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# The Origin of Technical Change; Knowledge Generation, Opportunities and Entrepreneurship

by

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# The Origin of Technical Change; Knowledge Generation, Opportunities and Entrepreneurship

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"Technological possibilities are an uncharted sea. We may survey a geographical region and appraise ... that the best plots are first taken into cultivation, after them the next best ones and so on. At any given time during this process it is only relatively inferior plots that remain to be exploited in the future. But we cannot reason in this fashion about the future possibilities of technological advance. From the fact that some of them have been exploited before others, it cannot be inferred that the former were more productive than the latter. And those that are still in the lap of the gods may be more or less productive than any that have thus far come within our range of observation. ... There is no reason to expect slackening of the rate of output through exhaustion of technological possibilities." Capitalism, Socialism, and Democracy, p. 118. 1942

## Introduction

The introduction of new and better goods in the economy has long been recognized as a key source of economic growth in the literature.[1] This process, however, has only been introduced in macro-economic growth models in the relatively recent contributions of Grossman and Helpman (1991) and Romer (1990). In their models a specialized Research and Development sector produces the blueprints for new goods or intermediates to capture the monopoly rents that such ideas generate in imperfectly competitive markets. Implicit in these models is the assumption that the opportunities to invent new or improve upon old goods are inexhaustible and freely available to the R&D sector. In other words it only takes effort to turn such opportunities into a new product.

But rethinking that assumption one realizes that, like innovations, opportunities do not fall like manna from heaven. In fact the opportunity for a new product only emerges when all of its components exist. Furthermore an opportunity is ready to be developed into a product when an entrepreneur (firm or person) has the vision to bring together all pieces of required and helpful knowledge and combine them with the financial, material and human resources needed to develop the idea into a product. This in turn presupposes that the knowledge, finance and resources are available to him. And even then it is the market and a considerable share of luck that determines which innovations succeed and which fail.

If all elements in this train were to be or become available gradually over time there would be no problem in abstracting from them. However, the evidence suggests otherwise. Jaffe and Trajtenberg (2002) use patent citation data and find clusters of related patents. A few core patents do not seem to cite many earlier patents but a cascade of patents follows, citing these core patents heavily. Then patenting slows down and some new core patents have to emerge for a new cycle. Evidence on the evolution of the patent-R&D ratio suggests that this process is not the result of cycles in R&D activity. Moreover, sustained periods of observed productivity slowdown at constant R&D efforts suggest that R&D switches between observable process innovation and unobservable product innovation. The ambiguous relation between entrepreneurship and economic development as uncovered in Carree et. al. (2002) is also consistent with cyclical, not exponential growth. Cycles in employment and more importantly for this paper in gross job creation rates as uncovered by Davis, Haltiwanger and Schuh (1996) are also indicative of a cyclical rather than a smooth flow of new products and services.

In this paper I will focus exclusively on opportunity in the narrow sense and therefore on the origin of the required knowledge. For the purpose of this paper an opportunity exists when all required elements of knowledge are “out there” and await the arrival of a keen entrepreneur to recognize, combine and exploit them. The origin of knowledge is therefore ultimately the source of economic growth and as such its understanding is of crucial importance to economics in general and macro-economic growth theory in particular. I leave all downstream complications of where the required entrepreneurial vision, venture capital, raw materials, intermediate products and finally skilled labor come from for future research.

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[1] See for example Schumpeter (1942) who wrote: “The fundamental impulse that sets and keeps the capitalist engine in motion comes from the new consumers' goods, the new methods of production or transportation, the new markets, the new forms of industrial organization that capitalist enterprise creates.” p. 83.

## The Origins of Knowledge

Broadly speaking knowledge is generated in two ways. Scientific research in laboratories and universities might be the first way that springs to the modern mind but in fact learning by doing has been the dominant source of knowledge accumulation over most of human history and remains incredibly important even today. The two types of knowledge thus created can be classified broadly as fundamental and instrumental knowledge respectively. Consider for example fire. The instrumental knowledge of fire allowed cavemen to use it to warm themselves and cook their food. Also they learned by doing that fire needed to be fed with combustibles and that blowing into the flames improved the burning. Over the centuries that instrumental knowledge has expanded and many commercial opportunities emerged. Bread, metallurgy and blast furnaces are but a few examples that were developed mainly on the instrumental knowledge of fire alone.

The fundamental knowledge of fire, despite efforts by for example the Greek philosophers in Antiquity and a few scattered scholars in the Middle-Ages, hardly developed before the 19th century. The difference is between knowing and understanding fire. Learning by doing is obviously very effective in generating instrumental knowledge but generates little fundamental understanding. Science on the other hand is entirely focused on generating fundamental knowledge, which has no clear direct application.

Still fundamental knowledge in general is a richer source of opportunity as it is understood in this paper. Scientists put very little effort in finding commercial applications (over and beyond the development of scientific instruments) and science may be said to produce new commercial opportunities as an unintended side-product.[2] Modern endogenous growth models focus on the non-rivalry of knowledge and the economics of scale that therefore exist at the aggregate level. As instrumental knowledge is often tacit whereas scientific knowledge is always published (see below) one would expect that the knowledge they refer to is fundamental. But the process they model resembles accumulative learning more than it captures the process of fundamental knowledge evolution itself. This is increasingly important as science and fundamental knowledge are quickly replacing learning by doing and instrumental knowledge as the main source of opportunity for commercial innovation.

As fundamental knowledge creation takes place in what we may label the scientific community, the incentives and constraints as well as its institutions require further discussion before the emergence and evolution of opportunity itself can be modeled. Obviously traditional and modern growth theory only offers a few useful starting points in this respect. To dig deeper we need to consider perhaps less orthodox strands of literature that are relevant here. First I will discuss insights from Thomas Kuhn and link his seminal essay on scientific evolution to Stephan’s (1996) JEL-article “the economics of science” that explicitly addresses the incentives and constraints that motivate scientists. We will see that these differ quite significantly from those in commercial R&D and therefore knowledge evolution follows a very different pattern than commercial innovation driven new growth models predict. Of course, when fundamental economics differ, so must and will the institutions that frame the scientific community.

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[2] Some or even many scientists may try to turn their own fundamental knowledge into opportunities and products by starting up a business, however, in my definitions (and those of Thomas Kuhn) they then seize to be pure scientists as they become entrepreneurs and later on producers.

## The Evolution of Science

Any useful survey of the literature on science and scientific activity should start with Thomas Kuhn's (1971) seminal monograph on the Structure of Scientific Revolutions. In it he describes and explains the cyclical pattern in scientific activity and the characteristics of fundamental knowledge generation while touching upon the underlying incentives, constraints and institutions.

Kuhn first argues that scientific knowledge, a constellation of facts, theories and methods, evolves historically and not, as textbooks would suggest, accumulate in a linear fashion from ignorance to full knowledge of the truth. A scientific discipline is born with the emergence of its first paradigm; a scientific achievement or insight that for a period of time is broadly shared among those engaged in scientific research. Once a paradigm has emerged the discipline will never be without one, although it typically does not remain the same over time. It is the existence of a paradigm that makes a discipline "scientific".

In the textbooks older paradigms are usually omitted or presented only to illustrate the mistakes that were made in the past, suggesting that they were mere stepping stones on the road to uncovering the full truth as is represented in the state of the art theories and methods. But in their historical context these paradigms were as valid as the most recent ones. Their function is therefore not to embody absolute truth or even necessarily bring the discipline closer to the truth but merely to guide further scientific activity. By implication the most recent theories and paradigms are also a mere guide to scientific activity despite the fact that they are presented as the truth. Scientists, however, have to believe in that presentation to be able to engage in what Kuhn calls "normal science". The belief in the paradigm allows scientists to skip discussing the fundamentals and get right down to details. A lot can therefore go without saying, which leaves more room for making advancements on details. Typically the paradigm also develops a vocabulary and set of specialized instruments and methods that exclude even the learned laymen from the debate. The belief in the paradigm is required to rationalize the often high investments to be made in becoming a member of the scientific community.

Kuhn likens normal science to puzzle solving. Like any normal puzzle there are rules and an expected and assured outcome under the prevailing paradigm. To solve a puzzle is a testimony to the scientists ingenuity and is rewarded by peer recognition and status under normal science. To fail at a puzzle is not attributed to the puzzle or the rules but to the scientist. If he steps beyond the bounds of the paradigm for a solution his peers will not reward but ignore him, however right he may prove in the future. He is then no longer regarded as one of them, a scientist.

To quote Kuhn (1971): "Normal science, the puzzle solving activity we have just examined, is a highly cumulative enterprise, eminently successful in its aim, the steady extension of the scope and precision of scientific knowledge" p. 52. But even as the paradigm allows for normal science to emerge, it thereby plants the seeds for its own demise. Under normal science, scientists seek and find facts to generalize, articulate and refine the core theories of the paradigm. In doing so, they encounter two types of results. Anticipated results that strengthen the incumbent paradigm do not call for theoretical revisions and refinements. Unanticipated ones are initially regarded as failures of the scientist or his instruments, not of the paradigm. They require further scrutiny and possibly refinement of the core theories. If the unanticipated result resists this absorption, however, it becomes an anomaly in fact (discovery) or theory (invention).

Initially such anomalies are ignored by the mainstream but as they accumulate the paradigm faces a crisis. However, the incumbent paradigm is not abandoned unless a new one stands ready to take its place. That one will emerge in time is tautological because scientists cannot remain scientists and abandon the old paradigm without accepting a new one. When confronted with a crisis, however, the rules of the incumbent paradigm are stretched, speculative new theories are formulated and discarded and the paradigm is pushed to its limits to exactly pinpoint where it fails. During the crisis more than one alternative paradigm may emerge and compete for dominance for a while. In those episodes a lot of energy is "wasted" on debating fundamentals and justifying one's choice of paradigm. Normal progress stalls. Kuhn stresses that such a conflict of paradigms cannot be resolved by anything other than persuasion. The crisis ends only when most scientists are persuaded to adopt a new paradigm. Those who refuse to follow the mainstream are not necessarily wrong but will henceforth be ignored and regarded as either old fashioned, irrelevant and wrong.
Obviously there is a lot of resistance to paradigm change as vested interests, reputations and research programs have been built on the incumbent one. This, and the sudden resolution of paradigm conflicts, explains the revolutionary and pervasive effect of the occasional paradigm shift when it does occur. It also ensures the fact that a revolution ultimately means progress. Scientists cannot be convinced to abandon the incumbent paradigm if the new one does not at least promise to explain a considerable part of what was known under the old paradigm and some anomalies. In that sense the new paradigm must be judged "better" before it will be adopted by the entire scientific community.

The first task set to the scientists who adopt a new paradigm is to reformulate all data, theory and knowledge previously accumulated into the structure of the new paradigm. Measurements are aimed at verifying the new paradigm and well known phenomena must be recast in the light of new theories. A lot of this has been done in the persuasion battle for scientists' allegiance, but still much remains to be done. This activity is necessary to establish the new paradigm but very unproductive in terms of generating new commercial opportunities. Arguably Newton's fundamentally new understanding of the pendulum-motion caused a revolution in physics but not in clock making. In this stage it is the absorption of old anomalies that does allow for increases in commercial opportunities. As normal science is re-established the rate of opportunity creation also picks up. So Kuhn describes how and why fundamental scientific knowledge starts to progress at a significant rate but in a cyclical fashion after the establishment of its first paradigm.

## Scientific Institutions

But Kuhn also addresses the institutional setting in which scientific activity of this kind can thrive. He mentions the importance of a relatively closed and intensively communicating scientific community that has the resources to finance its activities and can decide on how to allocate those resources relatively autonomously. This implies that the scientists decide among themselves what is and what is not science, what will and will not be financed and who is and who is not recognized as a fellow scientist. In addition, since peer recognition is so crucial, the role of priority is mentioned. Stephan (1996) refers to Merton (1957, 1968 and 1969) who argued that scientists aim to establish priority of discovery by being the first to publish their findings in a recognized publication. As the scientific community also decides what is and is not a recognized publication, this too helps to isolate the community from outside influences.

The recognition of peers comes in the form of fame (Planck's constant, Haley's comet), prizes (Nobel Prize, Spinoza Prize etc.), recognition by Scientific Societies and National Academies of Science and finally by publication in prestigious journals. The importance of contributions is then frequently measured by citations and positions in the citations indices help establish one's reputation as a scientist. Ultimately reputation translates into wages, job opportunities, tenure and fringe benefits. This structure may differ in the details between countries and institutions but its key attributes characterize the global scientific community.

In this respect it is important to note that science shares the "winner takes all" characteristic with patent races that for example underlies the model of creative destruction in Aghion and Howitt (1992) but also that scientists are driven by different types of rewards. Not the expected profit flows but the expected peer recognition and intrinsic motivation to solve puzzles drives the scientist.

This causes him to stay within the bounds of the paradigm as long as his peers do and to switch rapidly once a critical mass decided to switch. This herding behavior in switching is effectuated because scientists concentrate on puzzle solving, taking the rules and anticipated possible outcomes for granted. When the paradigm shifts, so do the puzzles and rules for solutions but not the rewards, both intrinsically and externally, for solving them.

The question now remains how the scientific community can switch when all individual scientists have no incentive to do so. One could think of this process as a rational speculative attack as for example modeled in Krugman (1979). In hat model the gradual erosion of monetary reserves causes a sudden speculative attack prior to the actual depletion of reserves. The fundamental force that weakens an incumbent paradigm when all scientists are engaged in normal science was described above. As anomalies accumulate a crisis becomes ever more likely, making it less and less attractive to invest heavily in the incumbent paradigm and more attractive to engage in looking for a new paradigm. The rewards to the latter in terms of reputation and satisfaction increase and become less insecure whereas the puzzles of the incumbent paradigm become harder to solve and less likely to yield rewards in the future.

Institutions that are built upon reputation therefore explain the fact that young scientists, who have no reputation, are the first to switch and usually become the leading scientists of the next paradigm. To be among those leading scientists means to enjoy a high status indeed, as the rewards in science are distributed extremely unequally. The winners take most; the rewards for second place are marginal. 6% of all publishing scientists produce 50% of all publications and consequently lead the citations indices and collect all prizes.[3] What remains for the remaining 94% of publishing scientists is the gratification of solving an occasional puzzle and usually spending a lot of time trying to keep up and, certainly not unimportant, training the next generation of scientists.

There are therefore some regularities and individual decisions still play some role in the resolution of the paradigm-crisis but the process is equally adequately described as a randomly arriving event of random length that causes normal science to seize and once resolved allows for the inclusion of anomalies, usually at the loss of abandoning previous theories and understanding.

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[3] See Stephan (1996)

## A Preliminary Model

Macro-economic models of growth have always aimed to explain the long run upward trend in GDP per capita by aiming for a constant long run growth rate. As a result these models abstract from the concept of economic cycles and concentrate on the accumulative types of knowledge generation that may support such a constant growth rate. As was mentioned above, instrumental knowledge accumulates smoothly. Arrow (1962) already realized that learning by doing could be a powerful engine of economic growth. New growth theory now builds on that basic principle by considering for example learning by learning (Lucas (1988), learning by inventing (Grossman and Helpman (1991) and Romer (1990)) and building on previously attained quality levels (Aghion and Howitt (1991)). In new growth models new knowledge is usually created by building on what has been discovered in the past. This is reflected well in the specification of the innovation function. That function describes the relation between R&D inputs, usually labor, and the number of innovations produced each period. In a standard variety expansion model such as Romer (1990) and Grossman and Helpman (1991) that function has the general form:

$$\dot{n} = nf(R) \tag{1}$$

Where $n$ is the stock of innovations, the range of goods or intermediates in production. A dot over the variable signifies the time derivative and $f(R)$ is a function of $R$ the amount of R&D inputs. It is useful at this point to interpret $R$ as applied or entrepreneurial R&D that seeks to develop new products or intermediates. The stock of innovations then equals the range of products or intermediates and expands at a constant rate in steady state equilibrium (where $R$ is constant). If utility or final goods production is assumed to exhibit love of variety, then this translates into a constant growth rate for the economy. Note that $n$ appears on the right hand side to reflect a knowledge spillover from the past. Innovations made in the past make the generation of new innovations (proportionately) easier.

Such a set-up may capture the role of accumulative instrumental knowledge but ignores the cyclical nature of fundamental knowledge evolution. As it was argued above that fundamental knowledge becomes ever more important in generating entrepreneurial opportunities and this omission needs to be addressed. Therefore the model suggested below will include the scientific community as it was described above. Its role will be to provide the raw material or “opportunities” for the entrepreneurial R&D in equation (1). Assume:

$$\dot{n} = (n^P - n)f(R) \tag{2}$$

where $n^P$ is the stock of potential innovations or “opportunities” developed by scientists.[4] As the stock of innovations cannot exhaust the stock of potential ideas, entrepreneurial R&D will run into strong diminishing returns as the number of realized ideas, $n$, approaches the number of potential ideas, $n^P$. On the other hand, as the number of potential ideas expands relative to those realized, entrepreneurial

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[4] To be precise, the scientists do not develop off the shelve opportunities but rather develop knowledge that visionary entrepreneurs can use to formulate product ideas. One might therefore wish to put some opportunity/idea production function between $n^P$ and knowledge, the output of scientific activities. Entrepreneurial talent and effort may be inputs in this production function as well.

activity becomes more effective per unit of labor and therefore more attractive. For now I assume this to be a linear relationship for simplicity. The idea that basic science provides the raw material for applied, entrepreneurial science is not new, but it is, to my knowledge, new to mainstream growth theory.[5] Downstream the model can follow the standard set-up of an innovation driven endogenous growth model as was described in for example Grossman and Helpman (1991). Imperfect competition implies that rents reward successful entrepreneurs and this explains why they can borrow to finance innovative investments. The more interesting challenges lie in modeling the evolution of $n^P$ as the outcome of the activities in the scientific community.

To do so I will first introduce some notation. Assume that an exogenously given number of people, $S^*$, engages in scientific activities. As was argued above they are driven by an internal motivation to solve the puzzles set by a paradigm and an external motivation to seek priority and peer recognition for their ideas. Output hence comes in the form of ideas and yields utility to the extent that curiosity is satisfied and peer recognition gained. We could capture the motivations in a utility function for scientists and assume that they maximize it by optimally choosing to engage in scientific activities. This would endogenise the number of scientists but when ideas are assumed to be homogenous and only one type of output is allowed then that maximization problem is trivial and we will not pursue it here. All scientists, $S^*$ choose to engage in science. We now need to specify how they produce fundamental knowledge under the regime of normal and crisis-science.

### Normal Science

Consider the value of doing normal science to the individual scientist. Under normal science the scientist can choose one of the puzzles set by the paradigm. His output is then either an anticipated solution or unanticipated anomaly. I assume that the former adds one unit to the existing stock of knowledge, $K_t$, immediately. The latter adds to the stock of anomalies, $A_t$. To capture this process it is convenient to assume that all normal scientific activity generates anomalies as an unintended side-product. This implies for the individual scientist:

$$\begin{aligned} \dot{K}_{it} &= (K_t - K^I{}_0)^{\alpha} f(S_{it}) \\ \dot{A}_{it} &= (K_t - K^I{}_0)^{\beta} f(S_{it}) \end{aligned} \tag{3}$$

where $K^I{}_0$ is the level of useful knowledge at the adoption of the incumbent paradigm and function $f$ exhibits the usual properties, $f(0)=0$, $f'(.)>0$ and $f''(.)<0$. In addition $f(.)$ is normalized such that $f(1)=1$. By assuming $0<\alpha<1$ and $\beta>1$ we ensure that, as the paradigm ages, anomalies will accumulate faster than knowledge. Under normal science only anticipated outcomes are valuable as anomalies are ignored by peers. If we denote the scientist's utility value of one anticipated solution by $U^N{}_{it}$ we obtain:

$$V^N{}_t = \dot{K}_t U^N{}_t \tag{4}$$

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[5] A nice quote by Charles Stine, advocating basic research in a presentation to DuPont's Executives in 1926, shows that managers have realized the importance of science for growth early in the last century. He was quoted in Stephan (1996) to have said: "applied research is facing a shortage of its raw materials", p. 1209. Perhaps an instance of instrumental knowledge in economics.

where subscript *i* has been dropped because all scientist are assumed to be identical. The value of an anticipated solution is equal to the probability of obtaining priority for it, $P_t$, times the value attached to peer recognition, which we can normalize to equal 1. One might add some constant to reflect the gratification of solving the puzzle, although I know from experience that this is also heavily eroded by the knowledge that someone else beat you to it. For now I therefore include it in the normalized value of peer recognition.

Finally it should be noted that reputations are a lasting asset but when built under a paradigm, they loose their value once the paradigm shifts. As the reputation of a scientist consists of accumulated anticipated solutions under the paradigm we can model this by assuming the scientist discounts the value of solutions under the current paradigm by the flow probability that the paradigm will shift, denoted by $\theta$. This probability is positively related to the number of accumulated anomalies and will be specified further in the next section. We obtain for $U^N_t$:

$$U^N{}_t = \int_t^{\infty} e^{-\theta(\tau)\tau} P_t d\tau \tag{5}$$

**Crisis Science**

The steady accumulation of anomalies under normal science will inevitably provoke a crisis in the current paradigm. When a paradigm crisis arises, the scientific community abandons normal science and instead alternative paradigms are formulated, proposed and advocated. In the search for alternative paradigms the goal is not to solve puzzles under the incumbent paradigm. The focus shifts towards the limitations of the incumbent paradigm and therefore scientists only produce anomalies. In addition the quest at any point in time may yield theory that can serve as an alternative paradigm. To introduce the randomness of the length of the scientific crisis I assume:

$$\begin{aligned}
&\dot{K}_t = 0 \\
&\dot{A}_t = (K_t - K^I{}_0)^{\beta} f(S_{it}) \\
&\Pr(P^A{}_t) = \varphi(A_t)
\end{aligned} \tag{6}$$

Note that because *K* seizes to grow the number of new anomalies will be constant. The third equation captures the assumption that as the number of anomalies increases so does the flow probability of an alternative paradigm arising at time *t*. This assumption ensures the crisis ends in finite time.

When an alternative paradigm is formulated the aim is to *get it adopted*. As only one paradigm can be adopted alternative paradigm formulation has the characteristics of a patent race or tournament. The winner takes all. In addition the reward is enormous, both in reputation and intrinsic rewards. But the risks are high and the losers are usually doomed to insignificance as they staked their scientific reputation on a paradigm that is not accepted. At this point, however, it is hard to model the complete game among scientists and the battle for prevalence explicitly. Rather I will assume that the outcome of this battle of ideas is the emergence of a new paradigm that allows the scientific community to return to normal science with a new initial knowledge parameter.

$$K^A{}_0 \equiv \zeta K_t + \xi A_t \tag{7}$$

where $K^A{}_0$ represents the useful knowledge base that alternative paradigm $A$ promises at adoption, i.e. the number of problems it can explain. The assumption that $0<\zeta<1$ implies that some of the knowledge under the incumbent paradigm must be sacrificed. $0<\xi<1$ implies an alternative cannot explain all anomalies. To be adopted a paradigm must be convincing to the scientific community at large. This implies that an acceptable alternative must at least explain some anomalies. The reason is that the alternative paradigm cannot be inclusive of the old paradigm by definition (see Kuhn(1971)). If that is the case some knowledge must be sacrificed when a new paradigm is adopted and the alternative can only be convincing if there are sufficient anomalies it can explain to compensate for that loss. Therefore there is a trade-off between parameters $\zeta$ and $\xi$. Only the incumbent paradigm can explain all current knowledge, such that setting $\zeta=1$ implies $\xi=0$ by the definition of an anomaly. A trade-off like:

$$\xi = \frac{S_t(1-\zeta)^\beta}{1+S_t(1-\zeta)^\beta} \tag{8}$$

would have the desired properties if $\beta>1$ is assumed. Note that $\xi$ can only approach 1 from below as the number of scientsist goes to infinity. Out of the known problems facing science, $K_t+A_t$, an alternative must now explain a larger share than the incumbent paradigm. The condition for paradigm switching is therefore:

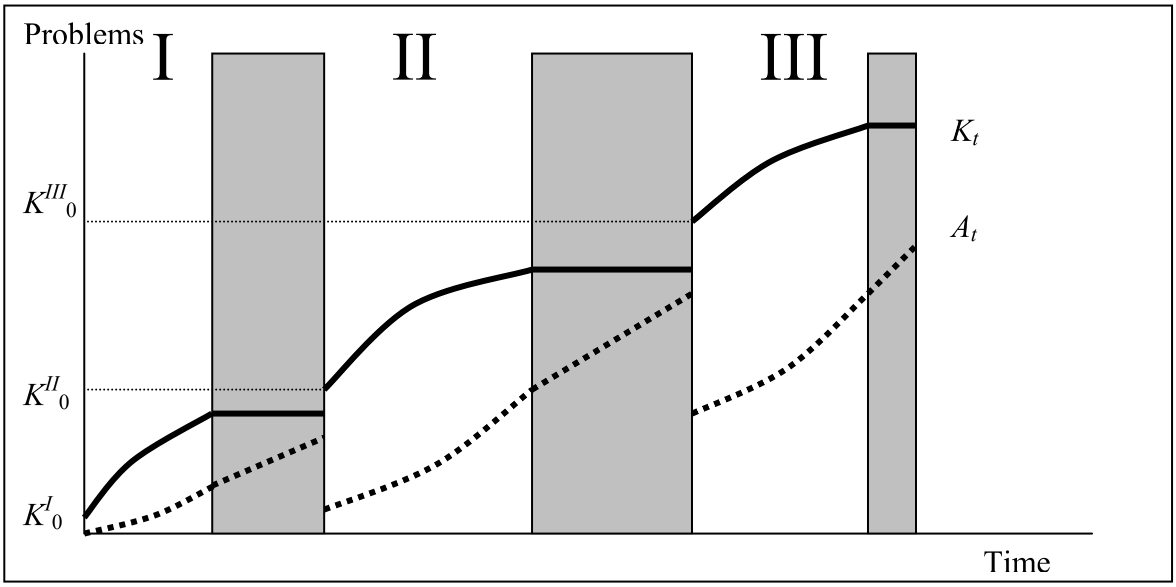
$$\frac{K^A{}_0}{K_t + A_t} > \frac{K_t}{K_t + A_t} \tag{9}$$

Now consider that every period the scientific community is in crisis, there is a positive and increasing probability that an alternative paradigm is proposed. If at that point it satisfies (9) it will be accepted and the crisis ends. In future work the process may be adjusted to include endogenisation or randomisation of parameters. Still the current set-up already captures the natural assumption that a higher share of anomalies in the number of problems will increase the attractiveness of alternative paradigms.

The flow probability of paradigm switching, $\theta(t)=\varphi(A_t)*\Pr(K^A{}_0>K_t)$, is now equal to the probability that a convincing alternative that satisfies (9) is proposed and drawn at time $t$. At the resolution of the crisis the scientific community returns to normal science but starting from $(1-\xi)A_t$ anomalies and a new incumbent initial knowledge level $K^I{}_0= \xi A_t+\zeta K_t > K_t$. Now consider the resulting dynamics.

## The Structure of Scientific (R)Evolutions

Figure 1 illustrates how the key variables in the model evolve over time. The figure shows the evolution of the key state variables $K_t$ and $A_t$ over 3 cycles. Initially, under paradigm I knowledge, K grows at a decreasing rate whereas the number of anomalies grows at an increasing rate according to equation (3). As $A$ rises, so does the probability of a crisis. When it emerges (grey area) the accumulation of knowledge stops while anomalies continue to increase in a linear fashion. As anomalies accumulate the probability of accepting a new paradigm increases. The length of the crisis is, however, a random variable.



At the end of the crisis paradigm II is accepted. Knowledge starts to grow from a new, higher level of initial knowledge. To reach that higher initial level a number of open anomalies must be incorporated, which causes the number of anomalies to fall. Once more the number of anomalies starts to grow at increasing and the knowledge staock starts to expand at a decreasing rate, until a new crisis emerges.

**Concluding Remarks**

Now the crucial point in this paper is to see that entrepreneurship can use the knowledge, $K_t$, accumulated in science as their raw material for thinking up new products and services. In that sense $K_t$ can be translated directly into $n^P$ and constitutes the universe of opportunities for the entrepreneur. Vision and entrepreneurial talent are now required to formulate commercially viable ideas for products and services from this stock of fundamental knowledge. As was argued above scientists are not primarily interested in or particularly talented for this activity. It is the entrepreneurs that seize such opportunities and bring them to the realm of commercial enterprise. It is however likely that entrepreneurial activity is easier and more rewarding when there is a large universe of opportunities out there. Hence, given the dynamics of scientific evolution, it is to be expected that in a paradigmatic crisis entrepreneurial activity stalls. The entrepreneurs are less succesful in generating new ideas and therefore more likely to engage in other activities. R&D labour would then reallocate towards productivity enhancement in incumbent firms.

Similarly a breakthrough in science causes a jump in $K$ followed by relatively steep increases in the early stages of paradigm exploration. This boom will also attract more entrepreneurial activity and reallocate R&D labour towards product development. These cycles in activity are likely to translate into cycles in economic growth, cycles in R&D activity and ultimately even in cycles in the labour market at large. Technological development is not a steady walk uphill, but a rocky ride on the waves of an unchartered sea. It should be no wonder then that we face waves of product innovation and face cyclical rather than steady state growth.

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