Lecture 4 – October 2nd, 2017

Topics for today
Dangerous Knowledge
The Rise of Big Science

Previously…

- Up to this point: because of inductive risk, the value free ideal is dead.
    - Because of the consequences of making errors…
    - Scientists in the process of doing science, if they are doing science properly, they should not have their own values involved
    - Because science is supposed to be sole fact
    - Values are those contained within individuals…
- Non-epistemic values are omnipresent and unavoidable in scientific practice.
    - There is no way to get them out of scientific inquiry
- *Is there still some sense in which science can be objective?*
    - *Even though there are epistemic roles in scientific inquiry*
    - *Can it still give us objective knowledge on the world that we can respect and take as true more so than in any other way???*
- *How should we organize science to best address the presence of non-epistemic values?*
    - *What measures can we impose on the way science is conducted to accommodate for that non-epistemic effect*

Dangerous Knowledge

- Knowledge that is not clear
- Because of the potential consequence of generating the knowledge itself…
- It is not clear whether we should engage in the research itself
- Question: Should we always pursue knowledge even if that knowledge could have dangerous consequences?
- You might think that the role of scientists is to go out in the world and figure out what they can figure out, but for the way science is situated in society, it has consequences
- The decisions that scientists make can have consequences to the citizens' lives of society
- Is it morally acceptable for scientists to peruse the knowledge they are trying to develop?

Slide 1

- Image: Will Large Hadron Collider destroy Earth? CERN admits experiments could create black holes
    - Author: Jon Austin
- CERN – large particle physics experiment
- Conducted in France and Switzerland
- Professor Miller used to work here (2008-2010)
- During that time, it was when the Hadron collider was about to turn on (for the first time)
- A new energy regime in hydrogen physics
- Exploring the unexplored territory in particle physics
- In time, it was getting closer to the beam being turned on
- But then there were protestors on the street
- They thought it was going to destroy the earth and universe
- Smashing particle with high energy would create black holes
- Black holes would suck up matter around and swallow the whole universe
- If you believe that would be consequence, you wouldn't want scientists not to research the question!
- Knowledge that is dangerous – so don't try to get it
- But nothing happened in this case, so don't worry
- This silly example motivates the idea that there are consequences about getting certain knowledge
- We might have second thoughts when trying to obtain knowledge
- Collecting knowledge may not be the greatest idea in certain cases

Slide 2

- Lakoff explains an example of where developing new knowledge may be dangerous
- If to "dare to know" is to undertake inherently risky experiments or to disseminate truths which may be misused, would it be wiser to resist the impulse to satisfy the cravings of curiosity when there is reason to fear that serious harm could result?
- A common thought amongst scientists:
    - Task of judging whether or not something is too risky to be used, is really the task of those who *apply* the science
    - Since most of the time people developing the application aren't scientists themselves, maybe all the moral responsible for deciding which research should be conducted falls upon the shoulders on those applying the science rather than those that are doing the science itself

Slide 3

- A thought: maybe those who apply science are morally responsible, but not the scientists themselves.

Slide 4

- Lakoff: fundamental/basic research is increasingly a direct source of technology.
  - People are doing basic science such as lab science (science that reveals the laws of nature) in the effort to develop knowledge precisely for technological application
  - The gap between basic research and technological output is too thin to really take the moral responsibilities off the shoulders of scientists doing the basic research
  - If there are bad consequences of that technological output, the people doing the basic research have it in their should have the ability to know this
- Moreover, scientists ask for funding on the basis that it will lead to new technologies.
  - Many labs motivate the need for the kind of basic research they are doing by advocating that it is going to have a particular technological output
  - Example: I want to write a grant that would allow me to study rat livers, people won't be motivated to fund your research because there are other things important in the world
  - Studies of rat liver get funded all the time though because people doing that research say we will understand cancer in humans better (things we care about more)

Slide 5

- The Manhattan Project: basic research with enormous practical consequences.
  - Big example
  - Basic research level
  - A complicated scientific project many of the best physicists of the 20th century
  - Professors from universities were summoned by governments to leave university and come work in the labs
  - Were told there were making a nuclear weapon
  - The people that were high up knew what they were working on
  - But others didn't have so much of a clear idea
  - They were told little bits of the problem…
  - Didn't know the large scale aim of the project
  - Project lasted several year and was successful
  - Managed to create a runway nuclear chain reaction - successful as an atomic nuclear weapon
  - Had enormous practical consequences (not only the consequence of making that weapon – which was used with catastrophic effects)
  - Profound effect on the way that economic and political dynamics have unfolded…
  - In 1917-1918, no one had an idea on what the outcome would be of this project
  - But that was the outcome of haven developed this knowledge
  - Basic research (even if you think has no practical application), it could have enormous practical application and consequences which will influence the course of human history and centuries to come

Slide 6

- "The key question is whether, or under what circumstances, it is morally justifiable to forbid certain types of research, either because the experiments are inherently dangerous or because the findings may be put to harmful use. Related to this central issue is the question of whether it is possible to develop universal criteria that may be applied in making the judgment to restrict research. Finally, there is the question of the process to be used in arriving at the judgment."

Slide 7

- Conditions?
  - Under what conditions should we think about restricting research?
  - Let's have a look at the Manhattan Project
  - Their aim was to develop an atomic nuclear weapon
  - It is plausible that such research would want to be restricted
  - But what lead to this development of the project and knowledge?
  - It is not clear…
- Can they be universally applied?
  - Whether those conditions on constraining make sense for all cases that may have potential development of dangerous knowledge
- How can they be applied?
  - Hard problem

Slide 8

- "Theoretically, at least, a better criterion is the calculation of risks and benefits: presumably a line of research which offers more benefits than risks is one that would be generally supported."
  - Lakoff argued this
  - More benefits than risks?
  - This would be supported
  - But this is very vague

Slide 9

- Consequentialism: the potential consequences of your actions are what determine whether or not your actions are morally acceptable.
  - Taking ethics into consideration (wide array of ethical positions)

- Funding particular research – what do consequentialists use?
- Can we come up with rational conditions?
  - When to fund, when to not fun?

Slide 10

- *A case: the discovery of penicillin…*
- Fleming was working on the influenza virus and left his lab for a vacation. He returned to cultures contaminated with mold, around which the cultures could not grow.
- Wasn't trying to discover penicillin
- Wanted to do research in influenza virus
- By accident, he found penicillin
- What were the practical consequences of this research?
  - Was able to treat infections
  - Antibiotic resistant strains
    - Harmful infection than was ever possible before penicillin was discovered
    - Should the scientist be held morally accountable?
- Could Fleming reasonably be expected to have anticipated them?
  - He wasn't even trying to find an antibiotic so why impose any sort of moral baggage upon him?
  - If they do the research and they had no idea that something is to be expected, and then all of a sudden something happens… it does not seem right to blame them

Slide 11

- "The trouble with this criterion, however, is that it is sometimes virtually impossible to know precisely what the risks and benefits are."
  - Lakoff said this…
  - So, if you want to compare the risks and benefits of some research areas (criteria for scientific inquiry that is morally acceptable) and use that as a means to know what to fund
  - But sometimes we just do not know what the potential consequences of our actions might be
  - If you want to rule out negative outcome, you want to make sure you don't want to do research that has that (and where positive outweighs negative)
  - Suggestion: stop generating knowledge altogether
    - Throwing out situations where knowledge imposes more positive outcomes
    - But also throwing away the ones with more negative outcomes
  - How do we prioritize what research to do under these circumstances?

Slide 12

- On this view of moral action, even what counts as ethical behavior is subject to uncertainty.
  - What it means to be morally right or wrong is subject to uncertainties that we do not know how to control
  - This is getting scary
  - We don't know to with certainty
    - When to accept/reject hypothesis
    - How to adjudicate between particular hypothesis without using non-epistemic values
    - What counts as moral/ethical behaviour in the context of the practice of science
- Does that mean that anything goes?
  - Intuitive answer: no, otherwise deep trouble
  - We want to be able to trust the information that science give to us about the world

The Rise of Big Science

- How do we organize funding, how do we use statistics
- In a way which guards us against the kinds of uncertainties that are built in science
- We are going to look at how this Manhattan project made us organize science
- The kinds of research that got conducted and the distribution of funding changed after WWII
- To control the way that values function in science

Slide 1

- Before WWII research was funded **privately**…
- Source of money for setting up a lab was from…
  - University endowments (private money)
  - Private industry
  - Wealthy benefactors
- If you wanted to do an experiment, you would have to go to them
- This was how it was up until WWII

Slide 2

- WWII
  - 50-80 million casualties (~3% world population in 1940)
  - Over 100,000 killed by the use of atomic weapons (radiation poisoning too)
  - ~40.000 Canadian casualties

- This world event had serious impact on people's psychological state
- Rethought the way the world should be organized…

Slide 3

- Scientists were heavily involved in the war effort.
    - Manhattan Project (work on atomic bombs)
    - Radar technology
    - Disease research (infections in soldiers)

Slide 4

- *Science the endless frontier… (required reading)*
- Report commissioned by President Roosevelt following WWII
- What lessons should be taken from the role that scientific research played in the war effort?
    - Had dramatic effects
    - It ended the war (due to the weapons being used)
    - It also cured/prevented diseases…
- How should they be applied going forward?

Slide 5

- *Vannevar Bush… (invited by Roosevelt to do the report with him)*
- American Engineer
- Director of Office of Scientific Research and Development during WWII
- He was in a position to oversee the scientific war effort
- Roosevelt asked for four things he wanted advice about

Slide 6

- One: What can be done, consistent with military security, ..., to make known to the world as soon as possible the contributions which have been made during our war effort to scientific knowledge?
    - What knowledge was generated during the war that we release to the world (without causing harm)?
- The diffusion of such knowledge should help us stimulate new enterprises, provide jobs for our returning servicemen and other workers, and make possible great strides for the improvement of the national well-being.
    - Role expected of science is not to generate knowledge only but make new business, provide jobs, great improvement (national well-being)
    - May have been different of what was conceptualized before the war

Slide 7

- Two: With particular reference to the war of science against disease, what can be done now to organize a program for continuing in the future the work which has been done in medicine and related science?
- The fact that the annual deaths in this country from one or two diseases alone are far in excess of the total number of lives lost by us in battle during this war should make us conscious of the duty we owe future generations.
    - He is saying res, we have lost lives in war, but one or two diseases killed way more people
    - Duty that we use what we learned from the war to try attack those disease so they don't have much of an effect on society

Slide 8

- Three: What can the Government do now and in the future to aid research activities by public and private organizations?
- The proper role of public and private research, and their interrelation, should be carefully considered.
    - Should we rethink whether pubic tax money should be used to fund research because public tax money was used during the war efforts (had a great effect too)
    - Remember: Before WWII – private research

Slide 9

- Four: Can an effective program be proposed for discovering and developing scientific talent in American youth so that the continuing future of scientific research in this country may be assured on a level comparable to what has been done during the war?
    - Not just an issue about funding, but Roosevelt was interested in developing an educated youth population that is capable of generating new scientific research going forward
    - Why? A lot of the young population was lost during the war…
    - Who was going to be young people to do science?

Slide 10

- *Bush: Societal significance of science… (reply to Roosevelt)*
- Discovery of penicillin "incalculable suffering which its use has prevented"
    - Note: Expectations that are being placed on science now is different from just knowledge being generated; now talking about having job, national well-being, relief of suffering
    - Not just about figuring what is true but making life within society better
- Radar in the war effort

- "more jobs, higher wages, shorter hours, more abundant crops, more leisure for recreation, for study, for learning how to live without the deadening drudgery which has been the burden of the common man for ages past."
- "We have no national policy for science."
  - What would it be like to operate in a society like this?
  - US vs. Canada
- "... we are entering a period when science needs and deserves increased support from public funds."

Two results of this report
- There does *enter* a national policy for science in the US
- Whole structure of the funding of science changed

Slide 11
- Basic/applied research distinction

Basic vs. applied research
  - After WWII, explosion of these terms being used
  - What counts as basic/applied research?

Slide 12
- *The war against disease… bush notes…*
- The death rate for all diseases in the Army ... has been reduced from 14.1 per thousand in the last war to 0.6 per thousand in this war (huge reduction ~ due to things like penicillin)
- The striking advances in medicine during the war have been possible only because we had a large backlog of scientific data accumulated through basic research in many scientific fields in the years before the war.
  - We just got "lucky" according to bush's mentality that we have the basic groundwork for getting relative applied research (the hard basic science was done already)
  - He advocates a priority should be put on basic research – research that is not oriented towards technological application
  - Having the basic knowledge of the quantum mechanics, made it possible to develop the atomic bomb

Slide 13
- An observation (in 1945): ~7 million persons in the US were mentally ill.
- This costs the government $175 million/year
- "Notwithstanding great progress in prolonging the span of life and in relief of suffering, much illness remains for which adequate means of prevention and cure are not yet known."

Slide 14
- "It is clear that if we are to maintain the progress in medicine which has marked the last 25 years, the Government should extend financial support to basic medical research in the medical schools and in the universities..."
  - How funding should be prioritized? How the structure of funding changes?

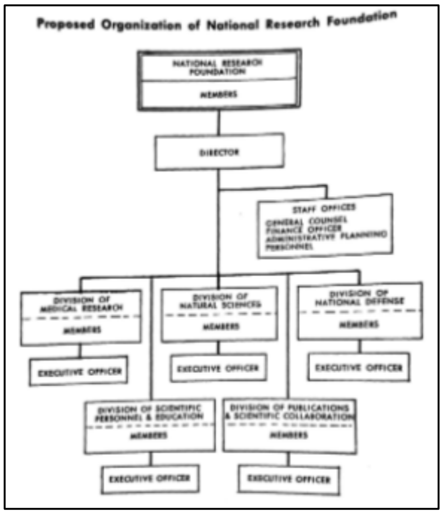
Slide 15
- *Science and the public welfare…*
- "Military preparedness requires a permanent independent, civilian controlled organization, having close liaison with the Army and Navy, but with funding directly from Congress and with the clear power to initiate military research which will supplement and strengthen that carried on directly under the control of the Army and Navy. "
  - What is the result of this?

Slide 16
- *Science and the public welfare…*
- "More and better scientific research is essential to the achievement of our goal of full employment."
- "A nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill."
  - Taxation should change also, in order to achieve goal of ways in funding

- "The internal revenue code should be amended to remove present uncertainties in regard to the deductibility of research and development expenditures as current charges against net income."

Slide 17

- How can the desired benefits be achieved?



There should be a national research foundation containing members…
There should be director…
Three group division: division of medical research, natural science and national defense…
Bush said there was no structure such as this that said who got money and for what…

Slide 18

- *US research funding…*
- US RESEARCH FUNDING NSF (National Science Foundation)
- NIH (National Institute of Health)
- DOE (Department of Energy)
- DOD (Department of Defense)
  - These followed the three division
  - Funding like this came about the following year

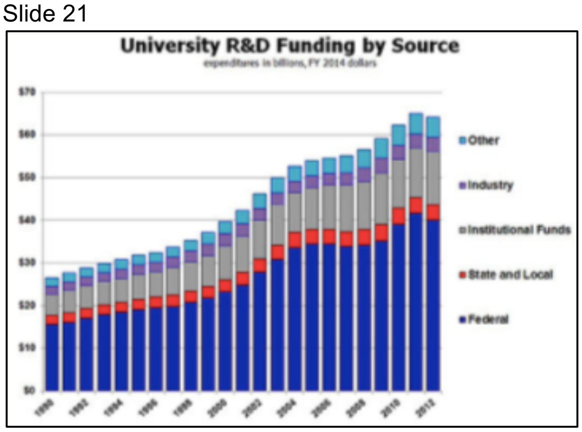
Slide 19

- *Canadian research funding…*
- NSERC (Natural Sciences and Engineering Research Council of Canada)
  - If you have a chemistry, biology lab…
- CIHR (Canadian Institutes of Health Research)
- SSHRC (Social Sciences and Humanities Research Council of Canada)
  - Economics and philosophy
- People have to go the government to tell them what they are doing and why it is important

Slide 20

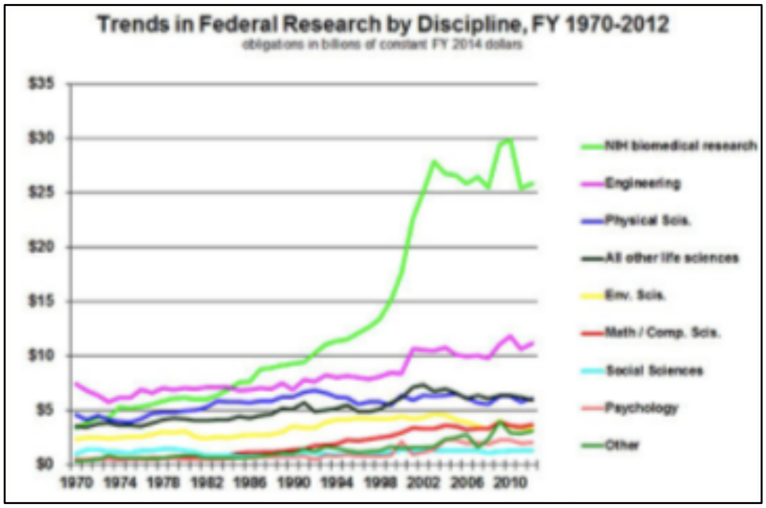
- Before WWII research was funded *privately* (much more freedom – pursued whatever)
- Now most research is *publicly* funded

Slide 21



- For NSF
- Expenditures in billions
- 1990-2012
- Dark Blue = federal research money (paid through taxes)
- Red = State local (geographical issues in state/province)
- Grey = university (may be private money)
- Now we have two-thirds of the research is funded by public funding

Slide 22



- In the US
- 1970-2012
- In billions
- Different areas of scientific research
- Top: biomedical research and engineering (main contributors)
- Explosion of biomed
- Amount of money spent on it, is much as every other combined

Slide 23

- If you run a lab, you need to apply for grants. To be successful you need to convince the funding agency that your research is worth spending their money on.
  - Can't fund everything

Slide 24

- How do values play a role in this process?
  - Disease prioritization
    - Widely-prevalent vs. rare (costs may be the same)
  - Economical values
    - Efficient way to increase well-being?
- To decide which research they are going to fund?

Slide 25

- How to prioritize basic vs. applied research?
  - Both costs money
  - Intermediate strategy?
- What duties do publicly funded scientists have to the public?
  - Do you have a moral responsibility to that?
- Are there different values at play when funding is private vs. public?
  - Government: take values of the whole public (want to maximize who you satisfy)
  - Private: you just need to satisfy that one corporation

Slide 26

- Philosophers need funding too – to fund graduate students
- I.e. John Templeton foundation (has an ideological angle – use philosophers to advocate)

Lecture 5 – October 16th, 2017

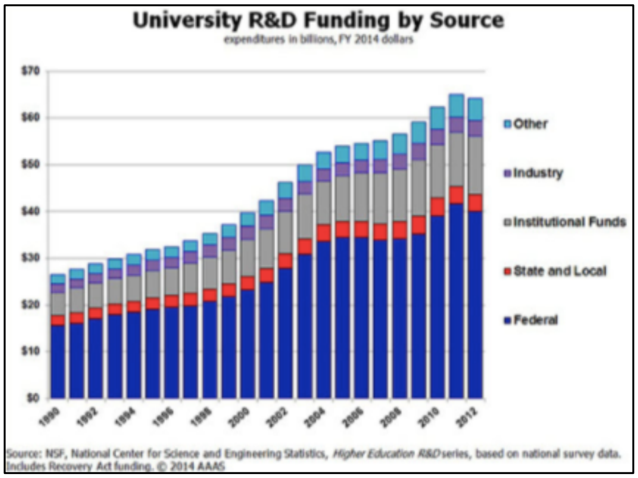
Topic for today:
    Funding Research
    Basic Research

From last class…
- Before WWII research was funded privately
- Now most research is publicly funded

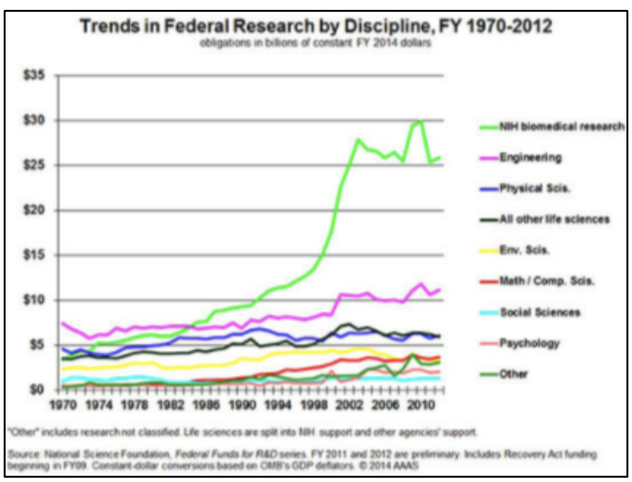
Question:
How do values play a role in the funding process?
- Transition in funding process after WWII

How should we prioritize basic/applied research funding?
- Something to think about

What duties do publicly funded scientists have to the public?



- From the US, NSC
- Public: federal, state and local
- Transition seen in the 1990's; largely funded by the federal



- Explosion of biomedical research in 1993
- Consistent with the story we are going to say today

In the book, first two chapters of Frontiers of Illusion
- Sarewitz provides a series of serious challenges to the received wisdom about science funding.
    - Why do we choose what research we fund?
    - This is an important question
    - There is this perceived wisdom that you let scientists just do their thing; the more you let them do their thing the more knowledge they generate and the better off we are…
    - This is a naïve view
    - A view that many people hold still…
- "The goal here is to reveal and evaluate the assumptions and interests that underlie the current system and that justify the government's role in supporting and promoting research and development."

- That was how Sarewitz described his goal
- His reading was challenging… those arguments undercut the professor's own values
- Undercut it in a way that he wasn't able to argue…
- Cut down on the prof's ideas on deeply cherished the role of science in society…

The Research and Development Trinity

- Basic research is the investigation of natural phenomena; it is often referred to as "pure science" because it is supposedly carried out independently from any consideration of practical utility.
  - Examples: proving statistical theorems, physics, pure science in biology
  - Mice in labs (being fed low vs. high fat diet) and looking at bone marrow in nice
    - You learn with what happens in the bone marrow
    - Come up with ways to manipulate human diets to expose certain things
    - Transitioning this information into applied research
    - Aiming for obtaining practical knowledge
- Applied research is the effort to use existing knowledge to solve particular problems - for example, determining the medical utility of penicillin.
- Development is concerned with making the products of applied research usable by society-for example, by devising techniques for mass production of penicillin.
  - I would probably fall into this…

Funding Research

Slide 1

- How does basic research contribute to the public good?
  - Sarewitz posed this…
- What is your naïve response to this?
  - How does doing basic research contribute the public good?
    - Developing this basic knowledge provides a groundwork for trying to understand how to apply the knowledge in new particular context (curing diseases, mitigating climate change, etc.) – improving the public good

Slide 2

- Sarewitz: Basic research contributes in ways which we do not reliably anticipate.
  - Against the common wisdom
  - Contributes to the public welfare in ways we do not know
- Sometimes the contributions are **positive** and sometimes they are not.
- Basic research wen into the development of television but there were some practical applied research to actually produce it, so it can be used in society
- When we do the basic research, we generate new kind of knowledge and the ways in which having that knowledge can contribute to the public good can either be positive or negative
- Examples: television, nuclear technology, the internet.
- Television
  - Positive: cultural enrichment (people living in far areas can have access to programming); dissemination of knowledge quickly (weather, emergencies)
  - Negative: Disseminating ideologies (world would be better without it); enabled political organization to rise to a level of prominence that they wouldn't have had
- Nuclear Technology
  - Positive: power plants (clean way of developing electricity)
  - Negative: basis for disputes (nuclear weapons)
- Internet
  - Positive: facilitates communication between scientists (nobody else)
  - Negative: same as television, cyberbullying, etc.

Slide 3

- Sarewitz talked about five myths…
- The myth of infinite benefit: More science and more technology will lead to more public good.
- The myth of unfettered research: Any scientifically reasonable line of research into fundamental natural processes is as likely to yield societal benefit as any other.
- The myth of accountability: peer review, reproducibility of results, and other controls on the quality of scientific research embody the principal ethical responsibilities of the research system.
- The myth of authoritativeness: Scientific information provides an objective basis for resolving political disputes.
- The myth of the endless frontier: New knowledge generated at the frontiers of science its autonomous from its moral and practical consequences in society.
- We are going to look at Sarewitz arguments to all of these myths…

Slide 4

- What Sarewitz means by "myth":
- Not the same as what we call a myth…

- "Myths of course serve a vital function in any society, by simplifying complex processes and making them comprehensible, and by encapsulating broadly held sentiments and thus creating a unity of vision and a shared sense of community and purpose."
    - His 5 myths capture the naïve conceptions some people can have (including you)
- "Thus, the question here is not simply the degree to which the policy myths are "true" or "false" but what ends they serve and how they affect the policy process and society at large."
- There is an interplay between science and society, non-epistemic values play a role in internal practise of science - what is the nature between the interphase of science and society is supposed to look at
- This is a productive way to think about and revise our commitments about the role of conducting scientific knowledge plays in contributing the pubic good and the policy process and how it affects members of society at large

Slide 5
- Sarewitz: "A new perspective must emerge in which science and technology are understood to be agents of context alteration, not merely simple steps upon which humanity seeks to ascend above the latest array of crises."
    - Abandon naïve thoughts on the role of science in society
    - What does it mean for science to be used as agents of context alteration?
        - The professor wouldn't say this

Slide 6
- The myth of infinite benefit: More science and more technology will lead to more public good.
- Television: it will be hard to come up an objective criterion
- Come up with all of the positive and negative influences that TV have had
- Doing any basic research just by default will naturally lead to situation where we have more positive benefits than negative…
- Bush said something like this…

Slide 7
- Recall Bush: "more jobs, higher wages, shorter hours, more abundant crops, more leisure for recreation, for study, for learning how to live without the deadening drudgery which has been the burden of the common man for ages past."

Slide 8
- Sarewitz: In the US, research funding has increased
- But where is the societal benefit?
- If the myth of infinite benefit is true and US is doing the most basic research, you would expect that the welfare of US citizens would be pleasant
- "Since the late 1960s, however, Americans have worked longer hours for lower wages, leisure time has decreased, concentration of wealth and the gap between wealthy and poor Americans has increased, the percentage of new jobs that are low paying and require a low level of skill (and, presumably, a high level of drudgery) has increased, unemployment rates have, on average, been higher, and overall indicators of social well-being have declined."
- All these trends he pointed out are actually getting worse
- Doesn't stack up with the idea that doing research is supposed to improve our situation

Slide 9
- "Since 1970, no major segment of the national research budget has increased as rapidly as that for biomedical research. In the 1980s, research funding for medical sciences rose 41 percent after inflation; for basic biomedical research the increase was nearly 80 percent."
- If myth #1 was true, you would expect that health and well-being of these citizen would have improved over that time
- "Despite these increases, the U.S. health care system is widely acknowledged to be in need of comprehensive reform as medical costs rise unabated (driven in part by the costs of new technologies and drugs), access to medical care increasingly becomes a privilege of those who can afford it, and public dissatisfaction with the system plays a growing role in national politics."
- Personal anecdote: professor have idiopathic condition where he gets bad headaches and faint; go to ER, and get MRI; fortunately, he had good healthcare; all was covered by healthcare plan; each time that happens, you get a bill of what you paid and what the insurance company pays ($1000); MRI's are great but too expensive to produce and maintain; burden of the costs of his situation gets paid out by everyone in some sense
- For people who need basic, standard care, it is not clear that someone like the professor is getting this service basically almost getting free by getting charged to insurance – is this a benefit to everyone?
- MRI's are expensive - sinks in the healthcare budget…
- You need to make decisions on who gets what kind of coverage
- Sarewitz point: when you generate technology, an attempt is being made to make the best care for an individual but doesn't always lead to situation where everyone on average gets better care

Slide 10
- "The United States government devotes more than 30 percent of its civilian R&D budget to health research, while other industrialized nations typically spend less than 5 percent. In absolute terms, the United States spends ten times more on health R&D than its nearest competitors, Japan and Germany. All the same, it lags behind other industrialized nations in many basic health indicators, ranking seventeenth in feto-infant mortality, nineteenth in maternal mortality, eighteenth in life expectancy from birth, and tenth in life expectancy from age 65."

- You can do all the research you want but if you don't effectively mobilize that research and solve real societal problems in a population – the research by itself isn't going to get you far in the greater good of society

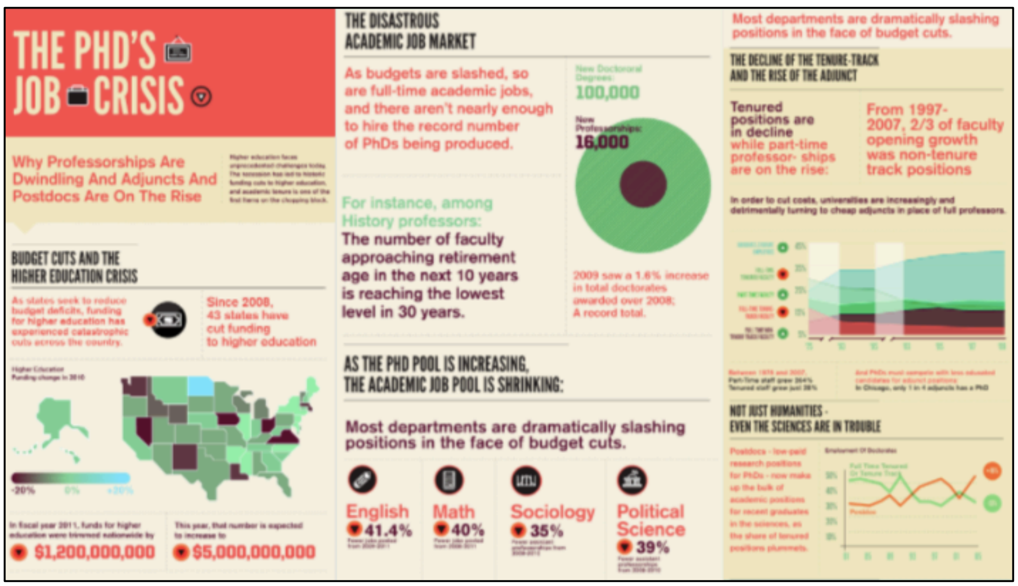
Slide 11

- "As a whole, the myth of infinite benefit isolates research, the purveyor of benefits, from the rest of society, the source of our ills."
    - In the objection raised earlier
    - According to myth #1: Job for scientists to come up with basic research, the job of everyone else to try and figure out how to deploy that research effectively to obtain positive outcomes of society
    - Sarewitz argued this wasn't the case in the US
    - Perhaps the benign conclusion drawn: absolute amounts spent on research is irrelevant on how society will be able to meet challenges
- "Yet if several decades of growing research and development expenditures have been accompanied by growing signs of societal stress in areas as diverse as economic performance, health care, and environmental quality, and if, as is widely acknowledged, the United States has the best research system in the world, then perhaps the most benign conclusion to be drawn is that the absolute amount spent on research is irrelevant to society's ability to meet its most pressing challenges."

Slide 12

- "Concealed within the myth of infinite benefit is political reality: a growing research system creates a growing research constituency and a growing demand for federal R&D funding. One of the purposes of the academic research system is to produce new scientists."
- Each new scientists has to train more scientists (that is their job)
- Physics and biology – 200 PHD students (in the end no jobs for most of them)
- One spot is only open; a position to become a professor after the current professor retires

Slide 13



- In a given year, across all disciplines, there are a 100,000 PHDs, positions for professorships is 16,000
- No guarantee that there is a job waiting for you
- Harsh reality
- Research system has arrived at this
- Difficult time in being in academia

Slide 14

- Sarewitz: How can we begin to evaluate the balance between benefits reaped by science and problems imposed?
- Does this balance depend in any way on the research itself or on the structure of the research system?
- What types of societal problems are likely to be responsive to more research and what types have proven relatively insensitive to scientific and technological advance?
- What types of research have proven most effective in addressing social problems?
- How are the long- term goals of a democratic society best served by research?
- In light of ongoing fiscal constraints, how large should the R&D system be, and how should finite resources be allocated, in order to make the best use of available funds and ensure the best return to society on its investment?
    - This is the question that we will think about in Case 2
    - Given the limits and constraints on money we have available to do research and the fact that Sarewitz proposes, this establishes that the total amount spent on research does not tell us how much good is generated by doing that research
    - Question: how should we use those research those dollars we have, and which research should we be funding as a result?

Basic Research

Slide 1

- The basic/applied research distribution

  - Basic and applied research
  - Development falls in later
  - End of WWII
  - 1943 – graphs ticked upwards

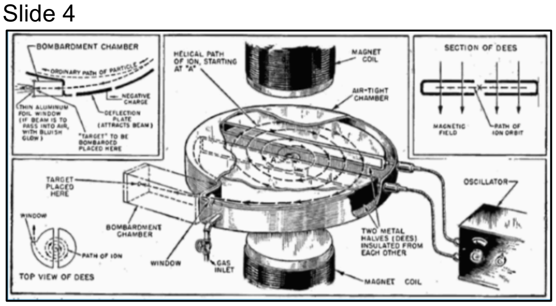
Slide 2

- At the end of 1800's
- J.J. Thomson discovered the electron using cathode ray tube
- First device that could be a particle accelerator
- Follow now is…
- Exposing electron to an electric field – deflection in the electric field
- Quanta-mechanics – understanding properties of electrons

Slide 3

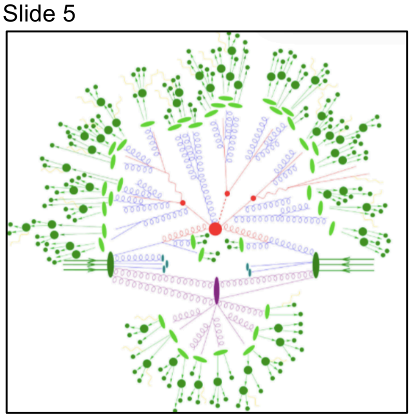
- 1930 Ernest Lawrence develops first cyclotron
- Form of a particle accelerator
- Expose ions to an electric field + magnetic field – speed up ions to verify high speed
- Dumped out and into a target
- To see the features of an internal structure of the ion
- Strategy used at CERN too – to discover what they are made up of

Slide 4



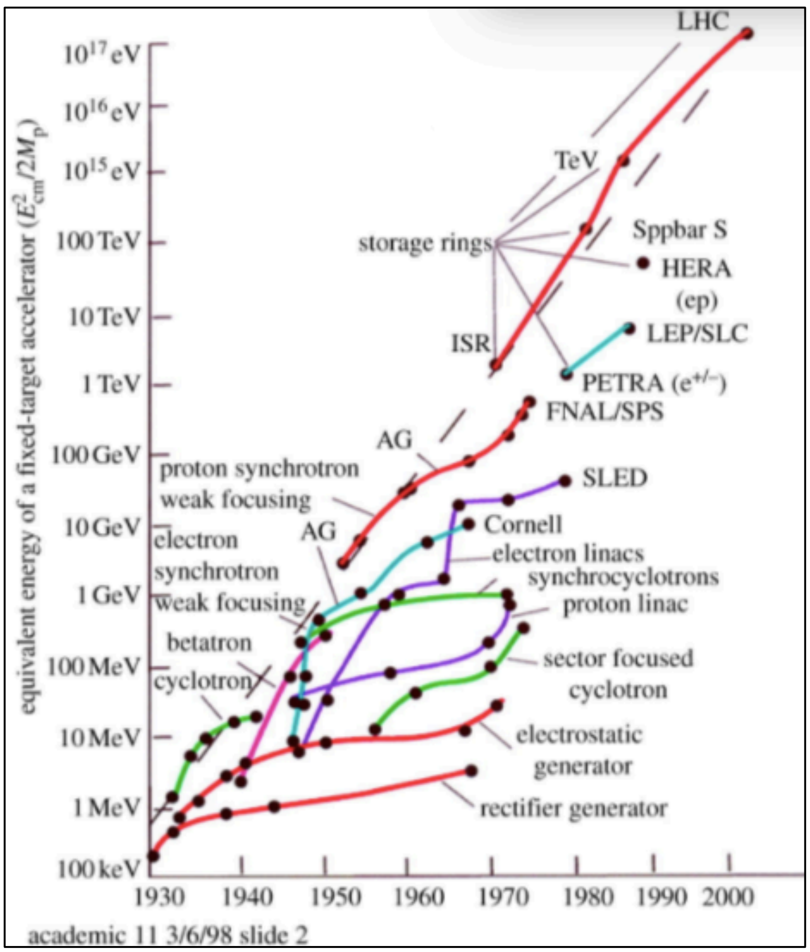
- Used for basic research
- Immediate application during the WWII
- First used inside nuclear weapons
- Nuclear chain reaction
- Basic research -> spits out additional knowledge of nature + aspects how we generate nuclear technology

Slide 5



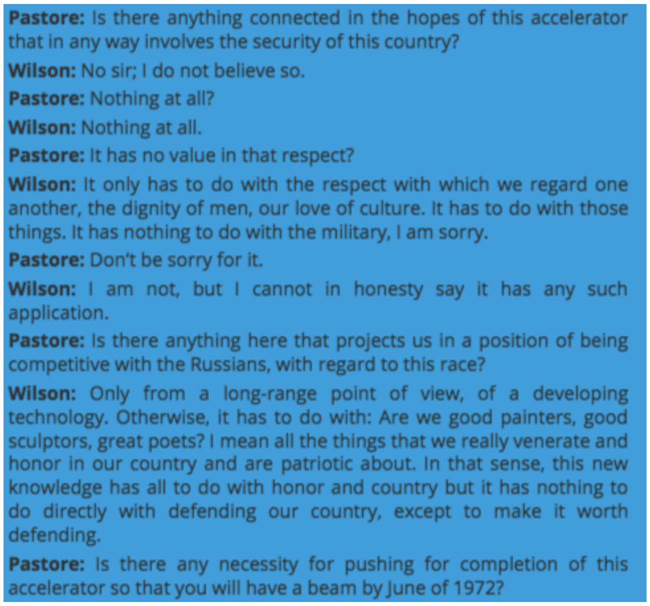
- Crash two protons at high speed
- Usually done at CERN
- Protons – stable individual units
- Protons are bags of stuff that stuff going on inside
- Protons have a lot of physics happening inside
- Two protons close to each other?
- Quarks happen
- One of these protons eliminate gluons
- This produces a chain reaction of something called hadronization
- Crash two protons – a runaway chain reaction
- Violate conservation of energy?
- These things have output energy of mass
- How could it go to two protons and produce a bunch of things with more mass?
- Kinetic energy that protons initially have is enormous…
- Near the speed of light
- What goes on in these processes – output
- Event in which boson was identified indirectly through the decay products
- Signature: what comes out and from where - calculate whether or not a boson has been there to produce the pattern that was observed
- Strategy is pretty simple: If you want to see the internal structure of something, just crash it with something else and see what comes out of it
- You get more info about what is inside the harder you crash it

Slide 6



- Y = available energy that a particle get collided together at (each data point are kinds of particle colliders)
- Cyclotrons, betatrons, etc.
- Things you can fit into the very room
- Localizable experiments became obsolete – need a whole lot of money
- People need new strategies
- Developed fermi-laboratories
- To see LHC
- Create large particle accelerators with huge magnets that were able to accelerate the collision items up to the speed of light
- Able to see what was inside the photons
- First time: fermi-national laboratories - large scale experiment that needs its own dedicated place

Slide 7



Pastore: Is there anything connected in the hopes of this accelerator that in any way involves the security of this country?

Wilson: No sir; I do not believe so.

Pastore: Nothing at all?

Wilson: Nothing at all.

Pastore: It has no value in that respect?

Wilson: It only has to do with the respect with which we regard one another, the dignity of men, our love of culture. It has to do with those things. It has nothing to do with the military, I am sorry.

Pastore: Don't be sorry for it.

Wilson: I am not, but I cannot in honesty say it has any such application.

Pastore: Is there anything here that projects us in a position of being competitive with the Russians, with regard to this race?

Wilson: Only from a long-range point of view, of a developing technology. Otherwise, it has to do with: Are we good painters, good sculptors, great poets? I mean all the things that we really venerate and honor in our country and are patriotic about. In that sense, this new knowledge has all to do with honor and country but it has nothing to do directly with defending our country, except to make it worth defending.

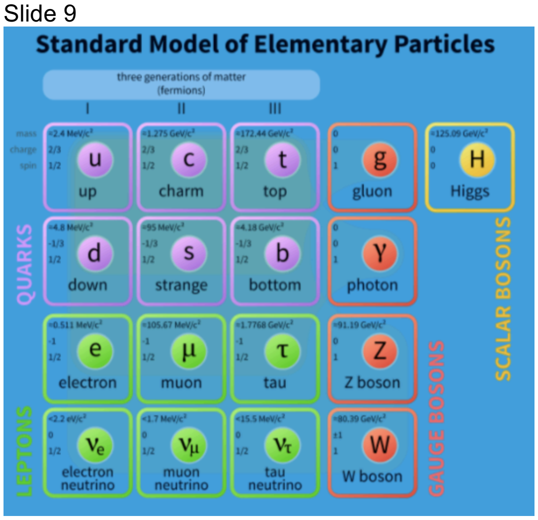
Pastore: Is there any necessity for pushing for completion of this accelerator so that you will have a beam by June of 1972?

- Testimony between congressman and scientists about why the US government may fund such a particle accelerator
- Interesting first question
- New knowledge has to do with honour and country and nothing to do with defending our country except to make it worth defending
- This was how basic research was thought of at that time
- For the same reasons of generating such structures, they decided to fund an expensive particle accelerator

Slide 8

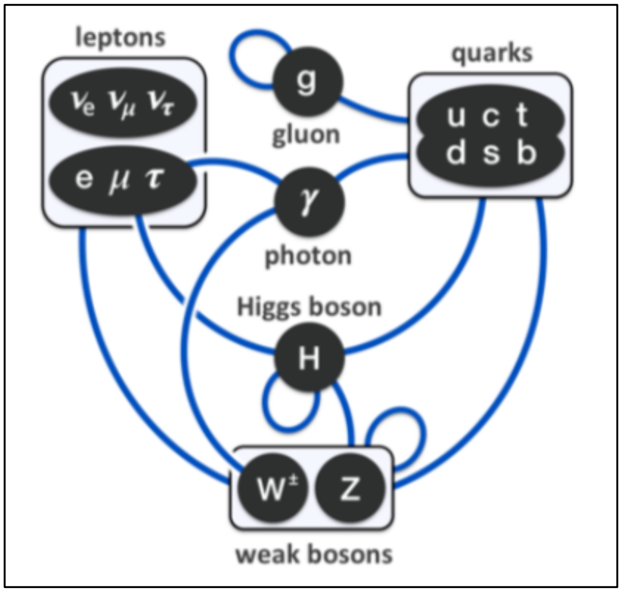
- The Tevatron at Fermi National Laboratory
- One of the first sites dedicated to the construction of a particle accelerator
- Deep down below tracks, there are magnets
- Tunnel underground where magnets are (generate super high velocity for collisions)
- Why far underground?
- Remove interference of environment
- Account for the fact that were being bombarded by so many particles from outer space
- This effects the kinds of signals that are being detected in these experiments
- Expensive to dig out tunnels
- In the end when we look at decay products
- Learn about the fundamental features of the universe

Slide 9



- Everything we have learned so far from doing particle physics
- These are the things that control fundamental interactions in nature
- Example: Electromagnetism – can't have finger go through table (electrons pushing back)
- Other examples: Weak nuclear forces mediated by bosons; strong nuclear forces by gluons
- Kinds of these we've learned in the process
- Higgs explains to us why some parts of this picture have mass and why other things don't (electron have mass; photons don't)

Slide 10



- There was an effort in the US, after the Tevatron reached the amount of energy that it could possibly produce, there was another attempt of a particle collider (super-conducting super collider)
- It was funded by US congress initially
- Now if you go in Texas, you can find a monument to the tunnel that actually got built (req for the exp)
- Congress pulled the funding partway through the project

The fall of SSC

Slide 1

- Sarewitz - to its supporters, the collider was many things:
- it was the ultimate physics experiment, an opportunity to develop a "final theory" that could unify all known laws of physics;
    - If the Higgs was there, it would solve the possible
- it was a testament to humanity's quest for knowledge and enlightenment and a laboratory for the education of America's next generation of physicists;
- it was a further demonstration of American pre-eminence in science and technology as well as a potential source of technological innovation that could yield great economic and practical value;
- and it was a sizeable public works project that would create thousands of jobs and bring billions of dollars to the Texas economy.

Slide 2

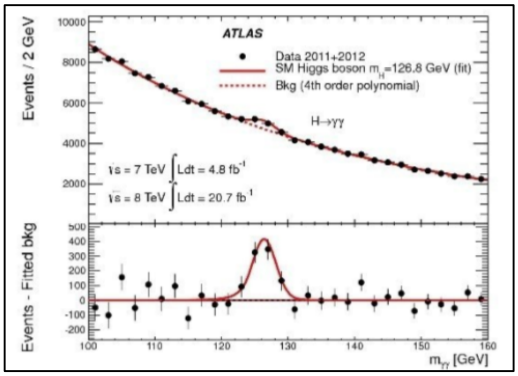
- Only 14 miles of the planned 54 mile tunnel were completed.
- The US congress pulled its funding part way through the project.
    - One basic research project (sole aim to get a unified aim of what the world is made of)
    - The costs were not worth it
- Why?

Slide 3

- *Sarewitz and Riordan:*
- End of the cold war American economy was tepid
    - Motivation of racing with the Russians not a motivation of doing basic research at this point
    - Economy was tepid (not enthusiastic in the expenses of it); let alone expensive research on basic human concerns
- Cost overruns
    - Project was poorly managed
- Failure to adequately employ the resources of other countries
    - Some effort in funding this project to involve many other powerful scientific countries in the funding of this project
    - This process didn't go well – wasn't managed properly
- The rise of other scientific disciplines (the human genome project was just beginning)
    - Human genome project: example of a biological project falling under basic research
    - Mapping out human genomes

Slide 4

- The Large Hadron Collider (LHC) at CERN

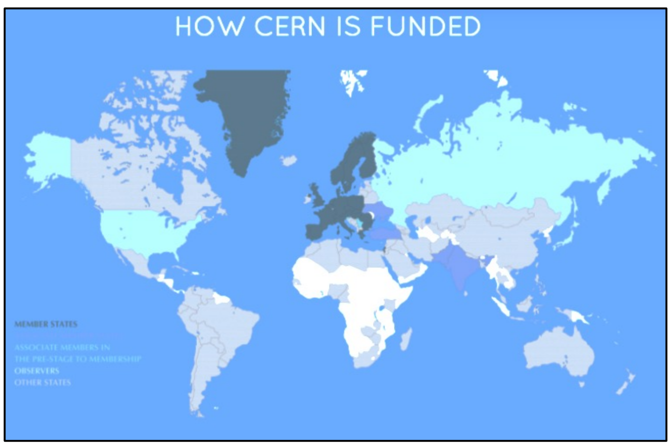


- 15B $ machine was this plot
- Info about fundamental constituents of nature and how they interact
- Bump = costs 15B dollars
- Is it worth it?
- What is the maximally efficient way to improve society?
- Benefits of finding/learning about boson are the kinds of benefits Wilson was describing
- The ability to generate thoughts like this makes it worth standing for Switzerland and France and worth defending – because there are resources for understanding this there – there is no application for this knowledge though yet
- How confident are we that this new knowledge will generate positive consequences in society
- Even if we don't know if there is positive consequences, would this basic research still be a good idea?
- We should still have **some** basic research available…

SSC

- US was aiming to pay for the whole thing
- CERN is funded differently

Slide 5



- Majority contributing to funding
- White countries aren't
- Should all basic research products, move to this model of complete international funding?

Slide 6

- Should we continue to develop accelerator technology?
  - LHC reaching energy frontier…

Slide 7

- Most of these are silly…
- Doesn't answer the question of why we should build more accelerators because there are already some available to us right now (especially, with the size of this room)

- Is your milk carton sealed? An accelerator did it.
- A lot of natural gas is wasted. Accelerators can fix that problem.
- Want your spinach E. coli free? Accelerators may have cleaned it.
- Can coal be a clean fuel? Yes, if you attach an accelerator to the smokestack.
- Antibiotics harm fish? Accelerators can turn pharmaceuticals into fertilizer.
- Your new computer has arrived. Thank an accelerator for building it.
- Accelerators make us live longer. They kill cancer.
- Can nuclear reactors be accident-proof? Yes, if particle accelerators control them.
- The world still runs on oil. Accelerators can find it.
- Accelerators keep watch for weapons of mass destruction.

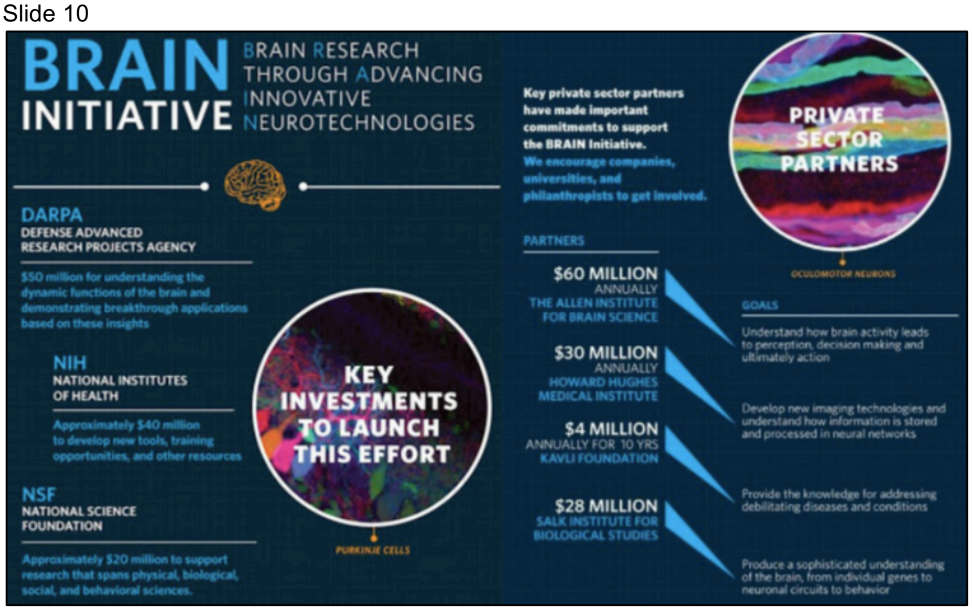
Slide 8

- Big science motivated by WWII; not only physics
- The Human Genome Project
- Huge, expensive collaboration
- Many intended applications
- Example of basic research
- Fundamental ingredient to future applied research – knowledge of how genetic information is unravelled…
- Project wrapped up in 2003
- Model is still here today

Slide 9

- Brain initiative?

Slide 10



- Breakdown of how this initiative which was launched a year ago
- Do what they did for the human genome project for now the brain
- Learn how the brain connects and encodes information
- Having this information to exploit info to generate positive consequences
- Cure brain illnesses, mental diseases, etc.
- Looking at annual budget, it is on the same scale as ???
- Contributors: DARPA, NIH, NSF
- Moral of story: context in which you see funding discretions, is when you are looking at biomedical research

Lecture 6 – October 23rd, 2017

Topics for today:

Funding Research II

Disease Research

Funding research II

Slide 1

- Basic research is typically not oriented toward any particular practical application.
    - The practical application that result tend to be incidental
    - Serendipity
    - There's happy accidents
    - In some instances we can take the output of basic research and apply them to real world problems that make a difference to people in societies
- For this reason, the funding of basic research is regarded as the responsibility of national governments rather than private corporations

Slide 2

- The standard view is that basic research contributes to the good of society by providing a knowledge of the structure of nature
    - According to Sarewitz
    - Gives us a pool of knowledge about the structure of nature
    - We don't know what the structure of nature will even reveal to know unless we do that research
    - For that reason…
- But we do not know what the structure of nature will be revealed to be until the research is done
    - That's why we are doing the research in the first place
- For this reason you might think it is impossible to predict how practical benefit will accrue from basic research
- Article on why we should be able to anticipate how and what forms of practical benefit can be derived from basic research

Slide 3

- You might even think that scientists need to be insulated from political, economic, and social pressures in order to do effective basic research
    - Sarewitz said this also
    - Just determining fundamental nature of nature blah, blah, blah and what you happen to be investigating and how you want that to be maximally affective… Scientists need to be insulated from political, economic, and social pressures
- Recall the value-free ideal!
- What was this again?
    - Scientist acting in their capacity as good scientist should not involve value judgements in their reasoning – non-epistemic values – this is the pernicious one; ones that have to do with social, political, economic issues – previously, we saw an argument that we do have clear-grounds that there is no way ignore the involvement of non-epistemic values in the progress of science

Slide 4

- Chapter 3…
- There is no argument towards this value-free ideal?
- Sarewitz: Funding decisions are largely protected from both external politics and expectations of practical application, while researchers are granted a large degree of autonomy in pursuing their investigations.
    - Idea is that: who researcher approach for money (national funding agencies), when those grants get adjudicated the content of those grants are judged by other members for the scientific community
    - Funding agencies collect grant proposal for what the research wants to be done, in that proposal research try to convince how it's going to revolutionize some particular fields of basic knowledge
    - Peer reviewers for that science foundation judge those proposals and on their scientific merits
    - Thus, what determines which grant gets funded, is by these peer reviewers (those who read the grants)
    - Policy maker… how are they involved? They guide how much individual grant agencies can be allocated
    - But what gets funded… is determined by scientists acting as peer reviewers
    - Content judged by other scientists in the community
- THE QUESTION: What are the promised benefits of this system?

Slide 5

- Nobel laureate in medicine:
- "The pursuit of curiosity about the basic facts of nature has proven throughout the history of medical science to be the most practical, the most cost-effective route to successful drugs and devices. Investigations that had no practical objective have yielded most of the major discoveries of medicine- X-rays and penicillin, the polio vaccine, monoclonal antibodies, genetic engineering, and recombinant DNA. These have all come from the pursuit of curiosity about questions in physics, chemistry, and biology, apparently unrelated at the outset to a specific medical problem."
    - Basic research is gonna give us the outcomes we want even though we don't direct the basic research at any particular outcome

Slide 6

- The myth of infinite benefit: More science and more technology will lead to more public good.

    - Refuted by Sarewitz: U.S had the highest national research budget; if we track the rise in research dollars and measure social wellbeing as a result of that, we see that while research funding as gone up, social well-being has gone down however during that time period
    - Spending more research dollars? We should ensure that social well-being should go up too…
    - But this hasn't happened
    - Sometimes in addition to having positive consequence the development of basic knowledge can have negative consequences too
- The myth of unfettered research: Any scientifically reasonable line of research into fundamental natural processes is as likely to yield societal benefit as any other.
    - Refuted by Sarewitz: absent any kind of info about how research is going to have practical benefit, he claims, that the default assumption is that any kind of line of basic research is going to be equally likely to generate public good and better social outcomes

Slide 7
- Sarewitz: "... the myth implies that the most efficient approach to basic research is to maximize the production of new knowledge-any new knowledge - and thus maximize the likelihood of serendipitous and revolutionary discovery."
    - If the connection between basic research and application of it is one that cannot be predicted…
    - Best thing we can do, is do as much research as we can
    - Each of those research is equally likely to produce beneficial outcomes
    - Will expect it to yield best social outcomes

Slide 8
- THE IDEAL OF THE UNFETTERED RESEARCHER
    - The only constraint on researchers should be nature itself
        - What he means is (when we have hypothesis about the world), the constraints on basic research should be whether or not that hypothesis is borne out in experimental investigations of the doubt
        - No one should care at all about the context of what researchers are working in and instead we should insulate researchers from social and political views

Slide 9
- A THOUGHT EXPERIMENT
    - Suppose there were such a thing as an ideally unfettered research system, subject to revolutionary, serendipitous breakthroughs, creating new knowledge in a manner entirely unpredictable yet inexorable, driven forward only by the interests and abilities of the researchers
        - Researchers just get money
        - All that is constraining them is whether or not their hypotheses are being borne out (proven) when they compare then to experiments in the world
    - The only controls on such a system (external constraints) would be the ability of the individual scientists, the resources available to them, and the existing state of scientific knowledge.

Slide 10
- A THOUGHT EXPERIMENT
    - There would be no way to judge the ultimate value to society of any particular line of research or even to guess the most likely type of contribution that a line of research would make.
        - This follows on from the idea that in this particular context where we are assuming there is no discernible connection between nature of some domain of basic research and the potential applications… if we can't predict anything then all of these things is likely to produce equal social benefits
    - Therefore, the most sensible approach to science policy would be to divide funds up equally among the best researchers in the various disciplines.
    - (Aside: the principle of indifference)
        - Deployed in statistical situations
        - A principle that is more or less intuitive
        - Is there a basis of adopting this principle?
        - If no reason to believe that any one of the option are more likely to happen than another, then assessing a uniform distribution over all possible outcome and ways of doing the research
        - To reiterate, we this the ideal of unfettered research (only constrained by fundamental structures of reality), and if we assume this principle of indifference, we're lead to think that all possible outcomes are equal… we should divide the funds equally amongst them
        - This principle once again says…
        - Absent knowledge is gonna make particular outcome happen…
        - Evenly divide probabilities over those outcomes
        - Issue: why should we distribute uniform probability at all?
            - We shouldn't act like we are using probabilities at all and understand the nature of the problem we are looking at

Slide 11
- Hint for case two: the hypothetical funding agency that you work for has adopted this consequence of the ideal of the unfettered researcher.

- The output is the claim: we should evenly divide our funding between all of the different categories we have for research

Slide 12

- Sarewitz: But that scientist is also part of a larger research system, and the organization of that system is a consequence of a complex set of still larger political, economic, historical, social, and scientific interactions. From a policy perspective, therefore, **a scientist (an unfettered researcher constrained by nature) is an almost incidental element of this broader context**, and a research problem reflects not just the intimate relationship between scientist and nature but the evolution of the research system as a component of society.
    - This is a way of denying the value-free ideal
    - Denying the claim that it is possible for individuals researchers to be only constrained nature
    - Something else will creep in that they have no way of control
    - Affecting the way their internal practise of science perceives to be

Slide 13

- Opportunities for **progress in basic research** may also **depend on important problems** or phenomena **revealed by applied research and technological innovation**
    - Invention of steam engine -> thermodynamics
        - In order to understand which engine was more effective, you need to develop another theory (thermodynamics)
        - Had a profound effect on physics in the invention of this stream engine
        - Why ice melts in water and why proteins fold the way they do
        - That theory wouldn't have developed
    - Advances in steelmaking -> better understanding of properties of metals
        - Industry of creating harder steel for less money… had an influence on a whole branch of basic-type science knowledge
    - Efforts to track U-boats -> mapping of ocean floor -> theory of plate tectonics
        - Bomb boats from WWII
        - What was causing variation on the structure of ocean floor?
        - Plate tectonics
    - 150 years of technological innovation -> climate change research
        - Development of steam engine, electricity
        - Need for basic science research into how that large emission of CO2 in the atmosphere… and is now a fundamental basic science problem that need to be solved and need to make new technological applications to resolve this issue
- In cases such as these, technology is the stimulus for basic research, not the consequence of it. The relation between fundamental research and application is one of complex interdependence. Each may drive the other forward.
- Therefore, it is not a one way thing…

Slide 14

- Moreover, Sarewitz points out
- Serendipity (we can't anticipate the kinds of things we dealing with) can occur in applied research and development as well.
    - Teflon was an inadvertent product of applied re-search on refrigerants
    - The microwave oven was an accidental outgrowth of research on radar
    - Mass production of penicillin was made possible because government researchers were trying to find a way to get rid of agricultural by-products that just happened to encourage growth of the penicillin mold.
- It is not the case that you are trying to develop a particular application from some body of basic research that you know for sure will happen BUT that outcome of that body of research can be completely different

Slide 15

- Sarewitz says…
- There is, in essence, a basic research market, driven not only by the curiosity of talented scientists (unfettered researcher) and the work of their predecessors but also by funding levels, job opportunities, public expectations, economic interests, and politics.
    - Have a role in the fettering of research
    - Researchers are extremely fettered
    - The can't escape the context in which they are producing knowledge
- The knowledge produced by basic researchers is an indirect reflection of the priorities of this market. The politics and economics of the research system are as subject to the whims of serendipity as the research itself.
    - It easy to get in to the mood of thinking that some of the things we have discovered is inevitable in a sense
    - Maybe someone somewhere may have already found we already found when there wasn't that priority

Slide 16

- An example: Particle physics after WWII
    - Would the particle physics research landscape look as it does today if there was never a Manhattan project and the war was won with biological weapons instead?
        - Would we have discovered bosons?
        - This is a hard question to evaluate
        - Particle physics would have looked different
        - We wouldn't have had any of these large colliders

        - We wouldn't have known to ask certain things about Higgs bosons
        - This research question probably would've never gotten posed either

Slide 17
- Sarewitz had another argument…
- Directions of basic research are also influenced by internal characteristics of the R&D system that may seem at first to have little connection to the actual pursuit of scientific knowledge

Slide 18
- We will discuss the myth of accountability (Sarewitz Ch. 4) in the final unit of the course

Funding biomedical research

Slide 1
- Debate: how to think about which disease reached their peak in fund, given there is a finite amount of resources dedicated to eradicating disease
- Flory and Kitchener paper
    - Morals same today as it was twenty years ago
- In 1995, 56 million people died.
    - Violence ~ one million
    - Famine ~ six million deaths
    - Disease ~ 40 million
    - Malaria ~ one million
    - Tuberculosis ~ two million
    - AIDS ~ three million
    - Respiratory disease ~ four million
- All of these killed more people than violence
- Some of these deaths can be preventable
- Many deaths are disease deaths…
- Most of those are due to old age
- Diseases at the end of one's natural life span
- Not really curable or reversible process
- Malaria, TB, AIDS, respiratory diseases – take lives much earlier

Slide 2
- TWO SENSE OF PREVENTABLE
    - 1) In the current state of our knowledge, available techniques exist for responding to the diseases mentioned above and for saving those who suffered from them.
        - Do not need to do anything new; already have basic knowledge and techniques to save those people

Slide 3
- 2) Second: scourges that we might hope to overcome in the course of future biomedical research. Part of the world's burden of disease could have been alleviated if those who died had had access to drugs or treatments routinely available to others in different places. Another part could have been lightened if there had been more thoroughgoing efforts to discover methods of combating disease, methods that the actual course of biomedical research has so far not yet found.
    - It might be that we have techniques that are effective in treating some forms of disease
    - But it could be the case that additional disease research could reveal different techniques for overcoming disease i

Slide 4
- In many situations, deaths from disease in less affluent nations result from problems with infrastructure, not a lack of medical technology.
    - Problem: not that we don't understand and disease, but the conditions on the ground in the society in question… those treatment we have available are simply not effective because of problems of infrastructure
- Malaria can be controlled through the removal of stagnant water sources where mosquitoes breed, use of mosquito nets, and availability of an effective public health system.
    - If people have access for this, it reduces the effect of disease in a population
    - Removal of stagnant water sources: extremely effective but only effective with proper infrastructure; ability to manipulate local environment with technology but not available in a lot of the places suffering with malaria
    - Mosquito nets
    - If people had access to these, it reduces the effects of these diseases

Slide 5
- The situation with tuberculosis is similar
- Inconsistent use of antibiotics has contributed to the spread of drug-resistant tuberculosis, but even most resistant strains can be beaten by a public health system that applies existing therapies quickly and thoroughly. As matters stand, millions of tuberculosis (TB) patients receive incomplete courses of drugs, bringing temporary respite at the cost of giving the TB bacillus an opportunity to evolve antibiotic resistance.
    - Drugs can be expensive…
    - Partial treatment makes it worse…
    - Allows for strong bacteria which aren't killed off can breed and make stronger strains

- If well-funded, trained clinicians reached all these patients, the emergence of resistance could be slowed and the vast majority of tuberculosis deaths prevented without a single new technological idea.
    - This problem doesn't require any new technological advance
    - More of developmental research problem
- Conclusion: error to think that simply because the strategy of combatting disease is available in a particular privileged region of the world, medical research in alternative strategies is pointless

Slide 6

- KITCHER AND FLORY'S CONCLUSION
- It would thus be an error to think that, simply because a strategy for combating a disease is available in a particular, privileged, region of the world, medical research on alternative strategies is pointless.
    - You might think that since the process resulted in practical resolution to a particular disease (cure) there is no need to spend research money on how to treat that disease
    - Figure how to cure? Resolved problem… don't spend anymore
    - But this is a problem as stated by Kitcher and Flory
    - Shouldn't stop research even though we have a method to resolve it is wrong
    - There is a good reason on why we should continue on how to treat diseases even once there is an available mechanisms is solving them in affluent societies

Slide 7

- How should we decide how to prioritise which diseases to work on?
    - We only have a finite amount of money
    - Could we use principal of indifference…
    - Take all of the uncured diseases and uniformly allocate funds across all of them
    - Is that a good idea?
    - I do not think that is reasonable
    - The class agrees
    - What would be a practical idea then?
    - Can't equally divide money to all of them
    - Some diseases affect huge fractions of a population but rare ones that affect a small fractions of population
    - Maybe look at how many people are suffering relative to the disease
    - Or maybe based on the severity of diseases?
    - Some diseases influences the quality of life (incapacitated)
    - Not all research dollars will give each case the same impact
    - If a problem is harder to solve that will cost more money than problems that are easier to solve
    - Hard to come up with objective measure for this kind of question
    - You need to think about whether or not we have an obligation, in an affluent society, to contribute to disease research to diseases that doesn't affect our population severely
    - Treat all diseases equally?
    - Question of ethics?
    - Hard to come up with an objective measure

Slide 8

- Two proposed objective measures by the two authors (K + F)
- But we need to think about this as well: what are the obligation of researchers to the society that is funding the research and the researchers' obligations to global order
- This is what K + F is trying to advocate
- A proposed metric (fair share):
    - (Total research dollars available) x (Fraction of deaths caused by that disease)
    - That tells you the amount of money you should allocate to that part of research
- This is called the fair share metric

Slide 9

- Malaria fair share:
  $70 billion x (1 million / 40 million disease deaths) = $1.75 Billion
- Actual spent:
  $85 Million (<5% of the fair share)
    - Didn't spend as much
    - That's less than 5% that what should have been spent
- We already have some things to prevent malaria (infrastructural resolutions)
- But look at what was actually spent…

Slide 10

- Tuberculosis fair share:
  $70 billion x (2 million / 40 million disease deaths) = $3.5 Billion
- Actual spend:
  $33 Million (<1% of the fair share)
    - Wow, less than 1% than what should have been spent

Slide 11

- WHY?
- Why so low…?
- Research dollars (from the 70B figure) come from the wealthy parts of the world
- Most of the suffering of TB and malaria happens elsewhere (poor nations)
- Not so much suffering in the rich areas…
- We don't we spend research dollars on them?

Slide 12

- Research dollars come almost entirely from the wealthy parts of the world, and the suffering from malaria, tuberculosis, and a large number of other infectious agents happens elsewhere.

Slide 13

- There is something called the…
- The 10/90 gap
  - 90 percent of humanity's burden of disease receives only 10 percent of the world's health research resources.
    - 90% of disease deaths that actually kill people gets 10% world health research resources
    - Where does the other 90% of those research money go?
  - The skewed distribution favors the diseases of the affluent world-cancer, heart disease, diabetes.
  - How much of the resources aimed at these diseases could be diverted without appreciable loss is a serious empirical question.
- Still exists today
- How much of that 90% of the research dollars could be reallocated without changed the level of success that we are achieving with respect to cancer, heart disease and diabetes
- For some forms of cancer, heart diseases, diabetes there are also infrastructural ways to resolve… just like TB and malaria, even without developing new technological/medical knowledge
  - Diet
  - Exercise
  - No smoking + other drugs
  - No easy out
- This shows that we do not need technological solutions to reduce the rate of these diseases as there are infrastructural ways of doing so… but not advocating to stop cancer research, no, no, no, no, nope
- But what is being said is that we can't dismiss doing more research just because of those infrastructural ways/existing methods that resolve those problems in affluent societies
- We should continue to do research
- In affluent societies, that is not a problem

Slide 14

- The two authors K + F raise two objections against their view
- 1) There are two obvious objections to the analysis presented so far, both of which would charge our talk of "fair share" with persuasive definition. The first would emphasize that the direction of scientific research cannot simply be a matter of the urgency of the practical problems; the second would question our measure of the global disease burden.
  - Going to focus on the second one the most

Slide 15

- Note what Flory and Kitcher do here: they raise an objection to their own argument and then proceed to show why their position is still valid.
- This makes an argument more persuasive!
- Very effective

Slide 16

- 2) Second objection: the measure of disease burden used in fair share is too crude.
  - "You might not like the way we measure global disease"
- It would be correct to note the possibility that some diseases inflict more suffering for a given mortality rate than do others: a disease that kills some number of people late in life may cause much less suffering than one that kills the same number of people but strikes earlier and inflicts long periods of disability in many nonfatal cases.
  - Fair share doesn't pick up on these things

Slide 17

- The two authors consider a new metric (DALY → Disability-Adjusted-Life-Year)
- The idea is that we should look for the number of years of life lost because of the disease.
- Life is lost not simply through early mortality but also through the discounted value of years of life lived with disability, where the discounting is based on widely shared views about the value one would attribute to a future year of one's life in the diseased state as opposed to the value one would assign to a disease-free future year
- If you are 85 and die from a disease – the following year doesn't count as much as…
- If you are 25 and die from a disease – counts (they miss out on core years – the years people judge to be that are most valuable in life)
- This metric adjusts for this
- But there are multiple ways to capture this notion of how valuable a given of one's life is supposed to be

Slide 18

- That idea can be implemented in multiple different ways:
- The age egalitarian assumption takes a year of human life to be of equal value **regardless** of location, race, social background, or level of education, and also incorporates the claim that death is **premature** if it occurs before a standard of 80.2 years for women and 80 years for men.

Slide 19

- Alternatively:
- The age-**in**egalitarian assumption does place a different value on years of life at different stages of a human being's existence: initially, the value of a year rises rapidly with age, peaking in the mid-twenties, and thereafter gradually declines to about a third of its maximum value by the age of 80.
  - Value of life decreases as you age
- This assumption is explicitly based on studies of how people actually value different stages of human life; part of its rationale is the idea that adults of the most valued ages can make the greatest contributions to their societies and to the well-being of their dependents.
  - So basically, how much the person contributes to society, not only their own selves

Slide 20

- This captures things more better – of part of the points we have raised
- But how do things stand when we replace mortality rates with DALYs lost?
- Roughly the same
- Still have 1% for TB, 5% for malaria
- Even once you try to be fancy and incorporate certain factors, we are still not allocating research dollars effectively

Slide 21

- Case two hint:
- Is there a way to motivate a metric like *fair share* or *DALY* to determine an appropriate balance between basic research, applied research, and development?
  - K + F are trying to develop allocation across different diseases and how you think about these different diseases
- Alternative approach would be to propose a metric with respect to how we should allocate funding across basic, applied, developmental research…
- Kinds of motivation one would want to give to that proposed metric may not be different to the motivations that Sarewitz mentions in terms of prospect of different kinds of research creating different kinds of positive, social applications