there no reality in Polanyi? Gonzalo responds that what really counts in Polanyi is community.

John Moore asks whether "truth" is whatever survives scrutiny? Gonzalo responds that Polanyi was

quite a relativist.

Joe Martino says that scientists assume that there is a knowable order. Experimental science is based on the post hoc, propter hoc fallacy. John Moore responds that Popper gave an alternative account.

Virginia Postrel says that the notion of a hidden truth is important here. How is this like, and not like, a market? In a market, there are many motives. Sorting in science has something to do with hidden reality.

## Session 5:

William Warren Bartley, III. "The Entrenchment of False Philosophies." Ch. 8 in: Unfathomed Knowledge, Unmeasured Wealth: On Universities and the Wealth of Nations. LaSalle, IL: Open Court, 1990, pp. 117-149 [ISBN 0-8126-9106-7]

*Bartley argues that scientists have no different motives from others, and that academic institutions encourage conformity and lethargy. His remedy is to recommend a true marketplace of ideas.*

John Danford suggests that when Bartley's paper was written, in 1990, universities were in a period of depression. Virginia Postrel responds that the examples and evidence that Bartley uses come mainly from the early 1970's.

Joel Mokyr asks what the output of universities is? John Danford answers that it is the generation of new knowledge. Virginia Postrel responds that the answer is more concrete: Bartley argues that scholars do not publish anything. She finds it reprehensible that Bartley uses such old data. Joel Mokyr adds that counting articles is an absurd way to measure productivity. Robin Hanson mentions that counting citations is better. Mokyr says back that the number of citations doesn't matter---only the opinions of the few best.

Terence Kealey reports the finding that there is a high correlation between the quality and quantity of research output.

Robin Hanson describes Bartley's piece as "an inexcusable rant." Ed Larson describes it as a very provocative piece, if not very good.

Art Diamond defended the piece as being courageous, and true in its central message that academic institutions often permit the entrenchment of error. Diamond also suggested that some of Bartley's suggestions for improvement, such as the abolition of academic tenure, are worth discussion.

John Danford steps in to second the notion that the humanities, at least, are in terrible shape. Gonzalo agrees that the universities are in a "fascist slump." But Gonzalo thinks that Bartley's pot

shots don't really work---there isn't any substance to his attacks.

David Levy says that Bartley's history of economic thought is dreadful. One case *is* worth reflecting on: U.S. universities behaved terribly when neither Ludwig von Mises, nor Hayek could get jobs in the United States. Hayek got a job in Chicago with the Committee on Social Thought, but without a salary. These were peers of Keynes and Schumpeter, but they couldn't get jobs in universities. You need lots of kinds of institutional support because each has blind spots.

Steve Postrel suggests that you need historical perspective to know when you've been in a slump. Theodore Sturgeon once said that 'It's true that 90% of science fiction is crap, but 90% of everything is crap.' So people like Bartley can always find evidence.

Steve Ostro discusses peer review of manned flight research. Projects are ranked and only the top 5% make it.

Stan Liebowitz suggests that students like business, but not economics. If we believe in markets, then maybe there's a problem with economics. There are no citations to a lot of research in economics.

Susan Feigenbaum says that there is something good in the article---the notion of intellectual cartels. How do new schools of thought win? How do they capture the funding agencies. David Levy adds that Liberty Fund helps to induce competition into intellectual cartels.

John Moore mentions that in the NSF there is a bias towards quantitative work---and this effects economics.

Virginia Postrel refers to Bartley's position that the competition to universities is often from non-university institutions. She refers to Bartley's quote of Lester Thurow saying that some universities should be eliminated. Exceptions of the competition coming from within academia are the Public Choice and Chicago schools in economics. Virginia believes that cartels can arise from state, or non-state sources. She says that Bartley hates the university so much.

Terence Kealey asks if it is possible that what Bartley says is true? Everyone wrote hundreds of papers, and yet you have a slump? Terence is sympathetic to Bartley.

Joe Martino asks how would we know if sociology and economics are sciences?

Joel Mokyr asks how would we know we're in a slump? He says to Danford that post-modernism is a market solution. The outcome is simply something that Danford does not like. We have a system that is sufficiently open, if you can persuade people. No one is teaching general equilibrium.

Robin Hanson says that we're asking the wrong folk if the system is working.

Art Diamond suggests that given Ostro's report that there's a lot of really good stuff in astronomy being rejected, the astronomers have an incentive to effectively evaluate themselves, to increase their overall funding.

Steven Postrel says that there has to be an authority. Let them publish, just give them less weight. Astronomers who can't get time on the telescope are a more difficult issue. If allocation of scientific resources is a political process, then there's no invisible hand.

Gonzalo Munevar says that in the humanities you can show a slump---they still do the same stuff because of inertia. Post-modernism won the day through intimidation, not through meeting a market test.

John Moore believes that there are standards of validity in the arts and humanities, but they are not testable in the way that physics is. What is this market? How do we translate from the market for goods into the market for ideas? For instance, who's doing the demanding in the market for ideas? And how does this translate into hiring decisions?

Steve Postrel says that assistant professors need to be responsive to academic demands. John Moore says the process works differently than as presented in the Republic of Science.

Joel Mokyr agrees that Steve Postrel is right that the market for ideas is not perfect. But it is not a total failure. Post-modernism has persuaded. Ottoman science never came up with autonomous universities. While English universities were in a slump, you had Scottish universities.

Gonzalo counters that when teaching Shakespeare gets you fired, that's not 'winning in the marketplace.' {I believe that he gave an example he knew of where this happened.}

Stan Liebowitz says that there's always some politics and intimidation in academics. A colleague of his who had been in a concentration camp says that the only thing that comes close in her life to that experience, is her current experience in the university. Stan's daughter wants to study literature. It's hard to find universities that will tell her what she will find in their programs. We need truth in advertising. John Moore responds that the Intercollegiate Studies Institute has a guide that would be useful.

Virginia believes that post-modernism is very large and very diverse. Not all of them use intimidation. Some strains are critiques that are useful---there are some useful history and cultural studies. Virginia also comments on Steve Ostro's description of the grant selection process in his field. The process has been going on with very limited funds; people know that they have a very small chance of being selected. The selection occurs from among the scientists who think that they might have a shot. This is a very unrepresentative selection process. Steve Ostro responds that his point was to give a quick counterexample to Sturgeon's law {that Steve Postrel had mentioned earlier}.

Gonzalo Munevar asks Virginia Postrel how did post-modernism win? Virginia responds that it won for different reasons in different places.

Joel Mokyr believes there have been cases of abuse of power by large corporations. He's been part of intimidation within his own department when he joins his colleagues in rejecting 'Marxist claptrap.' The question is whether in the long run we keep good stuff out?

Ed Larson asks how you evaluate the institutions that you spend money on? At his university, every department has been asked to come up with its own measures by which that department will be evaluated. His university is “free” to students. The business school is using success at finding students jobs as a measure. Others are using the number of grants received. It’s a real puzzle.

David Levy offers to bet Ed 25 cents that each department picks the measures that the department looks best at. We should do some theory here. There are an enormous number of citations generated by only a few scholars. OK, how do we get a scientific opinion on a candidate or an article? You ask the highly cited scientists. Might this be a generating mechanism for a cartel? All you need is Samuelson and a few others, and you have a cartel. Stan adds that all you would need for a cartel would be Samuelson and Friedman.

Robin Hanson wants to respond to Mokyr’s position. He asks Mokyr to look at music, and asks whether he would want to claim that music popularity is a guide to truth in music? Stan Liebowitz adds, what about the popularity of astrology?

John Danford responds that post-modernists deny that there is truth.

Virginia says it doesn’t matter what you teach lawyers, they just want to get rich. A lot of folk in the university are not tenured. But some people, mostly tenured faculty, are fighting back. She’s on a board that is an example. The university is an evolved institution.

Gonzalo Munevar says that the traditional academic justification for the tenure process was that you need a pre-tenure period for faculty to prove their competence. Once proven, the faculty were on their own. They had academic freedom and were untouchable. John Moore says that that’s the theory---but two future Nobel Laureates were forced out.

Terence Kealey says he tried to think of an intellectual cartel and came up with racist eugenics. Cold Spring Harbor was created as an eugenics lab. How did the universities cleanse themselves of this? They did not---the cleansing was all *external*. Some departments were closed down. It took WW2 to cleanse them.

Joel Mokyr agrees with Terence that universities should never be allowed to be monopolies of knowledge. Terence’s own work demonstrates this. The question is whether *on balance* western societies would have been better off without these institutions. There have been many bad episodes. But he goes back to David Levy’s multiple institutions; checks and balances.

Stan Liebowitz mentions that universities colluded on scholarships for a long time. If they could do that for so long, maybe they could maintain other cartels for a long time too.

David Levy mentions Galton and Pearson’s work on race sterilization. He says that Hitler made anti-Semitism impossible for at least two generations. What kind of feedback mechanisms were required for Auschwitz to work? It would be really interesting to study the external constraints.

Virginia Postrel mentions that desegregation is another example of the importance of competing

institutions.

Terence Kealey claims that no Catholic country practiced eugenics because the Catholics distrusted science. David Levy proposes a narrower explanation: Catholics weren't neo-Malthusian, so they opposed eugenics.

John Danford suggests that we return to funding issues. He raises the \$20 million grant that Yale returned to its donor.

John Moore explains that the donor wanted to control hiring. Joel Mokyr says that universities will turn down money where a specific person is named to a position. John Danford responds that that is just a pretense.

Virginia Postrel says that universities are not obligated to take money.

John Moore points out that Bartley believed that the intellectual cartels had infiltrated the government, and he *agrees*.

Gonzalo Munevar suggests that there is a diversity of funding sources. But Susan Feigenbaum suggests that foundations become risk averse as they age.

Virginia Postrel wants to go back to Babbage. Receiving foundation money does not solve the problem of prestige. You can't do your research at Yale.

Gonzalo Munevar responds that if you want to do research on a new way to generate electricity and you succeed, then you're in. And Mokyr adds that you'll have an office at MIT.

Stan Liebowitz suggests that we have both public and private universities. The fact of the private ones may explain why we have a better system.

David Levy mentions the Anderson et al Journal of Money, Credit and Banking paper showing how difficult it is to replicate other scientists' work. The NSF program officers paid for this.

Steve Postrel believes that one problem is obscurantism in style. Plenty of bad stuff is dressed up in bad language. Often editors impose this. Should the ordinary man be able to read all science?

John Danford mentions the Alan Sokol piece {that satirically and controversially imitated the neo-modern style so successfully, that it had convinced the neo-moderns}. Try reading Derida.

Steve Ostro reports that there is incredible pressure on scientists to popularize what they do.

Steve Postrel says that Jared Diamond has said he can't read relevant Science articles because the whole articles consist of nouns.

Virginia Postrel notices that we have stumbled on a justification for government funding of science. The Federal Reserve Banks make their research popularly accessible. Ostro is made to make his

work accessible too. The general citizen can't read chemistry articles.

Robin Hanson believes that obscurity peaks with the Ph.D. and falls afterwards.

John Moore believes that the public supports scientific research at the same time that they believe the sun revolves around the earth.

Ed Larson says that private foundation research grant proposals are also very clearly written. Susan says the cloning literature is clearly written.

Terence Kealey says the public understanding of history is also very low in spite of clear writing.

Gonzalo suggests that philosophers of science are obscure.

Joel Mokyr closes the session by asking that we not over-generalize---the best writing in economics is generally done by the best economists.

## **Session 6:**

Terence Kealey. "Dr Pangloss was Right." Ch. 12 in: The Economic Laws of Scientific Research. New York: St. Martin's Press, 1996, pp. 303-345. [ISBN 0-312-12847-9]

*Kealey's book presents an erudite, wide-ranging, controversial and witty case for ending government funding of science. He argues that private industry provides substantial and effective support for science, and that, in any event, much important technological progress is not due to progress in science.*

John Danford begins the session by noting that sometimes at Liberty Fund colloquia, there are not readings for the final session. Even though we have a reading, he wants us to feel free to talk about anything.

Joel Mokyr, perhaps with a tiny note of irony, says that he finds Terence charming; he's convinced.

John Danford cites Mokyr's writings as having taught us that unpredictability is a key feature of technological progress. There is no evidence of diminishing returns. Terence Kealey's reading says we should "relax." But if there is a major lesson in Mokyr's work, it is that we should not take technology for granted. Danford raises another question related to the decline of culture---what are the effects of science without moral structure?

Ed Larson points out that after saying to relax, Kealey goes on to give preconditions for relaxing, such as sensible commercial legislation.

Joe Martino wants to discuss the phrase “technological progress.” Many view “progress” as terrible.

Mokyr says there are two dangers: those who dislike science, and vested technological interests. For the second danger, the state may be an arbiter. In every society, the foundations of technological progress are shaky.

Steve Ostro says that we seem to be moving toward a society where everything is the right of everybody. He asks a question of Terence: have you really met so many despicable scientists?

Terence responds “yes,” he’s met many. Joe Martino adds that scientists on average are no worse and no better than others. Terence disagrees, asserting that scientists on average are less nice than the average person.

Virginia Postrel plugs her article on SAT tests in tomorrow’s L.A. Times. She says that the sort of society that is flourishing in markets, skeptical and materialistic, undermines the conditions for markets.

John Danford asks whether capitalism is responsible for the Columbine school shootings?

Art Diamond mentions Schumpeter’s argument that capitalism sows the seeds of its own destruction.

Joe Martino says that one of the things governments do is to freeze technology, e.g., in the past railroads, currently cable TV. They freeze to protect the current interests.

Gonzalo Munevar is concerned with a couple of aspects of Terence Kealey’s position on Smith versus Bacon. Gonzalo is not fully convinced. The evidence is that several times science has just made it by the skin of its teeth. Even if so, it’s just an argument for the *survival* of science. Bush was arguing for *furthering the success* of science. There are cases where the government should support science; where the science is the sort of thing that otherwise won’t be supported.

John Moore says that there are real dangers in some sorts of technical change. Genetic engineering will modify human characteristics. What this drives home is the importance of the moral framework. In the face of real danger, people will ask for government regulations.

Jennifer Thompson adds that the sort of people being trained to regulate these technologies are those without the tools to do so. Susan Feigenbaum suggests that with advance reproductive tools, the problem is incomplete property rights.

David Levy warns that we need to be careful: those who bear the brunt of creative destruction will organize against it. Public choice theory is relevant. One question is: why did the Reagan administration send a lot of money to M.I.T.? The answer is that M.I.T. folk are competent.

Terence Kealey points out that legislatures can be capricious---birth control pills were illegal for many years.

Art Diamond claims that successful human cloning would increase freedom.

Susan Feigenbaum says that advanced reproductive technologies have received bad P.R. because of some of the resulting families with seven children where the parents are unable to take care of the children.

Virginia Postrel believes that what people worry about has nothing to do with the technology.

Gonzalo Munevar says that creating human monsters is a possibility.

John Moore worries that the political consequences from the reactions to the new technologies may be bad.

Steve Postrel believes that the funds will flow to the new reproductive technologies, because there is a demand for them. The issue is what will the regulatory conditions be? Other technologies have been skewered, e.g., the radiation of food. Often folk see regulation as a shelter; regulatory agencies could speed up the rate of technological adoption.

Art Diamond mentions that Monsanto wanted the government to become involved in genetically modified crops in order to provide the sort of assurance that Steve is discussing. Steve adds that at first Monsanto had said that the government should stay out.

Ed Larson believes that the banning of genetically modified food is a protectionist cover to keep American food out of the European markets. There is not a real threat from this area. Some opposition to genetic modification comes out of religious regions---people would so much want to enhance their children. Europe bans organ sale; so Europeans go to India. We're so far into this revolution that the benefits are not going to be overturned.

Joel Mokyr states that Art Diamond made us read the wrong chapter from the Kealey book; there are better chapters. He believes that many of Kealey's objections to government funding also apply to private funding. At Mokyr's university one problem is that they are building too many buildings because that is what donors want to contribute for. Donors do not donate for janitors or for the heating bill. Private and government funding complement each other.

David Levy reports that some private foundations behave badly. As an example, he mentions that the Rockefeller Foundation said to the University of Virginia that they needed to get rid of Buchanan and Tullock or else lose Rockefeller Foundation funding. Joel Mokyr adds that some foundations won't fund scientists over age 45.

Steve Ostro says he used to read science fiction while in graduate school, but now he doesn't need science fiction since the exciting stuff is what he's doing. He mentions that one idea that used to be science fiction was to have a tiny chip that can be attached to anything to track it. The

chips could be ingested, and some versions might perform medical functions. This is now close to reality and some possibilities of it are very scary.

Gonzalo says that people sometimes make bad choices.

Joe Martino says that regulation tends to retard technological advance, although sometimes this is not the case, as with airlines.

Robin Hanson wants to return to the fundamental question: should government finance science?

Terence Kealey says that America is different. We lose the role of government as a referee. Terence also adds that science can deaden one's finer sensibilities.

Virginia Postrel responds to Mokyr that there are principle-agent problems in private giving to universities. People often want to give money for new things. Giving money for a special-purpose building may increase the chances that the money will be spent in the direction that the donor prefers.

John Moore asks what science we are referring to when we say that science is self-correcting, but not self-equilibrating? He takes it that mainly we're talking about basic science. What agencies make the best decisions? Different agencies have different biases.

Steve Ostro asks if the government stopped funding genetic research in 2002, what would be the results? Steve Postrel answers that it might be sort of like the case of SETI. When government pulled the plug, private funding came in. The government always has a computer involved, so they can say they were involved if life is found.

Joel Mokyr says that the issue is more than one of crowding out; the issue is where the money goes.

Steve Ostro says that some of NASA has enormous public support.

Joe Martino discusses the optimal amount of funding. If you have too much funding, you may have to dig too deep into the pool of scientific talent. If money is the constraint, we can't have too much. Ed Larson adds that scientists are so much underpaid.

Stan Liebowitz responds to Joel Mokyr that Mokyr's argument for pluralism of funding institutions is based on the premise that each type of institution has some degree of homogeneity. Stan believes that private support of science is very much more heterogeneous.

Terence Kealey says that millionaires would love to sponsor Mars missions. If you're drowning off the coast of Britain, you're rescued by a *voluntary* organization. Either Hewlett or Packard left 4.5 billion to science a few days ago.

John Moore says that if you look at time series for government R&D, you don't see crowding out.

Stan Liebowitz suggest that Joel Mokyr's point must be that there must be some point where you don't want more basic research.

Gonzalo Munevar says that you want to fund *unusual* research; that's where most advances come from.

David Levy asks if there is a market failure? The status quo involves significant government funding. Is there a government failure that suggests we should change the status quo? There is not much controversy; the current government funding is fairly popular; how unhappy are we?

Virginia Postrel asks what the current pathologies are? The politically high profile gets funded. Mike Milken gets prostate cancer, so now there is money for it. U.S. pluralism corrects many pathologies. Do people feel that they're getting their money's worth? It's not clear.

Robin Hanson makes a theoretical observation related to Joe Martino's most recent comment. Do the huge differences in the productivity of scientists mean that much of the money for funding science goes into bidding wars to hire the best scientists?

Joe Martino says that government funding has corrupted the scientific establishment; that by itself is a good enough reason for stopping it. If the investment in science pays for itself, why does the government need to do it?

Terence Kealey says that in 1890 the U.S. was the richest country in the world; it's an amazing story. Three fourths of funding for science in 1940 was private. One half of funding for science in 1960 was private. You panic in 1967, and the share of government increases substantially.

John Moore says the main issue is a little hard to settle with data.

Joel Mokyr asks what will happen if we cut off government funding of science? He answers that if we cut it off, the money will go to some other pig in the trough.