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In defence of the linear model: An essay

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ABSTRACT

This essay discusses the strength and weaknesses of the so-called linear model (LM) of innovation. It is a reaction to the habit of criticising it as over simplistic, mechanistic, or simply blatantly wrong. We argue that, while some criticisms are of course well grounded, many others are instead based on loose interpretations and unwarranted assumptions. In order to separate the wheat from the chaff, this essay first presents a comprehensive description of the linear model and differentiates it from the caricature many refer to. Second, we discuss the main criticisms put forward and argue that many of them are not at all destructive, but can be easily accepted within a refined version of the LM. Third, we discuss the policy implications often derived (or said to derive) from the LM to argue that the LM itself is distinctively policy-neutral. Other assumptions have to be added to justify alternative policy implications.

1. Introduction

This paper was prompted by the increasing dissatisfaction with the current trend in the economic and social studies of science, technology and innovation, towards generalized criticism of the so-called 'linear model' (LM). Even cursory perusal of the introductory sections of many of the papers published even in the most prestigious journals in the subject (let alone working papers) shows that almost invariably they include statements such as: 'it is now well established that the LM is wrong . . .'. In 1996, Christopher Freeman already lamented that 'No model of the innovative process has been more frequently attacked and demolished as the so-called linear model of innovation. At one time it was almost impossible to read a book or an article on technology policy or technological forecasting that did not begin or end with such a polemic' (Freeman, 1996, p. 27). The situation has not improved. Thus, it becomes a legitimate question to ask why is the LM continuously criticised if it is so patently wrong.

The sense of unease and dissatisfaction is compounded by the fact that it is difficult, in the critical literature, to find a precise definition of the so-called linear model. The term 'linear model' is used in different ways, to imply different meanings and characterisations, and can be tailored to the aims of a particular work. Also, critiques of the LM encompass a variety of different – and often mutually incompatible – arguments and implications, which do not derive from the model itself, but rather have their roots in other approaches. Accordingly, the disgrace of the LM among academics has opened up a Pandora's box in terms of the sheer range and variety of policy and managerial implications advocated and implemented on the basis of some of the supposed shortcomings of the LM. But when everything becomes possible, a sceptical reaction becomes legitimate.

Criticism to the LM is so widespread that we feel unnecessary to pinpoint precisely names and references. If anything, it is only fair to acknowledge that the authors of this paper have been frequently guilty of 'linear model bashing' (just as one example Dosi et al., 2005). Hence, we put the blame on ourselves. We do in fact believe that the LM has serious limitations as a conceptualisation of the innovation process valid in general and that significant progress has been made in the past 25 years or so in providing a much deeper understanding of how innovation is generated and proceeds. But it is precisely because of this that we feel we can rightly claim that critique of the LM has gone too far and, in some cases, has been instrumental in creating confusion rather than a better understanding.

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In this paper, we seek to probe the deeper reasons for our unhappiness with undisciplined critiques of the 'infamous' LM and to clarify their most relevant shortcomings. We shed light on those features of the LM which might still have some interpretative and normative validity. While we do acknowledge all the limitations implied by unwarranted linearization of non-linear processes, we also must remind ourselves that one of the fundamental aims of researchers is to develop conceptual models that creatively and intelligently simplify reality.

This paper is not a historical reconstruction of the origins of the linear model. The history of the linear model has already been told by, among others, Godin (2006), Edgerton (2004) and Hounshell (2004). While we rely on this literature to ground our arguments, we do not contribute to it. Neither is this an 'empirical' paper which claims to substantiate the validity of the LM. While we do provide examples, we do it in order to clarify our arguments, not to demonstrate them.

Rather, this paper is strictly an essay whose aim is identifying and correcting the 'common sense' understanding of the linear model, which we find loose, imprecise and terribly unfair to a vast community of scholars and policy-makers who have greatly advanced our understanding of the dynamics of science and technology. We agree that the 'common sense' version owes its origin to the supporters of the LM themselves at the height of its popularity. They were certainly responsible for a rigid, unflexible presentation of the arguments on which the model is based, often in order to make their case when allocation of resources was at stake. But time is ripe, we believe, to separate the wheat from the chaff. Specifically, in this paper we argue in (partial) defence of the linear model building upon four building blocks.

- First, we claim that the LM – as usually referred to – is a rather oversimplified version of a much richer and subtler set of propositions. Moreover, these propositions are not necessarily and logically linked to each other. Critiques are advanced in some cases towards some of these propositions and against all of them in other instances. Much criticism of the linear model remains however often undisciplined and generic, including incoherent or mutually contradictory arguments.
- Second, we argue that not all the critiques are really destructive and that many of the standard objections to the linear model might be easily accommodated within it.
- Third, we claim that the really destructive critique to the LM has to do with the recognition of the systemic, dynamic, interactive nature of innovation. At the same time, however, we raise a warning that critiques along this line run the risk of leading to an alternative model (as extreme as the standard version of the LM) 'where everything depends on everything else', if the specific structure of the system is not fully and clearly specified.
- Fourth, we argue that the linear model as such does not imply any specific normative prescription (except a rather general support to basic research) and therefore it should not be blamed on these grounds.

These arguments are developed in the paper with reference to three distinct dimensions of the LM: the cognitive representation of the innovative process, the identification of the actors involved and the normative implications of the model. Thus, in Section 2 of this essay we sketch a brief history of the LM, identifying the main arguments put forward by Vannevar Bush and by other authors (Maclaurin and Furnas) who made a fundamental contribution to the construction labelled the LM. In Section 3 we identify the main critiques advanced against the LM and point to various instances of loose interpretations which attribute to the LM problems of which it does not unduly suffer. Section 4 discusses the different, and conflicting, policy implications which have been 'grounded' in the LM. Section 5 concludes the essay.

2. The origins of the linear model

2.1. Did the linear model ever exist?

The LM of innovation is often treated as a sort of 'folk model', whose authors and meanings tend to remain nebulous. For example, Edgerton (2004) argued that the linear model is but a rhetorical device used to avoid 'critical engagement with the much richer models of innovation developed by academic specialists in innovation' (2004, p. 31). While many off-the-hand rebuttals of the linear model are consistent with such view, Godin (2006, 2008a,b,c) put forward the idea that the linear model is a complex set of constructs introduced over time by well different authors to explain and legitimate the activities of different professional communities. First, throughout the first half of the 20th century, natural scientists contributed to it by identifying basic research as the source for applied research or technology.[1] These works were already pretty influential. Second, between the 1920s and the 1960s industrialists and social scientists (mainly from business schools) extended it to include also activities focusing on technological development. According to Godin (2008c), the first version of the complete LM was suggested in the 1920s by Maurice Holland, director of the Division of Engineering and Industrial Research of the U.S. National Research Council.[2] Holland systematized a view that was already present in industrialists of the time, as is witnessed by the book of Mees (1920), director of the Research Laboratory at Eastman Kodak. Later within this stream one finds the classical analysis conducted by Furnas (1948).

Third, from the 1950s applied economists further extended its scope linking to it issues of diffusion and use. Godin (2006, 2008a) analyses how the linear model was then taken over by economists (such as MacLaurin) who provided the foundations to Vannevar Bush's '*Science: The Endless Frontier*' (1945), credited by many as the origin of the linear model.

According to Godin the LM is more than what is found in Bush's report for at least two reasons. First, the report contains only a 'rudiment' of the model, since the sequence of steps that lead from science to innovation is not articulated[3]; second, it presents only a macro-version of the LM (the 'aggregate LM', to use Freeman's (1996) expression). The aim of Bush, who focused only on the role played by science in fostering human progress, was to obtain wide and substantial financial and institutional support to basic

[1] 'From the time of the ancient Greeks to the present, intellectual and practical work always have been seen as opposites. The ancients developed a hierarchy of the world in which *theoria* was valued over practice. This hierarchy rested on a network of dichotomies that were deeply rooted in social practice and intellectual thought . . . A similar hierarchy existed in the discourse of scientists: the superiority of pure over applied research.' (Godin, 2006, p. 641).

[2] 'During World War I, the US National Academy of Sciences convinced the federal government to give scientists a voice in the war effort. A National Research Council was thus created in 1916 as an advisory body to the government. Very rapidly, the Council developed an interest in industrial research. In fact, the close links between the National Research Council and industry go back to the beginnings of the Council. Industrialists were called upon in the First World War's research efforts, coordinated by the National Research Council. After World War I, most big firms became convinced of the necessity of investing in research, and began building laboratories for the purpose of conducting research. In this context, the Council was part of the "movement" to persuade more firms to invest in research.' (Godin, 2008c, p. 7).

[3] 'Bush talked about causal links between science (namely basic research) and socioeconomic progress, but nowhere did he develop a full-length argument based on a sequential process broken down into its elements or that suggests a mechanism whereby science translates into socioeconomic benefits'. (Godin, 2006, p. 640).

research. The subsequent institution of the National Science Foundation owns much to Bush's efforts.

Besides tracing the intellectual origin of the LM in the literature that preceded Bush, Godin (2006) also argued that over time the LM gained strength in practice with the help of statistical offices, which used the main categories introduced by it to define methodological rules for collecting data (e.g. the OECD's Frascati Manual adopted in 1963 largely builds upon this literature).

Despite Godin's accurate account of how the linear model was generated, its intricacies, its intellectual foundations and policy applications, many studies of science policy and innovation have used the term devoid of empirical content and analytical complexity. This use is emphasized by Edgerton, according to whom the LM never actually existed as a well defined model, but only as a straw man to be condemned as simplistic and inaccurate. He credits earliest introduction of *the term* to Price and Bass (1969) and Langrish et al. (1972), who both criticised the model, but with quite different interpretations,[4] and observes that only after the 1980s the term became popular. Edgerton also rejects the claim of the influence of the LM on policy.

Edgerton's 'interpretation-by-denial' is criticised by Hounshell (2004), who maintains that the LM can be seen as a system of belief, a heuristic that simply states that the new knowledge generated by investment in fundamental, unfettered research will, at some point in the future, yield radically new inventions and technologies. Moreover, consistently with Godin, Hounshell argues that in the United States, from the end of the Second World War up to the early 1970s, the linear model was very real. It was used to make the case for the United States government's funding of scientific and engineering research at universities and was the basis of the R&D strategies of companies such as DuPont, which established the first fundamental research programs in American history (DuPont was specifically interested in basic research to produce new nylons).

2.2. 'Science: The Endless Frontier' – the main arguments

At any rate, the so-called LM is routinely identified with Vannevar Bush's 1945 report to the President of the United States of America: *Science: The Endless Frontier* (see Freeman, 1996; Stokes, 1995, 1997; Cohen et al., 2002; Hounshell, 2004, just to name some authors). We agree with Godin that Bush's report is not the correct reference for an exposition of the LM: it was a policy document, meant to raise support for public funding of basic research, which did not propose a fully blown theory, let alone a model. However, given that *Science: The Endless Frontier* is customarily referred to by the critics as the main origin of the LM, we start our discussion from this document. We stress here that this paper is focused on discussing the critiques of the LM, not in offering an original contribution to its genesis and history. Thus we need to concentrate on the very version of the LM the critical authors most commonly referred to.

In Bush's report, we find five main arguments, which are interconnected, even intermingled, but need to be distinguished. Identifying each of these five building blocks is the first step in our effort of 'deconstructing' the linear model.

First, Bush claimed that scientific progress is essential to technological innovation and economic development. New products, new industries and more jobs are founded on continuous additions to the knowledge about the laws of nature, and the application of that knowledge for practical purposes:

> We will not get ahead . . . unless we offer new and more attractive and cheaper products. Where will these new products come from? How will we find ways to make better products at lower cost? The answer is clear. There must be a stream of new scientific knowledge to turn the wheels of private and public enterprise. (Bush, 1945, http://www.nsf.gov/about/history/vbush1945.htm#ch3.5)

This essential new knowledge, he maintained, could be obtained only from basic scientific research, which 'creates the fund of new knowledge from which the practical applications of knowledge must be drawn'. Bush quoted two specific examples, health care and defence, and stressed that discoveries in these fields (such as penicillin and radar) often arose from remote and unexpected sources. He claimed that it was in the 20th century that basic research had clearly become 'the pacemaker of technological progress', since in the nineteenth century 'Yankee mechanical ingenuity. . . could greatly advance the technical arts' (Bush, 1945, http://www.nsf.gov/about/history/vbush1945.htm#ch3.5).

Second, Bush made a point of distinguishing clearly basic and applied research:

> Basic research is performed without thought of practical ends. It results in general knowledge and an understanding of nature and its laws. This general knowledge provides the means of answering a large number of important practical problems, though it may not give a complete specific answer to any one of them. The function of applied research is to provide such complete answers. The scientist doing basic research may not be at all interested in the practical applications of his work, yet the further progress of industrial development would eventually stagnate if basic scientific research were long neglected. (Bush, 1945, http://www.nsf.gov/about/history/vbush1945.htm#ch3.5)

Third, Bush argued that in order to sustain basic scientific research it was necessary to train a large pool of scientists and to strengthen the centres of basic research, which he identified with colleges, universities and research institutes. These institutions – according to Bush – provide the environment that is most conducive to the creation of new scientific knowledge and least under pressure for immediate, tangible results.

> Scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown. Freedom of inquiry must be preserved under any plan for Government support of science. (Bush, 1945, http://www.nsf.gov/about/history/vbush1945.htm#ch3.5)

It is in the institutions devoted to basic research that 'scientists may work in an atmosphere which is relatively free from the adverse pressure of convention, prejudice, or commercial necessity'.

Fourth, Bush suggested that science is a proper concern for government, because:

> It has been basic United States policy that Government should foster the opening of new frontiers. It opened the seas to clipper ships and furnished land for pioneers. Although these frontiers have more or less disappeared, the frontier of science remains. It is in keeping with the American tradition – one which has made the United States great – that new frontiers shall be made accessible for development by all American citizens. Moreover, since health, well-being, and security are proper concerns of Government, scientific progress is, and

[4] Price and Bass (1969, p. 802) criticise the LM as implying that innovation can be analysed as a 'rational' and 'orderly' process, 'starting with the discovery of new knowledge, moving through various stages of development, and eventually emerging in final, viable form'. Langrish et al. (1972) discuss and criticise two linear models of innovation, based on the 'discovery-push vs the need pull' distinction. See Edgerton (2004).

must be, of vital interest to Government. Without scientific progress the national health would deteriorate; without scientific progress we could not hope for improvement in our standard of living or for an increased number of jobs for our citizens; and without scientific progress we could not have maintained our liberties against tyranny. (Bush, 1945, http://www.nsf.gov/about/history/vbush1945.htm#ch3.5)

Fifth and last, Bush maintained that the most important ways in which government can promote industrial research and increase the flow of new scientific knowledge are through support of basic research,[5] the provision of suitable incentives to industry to conduct research, and by strengthening the patent system to eliminate uncertainties that bear especially on small firms. He also recommended that ways should be found to spread the benefits of basic research to industries that currently did not utilise new scientific knowledge.

2.3. Before and beyond 'Science: The Endless Frontier'

Much of recent criticisms against the linear model talks to the points sketched above. We shall discuss them again later in Section 2.4 referring to them as the LM *in strong form* (i.e. the version of the LM that, we think, most undisciplined critiques refer to). However, we wish to stress again that these points do not offer a comprehensive presentation of the arguments put forward over time by those authors who mostly contributed to a linear representation of the process leading from research to innovation (Godin, 2006). In this section, we briefly introduce two other main contributions, those of Maclaurin and of Furnas, which offer versions of the LM complementary to Bush's one, respectively focused on the meso/micro and micro/managerial sides the model. In Sections 3 and 4 we shall rely on Maclaurin's and Furnas' arguments to respond to some of the critiques to the LM.

Starting with Maclaurin, his main concern was understanding the role of innovation and entrepreneurship in the growth of industries and the economy. Notably, he carried out an important meso- and micro-level study of the process of innovation and the development of the radio industry (Maclaurin and Harman, 1949; Maclaurin, 1950), that he considered 'a direct outgrowth of a revolution in the science of physics and its applications to the study of electricity' (1950, p. 92).

> Radio is an industry in which scientists and engineers have forced the pace of technological innovation. As a consequence, there have been radical shifts in the product and its applications about every ten years since 1900. I believe that such industries will increasingly become the norm and that we can expect existing products to be rendered obsolete almost continuously for many years to come. (1950, p. 91).

In this type of industry, 'science and technology' can be broken down into five distinct stages: fundamental research, applied research, engineering development, production engineering and service engineering' (1947 and 1949: Preface, XVII). He also believed in the emergence of 'a class of scientific entrepreneurs' attempting to apply the latest advances in the physical and social sciences to the solution of their problems (1950, p. 112). Relatedly, he thought that special efforts were needed to ensure a flow of capital into start-ups active in introducing new products.

In a paper published in 1953 Maclaurin sketched a model of innovation and its diffusion which anticipates the following 30 years of research in the economics of innovation. His main argument is that in 'studying the determinants of investment in any advanced economy, it will be significant to assess the variations in the following factors: the propensity to develop pure science, the propensity to invent, the propensity to innovate, the propensity to finance innovation, the propensity to accept innovation. (1953, pp. 97 and 98).[6] Particularly interesting are the many conceptual subtleties introduced to support his view. With regard to pure science, we find that 'it cannot be assumed that pure science is undertaken without any thought of material ends... the doctrine of material progress has been too strong an element in our culture... The important point is that some scientists have been willing to speculate deeply and widely *without immediate practical objectives in mind*'. (1953, pp. 98 and 99). Moreover, with regard to the steps of invention and innovation, Maclaurin anticipated that science is not the only source of ideas for innovation, as 'new uses' may emerge for established products and the most likely source for such ideas is not the pure scientist, but rather the industrial innovator.

Maclaurin also developed a taxonomy of industries on the basis of the rate of technological progress (industries with very high, high, medium and low progress), where progressiveness depended on whether research and engineering were 'directed primarily to refinements in existing products rather than to radical improvements or the creation of entirely new products or processes' (1954, p. 180). By the time he wrote this, 5 years after the book on the history of radio industry, he had clearly in mind that many important industries of 'the second industrial revolution', like the auto industry, were 'far more concerned with style changes than with fundamental research on transportation' (1954, p. 185).

In conclusion, Maclaurin's account of the LM adds a clear articulation of the sequence of phases, but also unambiguously limits the interpretative scope of the model to science-based industries, such as radio. What remains to be seen is whether the managerial version of the LM, which was also elaborated in the 1940s, may be more justly credited for the LM bushing syndrome. A very influential exponent of this literature is C.C. Furnas, whose foundational contribution to the LM was published in a classic book on research management in 1948 (Furnas, 1948).[7] Furnas also started from the observation that it is the 'cross-over' of science and invention which lay at the heart of the new 'industrial revolution' which was becoming visible in the aftermath of WWII (Furnas, 1948, p. 1). In particular, he stressed the increasing role played by corporate R&D laboratories, without however implying that 'no advances can be made in industry without a formal research organization. Research organizations have not had in the past and will not have in the future a monopoly on sources of new ideas and improvements' (1948, p. 1).

In Furnas' text we also find the first diagram illustrating 'the steps that may be considered to be involved in transforming a new concept to a practical reality in the form of a new product' (Fig. 1).

[5] To assert the need of government support to basic research Bush also states that 'A nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill... We can no longer count on ravaged Europe as a source of fundamental knowledge. In the past we have devoted much of our best efforts to the application of such knowledge which has been discovered abroad. In the future we must pay increased attention to discovering this knowledge for ourselves particularly since the scientific applications of the future will be more than ever dependent upon such basic knowledge' (Chapter 3). We do not want to discuss this argument here, since we find it tangential to the core of the LM, which is the object of this paper.

[6] According to Godin (2008a, p. 349) 'Such a theorization or schematization of the technological innovation process as a "sequence" was the result of over a decade of Maclaurin's work on technological change. Maclaurin's communication was in fact the first full-length discussion and theory of what came to be called the linear model of innovation'.

[7] The origin of the managerial version of the LM is the management literature of the 1920s, as already said in paragraph 2.1. In the 1940s, before Furnas, also Stevens (1941) made a contribution. We refer to Furnas' one here due to the very wide recognition of his book in the field of research management.

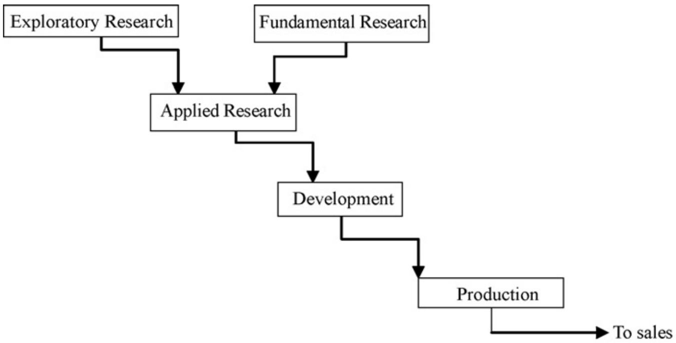

Fig. 1. Flow diagram: from research to sales.
Source: (Furnas, 1948, p. 4).

He considers the applied research phase, which is usually nurtured by the results of exploratory and fundamental research, as the most important for those industrial laboratories where the emphasis is on new products and processes. Development comprises 'the improvement, testing and evaluation of a process, material, or device resulting from applied research' and, in a broader sense, also 'the market evaluation of a prospective product' (1948, p. 8). In the production phase 'the research child grows to full maturity'. Feedbacks are also considered, since changes in production always bring problems that the research laboratories need to face.

Also in this version of the LM the arguments are both based on historical references and rich of nuances, which are completely lost in the inflexible and oversimplified model of the innovation process which is usually meant by LM.

To conclude, a number of contemporaries to Bush did provide finer grained analyses of the unfolding of the innovation process, e.g. identifying limits to the applicability of the LM across different industries, and also pointing out feedbacks across different stages of the innovation process. Nevertheless, memory of this broader – and deeper – discussion has faded. What we are left with is a common sense understanding of the LM, that is somewhat of a straitjacket, which, in depriving the arguments of any historical references and any nuance and subtlety, makes them appear an inflexible and oversimplified model of the innovation process.

2.4. The linear model in strong form

Here we try to summarize the main components of the 'Straitjacket model' – or 'linear model in strong form' – which became the main focus of 40 years of criticism. These components are the building blocks of the straw man used by many scholars of innovation to put forward claims about alternative models.

In particular, we distinguish the various arguments which have been made to coalesce into the compact graphical representation which has become to be known as the LM: Basic research → Applied Research → Development → Production → Marketing → Diffusion. Note that these arguments are not necessarily linked to one another – especially in terms of their normative implications – and it is possible to conceive of different, 'weak' forms of the LM based on combinations of only some of them. For example, it might be argued that science is the main driver of innovation but that there is not necessarily a clear sequence or feedbacks between science and technology. Similarly, representing the innovative process as a linear sequence from basic to applied research does not imply that the former should be publicly funded (or vice versa).

2.4.1. The LM in strong form – Part 1: The process

(1.1) A clear distinction can be drawn between basic (scientific) and applied (technological and industrial) research. The former is directed towards the understanding of fundamental principles; the latter is directed towards the development of practical applications of knowledge.

(1.2) Basic or fundamental or prior scientific research is the main or rather the unique source of technical innovation. Consequently, in order to innovate and to come up with new marketable products it is necessary and sufficient for firms to devote resources to R&D.

(1.3) New knowledge acquired through basic research trickles down, almost automatically, to applied research, technology and innovations, even within short time spans.

(1.4) The innovative process can be represented and conceptualised as a sequence of steps (or a pipeline) that starts with scientific research continuing through product development, marketing and subsequent diffusion of the innovation. In this process there are no feedbacks from later steps in the sequence to earlier steps.

2.4.2. The LM in strong form – Part 2: The actors and normative prescriptions

(2.1) There is a clear division of labour along the sequence among different types of agents who specialise in the various relevant stages. Typically, basic research is conducted in universities and public laboratories, while applied research and technological development are carried out by firms, especially large ones, which can afford expensive R&D investments.

(2.2) Universities can contribute to applied research primarily through the conduct of research and teaching, which represent their mission. Neither direct interaction with industry, nor encouragements to firms to develop the results of university research through some licensing mechanism which involves patenting are problems concerning universities. Rather, colleges, universities and research centres are the privileged locus of basic research precisely because they provide an environment free from prejudice as well as political and commercial pressures.

(2.3) The main prescription is that basic research – and therefore the agents performing it, typically universities – should be publicly funded, because profit-motivated private firms would not want to invest in such activities. This implies that the good delivered by basic research – new knowledge – should be in the public domain.

3. The critiques and their (partial) rebuttal

It is clear that most of the various arguments presented above deserved being criticised, especially since they were asserted in a rigid and inflexible form by the supporters themselves of the LM. However, while the importance of the contribution of many scholars for having attacked and destroyed the conventional wisdom that dominated the literature on innovation until the 1960s cannot be understated, we think that in some instances this critique is unjustified.

In examining the most important critiques advanced against the LM in strong form, in many cases we shall reassert their importance and validity, while discussing whether they are really destructive or rather they can be accommodated within a 'weak' LM (weaker in terms of boldness of the claims but probably more robust in terms of its generalizability). But we shall also show that in a few other important cases the critiques have indeed gone too far and tend to lead to improper generalizations.

3.1. The process: Research and innovation

Many of the critiques to the strong form of the LM have to do with the distinction between basic (scientific) and applied (technological) research, and how the former informs the latter (but not the other way around), i.e. propositions (1.1) and (1.2) above.

First, it is claimed that the distinction between basic and applied research is not clear-cut. Important contributions such as those of Rosenberg (1976, 1982), Rosenberg and Nelson (1994), Nelson and Rosenberg (1998) and Stokes (1997) have improved our understanding here. But, provocatively, a controversial but legitimate answer to this objection would consist in a further simplification of the LM, collapsing together in the same box basic and applied research.

Second, it has been noted that technological improvements often are unrelated to basic research (Kline and Rosenberg, 1986; Mansfield, 1991; Klevorick et al., 1995) and that in many industries science impacts on technological innovation only with very long lags. It has also been shown that technology frequently anticipates the scientific explanation (one of the best known examples being the defining of the Second Law of Thermodynamics in the 1820s by Sadi Carnot, which was driven by the need to understand the efficiency limits of the steam engine) and that not only is technology independent of new science, but it also provides essential inputs to scientific research, in the form of problems to be solved, instrumentation, etc. (Kline and Rosenberg, 1986; Pavitt, 1996; Sequeira and Martin, 1996). Other examples draw from fields such as vaccinology, where the development of a viable vaccine often precedes the 'scientific' understanding of why the vaccine actually works. These examples question the conventional time line orientation and suggest that the direction of causation should be – and in many cases is – reversed. Indeed, in some instances, it is suggested that a reverse LM (reverse, but still linear!) might provide a satisfactory approximation of how innovation proceeds.

While there are actually quite different sub-arguments involved in these set of remarks, many of these observations tend to acknowledge one way or another that incremental rather than revolutionary innovation is the main source of technological progress. This point has been emphasized repeatedly by sociologists and historians of innovation even before economists (see for instance Gilfillan, 1935).[8] But, to pick up just one important example, Kline and Rosenberg (1986, p. 282) in their path breaking and extremely influential article titled 'An Overview of Innovation', which is often seen as providing an alternative to the LM, make this point very forcefully:

> There is a tendency to identify technological innovation with major innovations of a highly visible sort... The fact is that much technological change is of a less visible and even, in many cases, an almost invisible sort. A large part of the technological innovation that is carried out in industrial societies takes the form of very small changes, such as minor modifications in the design of a machine that will enable it to serve certain highly specific end users better, or that make it easier and therefore cheaper to manufacture....

According to Kline and Rosenberg, the initiating step of most processes of technological transformation in today's world is typically design rather than research.

> Most innovation is done with the available knowledge already in the heads of the people in the organization doing the work, and, to a lesser extent, with other information readily accessible to them. It is only when those sources of information fall short of solving the problem that there is a need for research in order to complete a given innovation... The notion that innovation is initiated by research is wrong most of the time... Had the idea been true that science is the initiating step in innovation, we would never have invented the bicycle. (Kline and Rosenberg, 1986, p. 288)
>
> Contrary to much common wisdom, the initiating step in most innovations is not research, but rather a design. (Kline and Rosenberg, 1986, p. 302)

Akin but different from the argument that science is usually not the major source and initiating point of the innovative process, a significant body of research (e.g. Von Hippel, 1988, among many others) shows that users of products and processes are the developers of many important innovations that are later produced and sold by manufacturers. The mountain bicycle (Lüthje et al., 2005) is a case in point, which makes it clear that the sources of innovation are numerous and varied, and that the kind of knowledge required to innovating may be very local and 'sticky'.

We do certainly agree on the centrality of these points. Yet, there are *caveats* than can and should qualify the discussion.

First, the issue becomes now one of whether the LM applies only to a very limited piece of reality, as Kline and Rosenberg (1986, p. 293) suggest: 'New science does sometimes make possible radical innovations. These occurrences are rare'. It is worth recognizing that in the 20th century the emergence of major new technological paradigms has frequently been directly dependent on and linked to major scientific advancements: synthetic chemistry and continuous catalytic processes, solid state physics and transistors, molecular biology and biotechnologies being the most common examples (see e.g. Nelson, 1962; Dosi, 1988). It is also claimed that in the last two or three decades the role of science as a major source of innovation and as a driver of the high tech industries expansion has further increased. For example, Bonaccorsi and Thoma (2007, p. 814) argued that:

> 'In the 1990s, the notion that technological developments are increasingly dependent on advancements in science was proposed repeatedly. On one the hand, scientometric literature drew attention to the sharp increase in the number and share of non-patent literature citations in patents... On the other hand, industry case studies...illustrated important examples in which the very definition of industrial applications was only made possible by the discovery of new physical properties of nature'.

In our view it is not obvious that the role of science in inducing innovation has become generally more important (or not) in recent times. This issue is tangled and deserves more research and therefore we do not enter this discussion. But we propose that, although the generality of the LM is certainly reduced by the recognition that science is neither always, nor in the majority of cases, the direct origin and the main source of innovation (a point already acknowledged by the early LM theorists, such as Furnas and Maclaurin), science does remain an important condition and component of technological progress, and one that is fundamental in science-based industries. In any field of a certain complexity, such as biotechnologies and nuclear physics, progress without the new knowledge produced by basic research would be very difficult. Even in fields often pinpointed as examples of contexts where the introduction of innovation does not require basic research, things are trickier than they seem. Take the example of vaccinology, where a rather pragmatist approach has delivered great successes in the past (e.g. the vaccines against yellow fever and polio) even though the scientific understanding of the disease was lagging behind the ability of eradicating it. One might want to remember that much was learnt conducting experiments on humans (e.g. conscripts, orphans) with very limited consent (or none at all) imposing

[8] We owe this reference to an anonymous referee.

on them risks that are nowadays totally unacceptable on ethical grounds. Sound scientific foundations are necessary to make better predictions, minimize risks and, thus, ask for informed consent about the possible consequences.

Moreover, a 'LM in a weak form' (more respectful of what stated in the original versions of, for example, Maclaurin and Furnas reviewed above) would not imply that science is the *only* engine of innovation nor that scientific research *immediately* generates technological change. Rather, it would state simply – and more humbly – that scientific advance *in many cases* is a *major* source of innovation, recognizing *long temporal and cognitive lags*, and fully acknowledging that scientific advances are neither necessary (think of the bicycle) nor sufficient (think of the many problems that require resolution before a marketable product is achieved) for innovation to take place. Also, a 'weak LM' would underline that the impact of science on innovation does not merely reside in the creation of new opportunities to be (quickly) exploited by firms, but rather (and perhaps mainly) in increasing research productivity, and therefore the returns to R&D, through the solution and explanation of technical problems, elimination of research directions that have proven wrong from a scientific perspective and provision of new research technologies (Nelson, 1959; Mowery and Rosenberg, 1998). Put differently, the success of firm-level exploitation strategies might well depend on population-level investments in exploration strategies.

It should also be noted that science and basic research are notions that are often used interchangeably. But basic research does not necessarily coincide strictly with science, in the sense that it does not always provide a sufficiently reliable explanation of certain phenomena: vaccine development (and to a considerable extent most biomedical research) is an example. The process of discovery and development of new vaccines (or drugs) is still largely dependent on trial and error and experimentation without clear a priori understanding of how and why the vaccine (or drug) should work. Or, conversely, translation of the basic scientific understanding of the causes of a pathology into a new treatment is far from automatic: simply, the human body is far too complex to allow cures to be derived from first principles. However, basic research – even if not entirely 'scientific' in this sense – remains a fundamental component, and often a pre-condition, for subsequent product development.[9] Many, for example, have argued that the War on Cancer launched in the US in the early 1970s failed to deliver results, despite the enormous resources devoted to it, because there was no strong scientific understanding of cancer, its causes, and its evolution. The Manhattan Project instead had to solve tremendously complex engineering problems (related, for example, to the enrichment of uranium) but the basic science underpinning the functioning of the atom bomb was established in the 1930s. In this respect, science – and basic research more generally – could and should be considered as an essential reservoir of knowledge which underpins – often with long lags and perhaps even unconsciously – much of technological activities: all of us use Pythagora's theorem daily, without even being aware of it.

Last, the discussion on the role of basic research in the innovative process partly overlaps – but it does not coincide with – the old debate about 'technology pushed vs demand pulled' technological progress. While the strong form LM could certainly be held responsible for supporting a pure technology-push view, there is little in the model to suggest such a strong connection, at least to the extent that the LM is understood as claiming that science is one major source of innovation. This proposition does not necessarily imply that needs and demand are irrelevant in shaping the questions addressed by science. Here we can only refer the reader again to the seminal contributions by Rosenberg (1976, 1982), Dosi (1982), Rosenberg and Nelson (1994, 1998) and Stokes (1997). Despite their differences, all these contributions stress the point that science, basic and applied research are often strictly intertwined and in many cases inspired by practical applications. But the emphasis on *science* and *basic research* in the innovation process, reminds us that science and technology are not perfectly malleable to economic signals (Dosi, 1982). This is a fundamental lesson, which in our view can be derived from the linear model. Science (and technology) follows logics that are related to, but not entirely determined by, economic or social forces. Thus, for example, despite the need for and the investment in developing a vaccine for AIDS, we are far from success because our knowledge is not sufficient. The same applies to cancer research. It might be a simple message, but the 'rigidity' of the linear model is actually a very useful reminder that relative prices (and more broadly customers' wishes) do not explain everything. The inner dynamics of the science and technology system still plays an autonomous role in explaining what does, and does not, exist. This is a fundamental insight which we are unwilling to dispose of, and derives straightforwardly from the discussion inspired by the LM.

3.2. Bottlenecks

Another stream of critiques – related to propositions (1.3) and (1.4) above – of the LM in strong form arises from the recognition that the transfer of new scientific knowledge into technology and commercial innovation involves obstacles and bottlenecks. It is now widely recognized that knowledge does not flow smoothly among different stages of the innovative process and among different organizations and institutions. Nor does it flow freely among geographical areas. However, it has first to be noticed that such recognition is supported by two quite different arguments. One explanation is that knowledge (including scientific knowledge) is characterised by irreducible elements of tacitness. Thus, the transfer of knowledge requires close interaction, exposure to direct experience, face-to-face contact, etc. A second explanation relies on a radically different interpretation of knowledge, which is conceived as pure information and as such it has the characteristics of a public good. In this case, the introduction of incentives for the private exploitation of this knowledge – typically intellectually property rights – is deemed necessary in order to transform knowledge into innovation.

The argument (in both articulations), *per se*, does not necessarily damage the LM. The model can easily accommodate the existence of impediments to the flow or exploitation of knowledge and, as we discuss later, policies for the development of appropriate institutions that contribute to softening or removing these bottlenecks can be (and are) advocated and justified on the basis of the LM. In fact, one could argue that it is the linear representation of the innovation process that has enabled researchers to identify bottlenecks and allowed them to become the object of policy debate.

3.3. Feedbacks, interconnections and non-linearity

However, and more interestingly, the argument has been further developed into the notion that technological progress is often interactive in nature. Kline (1985) and Kline and Rosenberg (1986) synthesised this in the 'chain-linked' model. On the basis of their assertion of the fundamental role of design in triggering innova-

[9] The current debate on the strategies for developing a HIV-AIDS vaccine is revealing in this respect. While many researchers are convinced that it is impossible to develop a vaccine without having a complete scientific understanding of how and why it should work, an increasing minority is advocating a more 'experimentalist' approach, claiming that HIV-AIDS is just too difficult and complex to be fully scientifically understood and that alternatives should be tried despite their scientific base being not fully clear.

tion, they criticise the sequentiality of the process of technological change, stressing that the activities involved occur simultaneously and/or with continuous feedbacks among them. Thus, the innovative process cannot be represented as a sequence of steps; it should be seen as proceeding in parallel as a constellation of concomitant tasks required to deliver a marketable product, starting from an initial design.

This critique is certainly very compelling, since it challenges the very notion of linearity. Apparently it has little to do, as such, with the discussion on the role of basic research in the innovative process and more generally on the sources of innovation, since it applies broadly to all the activities involved in the process of innovation. Moreover, this critique is certainly destructive of a highly restricted and stylised micro-version of the LM: that is, the conventional view that the innovation process carried out within firms *always* starts with research, and is followed by development, then by production, then by marketing in an orderly sequence as a smooth, well behaved linear process. Yet, it might be argued that this is not what the 'fathers' of the LM would subscribe: in the above cited words of Furnas, 'research organizations have not had in the past and will not have in the future a monopoly on sources of new ideas and improvements'. More importantly, it must be emphasized that Bush's or Maclaurin's versions of the LM do not refer to daily processes of (more or less) incremental learning and change at the firm level, since they focus on the role of science in enhancing the long run development of the economy or science-based industries. And it is at this level of analysis, when one considers the role of long-term oriented research that the innovative process cannot be represented as a generalized co-occurrence of concomitant tasks, either within a single organization or across various entities. If the outcomes of research take a decade or so of further work to be translated into marketable products, feedbacks from downstream activities will impact on current or future research projects, but cannot influence the research carried out a decade earlier. Here what matters is not whether research is basic of applied, but whether explorations distant from *current* market pressures are needed to smash 'brick walls' and to deliver future products. This situation is extremely different from the case where firms' innovative activities are devoted to introduce minor improvements in response to well defined market demands, to which the critiques to the LM strictly apply. Thus de-emphasizing the role of research (which in most cases is absolutely correct) is the crucial premise of the view of a world of fully *interactive and simultaneous* activities directed to realize a given innovative output.

At a more theoretical level, it is useful to recall that linearity and (lack of) interaction are different concepts. Actually, the use of the term 'linear' is indeed misleading here, since there may be very different interpretations of it. In one version, linear is taken to mean sequentiality (as opposed to parallelism or simultaneity), in the time dimension. A second interpretation sees linearity as a synonymous of lack of feedbacks (either or both occurring simultaneously or over time). In a still different meaning, linearity implies that feedbacks are not self-reinforcing (the exponent of the equation linking activity x to activity y is equal to one). By using these interpretations interchangeably, many different structures can be generated. At one extreme, we might identify the strong form LM (a sequence of activities, occurring one after the other with no feedbacks of any sort). At the other extreme, one finds a fully interconnected system, where all the activities interact with each other, simultaneously and over time, through self-reinforcing mechanisms.

Clearly, there can also be vastly different structures that cannot be classified simply into the two extreme forms. Thus, first, a model may exhibit feedback loops but remain linear: many linear systems exist in theory and in practice and they can generate quite complex behaviour. Or, a purely sequential model might very well be characterised by non-linear relations: even the strong form LM could be compatible with this structure. Second, recognition of the interactive nature of the innovation process does not mean that all its components are completely and fully interconnected (and thus need to unfold in parallel) or that all the connections imply positive feedbacks and self-reinforcing mechanisms. Moreover, the timing of those feedbacks is also crucial. Hence, a system or a network can often be partially decomposed in (linear or non-linear) subsystems, linearly connected to each other. Indeed, fully connected systems are very unstable systems and partitioning pays off in terms of stability, predictability and sheer manageability. This understanding is a major contribution of system theory and network analysis, where the emphasis is precisely on identifying the fundamental properties of specific structures. It should also be noted that project management builds on a linear sequence: first, the main tasks to be performed are selected from alternative courses of action, by identifying those more likely to deliver the required result within given time and budget constraints. Next, one specific problem decomposition is implemented, and there is then a sequence of adaptation. Of course, projects are unlikely to actually follow such a simplistic map. The point is that the linearization of tasks provides project managers a map which enables them to trace progresses and problems. The map is not the territory. Nor is it meant to be. But one needs a map to understand when and where things stopped going according to plan.

Thus, linearization or partial block-linearization of a system might be in many cases a useful or even necessary analytical tool, even (perhaps even more so) when a systemic or network view of the innovative process is acknowledged. This approach allows the analyst to identify and focus (at various levels of aggregation) on the subsystems and relations which really matter and confer non-linear properties to the system as a whole. The alternative – in a form as extreme as the popular LM – would be an equally misleading and unmanageable 'strong form system model' where everything depends on everything else in a non-linear fashion at the same time. Hence, as soon as the systemic nature of the innovative process is (rightly, we wish to emphasize) recognized, it becomes at the same time necessary to identify, describe and analyse the specific structure of the system (or network) which is the object of study. This is the burden which falls on any systemic view.

3.4. The actors

With regard to appraisals of the organizations and institutions that support technological change (propositions (2.1) and (2.2) above), many have stressed that the LM underrates their complexity and variegated nature. Universities and (large) corporations are not the only relevant actors. Rather, there is a striking variety of organizations, both public and private, that participates in and contributes to the generation of technological innovation. The nature of these agents and the way in which they interact vary substantially over time and across countries.

These views have spawned an enormous and extremely valuable literature, which can be broadly identified with all the systems of innovation (national, regional, sectoral) approaches. Other streams of analysis which have gained audiences in academic and especially policy circles are the Triple Helix (Etzkowitz and Leydesdorff, 1997, 1999) and the New Production of Knowledge (aka Mode II) approaches (Gibbons et al., 1994). Despite their profound differences, these perspectives have in common the idea that firms do not innovate in isolation, so that innovation must be seen as a collective process involving other firms as well as a number of other non-corporate entities such as universities, research centres, government agencies, etc. A firm's capacity to innovate is further shaped by a large variety of institutions (including the financial system, laws and practices governing labour markets, etc.).

The behaviour and the specific nature of these agents and, even more importantly, of the relationships among them, have a critical influence on the way the system works and performs. Thus, in order to understand the process of innovation, it is no longer sufficient to analyse the behaviour of individual components of the system in a reductionist way, but it is necessary to unveil their relationships and the feedbacks among them. Especially when systems are conceived as evolving over time, perhaps in a non-linear fashion, complex dynamic analysis becomes useful and often necessary.

This is fine. There is little doubt that recognition of the systemic and interactive nature of the innovative process is major step forward in our understanding. However it should be noted that the conceptual issues raised by these new approaches do not necessarily destroy the LM. Actually, most of the arguments provided in the previous sections about the nature of the innovative process find a direct counterpart in terms of agents.

To begin with, in a first approximation, it would not be too difficult to extend the LM to many more organizations and institutions than simply universities and firms, provided that these new agents are located along the sequence of activities assumed by the LM. For example, it is extremely important to consider the role of professional groups and social networks, in remedying the bottlenecks of all kind that can occur through the pipeline. Beneath the main building blocks of any innovation system (say, at the sectoral level) we can find complex webs of social relations among individual scientists (witness the rapidly flourishing field of bibliometric analysis) and broader professional groups (witness the sociological literature on the role of Invisible Colleges, epistemic communities and the like, in scientific development). Networks and professional groups enable feedback loops to be managed and bottlenecks to be solved. It does not follow, though, that we cannot, or should not, model higher level constructs (e.g. university departments vs private R&D laboratories) as linear systems. As argued before, even within a system, significant relationships among agents may remain linear.

Nor is an emphasis on the interactions between these agents completely at odds with the LM. While it is certainly true that innovation results from the interaction of multiple agents, the division of labour among these agents persists, and collaboration occurs at distinct stages in the discovery/development process. Considering biotech and pharma, the network of relationships involved is characterised by a very specific structure, with a stable core of large companies (Big Pharma and large, vertically integrated early entrants in biotechnology, such as Genentech and Amgen) interacting with smaller and, especially, younger specialised firms. The network is both highly hierarchical and has a distinct orientation. That is, younger, smaller companies tend to be the originators of projects which are developed by older firms (Orsenigo et al., 2001).

Finally, in our view this stream of critiques of the LM may have gone too far in focusing too much attention on relationships rather than on the properties and characteristics of the individual components (nodes) of the system (network). This excessive focus on the relationships compared to the nodes is particularly evident with regard to the normative implications, as we shall discuss below.

3.5. *Does the LM actually work?*

The foregoing remarks prompt the question whether the LM might still be usefully applied at least within certain subsystems and in some technologies. Indeed, it is possible to conceive of a large variety of forms of the innovative process. One important step made by innovation studies over the past 25 years was that there is no such thing as 'science and technology', but that there are different sciences and different technologies each characterised by their own structures and dynamics, and that exceedingly simplistic generalizations may be misleading. The notion of technological regimes and related concepts goes in this direction. However, we do not want to end up saying that no generalization is possible. Rather, the construction of taxonomies (such as Pavitt's one, 1984, but one should not forget the early attempt by Maclaurin) and identification of different possible fundamental structures are important for a better understanding of the innovative process.

As Kline and Rosenberg (1986, p. 302) put it: 'Any model that describes innovation as a single process, or attributes its sources to a single cause, or gives a truly simple picture will therefore distort the reality and thereby impair our thinking and decision making'.

Yet, in some science-based and knowledge intensive sectors – like, e.g. the fields of life sciences and the sciences of the artificial – the idea that, in contrast to more traditional fields, new *science is the initiating point*, in a process of learning leading, after years, to marketable new products and applications, might remain an acceptable and useful approximation at least for selected levels of analysis.[10]

For example, looking at the recent evolution of the biopharmaceutical industry, all the critiques to the LM examined so far do not contradict the essential point that, despite some extremely long lags, scientific progress and more generally basic research have been major sources of technological advance in pharmaceuticals and other life-sciences based industries. Thus, basic research on statins in the 1950s and 1960s only started to yield practical results some 20 years later (Galambos, 2006; Grabowski and Vernon, 2000). However, these practical results did materialise. Recombinant DNA, monoclonal antibodies and the polymerase chain reaction, among a few of the more obvious examples, have allowed new drugs discovery and development and have become standard tools in biomedical research. Thus, when in place, science does indeed produce new innovative opportunities and better discovery techniques.

Moreover, basic research, however empirical, necessarily precedes drug development: it is possible to progress to the next step only when the previous problem has been solved. The classic model: basic research → target → hit → lead → proof of concept → in vitro experimentation → in vivo experimentation, and so on, reflects this simple fact. Feedbacks from clinical to preclinical research do occur and are an obvious and fundamental source of inspiration for future efforts. However, the cognitive and organizational sequence is largely preserved — necessarily, basic, pre-clinical research must come before clinical trials and subsequent phases of drug development (if anything, for regulatory purposes). Moreover, given the long time between basic research and clinical trials and, even more so, post-marketing surveillance, feedbacks are hardly concomitant. Just as the impact of fundamental research on the development of new products can take several years and even several decades, the reverse is also true: evidence from products impacts on basic research with long lags.

Another example comes from the semiconductor industry. Here we do not want to stress the well known fact that its existence depends on the basic research which brought to the invention of the transistor. Instead we find it more interesting to look at how the trajectory of miniaturization opened by the transistor paradigm could be translated into an incessant delivery of new products to the marketplace. At any new 'node' along the trajectory (namely at any advance in miniaturization), the initiating point in the process of coming up with new products is applied research, focusing on the design of new circuits. We find strong interactions between universities, research institutes and industry, continuous feedbacks among the actors, but at the same time the sectoral system is characterised by an essential linear sequence of tasks and a clear division of labour. The riskier explorations are performed

[10] A similar conclusion was reached by Cohen et al. (2002), who for LM intend Bush's version.

by the 'institutions of science', and are supported by public funding. Typically, academic researchers are concerned with radically new problems, with the aim of creating new products to be delivered in the long run (a decade or so). They inquire into the basic aspects of the methodologies applied and explore the fundamental causes that determine the functioning of material artefacts. The freedom to ignore short-term market demands is precisely what characterises this research, even though university scientists have to look to industry in order to give a broad direction to their work (Balconi and Laboranti, 2006). The research results delivered by universities are further developed by the more practical, specific and market oriented research typically performed within industry (Rosenberg and Nelson, 1994) under the pressure of pending clients' needs.

More generally, it is important to note that there are intrinsic limitations to the extent to which the innovative process could – and should – become more interactive. Problem partitioning continues to be the fundamental problem solving strategy used by scientists and engineers. References to Herbert Simon's very familiar seminal work would be redundant here. Moreover, one should not underestimate the fact that partitioning (and linearization) is necessary to control, monitor and attribute legal responsibility. Advocating better communication within and between development stages is one thing, but, in practical terms, is there a project manager that would agree to being put in charge of a fully connected project team? Also, firms engage in a huge number of processes that run in parallel. Extremely valuable feedback can 'spill over' from a mature project to a recent kick-started project. So, we can have feedback loops and interaction, and maintain an overall linear representation of an entire process.

In conclusion, at the core of these sectors linear patterns are discernible and can be fruitfully analysed. Clearly, these linear patterns are not sufficient for telling a complete and exhaustive story. They exist alongside with other non-linear components of the innovative process. We think that in reasserting the local descriptive value of the LM we are complementing, and not substituting, the picture of the process of technological change provided by Kline and Rosenberg and by the various versions of the 'systems of innovation' approach.

4. The normative dimension

It is especially on the normative side (i.e. proposition (2.3) above) that the LM has come under attack in recent years. In particular, critiques to the LM have been used as a basis for a new policy doctrine that recently has come to be widely accepted. This doctrine comes in different versions, uses different arguments and quotes as conceptual backgrounds approaches as different as Mode II, the Triple Helix and the System(s) of Innovation, but essentially it is based on three main elements, all of which are considered refutations of the LM.

In a drastic synthesis, one element refers to the issue of public support to basic research. As an extreme example, consider Terence Kealy's book (Kealey, 1996) advocating private, rather than public funding. In a milder version, sometimes labelled 'The Third Mission', a second stream challenges the traditional role of universities in society and in the economy and claims that universities should contribute more directly to industrial innovation and to (local) economic growth, by engaging more in applied research (especially research sponsored by industry) and above all in the commercialisation of discoveries realised within their laboratories. A third view suggests that scientific research has become increasingly multidisciplinary and involves different types of institutions, techniques and methods (Gibbons et al., 1994). Thus, universities are no longer the privileged institutions in scientific research, but merely one agent in a dense and ever changing web of relations among other agents. The implication is that closer and more flexible interaction with firms should be promoted, and that institutions appropriate to facilitate these exchanges should be created (see also Etzkowitz and Leydesdorff, 1997, 1999). More generally, this view suggests a shift of emphasis from science policy (i.e. how much should scientific research be funded, which disciplines should be favoured, which funding mechanisms should be used, etc.) to so-called 'innovation policy', where attention is mainly on the establishment and reinforcement of linkages and relationships among the relevant agents in the system. In particular, the role of intermediaries, such as technology transfer agencies, venture capital and/or the promotion of closer relationships between universities and industry, becomes the main focus of analyses and policies.

These three arguments are mixed in different fashions and yield different recipes. We do not enter here in the debate about the merits and demerits of this doctrine. We simply observe that they certainly contradict the normative prescriptions that traditionally are considered implications of the LM, but have very little to do with the LM as a *representation* of the innovative process. As argued above, the LM is totally consistent with the idea that bottlenecks to the transmission process may exist and ought to be removed instituting, for example, technology transfer offices or defining better intellectual property strategies. More generally, we suggest that the LM as such does not bear any immediate and obvious policy implication; nor do other alternative approaches, absent additional assumptions on the nature of knowledge, information and the innovative process, or lacking detailed knowledge on the weaknesses that in a system need being remedied.

To begin with, Bush's argument in support of public funding of basic research appears to be based on a quite vague notion that science provides a social return in excess of private returns, which in some way anticipates the so-called Arrow–Nelson thesis that research has the properties of a public good. But, more interestingly, Bush states that science is a proper concern of government for reasons that include but go beyond pure economic motivations. From this perspective, Bush's argument is if anything similar to Nelson's discussion of the limits of the 'market failure' framework for justifying public intervention in fields like education, healthcare, defence, etc. (Nelson, 2004).

Moreover, the converse is also true: not necessarily an alternative approach like, e.g. the 'system view', provides an unambiguous policy implication in this respect (see, for a discussion, Dosi et al., 2006).

Also the second stream of criticism (the Third Mission) has little to do with the LM. It could be actually argued that prescriptions for stronger involvement of universities in meeting economic and social needs, in the commercialisation of their research, etc., could be directly derived from a LM in weak form which recognizes bottlenecks and impediments in the flow of knowledge along the linear sequence: although basic research still comes first, it does not trickle down as predicted in the strong form LM because cognitive and/or institutional bottlenecks arise along the linear pipeline. This is precisely the case, in our view, of the arguments usually raised to support the Bayh-Dole Act in the USA and its transpositions in other countries: the notion that the establishment of IPRs on the outcomes of basic research provides the necessary incentives to its subsequent development and commercialisation rests on the idea that basic research directly leads to innovation, given appropriate incentives. Once again, prescriptions or counter-arguments related to the need to strengthen university–industry relations are based on considerations and assumptions concerning not the LM as such but the nature of knowledge (information as a public good vs tacit knowledge), or the incentives that drive firms to invest to transform universities' discoveries into new marketable products.[11]

The third stream of critiques to the LM – the Mode II approach – could indeed be taken as an extreme confutation of the LM, insofar as it addresses precisely the notion of linearity and constitutes a version of the critiques based on the interactive nature of the innovative process.

At this stage, we cannot but reframe here our previous arguments concerning the interactive nature of innovation in terms of policy prescriptions. First, as discussed by David et al. (1999) universities and public research centres remain fundamental agents in scientific research. And it is hard to see how a system of research can work properly when there is no recognized disciplinary base and research units are transient and continuously changing collections of different individuals. While problem-oriented, multidisciplinary research is certainly crucial, no system of research can function without underlying disciplinary-based organizations — as David et al. (1999) rather provocatively put it, who will pay the overheads? Moreover, while there is certainly evidence that more and more 'basic research' is being performed by organizations other than universities and that firms are increasingly eager to tap into (and link with) the best sources of new scientific knowledge,[12] these observations do not necessarily violate the (supposed) main normative prescription of the linear model, i.e. that government should fund basic research (which it still does, notwithstanding the new policy doctrines). If anything, they might be interpreted as a revisiting and updating of the LM with the addition that universities are no longer the unique locus of scientific research and that there is a variety of ways of triggering the research process.

Second, while emphasis on interactions represents a major step forward in the design of innovation policies, this emphasis might be excessive if it leads to neglect the importance of the characteristics of the actors. For example, it might be misleading to attribute the unsatisfactory performance, say, of Italy in biotechnology, mainly to weak relationships between universities and industry or the absence of incentives and organizational structures to promote academic spin-offs. While these explanations may constitute part of the story, they tend to detract from the simple fact that basic scientific research within universities is under-funded, regulated by feudal principles and produces too little good science. Thus, the weakness resides in a node rather than in its relationships with other agents (see Dosi et al., 2006 for a similar point).

Third, it might become exceedingly difficult to design, implement and evaluate policies in a fully interconnected system. On some occasions, a simplified representation of the process of innovation which decomposes a complex system of interactions into (linearly) interconnected subsystems might not only be necessary but also desirable. For example, in the policy domain, this view is expressed forcefully by Caracostas (2007), who stresses that the sheer simplicity of the LM makes it attractive for decision makers (managers and policy-makers) negotiating or advocating changes to the allocation of public funds for R&D activities. Of course, the strong form LM is certainly an excessively simplified decomposition. But the LM remains an essential constituent also in the policy-making arena.

[11] In addition, given the concerns that these new policy prescriptions have raised (see among others Mazzoleni and Nelson, 1998, and the *Research Policy* Special Issue on intellectual property rights matters affecting scientific research introduced by David and Hall, 2006), we would also claim that, were the LM actually at the roots of claims for preserving the 'Open Science ethos', then the LM (and particularly the sub-argument contained in Bush's vision on the need to keep basic research free from political and commercial conditioning) would be reinforced, not weakened.

[12] On this topic see the wide recent literature on 'asset augmenting' international investments.

5. Conclusion

In this essay we have first argued that the standard rendition of the LM (the strong form LM) does not do justice to a much richer and nuanced set of ideas developed over a long period of time and articulated at the macro-level (V. Bush), at the meso-level (Maclaurin) and at the micro-level (Furnas). While the idea that basic research is the key originator of innovation along a linear sequence of steps is common to all versions, the LM is actually constituted by the coalescence of different concepts and assumptions.

Second, we have tried to identify the main critiques which are routinely made to the LM. They apply to different components of the model:

(i) the role of basic research as contrasted to other sources of innovation;
(ii) the sequentiality of the process;
(iii) the absence of frictions and bottlenecks in the flows of knowledge;
(iv) the lack of interactions among the activities involved in the innovative process;
(v) the assumption that interactions between the sequential phases of the innovative process do not imply self-reinforcing mechanisms.

On these grounds, we have argued that most of these critiques are targeted towards an excessively simplified representation of the propositions that constitute the LM, namely the strong form LM, and use the LM essentially as a straw man.

Second, many critiques of the LM (in any of its forms) are unwarranted. The LM is blamed for many aspects, which frequently have very little connection with the model and could be very well accommodated in a slightly revised 'weak' version of the LM.

Third, the most destructive critique of the LM is addressed to the notion of linearity itself, namely the core of the model. The view that a broad theory of the research and innovation process should attribute crucial weight to the recognition that technological advance is often generated by interactions among differentiated agents and fragments of knowledge represents an important step forward in the conceptualisation of the process. However, the LM might on some occasions be interpreted as a linear subsystem within a broader decomposable system. And when such a simplification turns out to be illegitimate, then a precise description of the key non-linear interactions and feedback should be provided by the analyst. Otherwise, an outright rejection of the LM based on the notion of the interactive nature of the innovative process can lead to interpretations that everything depends on everything else, and everything occurs simultaneously. To avoid such an outcome, critics should take care to define, with a sufficient degree of precision, both the nature of the innovation process and the features of the relevant interactions in any specific context. This is difficult, challenging and demanding, but it is a necessary requirement for any theory built on the notions of interactions, systems and non-linearity.

Fourth, we argued that the irreversibility of time must be taken seriously: the fact that the processes of discovery and innovation in research intensive industries develop over a long (or a very long) time span sets a temporal order to interactions and feedbacks.

Fifth, we claimed that the LM as such does not imply any definite policy implication and therefore it should not be blamed on normative grounds. But the policy indications suggested by Bush need to be based on many other assumptions regarding the nature of knowledge which are not necessarily part of the LM. Indeed the LM can be used for supporting very different policies as much as other competing models are taken as a basis for radically different conclusions. We may add that if the central normative claim of the LM is that free basic research is fundamental for economic and societal reasons, then we fully agree and consider this conclusion an important element in favour of the LM.

In sum, we conclude that the LM is not guilty of many of the standard accusations. Thus, we should refrain from blaming it for every possible reason. Moreover, the LM is certainly not a general theory of innovation but it may well survive and be still useful, at least in some domains of analysis (such as science-based industries) and perhaps policy areas. It can also be taken as a part and a complement of broader, more general theories which recognize more clearly the dynamic, interactive nature of the innovative process. As Christopher Freeman suggested, 'elements of the vanquished and much derided linear model may yet come to the rescue of its successors' (Freeman, 1996, p. 38).

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