

## **Fruitful and Barren Developments in Economics\***

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### **Abstract**

The development of research in economics proceeds in five phases: (1) novel ideas and great insights appear; (2) the number of contributions explodes and the marginal returns of insights gained decrease; (3) the original idea is revitalized; (4) decreasing marginal returns to insights set in again; and (5) new directions of research emerge. These stylized phases are illustrated by Growth Theory, Constitutional Economics, Political Business Cycles and Laboratory Experiments.

The terms “fruitful” and “barren” are related to particular phases in the development of economic research. The barren periods are due to the specific institutional conditions under which scholars have to operate: the dominant need to publish and to get cited. These goals are defined by internal scholarly standards and requirements not devised to create new insights. It is high time to seriously reconsider the institutional foundations on which economic science is taking place. Some suggestions are made.

### **Zusammenfassung**

Die Entwicklung der wirtschaftswissenschaftlichen Forschung vollzieht sich in fünf Phasen: (1) Neue Ideen und Einsichten entstehen; (2) Die Zahl der Beiträge explodiert und der Grenznutzen zusätzlicher Einsichten schwindet; (3) Die ursprüngliche Idee wird wiederbelebt; (4) Es treten wieder abnehmende Grenzerträge auf; Und (5) neue Forschungsrichtungen entstehen. Diese stilisierten Phasen werden anhand der Wachstumstheorie, der Konstitutionellen Ökonomik, den Politischen Konjunkturzyklen und der Laborexperimente illustriert.

Die Begriffe „öde“ und „fruchtbar“ beziehen sich auf diese Phasen der Entwicklung der Wirtschaftswissenschaft. Die öden Perioden entstehen durch die spezifischen institutionellen Bedingungen, denen Forschende unterworfen sind: Der Zwang zu publizieren und zitiert zu werden. Die Ziele werden durch die internen wissenschaftlichen Standards definiert und fördern nicht kreative neue Einsichten. Die institutionellen Forschungsbe-

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dingungen in der Wirtschaftswissenschaft sollten dringend revidiert werden, wozu einige Vorschläge gemacht werden.

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## 1. Developments in Economic Research

This paper proposes that the development of many fields of research in economics takes place in five phases: (1) novel ideas and great insights; (2) explosion of the number of contributions and decreasing marginal returns of insights gained; (3) revitalization of the original idea; (4) decreasing marginal returns to insights and (5) new directions of research. These phases are stylized and will only be sketched here. The five phases are illustrated by three applications referring to developments of substance, and one relating to methods of analysis. It is thus argued that the phases apply to developments of both content and techniques of inquiry.

The phases and their application are considerably influenced by the personal experience of the author. Other scholars may well perceive different phases. Moreover, they may identify other applications in line with, or contradictory to, the phases here distinguished.

In order not to overburden the reader, the number of citations to the literature is reduced to a minimum (but additional literature is easy to track).

Section 2. discusses the five stylized phases. The following section provides applications to Growth Theory and to two areas of Political Economy (or Public Choice), Constitutional Economics and Political Business Cycles, as well as to Laboratory Experiments as a particular research method. Section 4. analyses whether, and to what extent, these phases of development are efficient or a waste of scholarly resources, and considers what can be done about them. The final section draws conclusions from the author's point of view.

## 2. Stylized Phases of Research Development

Five different phases of how research proceeds can be distinguished. They only provide a broad outline and are necessarily sketchy (see Kuhn, 1970). However, as will be shown in the next section, they can fruitfully be used to order several important developments in economic research that have taken place over the last decades.

*Phase 1.* One, and sometimes several scholars, introduce a novel idea into standard economics. It provides fruitful insights helping us to better understand the economy and society. Such ideas are rarely totally new. Earlier economists

such as Adam Smith, John Stuart Mill, Joseph Schumpeter or John Maynard Keynes may well have discussed them in their work. Indeed, there is almost never anything totally new under the sun (but there are, of course, exceptions such as the introduction of the computer or internet). What matters is that the economists engaged in research, as well as the students being taught by them, previously were not aware of the idea.

It has always been difficult to introduce a new idea into a science that has been so established as economics has been for a considerable time. What is considered acceptable is enshrined in textbooks which are equal or similar all over the world, and whose content the students have to reproduce. Upcoming scholars have a strong incentive to observe this canon because they otherwise risk being considered incompetent.

*Phase 2.* Some novel ideas are taken up by scholars sensing that they provide substantial new insights into how the economy works. The respective scholars may be outsiders but they are able to get the young generation of economists interested. This sets off an avalanche of papers and books. Some established scholars jump on the bandwagon and start to publish in the new area. As a result, more and more researchers engage in the pursuit. This leads to a huge increase in the number of publications on the topic. At the same time specialized academic organizations and scholarly journals are founded.

After some time decreasing returns with respect to the insights gained set in. It becomes more and more difficult to contribute anything new, and more and more publications deal with minor issues, mostly with methodological intricacies. The field becomes barren. But it often takes considerable time for the scholars engaged to realize that the additional insights gained are minor, or are even absent.

*Phase 3.* The development of research is revitalized by scholars able to bring in a new slant into the subject. They often can build on an excellent training in methods with which they bring new life to the idea which otherwise has become sterile. Sometimes this also involves considering what other disciplines contribute to an issue. Often the new phase consists in applying strict neoclassical theory in the formal manner cherished by much of the academic community. When a new development consists in a novel methodological approach, the corresponding techniques are generalized and extended to new substantive areas.

*Phase 4.* The introduction of the most advanced techniques of research results in a surge of publications and again decreasing returns in terms of additional insights. This development may go on for a considerable time in particular because few scholars ask what additional substantive knowledge results from the more formal techniques employed.

*Phase 5.* Some scholars begin to reject the whole novel idea and revert to the state of orthodoxy before the idea was introduced. The original insight may

even turn to the opposite. Others turn to totally different theories and approaches little or even unconnected to the original idea.

This discussion is extremely sketchy and is only of interest if applied to particular developments in economics. This is the subject of the next section.

### 3. Stylized Phases Applied

The three first applications of the phases distinguished refer to substantive novel areas of economic research, while the fourth applications refers to a methodological innovation.

#### 3.1 Growth Theory

The Harrod-Domar model (Harrod, 1939, 1948; Domar, 1946, 1957) viewed economic development as driven by capital. The rate of growth of the economy was taken to be proportional to the investment share, given the capital coefficient. This was an original insight compared to the then standard stationary models of the economy (Phase 1) and led to a flood of publications (Phase 2). A considerable extension took place when the neoclassical economists started to engage in the subject. They introduced the concept of a production function with labour, capital and technical progress as determinants of aggregate output (Solow 1956, 1957). The share of investment was optimized by maximizing a social welfare function subject to the production constraint. The “Golden Rule of Accumulation” (Phelps 1961)<sup>1</sup> and capital theory became the most prominent subject of a large number of technically well-equipped economists. Later technical progress was made endogenous in particular by relating it to changes in relative prices, as well as considering positive externalities and spillover effects (Barro, 1990, Romer, 1990, Grossman/Helpman, 1994). A specialized journal was founded (*Journal of Economic Growth*) (Phase 3). After some time the subject became visibly barren. The contributions almost exclusively dealt with mathematical intricacies but did not push the topic forward (Phase 4). As a consequence, a few economists started to engage in totally different aspects of economic growth, most importantly its relationship to the natural environment. Some of them even questioned the whole concept of economic growth, arguing for limits to growth, or even returning to the idea of a stationary economy (Hirsch, 1976). The relationship of steadily increasing income to subjective well-being has also been questioned by results produced in happiness research (Frey/Stutzer, 2002; Layard 2005). The Easterlin Paradox (Easterlin, 1974,

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<sup>1</sup> The Golden Rule was found at about the same time by several other economists, among them Joan Robinson (1956), Trevor Swan (1956) and Carl Christian von Weizsäcker (1965).

2001) even claims that raising income does not increase subjective life satisfaction over time (Phase 5).

### 3.2 Constitutional Economics

Buchanan and Tullock (1962) revolutionized economic thinking by rejecting the notion that politicians and public officials want to pursue the welfare of the population. Instead, they argued that these actors pursue their own interests in the same way as everyone else does. Their selfish behaviour can only be checked by constitutional rules because politicians and bureaucrats would oppose any restriction of their power in the current political process. These rules of the game can only be arrived at by a constitutional contract to which all, or at least a large number of individuals freely consent. This is possible only behind a veil of ignorance, where individuals do not know in which situation they will be in the future (Phase 1). This novel view of economic policy was adopted by younger economists and led to a surge in publications (see Mueller, 1997). A new society and new journals were established (*Public Choice Society*, *Public Choice*, and *Constitutional Political Economy*) (Phase 2). Economists committed to the neoclassical paradigm such as Persson/Tabellini (2002, 2003) entered after a considerable time and formalized the subject. A major contribution was made by Acemoglu/Robinson (2006, 2012). They sought to identify exogenous differences or changes in constitutions in order to analyse their effect on the economy (Phase 3). There are still many economists pursuing this line of endeavour but with few additional insights (Phase 4). Many economists returned to the situation before the advent of constitutional economics. They assume again, as a matter of course, that governments maximize the welfare of the population. A case in point is “Liberal Paternalism” (Thaler/Sunstein, 2003, 2009), which takes for granted that politicians and public officials propose options to individuals helping them to raise their welfare. Others reject the notion that human behaviour is shaped by institutions but argue that economic success is due to culture which is the more primitive concept (Guiso/Sapienza/Zingales, 2015) (Phase 5).

### 3.3 Political Business Cycles

Economic Policy based on the economics of Keynes assumes that governments make an effort to reduce business cycles. In this respect Keynesian Economics is in line with traditional neoclassical theory. This assumption was basically challenged by the novel idea that governments create rather than dampen business cycles in order to raise their election chances (Kalecki, 1943; Akerman, 1947; Frey/Lau, 1968; Nordhaus, 1975) (Phase 1). This notion led to an extensive literature mainly empirically analysing the various forms of politico-economic cycles (see e.g. the surveys by Frey, 1978, Mueller, 2003). In addi-

tion, endogenous election timing has been studied (e.g. Frey/Schneider, 1981) (Phase 2). Many neoclassical economists (e.g. Alesina, 1987, Rogoff/Siebert, 1988, Alesina/Roubini, 1997) formalized political business cycle theory and improved the econometric estimates (Phase 3). This induced a large number of contributions producing few new insights (Phase 4). Political Business Cycles are largely disregarded today and many economists assume again that governments endeavour to reduce business cycles. Economics thus reverts to the position before the advent of the idea of selfish politicians and bureaucrats (Phase 5).

### 3.4 Laboratory Experiments

In economics it has always been taken for granted that it is not possible to run laboratory experiments to study economic problems. German economists around Sauermann, in particular Selten (1961, 1970, see Ockenfels/Sadrieh, 2010), followed by others (Smith, 1962, 1965; Kahneman/Tversky, 1979) changed this view. They analysed typically economic decisions such as those relating to public goods and ultimatum and dictator games (Güth et al., 1982). These experiments produced outcomes at variance with existing economic theory, in particular demonstrating that individuals under many conditions are not purely rational persons maximizing their own selfish utility (Phase 1). The results induced many economists to relinquish the notion of the classical homo oeconomicus and to engage themselves in laboratory experiments. A large number of departments of economics in many countries established laboratories in which further experiments were conducted. After virtually thousands of experiments on public goods and ultimatum and dictator games had been run, new insights became rather scarce. Moreover, Experimental Economics became a subject of its own, focusing on internal validity rather than on external validity. Several societies were founded as well as a specialised scholarly review (*Journal of Experimental Economics*). This tendency has been strengthened by the need to keep lab experiments going because the university rectors and presidents would not appreciate seeing the very costly laboratories unused (Phase 2). More open economists have revitalised the method by engaging in field and natural experiments whose external validity is considerably greater (Harrison/List 2004; Levitt/List 2009). (Phase 3). It turned out to be difficult to run interesting laboratory, field and natural experiments resulting in a limited number of contributions providing new insights, in particular with respect to practical policy applications (Phase 4). A considerable number of economists now engage in Randomized Controlled Trials explicitly used to inform practical policy advice (Banerjee/Duflo, 2011). Other scholars have turned to different approaches, most importantly to Neuroeconomics (Camerer et al., 2005). The Big Data or Social Physics (Pentland 2014) movement constitutes another radical departure from laboratory experiments as it looks at real-life behaviour (see e.g. Helbing/Balielti, 2011; Helbing/Kirman, 2013; Lanier, 2013). It re-

lies on massive, real-time streams of data gained from all areas of life in which human beings act. The approach is based on statistical regularities involving the whole population and therefore taken to be true for almost everyone almost all the time. As a consequence, some scholars believe that the enormous size of the data collected makes it unnecessary to first develop a theory and to then analyze the data on that basis. Rather, the regularities derived from the data masses are taken to suffice to gain insights how society works. Social Physics is claimed to be no less than a third way besides Adam Smith's market and Karl Marx's classes (Pentland, 2014: 191) (Phase 5).

#### 4. Fruitful or Barren Developments?

The phases of barren developments of research in economics may well be *accepted* as a necessary feature of scientific endeavour. One may argue that periods of barren developments are needed in order to allow for fruitful periods producing important additional knowledge. It would indeed be mistaken to think that research can and should be planned in advance. New insights cannot be produced on command. To directly try to regulate and interfere into the phases here identified would most probably result in a failure. The barren periods must be tolerated as a precondition for scientific progress.

Another approach to the sequence of fruitful and barren phases of economic research may be to *advise scholars* to be more open, more flexible and more entrepreneurial. They should be more open to new ideas, and should not try to improve them by minor technical details. Rather, researchers should continually try to find out where promising novel ideas arise. They should leave an area as soon as the first signs of decreasing marginal insights appear. If scholars followed this advice the periods of fruitful scientific advance would be more prominent, and the barren periods of research shortened.

Such a purely normative approach sounds nice but is most unlikely to have any effect. Scholars are (more or less) rational actors and behave accordingly. They engage in those areas of research advantageous from their point of view. To be successful in today's economic research is not so much based on pursuing original ideas but rather on excelling in publishing in highly rated journals, and in accumulating as many citations as possible. The rankings of economics journals in terms of impact factors dominate the thinking of academic economists, in particular of those who enter academia. The conditions for a career in an economics faculty are so clearly specified and dominant that an individual scholar is obliged to follow them. Close to nobody wanting to get a grant, a scholarship, or to be seriously considered for a professorship can allow himself or herself to deviate from these requirements. Most young scholars have totally integrated these demands and do not consider them as restricting their autonomy (Feld et al., 2014).

The “publish or perish” rule does not motivate scholars to engage in scientific work leading to novel insights. Rather, they have a strong incentive to passively follow the crowd and to add some minor, often purely technical, points (Frey 2003). It has been argued long ago (Machlup, 1958) that there may be “too much research” devoted to issues of little relevance. A major reason for this behaviour is the restricted time period in which young scholars find themselves. In most universities they have to publish in leading journals within three, or at the most, five years. This requirement is difficult to achieve because the journals often take a long time (one year or more is no exception) after submitting a paper before they reach a decision. Even if invited to proceed, the author must prepare for between two and five rounds of revision. Each step takes considerable time, normally many months. It follows that young scholars must quickly come out with a paper ready for submission. They have no time to take up a possibly unsuccessful novel idea. Rather, they have an incentive to improve the already existing contributions, often by adding some slight variations or additions, which are easy to understand and to evaluate by the journal referees as they are uncontroversial. In contrast, it is well established that novel ideas such as those discussed in section 3. under phase (1) take considerable time to be acceptable to an average referee. This situation is worsened by the fact that many established scholars hand the paper they are supposed to referee themselves on to graduate students. The latter are well able to evaluate the technical features of an article but not a novel idea. They are afraid that their “boss” ridicules them if they support a particular idea as new when it is already widely known (except to themselves). Moreover, a novel idea is unlikely to be up to the technical standards the graduate students learn in their courses. This strong bias with respect to novel ideas existing in the present career system increases the length of barren periods.

There is by now an extensive literature regarding the major shortcomings of the existing career system in economics (and elsewhere) based on publications and citations (see e.g. Osterloh, 2010; Osterloh/Frey, 2014a, Osterloh/Frey, 2014b). It is sufficient here to mention only one. The impact factor on which the rankings of scholarly papers is based and which determines success in academia has fundamental weaknesses (e.g. Baum, 2011; Brembs et al., 2013, Osterloh/Kieser, 2015). If just one paper in a journal draws a large number of citations, while all the other papers published in the journal are never cited (and this is not an exception), the journal’s impact factor may be high. The number of citations divided by the number of papers published over the period of two years may reach a considerable number. As a result the journal is considered to be “leading”, though all the papers except one are totally disregarded by the economics community. This is an almost ridiculous error not to be committed by any scholar. The impact factor can also be easily manipulated, for instance by disregarding short papers, comments, and other communications from the nominator. This procedure jacks up the impact factor, which becomes even more unreliable as an indicator of scientific quality.

## 5. Conclusion

The terms “fruitful” and “barren” are taken not to relate to particular areas or methods in economics. Rather, they refer to particular phases in the development of economic research. The various phases can be considered to provide more or less additional insights into the world we live in. Phase 1 in which a novel idea appears, and Phase 3 in which the idea is more precisely formulated and empirically tested with advanced econometric methods, are mostly fruitful in this sense. The publications in Phase 2 become barren, as is the case in Phase 4 before scholars move to other areas (Phase 5). As pointed out, this evaluation is based on a personal view of the present state of economics. Other authors may see the situation quite differently.

I have argued that the barren periods in economics are due to the specific institutional conditions under which scholars have to operate: the dominant need to publish and to get cited. These goals are not directly related to knowledge helping us to better understand the economy and society. Rather, they are defined and informed by internal scientific standards and requirements not devised to create new insights. They may, but need not, do so.

It is high time to seriously reconsider the institutional foundations on which economic science is taking place. One possibility is to take the internet revolution seriously. It no longer is limited in space. Therefore, it should be possible to publish all articles online, and to leave it to the readers whether they are considered to substantially contribute to knowledge. The task of journal editors may no longer be seen to reject most papers submitted (90 to close to 100%) but to select those articles from the internet they find important. At present, this option is to a large extent blocked because the journals do not want to consider papers that have appeared on the internet. Such a procedure would give a sign of quality to potential readers (similar to journal publications thereafter included in readers).

Another possibility to reduce the length of barren periods in economic research is to engage outsiders in the evaluation process. Scholars from other disciplines, say political science, psychology, law or philosophy, could be asked to state what economics contributions they consider relevant. The signalling process of what furthers a scholarly career would then not solely reside in academia. It would be important to involve politicians, public officials and practitioners in the evaluation process. They should also be able to indicate what parts of economic research they find particularly relevant, not least because as taxpayers they provide the resources used in academic research. Care must be taken that these evaluations do not become victim to fashions.

A lot of thinking should go into devising appropriate institutions which re-establish the precedence of insights, relevance and arguments over rankings and measurements.

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