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Chapter 1 – Kuhn's Philosophy of Science

In his pioneering book *The Structure of Scientific Revolutions* Thomas Kuhn proposes a theory of the development of science that includes detailed work within frameworks that he named paradigms and great undermining if these paradigms which he called shifts. As we shall see, this new understanding of science has philosophical underpinnings that arise from his unique understanding of the way individuals and groups of people learn. Kuhn's book is an interdisciplinary work in which he recounts major discoveries, mostly in the field of physics, his original area of expertise, analyzes them, and uses them to offer a general theory of the changes of science.

In this chapter I first discuss what Kuhn calls normal science, the way most science is conducted within a framework of assumptions and rules. We will discuss why this framework is necessary for the development of science and how it impedes it. Then, I will discuss the paradigm shift, a term Kuhn coined to describe the scientific discoveries that require an undermining of assumptions but are necessary for great advances in science. Finally, I will discuss the philosophical claims that are necessary to understand Kuhn's argument – his views on epistemology, science and the role of the community in both.

Kuhn begins the description his scheme for the evolution of science by describing pre-science, a necessary phase that Kuhn actually does not view as science at all. Before a major discovery is made, or a field is established, there will be various people researching a topic with no systematic way of describing their experiments. Every experimenter will start from the foundations, in an attempt to find the right data, experiments and theory. The scientists won't communicate with one another, because they don't have a common language and do not have reason to believe that any one else's basis is better than their own.[1] This kind of research is characterized by the lack of prioritization of information, since there is no established way to know which facts will be crucial for developing a better understanding of the field, and which are superfluous. At the same the experimenters aren't challenged to reach out for esoteric data that is more difficult to find because they are still not sure what to make of the data that is most accessible.[2]

This state of confusion only changes when a great discovery is made that can create a framework for the field, a discovery Kuhn would call a paradigm. Such a discovery is able to answers major questions, while leaving them sufficiently open-ended so that they offer goals for other scientists, thus attracting "an enduring group of adherents away from competing modes of scientific activity."[3] Students then study the paradigm experiment in order to become experts in the particular field, adopting the underlying assumptions that enabled the success of that particular discovery. Subsequently journals, societies and departments proliferate in which problems ensuing

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[1] Thomas Kuhn, *The Structure of Scientific Revolutions,* The University Chicago Press, Chicago, 1996 (henceforth K), p.13.

[2] Ibid, p. 15

[3] Ibid, p. 10

from this great discovery are discussed and worked out in detail.[4] Scientists become more specialized, as they discuss these particular questions and mostly address the other experts in their writings, trying to answer the questions of this paradigm and the developing more intricate apparatuses and jargon to help them. According to Kuhn, the great majority of scientists work within a paradigm, which is why he labels this work Normal Science.

The framework set by the paradigm is not limited to scientific puzzles to be solved. Accepting the results of a certain experiment also changes the assumptions with which one comes to the field, and to scientific work as a whole. Thus a paradigm becomes not merely a certain experiment, but a set of scientific rules about how science should be conducted, what instruments are preferred and how they should be employed. It also dictates quasi-metaphysical claims about what the nature of the world, such as the belief that the world is made of atoms.[5] Kuhn accepts that not all of these are fully shared by scientists working within a certain paradigm. He explains that limits of paradigms function the way families of words work – there is a crisscross of associations and characteristics, that are shared by the group overall, though they are not shared by all its members.[6] Additionally, the paradigm goes beyond these rules: "those rules may not by themselves specify all that the practice of those specialists has in common,"[7] since the definition of a rule enables it to apply across fields and

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[4] Ibid. 19

[5] K p. 40

[6] Ibid, p. 44-45, as based on Ludwig Wittgenstein's understanding of groups. He explains, for instance, that group games doesn't have any set of characteristics, but rather than its members have a few common characteristics that are not shared by all.

[7] Ibid p.42

paradigms, while the character of the paradigm is unique to it.[8] Finally, scientists themselves are often unaware of the paradigm they are in (more about this below), since they take its characteristics for granted and don't debate it. This is because "paradigms may be prior to, more binding, and more complete"[9] than the rules and experiments the scientists conduct.

Kuhn argues that the framework the paradigm provides is necessary for the advancement of successful research. The paradigm defines for the scientist what entities the world is made of, what their relationship is, and what questions one can ask about them. There are three kinds of research within a paradigm:[10] using of the paradigm to answer scientific question, bringing its experiments to better precision and solving its intrinsic problems. As Kuhn notes, this is no small feat: "Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of complex instrumental, conceptual and mathematical puzzles."[11] Examples of this kind of work are the calculations of stellar position and magnitude in astronomy, gravities and compressibility materials in physics and boiling points or acidity of solutions in chemistry. The second form of normal science is comparing the discoveries of a paradigm with new data that is found or enabling its further precision. The development of a good telescope that could demonstrate Copernicus' prediction of the annual parallax or Foucault's apparatus that showed that the speed of light is faster

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[8] Ibid p. 49

[9] K p.46

[10] Ibid p. 25-27

[11] Ibid p. 36

in air than in water are examples of this. The third is empirical research to try to solve the puzzles and difficulties intrinsic in the theory. It is mostly through this framework that new questions can be asked, and discoveries which would not have been imagined before are discovered, as Kuhn puts it: “when the paradigm is successful, the profession will have solved problems that its members could hardly have imagined and would never have undertaken without commitment to the paradigm.”[12]

Yet for all of its importance and great success, normal science also has its limitations. Kuhn claims that “normal science does not aim at novelties of theory and, when successful, finds none.”[13] Scientists within a paradigm focus on understanding the paradigm itself, using it to understand nature, and therefore the nature of a paradigm is that it does not provide understanding of or acknowledge to facts that don’t fit into it and does not know what to do with them. Therefore, “Normal science… often suppresses fundamental novelties because they are necessarily subversive of its basic commitments.”[14] Strange discoveries that undermine the assumptions of a framework would jeopardize the project of understanding nature through the paradigm, which depends on the truth of the paradigm. Scientists will therefore often view data that doesn’t fit into their framework as a failure on the part of the experiment or the scientist rather than on the part of the theory,[15] and will label questions that cannot be answered by the paradigm as unscientific or relegate them to another field.[16] Paradigms

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[12] K p. 25
[13] Ibid p.52
[14] Ibid p.5
[15] Ibid p. 35
[16] Ibid p. 37

do not lead to the discovery of new phenomena's or theories that are very different from the work being done within them.

Since normal science limits change and discovery, frustration with normal science brings about what Kuhn terms paradigm shifts. "Discovery commences when awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science."[17] The frustration with a paradigm arises when it seems to not fully mirror nature as it is empirically observed, and when the problems it creates cannot be solved within its limits. One example Kuhn discusses is the disillusionment with the Ptolemaic astronomy prior to the advancement of Copernicus' theories. Ptolemy's system was successful at predicting the movement of planets and stars, yet constantly displayed seemingly local discrepancies between the observations and the prediction. Each one could be solved with additional complication in the theory, and yet when one looked at the system as a whole, it was becoming more complex than accurate. "Failure of existing rules is the prelude to a search for new ones,"[18] – scientists and non-scientists that preceded Copernicus were worried that the system was incorrect, and this attitude led to his discoveries.

There is no good definition for an anomaly, a problem within a given paradigm that undermines its authority, since every anomaly is different and scientific frameworks also have problems that are solved by further investigation. Yet looking at the history science Kuhn identifies four kinds of situations in which the problems with a given

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[17] Ibid p. 52-53

[18] Ibid p.68

theory will undermine a paradigm: 1. questions that call into question a whole theory, 2. a practical need for a solution to a technological or other application of the science, 3. a new theory that contradicts the old one, or 4. A long elapsed time that does not bring an answer. This kind of crisis can often be identified when different versions of the paradigm are developed, in an effort to find a way to solve the problem.[19] The solutions to the problems may become successively more complex without enabling greater precision, and often when solutions are offered they create further problems.[20]

The solution to an anomaly only comes when scientists begin questioning the assumptions of their paradigm, and their problems are solved “only when the paradigm theory has been adjusted so that the anomalous has become the expected.”[21] Often the only way to make a great discovery is to undergo this change, since facts and data that were not a part of the previous paradigm cannot be appreciated or even noticed by scientists who are working within its framework. The “distinction between discovery and invention or between fact and theory will, however, immediately prove to be exceedingly artificial,”[22] claims Kuhn. Scientists cannot understand the significance of certain data without the right theory that points to their significance. This is why discoveries such as the ones made by Copernicus, Newton, Lavoisier and Einstein did not only add to the knowledge of science before them, but also changed the way the previously discovered data and conducted experiments were understood,[23] as well as

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[19] Ibid p.71
[20] K p. 68
[21] Ibid p.53
[22] Ibid p.52
[23] Ibid p.6-7

the legitimacy of previously accepted instruments.[24] Moving into a new paradigm thus requires questioning of the accidents in the area of the paradigm being questioned, the success of the new theory in solving problems in the previous one, and the ability of the of the new paradigm to explain new phenomena or give a neater and simpler answer than the solutions of the previous paradigm.

One of Kuhn's most revolutionary claims is that paradigms must shift in order for science to develop, meaning that the underlying assumptions and rules of science must change in order for substantial new discoveries to be made. Kuhn argues that different paradigms are incommensurable, meaning that they necessarily contradict in some ways that cannot be resolved, and that one paradigm can hinder us from seeing the new discoveries that must be made, and therefore we must switch to another paradigm with new categories and principles. If the right understanding of nature is in place, there would be nothing new or remarkable about the discovery. [25] Yet great discoveries are great not merely because they uncover new facts, but rather they also redefine the way we view nature.

According to Kuhn, the shift from Newtonian to Einsteinian physics illustrates "with particular clarity the scientific revolution as a displacement of the conceptual network through which scientists view the world," because it did not introduce new objects, but rather reinterpreted the given ones.[26] Some people will argue that Einstein's theory is a more general theory, which includes Newton's theories. Yet Kuhn

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[24] Ibid p.59
[25] K p. 55-56
[26] Ibid p.102

shows that Newton’s laws would be completely different if they had been derived from Einstein’s, since the two theories approach mass, time and space differently . Though it may be sometimes useful to use Newtonian physics in a daily context, the framework that we use to view facts explained by the Einsteinian theories is completely different than the one Newton had.[27]

Great changes cannot occur without this paradigm alteration because the paradigms themselves set limits on the discoveries that are made. Logic cannot be used to overcome the limits of a paradigm, because both those within and without the paradigm could be fully logical yet be starting with different premises and values that prevent them from understanding each other. Different paradigms also answer different questions, so that an answer in one paradigm is useless in another. But these different values and questions held by the members and non-members of the paradigm also mean that scientists working in different paradigms will actually see and discover different data, since they are not looking for the same thing, and will even sometimes interpret differently the very same data, since “more than one theoretical construction can always be placed upon a given collection of data.”[28] Therefore, for someone to accept a new discovery they must be able to also accept the new theoretical construction, or paradigm.

The fact that new paradigms are usually born from the malfunctions of previous paradigms, and are based on answers to the questions that the previous paradigms

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[27] Ibid p. 102
[28] K p. 76

were not able to answer, shows that old and new paradigms are unlikely to naturally succeed each other. If the first theory could not solve a problem that arose between it and observations of nature, it seems that another theory must approach the question from a completely different perspective that will be contradictory to it.[29] Until this step is taken the unsolved problems are considered unimportant, too hard, or irrelevant – it is only with the offering of a new framework that is based on them that these questions can be given importance. Based on his historical analysis, Kuhn concludes: “though logical inclusiveness remains a permissible view of the relation between successive scientific theories, it is a historical implausibility.”[30] Although it is tempting to believe that all scientific discoveries are adding to our scientific knowledge, in fact scientific discoveries contradict and undermine previous theories.

The incommensurability of paradigms is also necessary because there is no way to assess science outside of its paradigms. Humans cannot invent a neutral language that could describe science independently of our understanding of it, and any attempts to create such a language, though fascinating, end up being paradigmatic languages that are stripped of illogical fallacies but not of their scientific assumptions.[31] If there is no language but that of paradigms, we can’t step out of the paradigms in order to speak of them neutrally and show how their differences can be explained and mitigated. At the same time, change can only come from within the paradigm, because without the paradigm people won’t see the intricate problems that plague a particular theory. It

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[29] Ibid p.97
[30] K p. 98
[31] Ibid p.127

only the focus on and belief in the paradigm that enable the scientist to see the problems in the theory, and yet it is the paradigm itself that also prevents the scientist to reach new conclusions.[32] Because of this bind, paradigms must change and shift in order for new discoveries to be made.

The theory of shifting paradigms is an interpretation of science that many scientists criticize, arguing that they don't see this process in their own work. Kuhn responds by ascribing this blindness to the way people learn science. Science is learnt from textbooks, which describe discoveries as the natural consequence of their predecessors, often even confusing lessons learnt from new discoveries with the incentives that lead to their discovery. Textbooks intent on teaching scientists, rather than historians, seek to instill the assumptions and rules of the current paradigm that scientists will need in order to work effectively in their field. Questioning or even identifying the paradigm might be unnecessary for many scientists, but at least historians and philosophers should be able to see the evolution not only of specific discoveries but also of paradigms that enable them. Though scientists may fear that this approach will undermine the infallibility of science,[33] Kuhn disagrees and insists that understanding will lead to appreciation of the way science changes and what these changes require.

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[32] Ibid p.65

[33] Such as physicist Steven Weinberg, as quoted by Errol Morris, The Ashtray: The Contest of Interpretation, *The New York Times*, March 10 2011, http://opinionator.blogs.nytimes.com/2011/03/10/the-ashtray-this-contest-of-interpretation-part-5/

Like the paradigms he assigns to scientific discovery, Kuhn's history of science also has its own paradigm, a philosophy that underlies his approach to history and his interpretation of the discoveries he describes. And just like the scientific paradigms he describes, if one does not accept the philosophical underpinnings of Kuhn's theory, it is difficult to accept his interpretations. In this part of the paper I will discuss three aspects of Kuhn's philosophy of science – his epistemology that is based on paradigms, his view of science as a construction of a worldview, and the role of community in both epistemology and science.

Kuhn contends that his paradigms are meant to oppose a prevalent philosophical approach to epistemology. He explains that science is "entangled with a dominant epistemology that takes knowledge to be a construction placed directly upon raw sense data by the mind."[34] According to this prevalent view that originates with Descartes, knowledge of the world is based completely on our empirical evidence from our senses of the natural world, which is considered independent of the perceptions of the person who is learning. Though Kuhn concedes that there are merits to viewing the world as distinct from the person who perceives it, he insists that human learning is also based on the framework that a person has in which he or she can interpret and define the empirical evidence. Errol Morris, in his five-part critique of Kuhn, questions this view of paradigms, and argues that it is logically flawed because we could not attest to the existence of paradigms if we ourselves were in a paradigm.[35] And yet Morris is mistaken

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[34] K, p. 96

[35] Errol Morris, The Ashtray: Shifting Paradigms, *The New York Times*, March 7 2011, http://opinionator.blogs.nytimes.com/2011/03/07/the-ashtray-shifting-paradigms-part-2/

because, as we've seen, Kuhn does not argue that we cannot detect paradigms. He thinks we can become aware of them, though we can never overcome them.

Kuhn's epistemology influences the way he views science itself. As discussed above, Kuhn opposes the idea that science is an accumulation of facts about the world. Rather, he believes that science is the human descriptions of nature, based on the facts humans distinguish and the questions they ask. The facts we see are limited by our human theories, because we cannot separate between our perception and the facts that we see: "scientific fact and theory are not categorically separable, expect perhaps within a single tradition of normal-scientific practice."[36] There can be no accumulation of facts without interpreting them as well. Science does not discover a truth that is independent of human understanding, because in a very narrow field, where assumptions are already shared and taken for granted, can we separate between theories and the facts we are using to prove them. Given this view of science, Kuhn's view of the incommensurability of different scientific paradigms is now not hard to defend. If someone believes that science is the discovery of nature, then it does not make sense that different discoveries would not accord with each other. But Kuhn's incommensurability is directed towards the only science he thinks exists – the construction of knowledge dependent on the interests and assumptions of the people who do it. This science can vary from person to person, from time to time, and even contradict, depending on the goals and needs of the people who are doing it.

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[36] K p.7

It is illuminating to compare Karl Popper's popular theory of falsification with Kuhn's theory of paradigms. According to Popper, the social sciences offer theories that can reinterpret any fact to fit them, and therefore there is no way to argue logically or empirically that theories in these fields are wrong. In the sciences, on the other hand, only a theory that could be tested and proven wrong can be adopted after it is tested. Kuhn would disagree, since according to Kuhn scientists also work within paradigms that cannot be falsified. Falsification may exist in hard science, where the experiment are clear and easier to define, and where negative results are discarded as not useful for furthering a paradigm, yet science as a whole "…can [n]ever be exposed to all possible relevant tests." Scientists "ask not whether a theory has been verified but rather about is probability in the light of the evidence that actually existed,"[37] since they cannot test every theory with respect to the endless ways in which it could be falsified.[38]

Eroll Morris attacks Kuhn on this point, saying that Kuhn must not believe in science's ability to discover truth, or in truth at all, if he thinks that we only learn through paradigms.[39] Yet Kuhn is not claiming that there is no truth, or anything about truth's existence. Rather, he is arguing that that through history and our experiences we see that human understanding is limited in every realm, including in the scientific one, and therefore humans cannot view any particular understanding they have of the world as the truth. This is important to Kuhn because only if we acknowledge the limits of

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[37] K p.145

[38] Kuhn responds to Popper on p.146.

[39] Errol Morris, The Ashtray: Shifting Paradigms, March 7 2011.

human understanding can we encourage further learning and understanding, especially in the realms that require an undermining of current knowledge.

An importance consequence of Kuhn's epistemology of paradigms, and an understanding of science which views it as construction of the human mind, is that community becomes central to the way we learn and do science. Paradigms require shared assumptions and language, which can only be possible among a group of people who are working together. As he says, "Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all."[40] This means that science cannot be a universal, neutral system as it is sometimes presented to be. Rather, as we've seen, it depends on the cooperation of a particular group, and it is limited by the assumptions of that group.

Morris wonders if this view of science is a product of a long history of prejudiced and close-minded regimes and communities that limited science. According to this view, Kuhn's paradigms may exist under totalitarian regimes, where the scientists are not allowed (either by the government or by the other scientists) to suggest theories that oppose the prevailing ones, but paradigms would not be applicable to much of the scientific world today.[41] This reading of Kuhn turns the paradigm and the communal aspect of science into a tool used to gain power, which ultimately negates and limits science, rather than an intrinsic characteristic of science that is necessary for scientific development. Yet Kuhn's argument is that paradigms are required because human

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[40] K p. 210

[41] Errol Morris, The Ashtray: Hippasus of Metapontum, *The New York Times*, March 8 2011, http://opinionator.blogs.nytimes.com/2011/03/08/the-ashtray-hippasus-of-metapontum-part-3/

thought is limited, and because humans cannot advance without a framework. It is only with the limits of the paradigm that are shaped by a community that science can be taught, and scientists can focus on more detailed and advanced questions. "The insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve,"[42] since without the paradigm every scientist would have to start all investigation all over again. Moreover, it is only the entrenched theories of the community that offer the revolutionary scientist something to work against, since great discoveries consist of a change in the way a particular community views nature.

History of Plasma Physics Fusion

Scientists first began to experiment with fusion in the 1920's. They discovered that if two nuclei collide into each other with enough energy, they can fuse into a heavier nuclei. Normally nuclei repel each other since they are positively charged, but if they collide when they are hot enough, and therefore moving quickly enough, the repellence is overcome and the nuclei fuse. When this happens a neutron is released, as well as excess energy. Scientists also discovered that some atoms are more likely to fuse than others: unstable nuclei that have too many neutrons and thus are not balancing the repulsion of protons in the nuclei have a tendency to fuse in order to become stable. Particularly, they found that the element tritium, which is unstable, naturally has a neutron turn into a proton, and an electron is emitted, thus becoming helium-3. This

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[42] K p. 164

became scientists preferred source of fusion. In the 1930's, a physicist named Hans Bethe discovered that the sun created energy by fusing atoms of heated hydrogen into helium. (picture Herman p.255)

Fusion was the second kind of nuclear reaction that humans discovered – the first was fission, where a nuclei was smashed to create energy. This earlier reaction discovered was put to use in atom bombs that were dropped by the US on cities in Japan in WWII. In 1942 the physicist Edward Teller first suggested that a fusion bomb, or Super Bomb as it was called, could be seven thousand times more powerful than a fission bomb. He suggested a design, the first suggestion of utilizing man-made fusion, in which an atom bomb would be placed at the end of a vessel of hydrogen. The energy from the bomb would create such power that would fuse the hydrogen, creating large amounts of energy in the process. Teller's suggestions were not pursued, since scientists didn't know how to contain the energy and sustain the fusion process, and preferred to focus on developing fission weapons to end the war.[43] It took ten years till the US developed a fusion bomb, Ivy Mike, which was first tested in 1952 on an Island in the pacific, in a race to beat the USSR scientists who were working on a similar design.

Though fusion keeps us alive through the radiation from the sun, creating fusion on earth turned out to be very difficult. The first problem was a paradox – fusion required that particles would be very hot in order that they be moving quickly enough to fuse; yet heated particles spread and move away from each other, thus as they get hotter they are less likely to collide. The sun's huge mass creates an gravitational pull

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[43] Seife, p.14-15

powerful enough to pull the particles together though they are incredibly hot – but scientist would have to find another way to do it. The other problem was the bottle – what material could contain particles heated to the temperatures necessary to enable fusion? In the fusion bomb the energy from different explosions bottled the deuterium tritium for long enough to ignite and explode, but this would not be sustainable for energy use where the fusion reaction would need to be continuous.[44]

Scientists began being interested in solving these difficult questions in the early 1950's. At the time the US was becoming more desperate for a source of energy, since energy consumption was rising and it was estimated that by the end of the century the US would become dependent on foreign oil.[45] Yet the decisive incentive for renewed interest in peaceful use of fusion energy was competition, and a fabricated one: on March 24, 1951, the Argentinean dictator Juan Peron announced at a news conference that Ronal Richter had discovered how to sustain a fusion reaction, which would only be used for peaceful purposes. The report was soon discovered to be untrue, but the announcement convinced the US government to start a program to study the possibility of fusion energy. The Atomic Energy Commission (AEC) decided to accept the suggestion of Lyman Spitzer, the thirty-six-year-old head of Princeton University's astronomy department, but insisted that the research on nuclear energy would remain classified, arguing that it was related to the work being carried out to develop nuclear weapons at government laboratories and thus exposing it could pose a security threat to the US.[46]

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[44] Seife, p.75; Herman, p.28

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[46] Herman, p.22

Spitzer’s revolutionary design was based on a state of energy called plasma. In the 1800’s scientist realized that when gas is heated to million of degrees centigrade it reaches an ionized state in which the bonds between the electrons and the nuclei of the atoms are loosened. Like gas, plasma does not have a definite shape or volume, but it is a distinct state of matter because it is electrically conductive, and magnetic fields cause it to form into particular shapes. In nature, plasma is found in flames, lighting bolts, and in most material in space, such as stars. It is extremely difficult to control, since its charged particles create magnetic fields, that in turn effect the direction the particles move in. This change in direction affects the magnetic fields, etc. Moreover, no material on earth could withstand the heat necessary for a plasma, and as soon as plasma particles would touch another material they would cool down, preventing a continual fusion. Yet since plasma is so hot Spitzer realized that it would be ideal for creating fusion, and his revolutionary suggestion was to use magnetic fields to contain it.

The Stellerator, the first suggested design for a machine that could create plasma fusion, was a metal donut-shaped bottle, surrounded by a wire. The wire was shot with an electric current, creating a magnetic field that prevented the plasma from touching the sides of the bottle, and kept it moving forward. The donut shape was meant to ensure that the plasma was not lost, but rather could be used continuously. While the plasma was moving it was heated up and injected with spurt of helium, so that fusion could occur. One of the main weaknesses of the design was that the wire surrounding the bottle was spread out on the outside of the torus and close together on its’ inside, thus generating an uneven magnetic field that created disturbances in the plasma that caused it to whip out of control. [47]

Another design, which was worked on at the government laboratory at Livermore, was the Mirror machine. Trying to avoid the unevenness of the torus shape, the Mirror Machine retained its tube bottle. In order to enable for continual use of the plasma, the magnetic fields of the machine were teaked, so that they were stronger in the edges and weaker in the middle, thus reflecting the plasma into the middle of bottle.

The third design for a fusion reactor was developed at this time in Britain, and brought by the scientist James Tuck to the government laboratory in Los Alamos. The Perhapsatron was based on a the pinch theory, which sent an electrical current through the plasma, rather than creating a magnetic field around it. The magnetic current caused the plasma to be pushed into the center of the bottle, thus heating and compressing it at the same time.

All of these designs had to contend with two major questions. The first was how to heat the plasma to the necessary temperatures. Not only were these temperatures hard to achieve, but the plasma proved especially hard to heat because the light electrons heated up more quickly than the heavy nuclei which were the ones necessary to accelerate in order to create fusion. The other, unexpected, problem was the problem of stabilizing the plasma. The plasma must be heated and charged evenly, otherwise any little defects very quickly turn into instabilities that cause the plasma to

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whip out of control, touch the sides of the bottle, and cool it off, preventing it from reaching the necessary conditions for fusion. Scientist tried to build bigger and bigger machines in order to overcome these difficulties.[48]

The AEC decided to sponsor all three of these suggestions under the umbrella of Project Sherwood, a classified program that was supposed to allot each design a few hundred thousand dollars per year. By 1955 the American government was spending five million dollars on Project Sherwood, and ten million by 1957. In this it heralded the change all over the world of the effort to use fusion for peaceful means. Simliar project were being developed in Britain and the USSR. Hopes were high – in 1955 the UN held its first Conference on the Peaceful Use of Atomic Energy, where delegates claimed that fusion could be reached within two decades.[49]

The new field of plasma physics was publically acknowledged, and the secrecy of the American government lifted, and the USSR uncovered its discoveries, at the UN sponsored convention that took place in Geneva in 1958. Atoms for Peace brought together the leading scientists working on plasma from all over the world, to discuss their work and future collaboration. According to Herman, "the Geneva conference marked the birth of the world fusion community, and from that time forward fusion curiously became almost a sacrosanct kind of cooperative endeavor for both the scientists and their government backers."[50] This convention and the unveiling of the research program was especially important in creating a discipline for which students

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[48] Seife, p.104-106

[49] Seife, p.88-89

[50] Herman, p.61

could be recruited. Following the convention Princeton University opened the first gradate program in plasma research, and students were recruited at the American Physical Society. “Plasma physics became a recognized intellectual discipline in the mainstream of physics. There were suddenly a new infusion of ideas, and new infusion of people.”[51] Though there were no textbooks yet, since the plasma research was classified till then, Columbia University soon followed and opened its own graduate program, training graduates who could work in the field. In 1961 the UN’s International Atomic Energy Agency (IAEA) sponsored the first “Atomic Olympics”, where the machines and inventions of the worldwide efforts were presented.

After the great advances of the 1950’s, the 1960’s proved to be a decade of disillusionment. The plasma proved to be much more difficult to control than anticipated. Large sums were spent on building bigger machines, such as the C-Stellarator in the US, yet the larger machines seemed to suffer from the same instabilities that were beleaguering the smaller ones, so that the plasma was not confined long enough to create fusion. The Perhapsatron was good at confining the plasma, but was not good at compressing and heating it, while the Pinch machine was able to compress and heat, and even reach fusion, but not maintain this situation. The many questions marks caused scientists to become more conservative, and they “started to pull back from its original, concrete goal of a commercial fusion reactor into the more rarified world of atomic physics.”[52] This change was symbolized by the

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[51] Herman, p. 63
[52] Herman, p.79

retirement of Spitzer, father of the first Stellarator. Spitzer explained that he was not fit to help in a field which had turned to minute mathematical calculations and experimentations rather than bold designs that would produce machines that could produce energy.[53]

The breakthrough came from an unexpected source. The Russians had been working for years on a design called the Tokamak, which combined the strengths of the Perhapsatron and the Pinch machine. The Tokamak was made up of a torodial tube, like the Stellerator, but it both ran an electrical current through the center of the plasma and had a coil around the bottle generating a magnetic field. American and British scientist discredited the Tokamak since they thought it was too complex for experimentation and would not be able to produce enough energy even to match the great energy required to send the current through the plasma. Yet though the different electrical currents were difficult to calculate and control, the Tokamak was nonetheless able to achieve a more stable plasma, with a temperature of ten thousand degrees instead of the few million that were achieved on other machines. The squat shape of the Russian machine allowed for wider plasmas and a shorter trip around the donut. Better cleaning of the metal tube, which prevented cooling, and better calculations of the alignment of the magnetic fields, also contributed to the success of the advanced machine. (picture?)

Russian scientists had been working on the Tokamak for almost two decades, when its success was finally proven at the Plasma Olympics that took place in Russia in

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[53] Herman, p. 80

1968. Though the American were still dubious of the success, the AEC directed Princeton to put away the Princeton Stellarator, which had just been completed after 8 years of work, and convert it into a Tokamak, so that they can disprove the Russian and stop them from reaching fusion first.[54] After the Tokamak's success was proven by a British delegation in 1969, Tokamaks were initiated across Europe, the US and in Japan.

The success of the Tokamak started a new age of plasma research in the US, combining big hopes and a lot of money into the era of Big Science. In 1970-1972 five Tokamaks were approved in the US, each estimated to cost tens of millions of dollars. These machines were meant to be much larger that previous machines, in order to overcome the plasma instabilities. Thus the Princeton tokamak could hold a plasma one meter across – 45 times bigger than the plasma created in Spitzer's machine. But this was just the beginning – in 1974 Princeton got a grant to build the Tokamak Fusion Test Reactor (**TFTR**), the biggest tokamk ever built. Though scientists did not think they were ready to do research on such a machine, which was estimated to cost $200 million[55], the international competition convinced them that they should try building one anyway. Though large grants were also given to other projects, over time the US made drastic cuts to the other designs, so that "they obliterated almost everything that wasn't part of a tokamak project; the nation put almost all its magnetic fusion eggs in the tokamak basket."[56]

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[54] Herman, p.94

[55] Herman, p. 108; Seife reports that the estimate started with $300 and grew way beyond that, Seife, p. 163-164

[56] Seife, p. 203

US support of fusion research was not merely a craze of a few scientists. Energy creation became a presidential concern in the 70's, and starting with Richard Nixon's presidency fusion was the preferred path to energy. It had two promising aspects: the first, it was more environmentally friendly, since it did not create much radioactive waste (though the metal machine did have to be replaced every few years). The second, it was safer, since it was so hard to continue the fusion process, and any cooling or discontinuation of fuel would automatically end the reaction.[57] In 1980 president Jimmy Carter signed an agreement to double the annual fusion budget of $400 over the course of seven years.[58]

Notes on Herman:

Key terms to appear in scientific descriptions of plasma-physics:

Plasma
Fusion
Lasers
Breakeven
Tokamaks
FRC
Instability – when the plasma goes out of control because small disturbances in the flow of particles grow and breaks out of the magnetic fields that are holding it

"Anyone stumbling on this fraternity would have discovered men who measure time and commitment in decades." Or longer. People talk of the ITER era, already 25 years. 1 American fusion pioneer Richard F. Post, "Once we have learned how to tap it, fusion can supply man's needs for energy for thousands of millennia, until, and even after the sun grows cold.Baloney" 5 until this plasma existed on earth only when lighting heated the atmosphere into fleeting plasma

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[57] Herman,

[58] Seife, p. 164

Herman – “Gripped by a sense of urgency, the scientists claim they were thwarted in their efforts by political and economic short-sightedness.” When it was their dimwitted greed, scientific naivety and abominable judgement9

New language (p.11):
Mode, MHD and DCLC instability, resistive kink instability, 10 million degree plasma is “cool”, grain of dirt is a “boulder”, plasma that floats 25 centimeters into the chambers surface is “lost”. “It was apparent from the language of plasma physics [nice insight] that the field was still a new, evolving science. In a desperate effort to communicate with one another about new sights and ideas, the physicists appropriated whatever words and images came to mind.”

Timeline:
1920’s – first experiments
1950’s – first experimental fusion devices (p. 25)
1951 – Spitzer, Princeton, **Stellarator** (explanation p.20-21, $1million p.23), at first don’t know how to measure (p.28)
At the same time Sakharov invented the **tokamak** in Russia (magnets from inside and outside, p. 36)
ZETA in Britain
later machines took a decade to build
1958 – UN sponsored international convention on fusion, **Atoms for Peace** in Geneva
“The Geneva conference marked the birth of the world fusion community, and from that time forward fusion curiously became almost a sacrosanct kind of cooperative endeavor for both the scientists and their government backers.” P.61 Countries were willing to share as long as no seemed to have a clear advantage 61
Secrecy is lifted, makes it easier to recruit young scientists, graduate program is opened at Princeton University, “We could finally tell our colleagues in the physics department [plasma physics is rarely in the physics department. It is not enough high brow] what we were doing.” Said Ed Frieman, Spitzer’s top theorist. [When Furth became diretor of PPPL, Ed left to become head of the Scripps Oceanographic Inst in San Diego.]“Plasma physics became a recognized intellectual discipline in the mainstream of physics. There were suddenly a new infusion of ideas, and new infusion of people.” 63 this change seemed to happen not because of the science but because of the politics. There are no textbooks yet, because classified, but Post goes out to recruit people at the American Physical Society. Gross hears him and starts the fusion research program at Columbia University and producing graduate students in the field. 64-65.
1961 – First “Plasma Olympics” of the UN’s International Atomic Energy Agency (IAEA) mirror machines Post, “Fusion is to big an objective to think you’re omniscient about how it’s going to be solved.” P. 71
the 60’s were a hard decade, where it seemed that fusion will not be attained because of instabilities $36 million build the C-Stellarator in the US, in hopes of increasing confinement time of the plasma p.72. yet experiments show that the larger machine had the same problems - the hotter the plasma the faster it slipped away. Rethinking

the basics: Harold Furth at Livermore: “People were calculating these wonderful machines, and they turned them on and they didn’t work worth a damn… that is because within those laws of classical physics and electromagnetic theory there’s room for all kinds of phenomenon which people just hadn’t thought about. They only thought about the smooth equilibrium, but, in fact, the plasma were capable of all kinds of nasty, turbulent behavior.” P.73 yet the theory of “finite resistivity” cannot be put to use because they don’t know how to calculate it. P. 74.
“scientists were caught between the instinct to understand before acting and the desire to progress quickly towards a concrete goal.” P.77 “After such high hopes nad so much failure, a conservatism had gripped the community. Rightly or wrongly, it started to pull back from its original, concrete goal of a commercial fusion reactor into the more rarified world of atomic physics.” P.79 (balloon to moon allegory)
Spitzer leaves – he was good at the ideas but isn’t good at the extensive and detailed math now needed (end of a paradigm shift?) [Garbage – self-aggrandisement by theoreticians Theory , detailed math, rarely lead the major breakthrough!] p. 80

1968 – only Soviets are working on tokamaks, other scientists don’t think they are practical because 1. Not good for experimentation 2. Not constant energy source (p.83). yet a **hotter and more stable plasma is being achieved**. Works better than the Stellarator bc of 1. Fat, squat shape; works better now bc 1. Better cleaning 2. More precise magnetic field alignment. While results are still being tested, Princeton follows the Atomic Energy Commission to put away the Stellerator and build a tokamak, mostly to disprove the Soviets. Move from “a few million degrees on the old machines to 10 million degrees on the tokamak.” Right shape, and lasers to measure the temperatures (Britain).
The soviets don’t quite understand what the plasma is doing in the tokamak, but bigger is better because they think that “scaling laws” mean that if the plasma is bigger, it takes the particles longer to get to the edge of the plasma and cool off.

US decided to go with the tokamak even though not clear how works – Big Science. In 1970-1972 five tokamaks are approved, which are estimated to cost tens of millions of dollars each. P. 99. Princeton’s is the biggest, meant to hold a plasma a meter across, 45 times bigger than the plasma made by Spitzer’s team. 102
Power shortages push Nixon to make nuclear power the centerpiece of his energy plan. At first foucs on fission, but fusion is safer bc (p.103) 1. Doesn’t create radioactive waste products 2. Little fuel means that if stop feeding it stops ? 3. Very hard to sustain, if goes wrong cools off and stops. The way to do it is look for the practicality, not the science.

1974 – Princeton get’s the money to build the Tokamak Fusion Test Reactor (**TFTR**) which is the biggest ever built, but not big enough to actually do fusion. Princeton does this because of the competition, not because they think they are ready to do the research on this scale (p. 108). Will cost $200 million. Another grant is given to Livermore to build giant mirror machine that will cost about $100 million. [The mirror machine was shut down without ever making a plasma.]

“competition was a vital force in keeping the spirit of the fusion mission alive… Without a rival, a scientist facing a lifetime’s battle with the plasma might give in to intellectual exhaustion. As an applied science, fusion’s goals were well defined and calculated. The scientists could easily chart their own progress and measure themselves against others.” P.152-153 normal science? Different because it is applied science?

p. 157 – in 1983 MIT achieved minimum density and confinement time necessary for fusion – though not in temperatures that would allow for breakeven (fusion for long enough that would create more energy than used). [Look how long it has been – so little progress!]

1984 – creation of the **JET** (Joint European Torus) international reactor

in the 80’s the worlds’ fusion researchers were spending about $1.3 billion dollars per year, mostly backed by governments.
A lot of competition, since countries are working on the same machines. A lot of practical experiments to be done on these big machines that are being built. “It was all a very human attempt to impose an order on the turbulent, seemingly haphazard movements of the gaseous plasma – to find logic and consistency where perhaps there was none at all. Behind the effort was an unquestioning belief that an order, a pattern, indeed a law existed that explained in mathematical terms the writhing of the plasma. Moreover, the group believed it had the collective intelligence to perceive that pattern. [If it worked, a child could see the pattern.]” P. 145 “The Princeton plasmas were the most fully described plasmas in the world, and with each shot came reams of data.” {True]P. 146

1984 Olympics in England – “Data were more plentiful than ideas.” (H 161) “All improvements had been arrived at by “brute force”, by applying more powerful heating methods and magnetic systems or by using larger machines. The reporters to the international audience did not contain advances in the theory of plasma behavior.” 168
Furth points out the need to go back to understanding the basic physics “The world was wrapped up in a hardware competition. It should also be wrapped up in a competition of ideas.” [Good point Harold. Still valid today.] 169
Princeton is unique because of its neutral beams.[Others got them later.]

p.184-187: the radioactivity of fusion – much less then fission in some areas. Less then oil? ()

- Fuel: radioactive tritium (not used yet, during experiments)
- Container of the reactor becomes radioactive from the flying neutrons, would have to be buried but could be salvaged after 100 years

1983 Lawerence Lindsky Lidsky publishes an article in Technology Review (MIT).[Read the original. The last paragraph is perfect – he says only neutronfree fusion is worth pursuing.] He thought that the fusion reactors are too complicated, and could not be used for creating power. Need “small, nonbreeding fission reactors, burning natural uranium from seawater” (188). 191:

- Fusion more complicated and therefore more expensive than fission
- Engineering problems: radioactivity of the vacuum vessel, magnets on the outside must be cooled to almost zero and therefore are sensitive to all heat from the outside
- Complexity will cause constant malfunctions which will not happen in the simpler, though more dangerous, fission

Response of the fusion scientists: shoot his arguments down (led by Howard Furth, director of Princeton’s lab)

193-204: private backers in financial and spokesmen capacities.

194: get rid of the shielding and create a small, powerful and disposable tokamak (Robert Bussard, Inesco). OOOOPs – I forgot Bussard worked on this

Because of the budget cutting of the Reagan era the non-immediate fusion program is vulnerable 181

1. been going on for a while – always twenty years to success
2. less of an oil crisis
3. Reagan cutting budgets
4. Changing theory of what to fund: “Funding basic research, as opposed to applied science, was a more natural role for government.” 205 Keyworth, Reagan’s science advisor, wants more understanding rather than engineering

In a speech in 1985 to the American Nuclear SocietyL Robert Hirsch (director of the US fusion program from 1972 to 1976) also comes out against the impracticality of the tokamak He certainly did – scientists have become more interested in proving the tokamak’s success than in finding the solution of how to create a useful reactor He got that right “the point of fusion is not to make tokamaks work, but to make a product that is going to be useful, economic, reasonable and desirable when it’s done… You’re trying to make a practical result. I think the program has lost sight of that.” Right on Bobby!!! (p.211) claims that he wants what’s best for the program, but viewed as a threat.

Questions for Professor Cohen:

1. What does he think about the theories of small tokamaks – Lindsky and Bussard, MIT
2. What is the difference between the reacotrs (such as JET) and fusion test machine (ITER)? (Herman 224)
3. Is what he is doing more engineering or science? Would the paradigm shift theory apply to engineering?

p.216 – mistake of cleaning the vacuum vessel more than usual allows for soaring temperatures, going from 80 million in 1980 to 300 million by 1988. Challenge now is "to improve the confinement time and density of the plasma at these high temperatures and neutron levels". (Furth, p.218-219) but "Such hot plasmas were not very dense and did not last more than five-tenths of a second before escaping the magnetic bottle" (how long do they need for ignition of fusion?) p.227

Tokamaks need to be rebuilt because of radioactivity, and the government is cutting the budget…

Craze over "cold fusion" that turned out to be a lie (p.228), but brough fusion on the public's attention (234).

Herman: "it is unfortunate that years were fritted away in efforts to define plasma *before* building new machines." 235 yes – as you note she meant big machines. But little ones could have been built. The tokamak folks did not want any competition.
"an evolution of how scientists think about fusion is what is really needed." 235-236. People need time to evolve the way they think about something, before they can get to the ideas that seem so obvious at the end. Many of the older, retiring scientists, are quoted as regretting that they didn't build bigger machines, though Lyman Spitzer points out honestly that no one wanted to give the fund without better understanding and better results 238. He says that we haven't yet proven that fusion is feasible physically, but that "A fifty percent probability of getting a power source that would last a billion years is worth a great deal of enthusiasm." 238-239 How about 0.1 % probability?

Herman calls for a commitment to an international reactor, Baloney – competition makes ideas grow. Committees kill ideas. the one that is being planned by ITER, because she thinks that only joint work and construction can prevent the effort that has been put into fusion until now from going to waste Baloney- Gresham's law Bad money drive sout good money. Stop wasting money on tokamaks. – the former generation is growing old and are not attracting new students we actually are attracting excelent students because there are no exciting new projects. "I have come to feel that the fellowship [of fusion scientists] – there is no fellowship only a few leaders manipulating the funds hence the herds of docile sheep-like physicists has itself to criticize for the unmet promise of fusion." She claims that the scientists are lost in the beauty of the plasma, rather than excited to forge forward and try to build a reactor that would work. 240

Though Herman is right that if there are no new directions Yes that is the problem, no clear alternate – BECAUSE the tokamak leaders prevent even small projects from making progress to challenge the tokamak, transmission of experience and attraction of new students will dwindle. And yet it seems problematic to build bigger and bigger machines, when it is not proven that bigger is necessarily better. This would be the right direction to go if we saw that better results were consistently found in bigger machines. Yet this is not the case, and moreover, the end goal is not to have huge machines that would be very complicated and expensive, and could not feasibly be replicated in order to make a profit. If the machines are so big and have not worked, perhaps it is time to try something that would be close to the final goal?

It seems that Herman is blaming scientists for falling into “normal science”, Nice connection -Astute the solving of problems within the same paradigm that cannot bring to a new paradigm. And yet her call for larger machines would not allow for a new paradigm either. Rather, she is really lamenting the love of science she misunderstands scientists – they would LOVE to do both – pretty physics and useful physics that comes in the way of engineering solutions to problems. And yet perhaps what is needed is not less theory, but rather new theories or pre-theories, sometimes called ideas . Perhaps the calls of Bussard and Li~~n~~dsky and Hirsch should be heard, saying that new directions must be found that will bring scientists close to the final reactor they want to achieve.

Should I address in this paper the question of engineering versus science? let’s see where your research goes It seems that much of the work that is done building the reactors (does Professor Cohen plan them himself? If Hashem pleases) has to do with engineering, while the discoveries made using the reactors would be termed science. And yet is this true? Perhaps realizing that one can confine plasma with certain magnets is a scientific discover, not an engineering one? Perhaps any discovery that allows to control something that has not been controlled before is actually science, rather than engineering that just repeats something that someone else has already done? Would Kuhn’s theory also apply to engineering? Probably Does it matter? He seems to think that his theories of paradigms already existed in other fields, and he is just applying to science the way we think and discover, that people think does not apply to science. Can we deduce from this that it would apply to engineering to, and therefore the differentiation of the two is not important when trying to fit plasma physics into his paradigm. And yet it could be helpful to mention/discuss the intersection of science and engineering when explaining plasma physics.

3. (for Professor Cohen) It’s interesting that unlike other sciences, plasma physics was born with a goal in mind. Are there other examples of this? Many – a clear one is medicine, another is biophysics another is condensed matter physics. Another is chemistry. Math had lots of practical motivations – measuring land, calculating …The Nobel prize was set up to reward useful science. One winner invented a better buoy.. What makes it science?