Proposal to Teach Lakatos-Feyerabend-Kuhn Philosophy of Science

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1. Synopsis

It is proposed that the Lakatos-Feyerabend-Kuhn philosophy of scientific research programme (abbreviated 'LFK Programme' or 'LFK Theory') be taught at the University of Minnesota.[2] The LFK philosophy of scientific research programme created and shaped modern philosophy of science. Nearly all modern terminology used by philosophers and historians of science was first used as part of the LFK programme. Examples are: 'paradigm', 'research programme', 'problem shift', 'scientific revolution', 'period of normal science', 'demarcation problem' and 'quasi-empirical nature of mathematics'. No seminar in LFK theory philosophy of science is currently offered by the University of Minnesota, but because outside of Professors Lakatos, Feyerabend and Kuhn, the most important advocates and proponents of the LFK Theory were former Minnesota Center for the Philosophy of Science (MCPS) directors Herbert Feigl and Grover Maxwell, it should be taught.[3] Surely, if former MCPS director Maxwell and Lakatos had not both tragically died at early ages, the LFK programme would currently be taught.

Since the LFK programme simultaneously philosophizes, respects historical analysis, and provides methodological prescriptions for the practice of science, it should be taught using the

2. This essay is the rewritten form of a proposal by the author to teach a graduate seminar on the LFK Programme. The original proposal was written in the first person. The current impersonal form attempts also to tell the story of the elimination of the LFK Programme in the US. Since the original proposal was submitted in late 2003 to the University of Minnesota faculty without any response, it is unlikely any LFK Theory will be taught in Minnesota. It is hoped that the LFK research programme will be taught somewhere.
3. Feyerabend[1994], 116-118.

seminar rather than the lecture instructional form. The seminar should be open to students of science, philosophy and history. Required texts should be two volume collected works by Lakatos and two volume collected works by Feyerabend.[4] These four volumes of collected works concisely express nearly all of the LFK programme. Attendees would previously have studied Kuhn's book on the structure of scientific revolutions.[5]

The class should be taught in casual style of Feyerabend. Also following Feyerabend, criticism must be encouraged - including criticism of the LFK methodological research programme itself.[6]

2. Disappearance of LFK Programme

This essay is critical of the academic reaction against the founders of the LFK programme and academic behavior toward the essay's author and other young scholars who wished to continue development of the LFK programme. The criticism is needed because it establishes that the teaching of a philosophy seminar can lead to rapid scientific progress in theoretical science especially at the University of Minnesota. It also explains why it is possible to convince the US Federal Court to award an injunction for mathematical software while the technical papers expressing the discovery do not get accepted in any peer reviewed scientific journal. The academic misconduct against the LFK programme is a story that needs to be told and documents academic behavior that should not occur in a democracy.

It is believed that the following explanation for the disappearance of the LFK programme and for problems encountered by young scholars of methodology in the 1970s and 1980s shows a pattern of conscious elimination of the LFK research program. This proposal assumes awareness of a number of historical events relating to the LFK programme. First, LFK programme advocates were not hired by any of the more distinguished US Universities.[7] Next, although Thomas Kuhn was eventually hired by Princeton and MIT, he was denied tenure by the UC Berkeley philosophy department in the early 1960s.[8]

Finally, after Lakatos death in 1974, the final step in the elimination of the LFK programme was elimination of anti-formalist mathematical thinking at Stanford University and UC Berkeley. George Polya at Stanford and Alfred Tarski at UC Berkeley were two of the most famous mathematicians of the 20th century. As they reached the ends of their careers they tried to perpetuate anti-formalist and heuristic epistemology. Lakatos thesis topic was suggested by Polya in the late 1950s.[9] Polya then continually encouraged Lakatos throughout his career at London School of Economics (LSE).[10] Feyerabend visited Stanford during the summer of 1968. Tarski also encouraged Lakatos and visited him often in London.[11] Stanford mathematicians and physicists who had emigrated to the US with assistance from the Rockefeller Foundation to escape Nazism in the 1930s encouraged and facilitated the LFK programme by supporting and agreeing to be interviewed for the Archive for the History of Quantum Physics (AHQP) project. Thomas Kuhn was the editor and interviewer.[12]

4. Lakatos[1978], Lakatos[1978b], Feyerabend[1981] and Feyerabend[1981b].
5. Kuhn[1962].
6. Feyerabend often expressed his view by baiting his critics to make mince meat of his arguments.
7. See letters from Lakatos to Noretta Koertge on her application for a teaching job at MIT. Cf. Section 11.3 archive summary list item 1 and archive 13/489:58 and 13/489:117-118.
8. Andresen[1999], 564.
9. Lakatos[1976].
10. <Need Polya letter references from Lakatos Archive>.
11. Feyerabend[1994], 130.

The most important result of Polya and Tarski's anti-formalist research programme was creation of a UC Berkeley computer science department (CS) that was established as part of the school of literature and science (L&S) rather than as part of the engineering school.[13] The department was established to study basic philosophical questions relating to the algorithmic part of mathematics in the natural philosophy tradition. The department was successful beyond the the wildest hopes of the Polya-Tarski-Lakatos anti-formalist research programme because the P=NP? question and many still superior algorithms were discovered and before the department was annexed by the engineering school's electrical engineering (EE) department. The annexation used the same tactics used by Nazis in the Austrian Anschluss including using the name EECS to label a department which immediately was changed to study engineering problems. The 'counter reformation' that eliminated the LFK programme functioned by allowing the founders of LFK to publish but prevented younger scholars from continuing development of the LFK programme. The result of this elimination is that progressing anti-formalist mathematics and science research programme's were replaced by the current degenerating formalist and anti-scientific study of engineering.[14]

A simple solution that would re-open scientific research and eliminate suppression of anti-formalist ideas would be to eliminate all granting of ph.d degrees by engineering schools and especially by EE departments that have historically seen any topic studied in universities as targets for annexation. for the solution to succeed, all academic departments must be monitored to eliminate awarding ph.ds involving for profit engineering and business endeavors.

3. What is Modern Philosophy of Science

Ancient Greeks studied philosophy of science and mathematics, but science and mathematics were not separated from general philosophy. History has been recorded and interpreted since the beginning of human language, but history and philosophy of science were not studied as a separate discipline until the 19th century.[15]

The first systematic study of philosophy of science as conscious study of method probably began near the end of the 19th century. Feyerabend attributes the first conscious study to Ernst Mach[16] Also, the European founders of modern physics especially researchers at universities in which physics was studied as natural philosophy used methodological analysis. It is possible that methodological study occurred even earlier in Minnesota because explicit methodological study is present in the writings of Thorsten Veblen.[17] The Mayo Clinic in Rochester studied medical

12. Kuhn[1963]. Also see Shockley[1966] for a anti-formalist introductory mechanics text book. One of Shockley's favorite topics in his freshman seminar was warning students to avoid EE departments at all costs.
13. Grouping mathematics with natural philosophy was universal since the beginning of western thought but was changed as part of the elimination of anti-formalist research in US Universities in the 1970s.
13. This essay's author received a math and physics B.S. from Stanford and had passed his orals in the UC Berkeley L&S department before the annexation. As a Stanford undergraduate, the essay's author was actually encouraged to avoid studying set theory. See below for more detailed discussion of how the elimination of the LFK programme has destroyed theoretical science in the US. Much of the discussion below involving anti-formalist mathematics comes from the author's recollections. Therefore, much of this essay should be viewed as the author's testimony.
14. The elimination of the LFK programme has been marked by academic white collar lawlessness and refusal to provide protection of the legal and constitutional rights of anti-formalist scholars just as the Nazis gained power by white collar crime before their true colors were revealed.
15. This commonly assumed fact is actually an open historical question because such explicit study may have occurred much earlier.
16. Feyerabend[1981b], 89-98.
17. Mestrovic[2003].

methodology at the end of the 19th century and kept systematic medical records for all of southeastern Minnesota and northeastern Iowa by 1905.

In spite of the anomalies created by Einstein's theory of relativity and modern physics in general, until the 1950s mainstream philosophy of science meant study of proven knowledge. During the first half of the 20th century, study of philosophy of science meant study of formal logic. After WW II the study of philosophy of science as explicit study of methodology using historical case studies of scientific discoveries emerged. Although Popper's theory of naive falsificationism was first described in 1934,[18] it did not become popular until the 1950s. Lakatos characterizes Popper's discovery as understanding the implications of the fall of Newtonian Physics:[19]

> Popper's distinction lies primarily in his having grasped the full implications of the collapse of the best-corroborated scientific theory of all times: Newtonian mechanics and the Newtonian theory of gravitation. In his view virtue lies not in caution in avoiding errors, but in ruthlessness in eliminating them.

4. The Lakatos-Feyerabend-Kuhn (LFK) Research Programme

During the 1960 and early 1970s there was a renaissance in the philosophy of Science that culminated in Imre Lakatos' theory of scientific research programmes. The theory was both descriptive because it explained historical scientific progress and prescriptive because it provided methodological analysis to guide scientific practice. The research programme abstracted and superseded all previous work in the philosophy of science because it was expressed in terms of the age old debate between rationalists and sceptics. It superseded all previous work because the theory itself is a research programme that can test philosophical and historical theories of science while itself being testable.

Lakatos' Ph.d. thesis showed that historically mathematics had been studied as a quasi-empirical activity. By the mid 1960s, the quasi-empirical nature of mathematics as an alternative to the axiomatized formalist view was accepted by nearly every practicing mathematician. The Lakatosian programme in mathematics can be viewed as a reformation against the one true 'religion' of mathematical certitude. Lakatos then moved on to study philosophy of science. By 1960, Feyerabend had already progressed from studying Wittgenstein to methodological anarchism (renamed as methodological dadaism later because of the negative connotations associated with the word 'anarchism'). Historian (but trained scientist) Thomas Kuhn published his epic book *The Structure of Scientific Revolutions* in 1962.[20] The book was the first widely discussed challenge to the view of science as continual accumulation of ever improving knowledge and strongly influenced research methodology during the 1960s. The collaboration at the Univerity of California Berkeley between Feyerabend and Kuhn is widely known, but it is not widely known that Lakatos and Kuhn saw their work as part of the same research programme. Letters in the Lakatos archive at LSE make the connection clear.[21]

For example, in his book on Kuhn's philosophy, Hoyningen-Huene explicitly waives systematic exposition of Kuhn's influence and waives parallels with the work of other authors in either direction.[22] Hoyningen-Huene never mentions the term 'research programme' and even

18. Popper[1959] is updated English version. Originally published in German in 1934.
19. Lakatos[1978]d , p. 8.
20. Kuhn[1962].
21. Cf. section 11.1 archive summary below and 13/512:148-149 and 13/512:163.
22. Hoyningen-Huene[1993], preface xxx.

claims Lakatos misunderstands Kuhn. Laudan[23] discusses Kuhn (pp. 73-76) and Lakatos (pp. 76-78) in separate sections and calls Lakatos theory ’an alternative theory’ (p. 76). However, tellingly he seems to claim that Lakatos refutes Kuhn and Kuhn refutes Lakatos. This proves the two theories are part of the same LFK research program (pp 73-78). The Lakatos logic of Scientific discovery was then proposed in the late 1960s to defend scientific rationalism from the sceptical criticism of Feyerabend and Kuhn. A cornerstone of the LFK programme is use of the word ’logic’ in its traditional intuitive sense that goes back to the the Greeks. The theory shifts the meaning of ’logic’ away from its meaning as formal mathematics used by logical positivists and other formalists. The founders of the LFK programme viewed themselves as independently working on different sections of the same ’building’.

The different elements of the LFK research programme are best described by the founders themselves. Lakatos succinctly describes the LFK programme in his essay on the history of science:[24]

> According to my methodology the great scientific achievements are research programmes which can be evaluated in terms of progressive and degenerating problemshifts; and scientific revolutions consist of one research programme superseding (overtaking in progress) another. ... The basic unit of appraisal must be not an isolated theory or conjunction of theories but rather a *’research programme’*, with a conventionally accepted (and thus by provisional decision ’irrefutable’) *hard core* and with a *positive heuristic* which defines problems, outline the construction of a belt of auxiliary hypotheses, forsees anomalies and turns them victoriously into examples, all according to a preconceived plan.

In a letter to Paul Feyerabend dated March 2, 1973 Lakatos defends his methodology against Feyerabend’s criticism:[25]

> [...] On studying your *Against Method* I am increasingly worried. To my mind you have a basic weakness in your position which is at least as bad as mine. If you were consistent you would have the courage to be a sceptic. For the first time to my knowledge, you now say that epistemological anarchism cannot be equated with scepticism. If so, I shall prove that epistemological anarchism is double faced. One face is the face of a sceptic, the other is the face of a Kuhnian authoritarian. I am terribly sorry about this, but you either return to complete scepticism or I shall show that you are inconsistent. All that I can promise is that I shall do it with a light touch so that you will be killed and most people will believe that you are being praised ...

In his 1962 book on scientific revolutions, Kuhn began development of the methodology of the LFK programme:[26]

22. *Ibid.*, note 142, p. 152.
23. Laudan[1977], page numbers in parentheses below refer to this book.
24. Lakatos[1978], 110. A footnote refers the reader to his essay ’Criticism and the methodology of scientific research programmes’ (Lakatos[1968]).
25. Lakatos[1999], p. 323. Quotation appears on back cover of book jacket.
26. Kuhn[1962], 2nd ed., p. 6.

> In these and other ways besides, normal science repeatedly goes astray. And when it does-when, that is, the profession can no longer evade anomalies that subvert the existing tradition of scientific practice-then begin the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science. The extraordinary episodes in which that shift of professional commitments occurs are the ones known in this essay as scientific revolutions. They are the tradition-shattering complements to the tradition-bound activity of normal science.

In a letter to Lakatos dated February 10, 1973, Feyerabend expresses his sceptics role within the research programme:[27]

> So--forget about rationality and find out what is was that made everyone accept Einstein's research programme and abandon Lorentz's. 'Everyone,' this means a few big shots in England, Germany, France, for the rest are content with the Lorentz transformations and E equals mc^2 tacked onto it; that is, they are content with some purely formal tricks and would not even know the difference between Einstein and Lorentz. That Lorentz turns out to be not ad hoc at all, but progressive, pleases me very much and cheers me up on an otherwise rainy day. 'That rationality must lie in the *extra merits* of Einstein's theory,' says sir Epsilon Omega [Zahar] and I am interested indeed in what these merits are going to be.

5. LFK Programme More Than Simple Continuation of Popper Falsificationism

As the senior professor of philosophy of science at London School of Economics (LSE) during development of Lakatos methodology of scientific research programmes, Popper certainly played some role in development of the LFK programme. Popper was instrumental in bringing Feyerabend from Austria and in hiring Lakatos to teach as LSE. However, development of the LFK research programme involved abandoning Popper's naive falsificationism and replacing it with Lakatos methodology of scientific research programmes.[28] The LFK programme superseded the Popperian programme in the sense that it answered and solved the various objections to naive falsificationism and in the sense that the various modern criticisms of the Lakatos research programme actually criticize Popper not LFK. The main theme of this proposal is that any historical or philosophical theory that improves on the LFK programme must still study science in terms of research programmes.

Many modern scholars still attribute the LFK programme to Popper. The most extreme example of this mistake is the recent two part paper by Glas[29] on fallibilist philosophy of mathematics that claims fallibilist mathematics was discovered and developed by Popper and calls the LFK programme an 'application'[30] of Popper's philosophy. The two part paper rewrites history to erase Lakatos and mathematician George Polya, who was Lakatos' strongest early supporter, from the historical record. As Glas writes in the paper's introduction[31]

27. Lakatos[1999], p. 317 - part of section was underlined by Lakatos.
28. Cf. summary of Kuhn correspondence with Lakatos (archive summary section 11.1 and 13/512:148:149, 13/512:163) especially July 1969 letter from Kuhn to Lakatos (13/512:126). Letter shows that Lakatos and Kuhn understood there were problems with Popper's naive falsificationism and that it needed to be replaced.
29. Glas[2001] and Glas[2001b].
30. Glas[2001], 119.
31. *Ibid.*

> The Choice of titles--besides much else--already makes it unambiguously clear that Lakatos intended his studies in the philosophy of mathematics to be an application or test case of Popper's philosophy of science.

Also refer to the quotations from the LFK programme developers in section 4 above and to section 7.3 below that discusses the role of the LFK programme in the development of NP completeness computational complexity research programme for disproof of Glas' claim. Glas' claim is ludicrous on its face but possibly shows Glas' actual intention is to defend formalist mathematics from the LFK quasi-empiricist programme by simply side stepping Lakatos' arguments.[32]

Another example occurred at the 2004 HOPOS conference. William Shields presented the paper 'Karl Popper's Quantum Ghost'[33] that not only attributes Feyerabend's work to Popper but goes so far as to attribute the discovery of quantum physics to the philosopher Popper. Shields writes 'How is it that physicists in the new millennium are invoking Karl Popper's name, conducting experiments suggested by him, and arguing over the the meaning of the results?'[34] The paper attributes Feyerabend's 1950s[35] work to Popper without even one bibliographic reference to Feyerabend.

All three founders of the LFK programme had ambivalent feelings toward Popper and toward naive falsificationism. The attitude toward Popper's theory are best expressed by Feyerabend and Lakatos themselves. Feyerabend was critical of Popper's theory at least by the late 1950s and attributed most of Popper's discoveries to Ernst Mach[36] and J. S. Mill.[37] As Feyerabend states in his review of Laudan's book *Progess and its Problems*:[38]

> What remains? *Popper's original problem solving model* freed from the cumbersome logical machinery which Popper himself and some of his more distant pupils have superimposed on it.

Feyerabend explicitly expresses why the LFK theory supersedes Popper in his review of Popper's book *Objective Knowledge. An Evolutionary Approach*.[39]

> For although Popper promises a 'full answer to [his] critics' he never even mentions the decisive objections of Kuhn and Lakatos. Nor is it clear who the 'critics' are whom Popper has in mind though, judging from the arguments he offers, they seem to come from the backwoods of Oxford.

Feyerabend's various discussions of the problems Popper has with Niels Bohr's physical theories that the LFK programme solves show the superiority of the LFK programme and explains the reason the LFK programme still contributes to scientific development. For example, In a 1968 letter to Lakatos defending Bohr against Popper's criticism, Feyerabend writes:[40]

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32. The author observed another example of this misunderstanding when attempting to teach a seminar on the LFK programme at Stanford. Stanford faculty refused to allow a LFK seminar to be taught during the mid 1990s because instead they were having an education department professor teach a class on Popper. Correspondence discussing seminar with Stanford is available. The refusal shows an irrational unwillingness to teach the LFK programme because Stanford has an unwritten rule that undergraduate alumni are encouraged to teach classes at Stanford.
33. Shields[2004]
34. *Ibid.*, 2.
35. Feyerabend[1981], 247-293. See also the entire part 2 of Volume 1 of Feyerabend's collected works.
36. Feyerabend[1981b], 89-98.
37. Feyerabend[1981b], 144 last paragraph.
38. Laudan[1977] and Feyerabend[1978b], 240.
39. Popper[1972]. See Feyerabend[1978b] for review. This criticism of Popper appears on page 169.

> My point is as follows: Karl may score against a few bum-physicists who have also written on philosophical matters, but he has not scored a single point against Bohr. Nowhere does Bohr *commit* the mistakes which Karl criticize, he knows that one could commit them, and warns against them and the ghost which Karl wants to exorcise is *nowhere* to be found in his writing.

Lakatos view of Popper's naive falsificationism was less consistent than Feyerabend's and changed over time as the LFK programme was developed. This quotation best expresses Lakatos' early view:[41]

> Popper's distinction lies primarily in his having grasped the full implications of the collapse of the best-corroborated scientific theory of all times: Newtonian mechanics and the Newtonian theory of gravitation.

In his 'Lectures on Scientific Method' Lakatos describes the problem with Popper's naive falsificationism:[42]

> What is wrong with falsificationism? ... Contrary to Popper, these anomalies are shelved instead of used towards rejecting the theory. It follows that falsificationism considers the growth of science as a whole and the behaviour of individual scientists as *irrational*. In other words, Popper cannot explain in rational terms the presence of anomalies.

6. Berkeley CS Department Excommunicated from Church of Science

To understand both why the LFK theory is no longer taught and why it should be taught at the University of Minnesota, it is necessary to study the details of the elimination of scepticism that at the time was mainly expressed by the LFK programme and Polya-Tarski anti-formalist mathematics. The analysis shows that the annexation halted progress in nearly every scientific and mathematical area that was attempting to answer basic questions. Without question, the LFK programme was the primary target of 1970s anti-formalism.

The elimination of the LFK programme in the area of applied mathematic and computer science (CS) is best shown by considering destruction of the UC Berkeley computer science formed in the European tradition that studies science as natural philosophy. In the late 1960s UC Berkeley established a computer science department that was separate from engineering and part of the school of literature and science. The department was small and selective. Faculty interest was mostly in mathematical aspects of computer science and many of its older members had contributed to the development of 20th century numerical problem solving. Most professors held joint appoints with the mathematics department, but the most popular professors had been educated in the Harvard applied math department and had joined the department from operations research. There were no development projects because engineering development projects such as design of a new operating system (CAL TSS) and designing new computer architectures were undertaken in the electrical engineering (EE) department. The department studied mathematics that was too concrete and too algorithmic for the mathematics department and too far removed from physical experiments for the physics department. There was also a computer science department in the Lawrence Berkeley Laboratory that was more aimed to provide computational support to physicists than to study basic algorithmic questions. In addition to studying computer

40. Lakatos[1999], 127.
41. Lakatos[1976], p. 8.
42. Lakatos[1999], 97,98.

engineering, the faculty in the EE department also attempted to study computer science but were mostly unsuccessful in obtains grants. Some of the L&S CS department faculty were interested in areas that applied the mathematics of algorithm and computational complexity such as study of computer languages programming methodology. The intellectual environment was one of scepticism of mathematical study of computation because the mathematical study was not concrete enough and scepticism of engineering because of its lack of content.[43]

The department's deep rooted scepticism resulted in research that would now be classified as developing the Lakatosian mathematical program that views mathematics as quasi-empirical and fallible.[44] In particular, there was strong scepticism toward the artificial intelligence (AI) research programme especially toward intelligence as formal computation. Polya's conception of heuristic was popular and expressed in development of concrete algorithms.

The department was a great success in the early 1970s in the sense that students were making progress, every professor was funded by NSF or other pure research grants, any graduate students that wanted TA or RA support received it, and nearly every Berkeley undergraduate chose to learn computer programming from that department. Many famous discoveries were made in that department. Some of the discoveries were:

1. Richard Karp extended Cook's result on NP completeness.
2. James Morris discovered a linear algorithm for string matching.
3. Ralph Merkle discovered the idea of public key cryptography.
4. Jay Earley discovered the modern version of programming language iterators.

However, there were signs from the department's beginning that the department's results were contrary to the prevailing scientific 'religion' at UC Berkeley and Stanford at the time. For example, there was interaction between Berkeley CS department and Stanford but interaction with the Stanford CS department was limited.[45] In the author's view many of problems that led to the excommunication from the scientific establishment of the Berkeley CS department members is related to a counter reformation against the Lakatosian fallibilist mathematical methodology and against the LFK programme in general. In other words, the department was making too much progress at disproving formalist beliefs.

Some of the problems began as soon as the department was established and some were not evident until the EE Department annexed the CS department and fired non tenured faculty and graduate students. Examples of scientific discoveries and anti-LFK programme behavior are:

1. **Karp's NP completeness result seen as less significant than Cook's**
   Although, Karp's result provided the quasi-empirical evidence that NP completeness is a good abstraction of the concept of computational hardness, the importance of Karp's result was not seen as important as Cook's result.[46] Cook does not view Karp's result as

43. UC Berkeley sceptical neurophysiologist Gunther Stent was a supporter of the CS department and many graduate students took his classes to fulfill there requirement for a minor outside computer science and mathematics. See Feyerabend[1994], p. 130 for Stent's connection to the LFK programme.

44. Obviously because of the encouragement Lakatos received from mathematicians, especially mathematicians in northern California, everyone assumed a common fallibilist mathematical conceptual framework.

45. The letters from Polya to Lakatos in the Lakatos show a similar lack of interaction between Polya and Stanford philosophy and computer science <GET LSE REFERENCE>.

45. The religious language used here is appropriate because the opposition to the LFK programme was irrational in the sense that scientific evidence was ignored in the name of the beliefs of engineering school professors. (Feyerabend[1981b], p. 307). Also see below.

less important than his because they studied at Harvard at the same time.

2. **James Morris denied credit for discovering linear string matching**
Although James Morris is now acknowledged to have discovered the linear string matching algorithm, in 1970 Donald Knuth and Vaughn Pratt published a technical report describing the algorithm but omitted Morris as an author. Vaughn Pratt had worked with Morris on analyzing complexity of the algorithm that Morris had already implemented. Pratt then moved from Berkeley to Stanford to work with Knuth. After complaining, Morris' name was added to the list of authors but even now the people who analyzed the algorithm's complexity are remembered as having discovered the algorithm. The original technical report was published as Morris[1970]. See Knuth[1977] for the paper that included James Morris in list of authors. An earlier Stanford technical report omitted Morris. Morris' name was only added after he complained, but once James Morris was denied tenure and forced out of academic research (he found a job a Xerox Palo Alto Research Center - PARC), history was rewritten[47]

To continue the science as religion analogy, Shasha and Lazere describe the discovery of linear string matching this way:[48]

> In 1968, Knuth moved to Stanford University which had become one of the top three computer science departments in the world (with M.I.T. and Carnegie Mellon). With graduate student Vaughn Pratt, he [Knuth] discovered a simple, yet extremely effi cient way to search texts for a string of characters. A similar method was discovered at about the same time by James Morris, so it is now called the Knuth-Morris-Pratt algorithm.

The account rewrites history to glorify the computer science's 'true' prophets because the account omits the fact the Pratt was a graduate student at UC Berkeley working with Morris when Morris discovered the algorithm.[49] Shasha and Lazere continue with a discussion of the algorithms 'inspiration.' They write that Knuth, Morris, and Pratt 'drew inspiration from the ideas developed by Robert Boyer and G. Strother Moore and advances in automata theory pioneered by Steve Cook.'[50] As a former student of Morris and Earley, The author's recollection is that the background problem context for Morris' algorithm came from studying Earley's optimal context free language parsing algorithm and Krohn-Rhodes semi-group complexity.[51] Earley's algorithm connects back to Chomsky's work on context sensitive languages.

3. **Ralph Merkle not given priority of discovery for public key cryptography**
As a Berkeley undergraduate, Ralph Merkle discovered public key cryptography and described it in a paper submitted to assistant professor Lance Hoffman's class on cryptography.[52] This happened in 1973 just as the department was about to be annexed by the EE department.[53] Merkle wrote up his result and submitted it to the Journal

46. For example, Cook was awarded the Turing Award many years before Karp. Also, the popular book on the history of the development of computer science by Sasha and Lazere (Sasha[1995]) only mentions Karp in one place in passing.
47. Shasha[1995], 98.
48. *Ibid.* 52.
49. Morris[1970].
50. Shasha[1995], 98.
51. See Arbib[1968] for the group of essays that best explain the Krohn-Rhodes theorem and semigroup complexity. See Arbib[1969] for the related graduate level text book.
52. Hoffman was also denied tenure after the annexation.

Communications of the ACM. He then moved to Stanford to continue work on cryptography. Merkle then worked with Martin Hellman at Stanford. Hellman's group further developed public key cryptography from Merkle's discovery. However, public key cryptography is now called the Diffie-Hellman algorithm. This is a case where the more self serving developers took credit for a discovery they probably did not understand.[54] The priority of discovery question is an interesting open historical problem because there is a claim that the idea for public key cryptography was discovered in a secret British government research laboratory in the 1960s.[55] This discovery would not be possible with awareness of the Polya-Tarski-Lakatos concept of the quasi-empirical nature of mathematics prevalent in the UC CS department. Therefore, if there were an earlier discovery in Great Britain, it is claimed there will be a connection to Lakatos and the the LFK theory.

4. **Discredited discovery of linear median algorithm**
   Also in the early 1970s, there was a claimed discovery by seven people from the Stanford CS department and the Berkeley EE department. They claimed to have discovered a linear time median (half of the input data are larger and half smaller) finding algorithm.[56] At the time (and also now), the best median algorithm involved sorting the values and then selecting the median. It turned out that the algorithm made some incorrect counting assumptions in the algorithm's performance analysis and result was not really linear. It was understood by every member of the Berkeley CS department that the algorithm efficiency claim was wrong because of its anti-formalist scepticism. In spite of the error, the work established the careers of the engineers who published it.

6.1 Eliminated CS Graduate Student Research

There were a number of students who were working on projects that would have furthered the LFK fallibilist and quasi-empirical programme, but the projects were ended when the department was annexed and no student received their Ph.d from UC Berkeley although some students received Ph.ds by working at other institutions. Teaching the LFK seminar will lead to progress in these areas even if the particular subjects are not explicitly discussed in the seminar. Here are some examples of basic scientific problems in need of study for which understanding the LFK programme is needed:

1. **Exponential time resolution theorem proving efficiency problem**
   One student named Jack Revelle was working on criticizing the AI research programme by proving that resolution theorem proving problem instances exist that require exponential number of steps. This result would have shown that the 1970s (and current) AI claim that intelligence is nothing more than theorem proving using the resolution method is false. Revelle was working on concrete problems that are outside of the class called NP. Currently, such problems are classified as monsters within the formalist computational complexity research programme and placed into a class called Co-NP

53. Merkle's result is an example of a discovery facilitated by the LFK programme.
54. Shasha[1995] does not even mention Merkle, p. 134n, But Merkle was listed as an inventor on the patent application filed by Stanford.
55. There is an article on the claim that the British Government Cryptography Organization (CGHQ) discovered public key cryptography in the January 1998 issue of *Doctor Dobbs Journal*. No copy of this periodical was located. However, if a web browser is given the string 'CGHQ and public key cryptography', many references are found, but most links are broken.
56. Cf. Blum[1972] for the published paper without the linearity claim.

(complement of NP class). This modern formalized approach eliminates the possibility of scientific and mathematical progress.[57] Revelle was making good progress when the L&S CS department was merged into EECS department and the P=NP formalization was decreed to be the 'one true religion'. Revelle had learned his complexity theory from Steven kleene at University of Wisconsin so ending of this research suggests that an LFK seminar would lead to progress at the University of Minnesota.

2. **Development of Krohn-Rhodes computational complexity model**
Douglas Albert was working with UC math professors Krohn and Rhodes on formal languages when he was thrown out of Berkeley. He started graduate school in the CS department just as it was being annexed and before he had a chance to pass his exams, a rule involving incomplete grades in academic classes was used to expel Albert. The behavior is unusual because incompletes are common during graduate study and if there are rules, they are not enforced. Albert was a sceptic toward AI and took Dreyfus' philosophy seminar on AI.

    There is even a later strange Lakatosian connection to Albert. When Lakatos arrived back in Hungary in 1950 after his visit to Russia, he found that his apartment had been rented[58] Albert was working at Intel in the mid 1980s on floating point hardware when he was fired by Intel in a similar manner. He learned that he was fired because when he returned from a visit to the Intel lab in Oregon, he found all his belonging from his desk and carrel piled in the reception area of the Intel building he worked in. Jackson's book *Inside Intel*[59] describes other similar events at Intel. The author believes the behavior pattern was started by the Berkeley EECS department in the 1970s.

    When Albert was fired he was working in his spare time with Krohn and Rhodes who still taught in the Berkeley math department and still studied algebraic computational complexity from the formal language viewpoint. Graduate students who attempted to study the Krohn and Rhodes complexity results were at minimum criticized and discouraged and later after the annexation threatened and dismissed. Expelling Albert was unfortunate because it resulted in the Krohn Rhodes theorem's connection to complexity theory elimination from scientific study.[60]

3. **Development of concrete Markov algorithms**
Colin McMaster was another student who did not receive his Ph.d. He also started just as the annexation was occurring so he never got to the point of starting his thesis. He was working with Gene Lawler in the area of Markov and stochastic processes. Lawler ceased producing many results after the CS department was merged with the EE department. This is a an example of suppression of anti-formalist knowledge by preventing access to students able to deal with the complicated algorithms Lawler studied. Although Markov algorithm study is common, such algorithms are not being studied in any anti-formalist and sceptical CS department.[61]

4. **Disproof of object oriented computer programming**
This essay's author's Ph.d. was not granted by UC Berkeley. The intended thesis called 'Pragmatic Versus Structured Computer Programming' showed that object oriented

57. Cf. section 7.3 below.
58. Lakatos[1999], 402.
59. Jackson[1997] discusses other similar events that occurred at Intel.
60. Albert has published a paper with Rhodes that is only indirectly related to complexity theory (Albert[1992]).
61. McMaster used Markov techniques to analyze complexity of the various algorithms discussed in the intended thesis (Meyer[1983]). McMaster's results were published as McMaster[1979].

programming is wrong or least no better than other programming methods.[62] The author's self perception during that period was as an advocate and student of the LFK programme because of Polya's popularity at Stanford. The author took Feyerabend's epistemology class and attended Feyerabend's philosophy seminar at UC Berkeley. UC Berkeley's refusal to award a degree to a sceptic of formalist programming methodology is unfortunate since there is no one developing LFK programme in computer science. The current situation is that there is a kind of split personality in the study of computer science. Every academic department studies and advocates formal object oriented programming and mathematical proofs of computer programs but the best computer programs (especially involving applications that use advanced mathematics) have been developed using the C language and the pragmatic approach.[63] A disturbing modern problem relating to absence of anti-formalist study of computer programming methodology is that many programming projects are being outsourced to other countries due to high costs and failures of projects in the US. The author believes this outsourcing is an attempt to hide the fact that object oriented programming is both an engineering failure and methodology a degenerating research program.

The Berkeley CS department story is quite unusual and is worth describing in detail because the essay's author was quite far along in his studies, had passed his orals and was hired as a lecturer by the L&S CS department before being fired after then annexation. Final result was that the EECS would not award the author his Ph.d. and the paper that contained the main result that was submitted to the refereed journal *Communications of the ACM* was also rejected.[64] Possibly most unusual aspect of the original work was that the famous computer scientist Edsger Dijkstra advised the author to to 'burn' his manuscript: 'I have only one advice: unless you want to act like Don Quixote, fighting ills of your own imagination, burn this manuscript, ...'.[65] The book burning suggestion was repeated by another famous Stanford computer scientist Robert Floyd and appears to be the basis for the paper's rejection. A thesis was written by the essay's author in 1983 after the author was hired by LSI Logic Corporation and had published a paper in a related area and was well enough established in a job at LSI Logic Corporation to complete the writing.[66]

It is possible to read the author's story and in fact the elimination of an entire department as counter reformation against the LFK research programme, i.e. elimination of anti-formalist study in computer science. Since Feyerabend had contributed to the thesis' main discovery, he was quoted in the thesis. The thesis also included quotation from another Lakatosian British philosophy professor Frank Cioffi and quoted Polya in the original 1977 paper. The responses to the paper can only be read as religious opposition to LFK quasi-empirical mathematics.[67]

The story began after the author received a B.S. degree from Stanford and decided to study computer science (CS) at Berkeley to work with what the author thought were the two best young professors in the computer programming language area. One was Jay

62. Meyer[1983].
63. Software developed at Bell Laboratories such as C and Unix and recent development of Linux by one undergraduate are examples of this dichotomy.
64. Meyer[1983]. chapters 1-3.
65. Meyer[1983], 15, last paragraph of section 3.5.
66. Meyer[1983], chapter 5 for the original paper. The thesis was reformatted into possible book form in 1989. The draft date of 1999 is the printing date because the document preparation software used originally had changed.
67. Meyer[1983], chap. 3.

Earley who had discovered the fastest (at the time and still) context free formal language parsing algorithm.[68] Earley left computer science to become a psychologist after not receiving tenure. The other young professor was James Morris who moved to an industrial job at PARC after also being denied tenure. He moved back into academia as a professor at Carnegie Mellon in the 1980s.[69] The author passed his orals with both of those professors on his committee.

The author studied philosophy of science in Feyerabend's UC Berkeley seminar because he saw the writing on the wall after Jay Earley and James Morris were forced out of the academic research establishment after they were denied tenure at UC Berkeley. The author needed a discovery that was so significant that the annexing EECS department would be forced to award a ph.d. degree. The author believed that by finding a failure of the application of the structured programming method in the book written by the founder of the method to justify it, such a discovery had been, but the assumption was incorrect.

The essay's author's first inkling that no degree would be granted occurred in the area of teaching. A lectureship was shared with Diane MacIntyre.[70] Class notes were written and used in their classes to teach beginning computer programming, but requested clerical support to help with publishing the notes was turned down by the faculty during the annexation. The author's lectureship was not renewed, but since the author had no trouble earning money consulting, he still hoped to write a thesis using the failure of structured programming discovery. The department attempted to prevent the author office access, but because the office was shared with Diane MacIntyre, MacIntyre found a loop hole in the rules and was able to preserve the author's office space during that final year needed to write up the failure of structured programming result and submit a paper to the *Communications of the ACM.* journal.

During that last year the author attempted to have his paper read by the only professor remaining in the programming language area named Susan Graham but she would not even read it.[71] It was given to Professor Graham periodically and her reaction was requested, but she was always to busy to talk to the author let alone to read the paper.

In May of that final year, the author remembers going to talk to Feyerabend during his office hours held outside under a tree.[72] Feyerabend's suggestion was to write up the result and try to have it published as a paper back book and to try to give a talk. Feyerabend probably already knew from various attempts to fire himself and attempt to eliminate the LFK programme that the paper would not be published in a refereed journal. The author then asked to present a talk in a CS department seminar describing his discovery. The request was turned down, but a complaint was made and the author was allowed to give a talk. However, the talk was scheduled at the same time as the weekly EECS department seminar. Only a few people attended the talk because all beginning EECS students were required to attend the weekly seminar and sign the attendance list.

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68. Earley[1970]. Also, Jay Earley's style of iterators is still superior to the modern ones (Earley[1975]).
69. Cf. James Morris' problem obtaining recognition for his discovery of the linear string matching algorithm in section 6 list item 2 above at this time.
70. MacIntyre did receive her Ph.d because her adviser was physics professor Frederick Reif. She was able to find a job teaching at Mills College but she was always unhappy until her death in the early 1990s about being forced out of the research system.
71. This may be unrelated but she was married to Michael Harrison the senior professor in the related formal language area. Both were journal editors for the Association of Computing Machinery (ACM) and it is believed both attempted to prevent the paper from even being refereed.
72. Feyerabend[1994], picture section p. 8 shows the tree and Feyerabend talking to students.

After submitting the paper, Edsger Dijkstra, who had written the book about the 'failure' and developed the algorithm used for disproof, was sent a copy of the paper. He found another interpretation of his his English text describing the algorithm in question which had the effect of making the algorithm inefficient. He described the algorithm in English rather than coding the algorithm in a computer programming language so he was able to reinterpret the reference subject of one of his subordinate clauses. The paper was then changed to use the new interpretation that made the point just as well because of the inefficiency. However, it is believed from later communications with on of the ACM editors that Berkeley professor Michael Harrison editor of another ACM journal had arranged to have the first version, not the revised version reviewed. When the rejection letter using the wrong old version arrived, an opportunity was granted to resubmit the new version. That version was also eventually rejected.[73]

Such academic gamesmanship is not so unusual[74] but the unusual part is that every CS department behaved in the same manner. The author applied for some academic jobs thinking Berkeley would not dare reject the author's thesis if the author were teaching somewhere else. The author was invited to present his work at Yale, but that invitation was withdrawn before a visit could be arranged. Naturally, the author still claims the disproof of structured programming using Lakatosian analysis is correct and has withstood all intervening criticism.

The author's thesis was submitted to EECS department in 1983, but after some discussions and delays it was rejected too. The rejection decision was also eventually appealed but it was decided to not grant a ph.d. in spite of completion of all requirement (albeit elimination by annexation of the author's original CS department had occurred). The author's appeal was finally denied by the UC Berkeley Chancellor's office.[75]

6.2 EE Department Annexation Corroborates Feyerabend's View of Science as Religion

The most obvious explanation for eliminating a department that was at the fore front in developing the anti-formalist part of the LFK research program is that the EE department faculty viewed formalist science as the one true modern religion. They viewed the sceptical and anti-formalist CS department as heretical and in a counter-reformation of 1974 excommunicated the department. Feyerabend holds the view that science is the modern religion when he writes:[76]

> Let us follow their example and let us free society from the strangling hold of an ideologically petrified science just as our ancestors freed *us* from the strangling hold of the One True Religion!

Feyerabend also sees money as a factor in misconduct in the name of true science as a religion.[77]

> The best single entity to get a modern scientist away from what his 'scientific conscience' tells him to pursue is the *Dollar* ...

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73. Meyer[1983]. It is still not clear if there was intentional sabotage to prevent reviewing the revised version. This also may not be relevant, but the first ACM editor Glen Manacher and computer pioneer Herbert Grosch both tried to help. Editor Manacher was replaced before the paper's final rejection.
74. Cf. Andresen's discussion of UC Berkeley philosophy department's denial of tenure to Thomas Kuhn for example (Andresen[1999], p. 564).
75. Appeal correspondence available.
76. Feyerabend[1981b], p. 307.
77. *Ibid.* 52.

However, this does not seem to be the reason for the annexation of the Berkeley CS department since by all measures it was monetarily successful: grants, popularity with undergraduate students, attracting students with outside financing, etc.

7. LFK Programme Facilitated 20th Century Scientific Progress

Feyerabend taught his seminar every year at UC Berkeley during a period when many currently dominant research programs were begun or under went rapid progress. The author attended the seminar every year after the EECS department annexation of the L&S CS department. There were at least as many attendees from scientific departments as philosophers. The author understood from the start that the topics discussed in Feyerabend's seminar were relevant to the author's computer science work because of the LFK theories' anti-formalism and because of its insistence on historical accuracy. The LFK philosophy of science ideas were common topics of discussion among UC Berkeley science area graduate students although only a few students actually attended Feyerabend's seminar.

The most important reason the LFK programme contributed to scientific progress is that it was both a continuation of methodological problems studied by the best 20th century scientists and influenced the behavior of those scientists. For example Feyerabend's papers on empirical problems in quantum mechanics[78] was influenced by Niels Bohr and resulted from working with David Bohm. Those papers in turn have influenced modern methodological discussions of quantum physics. The Feyerabend papers on quantum physics should be read as replying to Planck's essays on the future of science in his 1932 book.[79] In the other direction, Polya's encouragement of Lakatos study of mathematical heuristic[80] and Stanford's invitation to Feyerabend to teach during summer of 1968 at Stanford show the bidirectional interaction.[81]

Another example is Lakatos criticism of probablism[82] and his discussion of the 'inconsistent foundations' of the Bohr programme in quantum physics.[83] Although Lakatos work is not studied as it should be, probablism is now possibly the central methodological problem in physics and computation.[84]

The examples below express from the author's personal knowledge and mostly involve computer science because of the author's background, but it is claimed that any scientist who worked in northern California in the 1960s and 1970s could provide similar examples. If the LFK programme facilitated scientific progress, there should also be examples from Zurich after Feyerabend focused more of his teaching effort there starting in 1980. Also, there should be less research progress at UC Berkeley after mid 1970s after complaints by Dreyfus and Ayn Rand against Feyerabend made UC Berkeley less attractive to him.[85] In a January 1973 lecture Feyerabend writes, 'Now Hans Sluga and that drip Dreyfus are giving my TAs the third degree ... and I have suspicion that they sent somebody into my lecture with a tape recorder. Contrast this with earlier Dreyfus behavior before he received tenure. In a July 1971 letter Feyerabend writes to Lakatos about Dreyfus, 'And I repeat, he is a *very* enthusiastic about you and very popular with

78. Feyerabend[1978], pp. 207-333.
79. Planck[1932], especially 84-106.
80. Cf. letters from George Polya to Lakatos in the Lakatos archive. Letters in archive are translated from Hungarian. They show a pattern of constant encouragement of Lakatos.
81. Many of the founders of modern physics such as Felix Bloch, Linus Pauling and George Polya taught at Stanford at the time and were sceptics of formalism.
82. Lakatos[1981], 11-12, also Lakatos[1978b], 154-164.
83. *Ibid.* 55-68.
84. Cf. discussion of Computational Complexity research program in section 7.3 below.
85. Lakatos[1999], 311.

students here.'[86] Dreyfus came to Berkeley from MIT philosophy department by way of Rand Laboratory. See Feyerabend April 1970 letter to Lakatos describing Ayn Rand's 'attack'.[87] The current lack of teaching of the LFK research programme at the University of Minnesota and political problems in California offer unusual opportunities for scientific progress in Minnesota by again teaching LFK philosophy of science.

7.1 Break Down of the DNA-RNA-Protein Gene Model

The original work that led to Thomas Cech's 1992 Nobel prize for chemistry began during his graduate studies at UC Berkeley. Cech's Nobel prize work showed that RNA can act as an enzyme.[88] Although anomalies in the DNA-RNA-protein gene model existed since the late 1940s,[89] the strict functionality of various genetic components was considered proven before Cech's work. The discovery occurred against the background intellectual climate of conscious study of methodology. Cech's papers describing the results were initially rejected, but were accepted when the results were replicated in other laboratories. The discovery also shows the progressive character of the LFK philosophical research programme. Cech's aim was to improve quantitative chemical analysis. No solution can be perfectly pure so there will always be sources of contamination. It was believed before Cech's work that the imperfect measurements were not qualitative (function effecting?), but Cech's improved quantitative chemistry showed that the effect was not caused by impurities.[90]

7.1.1 Cech's Result Disproves Popperian Naive Falsificationism

Cech's result is an anomaly for any philosophical theory that does not focus on research programmes. It is also an anomaly for theories that preceded LFK programme such as Popperian naive falsificationism because there was no 'bold conjecture' to falsify. It is an anomaly to theories that followed LFK programme such as Laudan's *Progress and its Problems*[91] because it did not solve any recognized problem and because there were no competitors to compare it against. Feyerabend focused on this very problem in his review of Laudan's book:[92]

> Theories and research traditions are *evaluated* by their problem-solving propensities (p. 14). The evaluation is *comparative* (p. 71) 'what matters is not, in some absolute sense, how effective ... a tradition or theory is but, rather, how its effectiveness ... compares with its competitors' (p. 120): one chooses 'the theory (or research tradition) with the highest problem solving adequacy' (p. 109).

7.1.2 Cech's Result Disproves Janssen's COI Patterns

Michel Janssen's recent paper on common origin inference patterns (COIs)[93] also is contradicted by Cech's work. Again Jannsen's problem is proposing a historical research

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86. *Ibid.* 258.
87. *Ibid.* 198-199.
88. cf. Cech[1986]. Prize was shared with Sidney Altman. The story is known to the author because Tom Cech attended the same NSF high school summer science institute at the University of Iowa and both worked together in the University of Iowa Medical School radiology department before starting college.
89. Rennie[1993], p. 123.
90. Cf. 'On the quantum theory of measurement' in Feyerabend[1981a], see p. 207 for philosophical justification of claim that physical measurements are inherently approximate.
91. Laudan[1977].
92. Feyerabend[1978b], 231. Feyerabend's page numbers refer to Laudan's book.
93. Janssen[2003].

program that fails to consider scientific research programmes. He seems to call the LFK programme a theory in which one ’ends up indiscriminately putting episodes in the history of science on the Procrustean bed of one’s preconceived philosophical categories.[94] He also criticizes the LFK programme as being part of ’the cottage industry in History and Philosophy of Science in the 1970s with its case studies of scientific change.’ Unfortunately, because Janssen considers inference patterns and scientific discoveries outside of scientific research programmes, the set of COI patterns in the history of science is empty.

However, Janssen does make a bold and testable conjecture on the nature of patterns of scientific discovery and there is probably something to the idea that some scientific discoveries were accompanied by the psychological ’Eureka’ feeling[95] Another positive aspect of Janssen’s paper is that its effectively shows the emptiness of the work following the LFK programme. As Feyerabend describes the situation:[96]

> The situation is very different with the new breed of philosophers [and historians] of science that now populate our universities. They received their philosophy ready made, they did not invent it. Nor do they have much time or inclination to examine its foundations. ... we have now anxious conformists who try to conceal their fear (of failure, of unemployment) behind a stern defense of the status quo.

7.1.3 Enzymatic RNA Behavior Invalidates Waters’ Attempt to Define ’Gene’

Cech’s discovery in the early 1980s of Enzymatic behavior or RNA began the modern ’anything goes’ biochemical genetics research programme.[97] CK Waters’ mistake is that he fails to treat the concept of gene as part of a research programme.[98] Any attempt to define the concept of gene separately from on going scientific research is doomed to failure because it is impossible to predict future scientific discoveries. The author believes the biologist at the back of the room who pointed out during the question and answer period that retro-virus discoveries contradict Waters’ current definition of gene implicitly understood that scientific concepts must be defined in terms of research programmes. It is claimed Univerity of Minnesota biochemists would benefit from studying the LFK programme because it is currently the best research programme based philosophical theory of science.

7.2 LFK Explains Artificial Intelligence as Degenerating Research Programme

The study of artificial intelligence (AI research programme) has been marked by a pattern of continual grandiose predictions that in each case never came true. Billions of dollars of research funds have been wasted on a theory that has fit the LFK model of degenerating research program from its inception. Starting in the 1950s, the theory conjectured that intelligence was nothing more than theorem proving using the predicate logic resolution proving method. Then when that turned out to be false, other methods such as dependency networks were proposed. Each new ad hoc claim has failed just as predicted for a degenerating research programme. Recently, the AI programme has started claiming that its value is not its scientific progress but

94. *Ibid.* 458.
95. *Ibid.* 459.
96. Feyerabend[1981b], 88.
97. Cf. Rennie[1993] for summary of research programme written by a scientific journalist. Cf. Varmus[1987] for discussion by another Nobel Prize winner of the retro-virus part of the ’anything goes’ genetics research programme.
98. The criticism here is based on Waters’ Friday Oct. 10, 2003 history of science seminar talk titled ’What Genes Do’.

rather the great spin off technology it has created.[99] The AI research programme has come up with another inventive claim. It states that although the scientific part of the research programme is degenerating, a machine embodying intelligence will soon be built and therefore money should continue to be poured into AI research.

It turns out that a report exposing the degenerating nature of the AI research programme was written in London by John Lighthill the Newton Professor of Physics at Cambridge University in 1972 for the British government.[100] Lighthill surely must have been aware of the LFK programme and the concept of degenerating research program when he wrote the report. Unfortunately, the report was and still is completely ignored. Even though it could have save countless billions of wasted research spending.

The degenerating AI research programme provides support for the Lakatosian rationalist part of the LFK programme. The author gave a talk describing the degenerating nature of the AI programme in Feyerabend's seminar back in the 1970s and Feyerabend agreed that in this case Lakatosian prescriptive rationalism made sense and should be used to redirect research funding. The author's first encounter with the Berkeley philosophy department was in 1972 from a seminar taught by Professor Hubert Dreyfus on AI. The author attempted to argue that although Shank's linguistic dependency theory was incorrect,[101] it was not refuted by Dreyfus' existentialist arguments. Dreyfus refused to allow the talk. It is still claimed that Shank's theory can be disproved by LFK research programme analysis. The authors's recollection of Feyerabend's seminars is that Feyerabend would become a Lakatosian rationalist given a specific set of conditions in spite of his methodological anarchism. Teaching the LFK seminar will help University of Minnesota researchers obtain grant money now wasted on degenerating formalist research such as the AI programme.

7.3 LFK Programme Needed for Computational Complexity Theory Progress

The foundations of computer science is both an area that the LFK programme contributed to during the 1960s and 1970s and an area that is currently progressing at a slow rate and therefore would benefit from study of the LFK programme. The computation complexity research programme currently focuses on just one problem called the P=NP question.[102] It is related to the Church-Turing thesis that states that the term 'algorithmic' and problems that Turing machines can compute are the same.[103] The P=NP problem asks if the set of problems that can be solved in polynomially bounded time (called P) is the same as the set of problems that can be solved in polynomial time using a non-deterministic Turing machine (called NP). This problem is related to foundations of logic studied by Vienna circle philosophers such as MCPS founder Herbert Feigl because if P=NP, fast computer proofs of all propositions in propositional logic are possible and probability theory the becomes vacuous.

Development of the P=NP problem (also called NP completeness) in the late 1960s and early 1970s was influenced by the PKF research program. Before discovery of the P=NP formulation, a number of measures of computational difficulty had been proposed, but all suffered from serious difficulties. The P=NP problem was accepted because it seemed quasi-empirically

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99. The spin off claim is also false. Hopefully, teaching the seminar will lead to study of this problem.
100. Called the Lighthill report (Lighthill[1972]).
101. Shank[1970].
102. Cook[2003] defines the problem and discusses progress toward solving it in the 50th anniversary *Journal of the ACM* issue. The P=NP problem is the first of the seven million-dollar 'Millennium Prize Problems' listed by the Clay Mathematics Institute.
103. Cf. Sipser[1997] for an introduction to computational complexity theory. Section 3.3 of that book discusses the Church-Turing thesis.

(in the Lakatosian sense) better than previous alternatives at the time. The original discoveries treated mathematical proofs as thought experiments again in the LFK sense.[104] It is not clear if the thinking behind the P=NP problem was influenced by Polya who spent considerable time at UC Berkeley or by Lakatos' thesis.

There was an alternative Russian research programme in the computational complexity area called Perebor (brute force search) that was probably also influenced by the LFK programme.[105] By 1973 the P=NP research approach was dominant and a Russian student of Kolmogorov named Leonid Levin was credited with independent discovery of the P=NP problem.[106] Lakatos certainly influenced the Russian Perebor research programme because the book form of Lakatos' thesis *Proofs and Refutations* was first published in Russian in 1967 before it was published posthumously in English in 1976.

According to P=NP discoverer Cook 'The sad state of the mathematics of this field is that we can't prove these things. We can't prove P not equal NP. ...'[107] It is possible to view the P=NP programme as either a degenerating research programme using Lakatos' language or a theory in a period of crisis using Kuhn's language. For example, much recent work has involved simply categorizing the various complexity classes even though there are no proofs showing that the classes are different.[108] There are a number of anomalies within the P=NP programme that may have already been solved if study of the LFK programme had continued after the mid 1970s and LFK scholars such as Noretta Koertge had been hired by universities with active computational complexity research programmes. Many of the areas with anomalies were studied at MCPS until 1980. Some of the anomalous areas are:

1. **Problems related to consistency of probablism (inductive logic)**
   Many algorithms within the P=NP programme solve problems by producing solutions with high probability of correctness or efficiency. However, those algorithms ignore the work of Lakatos who continued work by Feigl and other members of the Vienna Circle[109] that showed foundations of probably theory are inconsistent. Kolmogorov complexity also is currently formulated in terms of probability, but it is possible to read Kolmogorov's work as explaining limitations of probability theory[110] just as Feyerabend shows that Bohr understood difficulties with the foundations of probablism in quantum physics.[111]

2. **Lack of connection to concrete problems with problem specific structure**
   The P=NP problem is studied using non constructive mathematics so that new thought experiments that result from study of concrete problems are not being discovered. See section 6.1 list item 1 above for Revelle's study of the concrete problem of testing the

104. Lakatos[1976].
105. Trakhtenbrot[1984]. The area is also called Kolmogorov complexity (LI[1993]). The Perebor program focused on program size rather than the number of steps needed by a Turing machine.
106. Cf. the section on Levin in Sasha[1995], 148-156. Although the book has inaccuracies, The author believes the direct quotations are correct. Also see section 6.1 list item 1 above because the stated reason for not granting a Ph.d. to Jack Revelle was the discovery by Levin. This case of claimed coincident discovery offers an interesting area for historical study. The Trakhtenbrot[1984] historical essay mentions problems with misunderstandings due to translation problems in both directions.
107. Shasha[1995], 156.
108. Hartmanis[2003].
109. Lakatos[1976b], 128-193. See Stadler[1997] for a detailed history of the Vienna circle. Feigl's biography appear on 624-630.
110. Li[1993], contrast section 1.1 with section 1.3. Differences of interpretation may be related to language translation problems.
111. Feyerabend[1981], 247-293.

resolution theorem proving algorithm that is the theoretical basis of the AI research programme.

A related example is study of concrete problems that require exponential (outside the class NP) worst case time to run. These problems are studied by studying the abstract class called Co-NP (complement of the class NP) rather than as concrete problems. This is a type of monster baring in the Lakatosian sense. It is also interesting because Levin originally studied such problems in his work involving discovery of the P=NP problem.

3. **Inconsistencies related to actual computer programs**
Some problems have been reduced to the class NP that arise in the area of computer programming such as the 'Span independent jump instructions program size minimization problem'. There are philosophical problems related to the quasi-empirical nature of mathematics because there is a difference between the formal numbering of problem instances needed to 'reduce' the problem to a Turing machine program and the intuitive concept of computer programs that compute something meaningful to humans. This is an example of LFK Programme conceptualization of mathematical proofs as thought experiments.

4. **Focus on P=NP to Exclusion of other Complexity Measures**
Problems that are not naively related to proving or disproving P=NP are not studied. Therefore linguistic problems that may lead to progress within the P=NP programme are not studied. One example is that context free language parsing is always studied and taught using a very weak type of automata called 'push down automata'. If parsing using the full power of Turing machines were studied and taught, progress in the study of linguistic problems relating to computational complexity would occur. Another example is the study of algebraic properties of automata.[112]

Recent work in quantum computation has tied computational complexity that grew out of the study of the foundations of logic to quantum physics that is at the foundation of physical thought.[113] This new area increases the importance of the LFK programme that concentrated on advances in 20th century physics.[114] The author claims the best philosophical work in the quantum computation area is still Feyerabend's work on the foundations of quantum physics[115] and the original work by MCPS at the University of Minnesota. It is important to again teach the LFK programme to revive MCPS and the LFK Programme.

8. Seminar Continues the Research of the Founders of MCPS

Another important reason to teach a seminar on the LFK research programme is that University of Minnesota faculty were early advocates and adopters of the LFK programme. The tradition goes back to the founding of the Minnesota Center for Philosophy of Science (MCPS) after World War II by Herbert Feigl. Feigl came from the Vienna Circle logical positivist tradition,[116]‘ but he recognized the superiority of the LFK programme very early and was one of it strongest backers. Univerity of Minnesota hired Feyerabend to teach just after he came to the US in the 1950s. Also Feyerabend and Lakatos were invited often to visit here in Minnesota to discuss the LFK programme. Feyerabend attributes Feigl with improving the clarity of his

112. Krohn[1965].
113. See Shor[2003] for a discussion of quantum algorithms and Yao[2003] for a discussion of quantum computing and the Church-Turing Thesis. Also Levin[2003].
114. See the author's 2003 Eurocrypt talk (Meyer[2003]).
115. Feyerabend[1981], 207-333.
116. Stadler[1997], 171-174.

thinking and attributes many of his ideas to Feigl. Although he states that Feigl's view was less radical than his.[117] It was not just Vienna Circle philosophers and founders of modern physics who understood the importance of the LFK programme but also Univerity of Minnesota scientists such as Grover Maxwell, William Hansen and Paul Meehl.[118]

The author does not understand why the current University of Minnesota faculty does not teach the LFK programme. There has been no disproof of the theory and it is difficult to even imagine what argument would be used to show that Feigl and other early member of the the MCPS were incorrect. The author claims teaching the seminar will lead to prize winning research at the University of Minnesota. It is even possible that because of Minnesota cultural thinking patterns, the LFK programme will produce even better results here than other places and that the founders of MCPS understood that in selecting their original research topics.

117. Feyerabend[1981], 41n, 31n. Also Feyerabend[1994], 116-118.

118. The correspondence in the Lakatos archive shows the importance those faculty members attached to the LFK programme. Cf. archive summary section 11.2 and 13/268:28-29, 13/268:26:27 (actually 3 pages), 13/610:47, 13/270:9 and 13/373:1-2.

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10. APPENDIX I - Summary of Correspondence from Lakatos Archive

As the introduction to the Lakatos Archive explains, Lakatos had a full time secretary and was a systematic record keeper. The attached letters are examples from the archive that I selected to assist in showing the importance of teaching Lakatos-Feyerabend-Kuhn philosophy of science programme at the Univerity of Minnesota. In addition to the Lakatos-Feyerabend correspondence published in the book *For and Aginst Method* (Lakatos[1999]), the archive has other very interesting material that merits further study. The archive is large and systematically recorded and indexed.

This proposal is being distributed in computerized .pdf format, but the correspondence from the archive only exists as paper copies of the letters from the archive. Therefore, this summary has been added to the .pdf format version. In addition, the letters can not yet be provided until permission from the Lakatos archive and the copyright holders has been received.

In the Lakatos Archive, document references have the form "cc/fff ppp" in the upper right corner of every page. Where cc is the collection number such as 13 for miscellaneous correspondence. fff is the number of the folder in the collection. The final number is the paper number within a folder. Pages are numbered sequentially so a 3 page letter will have one folder number but 3 page numbers.

The following letters are attached:

10.1 Kuhn Correspondence

1. **Kuhn June 1964 letter to Lakatos**
   Letter shows Kuhn's self perception as philosopher of science within the Lakatos-Kuhn-Feyerabend research programme Archive references 13/512:148-149.

2. **Lakatos July 1969 Letter to Kuhn**
   Letter shows importance of Kuhn's work in Lakatosian research programme and acknowledges borrowing 'an immense lot from your insights into the nature of scientific growth'. Kuhn recommended Lakatos for his LSE appointment. Lakatos archive 13/512:163 - quotation in par. 3.

3. **Kuhn July 1969 reply**
   Letter shows Kuhn's role in LFK programme's superseding of Popperian naive falsificationism. Lakatos archive 13/512:126.

10.2 Feigel and MCPS Faculty Correspondence

1. **Feigel 1964 letter to Lakatos**
   Letter shows Feigel already understood that formalist mathematics was a degenerating research program and that an alternative was needed. Lakatos archive 13/270:2.

2. **Lakatos July 1, 1970 letter to Feigel**
   Letter shows Lakatos' understanding that future philosophical and scientific progress could happen at the MCPS. Lakatos Archive 13/268:28-29.

3. **Feigel July 18, 1970 letter to Lakatos**
   Letter shows Feigel's contribution to Lakatosian research program by insisting on historically accurate treatment of early 20th century study of formal logic. Lakatos Archive 13/268:26:27 (*sic.* attached page marked 2 not numbered).

4. **Lakatos October-November 1970 Letters to Maxwell and Feigel**
   Letter refers to argument between Paul Meehl and Lakatos. More importantly, it shows that U of Minnesota scientist Grover Maxwell understood implications of Lakatosian research program. Lakatos Archive 13/610:47 (pages 1 and 2) and 13/270:9.

5. **Letter exchange between Umn professor Hanson and Lakatos April 1970**
   Exchange shows that understanding of the importance of LFK program extended outside the European-Vienna Circle sphere. Lakatos Archive 13/373:1 and 13/373:2.

10.3 Correspondence with Former LSE Students Teaching in US

1. **Lakatos January-February letters to Noretta Koertge**
   I believe letters show beginning of anti-Lakatosian counter reformation and explains current lack of scientific progress at University of Minnesota. I requested copies of additional Koertge correspondence from the LSE archive, but copies were inadvertently not made. This one letter from Lakatos to Koertge shows the problem. Lakatos Archive 13/489:58 and 13/489:117-118.

11. List of Letters from Lakatos Archive

1. 13/512 148-149: 3 June 1964 Kuhn letter to Lakatos
2. 13/512 163: 4 July 1969 Lakatos letter to Kuhn
3. 13/512 126: 7 July 1969 Kuhn letter to Lakatos
4. 13/270 2: 11 June 1964 Feigl letter to Lakatos
5. 13/268 28-29: 1 July 1970 Lakatos letter to Feigl
6. 13/268 26:27 (3 pages p. 2 unmarked): 18 July 1970 Lakatos letter to Feigl
7. 13/610 47 (2 pages): 8 October 1971 Lakatos letter to Maxwell
8. 13/270 9: 10 November 1970 Lakatos letter to Feigl
9. 13/373 1: 3 April 1970 Hanson letter to Lakatos
10. 13/373 2: 29 April 1970 Lakatos letter to Hanson
11. 13/489 58: 25 January 1971 Lakatos letter to Koertge
12. 13/489 117-118: 4 February 1972 Lakatos letter to Koertge