# Economics of Science

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**Abstract**

In this paper we argue that the incentive scheme faced by scientists induces them to promote their activity, publishing on-line their work-in-progress, participating or organising meetings and so on. Such actions produce a huge amount of externalities that may make easier for others to deal with the same topics. Recognizing this influence, we present four game theoretic settings which aim to replicate same features of the research world, such as the "fashions" or the separation between "common researchers" and "stars".

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# 1 Introduction

The primary goal of scientific activity is the production of knowledge. Most governments and firms pay a significant percentage of their budgets for the scientific research (around 3%-5% of GDP in USA, UE and Japan, with the exception of Italy that devolves just 1%). The aim of governments in financing research is to improve the economic growth.

The study of "knowledge", intended as an economic good, starts with Arrow, 1962. He shows that (codified) knowledge is a public good, i.e. non-excludible and non-rival. Indeed, it can be transmitted and it can be reproduced without costs.

After Arrow's contribution, we observe a first and short development period for the "economics of science". In this first period, the studies were concerned only with the appropriability of the economic value of scientific research or with the valuation of the innovation positive externalities on social welfare.

On the contrary, the "new economics of science" (where the leaders are David, Dasgupta and Stephan) is more concerned with the specific features of the scientific institution (i.e. the family of researchers and research centers) such as the reputation and the remuneration system or such as the way of creation and diffusion of knowledge in the scientific community.

In this paper, we present some empirical features of the incentive system faced by the researchers[1] in order to show that it also yields to externality-producing activities. Such externalities are able to explain several characteristics of the scientific world, mainly related with the topic choice of the researchers. We will formalize the relationship between externalities and topic choice by the mean of several game theoretic models.

We shall see that an institution, spontaneously arisen in the scientific community, the so called *priority rule*, makes research a winner-take-all race. Only the first that publish a new discovery, is rewarded. The colleagues recognize the paternity and cite the author whenever they employ his results. Citation (computed through appropriate indices) is the main source of reputation: a large number of citation means that the author's work has a great impact in the scientific debate.

Reputation is the direct reward of the scientific activity. We shall see that

[1] We consider what determines several actions and choices: what topic one studies? Where he publishes? How he makes public his findings?

it greatly determines the scientist's career: the so called *Matthew effect* is at work. Reputed researchers are enrolled at the top research centers, endowed with the best resources. Indeed, they are in the best position to make new important discoveries, increasing further their reputation and their influence. Reputation yields also economic benefits as well as social recognition (indirect reward).

Notice that it is necessary to make the new discoveries known to an audience as large as possible to maximize their impact in the scientific community and the author's reputation.

We point out that, to get this result, researchers have to involve (and actually do) in a number of "marketing" activities: they put their papers on-line, they organize or participate to workshops, they cooperate with others and so on. The "openness degree" of any scientist has to be as high as possible.

Our claim is that these marketing activities, in their complex, produce a large amount of externalities able to influence and sometimes to determine the individual's choices of research. It may be simpler to work on a field dealt by many researchers, where a huge amount of information, ideas, advises, insights is daily produced. On the contrary, choosing a new or a marginal topic requires a more difficult activity of information collection, of literature screening, of formalization effort and so on. Therefore, such externalities may induce a concentration (clusterisation) of many researchers over few topics[2]. Furthermore, notice that some scientists may choose a particular topic even if it is not their preferred, whenever externalities are strong enough to make it the choice maximizing their reputation.

We argue that these externalities, produced in the attempt of being visible, may be an explanation of the science evolution process through a sequence of paradigms (Kuhn, 1962) as well as of the, casually observed, fashions that characterize the scientific production at each date. Indeed, in both these phenomena, we may observe a fairly compact mass movement towards a given direction, or, in other words, a clusterisation of researchers over specific topics, methods and models.

[2] Competition playes a major role, given the priority rule. Working on a crowded field or topic requires a deep specialization to have the chance of a new discovery. On the contrary, working on a new issue may be profitable, although risky. However, the competition effect does not cancel the externality pattern described, i.e. given the externalities, more popular topics present a comparative advantage. Competition effect only parallels the externality effect, it does not eradicates it.

This paper is organized as follows.

Section 2 briefly mention empirical results on the scientists' productivity (the so called *Lotka law*). Sections 3 and 4 analyze respectively the *priority rule* and the *Matthew effect*. Sections 5 and 6 are devoted to the citation indices and to the consequences of their use. Section 7 contains a set of game theoretical settings, formalizing different aspects: clusterisation, separation in groups, positive externalities and competition. Section 8 concludes. Three appendices on the system of the economics journals, on the publication indices and on the co-word and co-citation analysis follow.

# 2 The Lotka law

Empirical studies show that scientific production is highly due to a small minority of researchers. Just a six percent of scientists writes a half of the published papers (Lotka, 1926). This phenomenon is generalized in all fields and at different times (Price, 1986). It seems a consequence of an uneven skills distribution, i.e. there are few people very endowed that carry on the scientific progress and a large majority of researchers fairly good but not very productive.

However the incentive and reward schemes of the science world may strengthen the skewness of the publication distribution on the authors. Moreover, "scientific productivity is not only characterized by extreme inequality at a point in time; it is also characterized by increasing inequality over the careers of a cohort of scientists, suggesting that at least some of the processes at work are state dependent (Stephan, 1996)".

# 3 Priority Rule

Sociologists were the first to study the rewarding systems within the scientific community. Merton (Merton, 1957; Merton, 1968; Merton, 1969; Merton, 1973) shows that the goal of scientists is to establish the priority of a discovery by being the first to communicate an advancement in knowledge.

The scientist's reward is the recognition, awarded by the scientific community, for being the first to find a new idea, a new method or a new result. The paternity of a discovery represents the prize of a race where the winner takes all.

Generally there is not a second prize for a scientist who is working on the same field, who finds the same result, uses the same techniques... but who, unfortunately, publishes his work (much or little) after the first finder. *Priority rule* is a device to manage the moral hazard issues connected with the research activity. Given the fact that knowledge is a public good, it is not possible to establish, with certainty, the origin of a work, whose content has been already published. The easiest way to avoid any problem of asymmetric information is, then, to reward just the first finder, rather than to make in place a prohibitively costly monitoring system. Moreover, from a social value point of view, "there is no value added when the same discovery is made a second, third or fourth time (Dasgupta and Maskin, 1987)".

The same system works, in the very similar domain of the patent race: a patent is granted only to the first inventor. The return of a patent is essentially economic, because it gives the exclusive right to use the protected innovation with the possibility of monopolistic rents.

In fundamental or pure research (that we consider here) the main return of a discovery is the publication on a reputed journal. A scientist, after the article draft (and sometimes even before the complete drawing up), sends it to one or more journals, to establish the priority. Here the referee process is a first and determinant judgement of scientific community about the quality and the correctness of the contribution. Once published, the whole scientific community gives the second and, often continuous, judgement, through citations, strengthening or attacking the content.

Notice that the incentive to publish, determined by the *priority rule,* has two additional social benefits. "First [...] it rises the social value of knowledge by lowering the chance that it will reside with persons and groups who lack the resource and the ability to exploit it. Second, disclosure enables peer groups to screen and evaluate new findings. The result is a new finding containing a smaller margin of error (Dasgupta and David, 1987)".

*Priority rule* is efficient because it gives maximum incentive to the researcher, since only spending all his potential effort on research, he maximizes the probability to be the first and so to obtain the reward. Moreover, *priority rule* is efficient because it allows to internalize all externalities connected to his activity. Citing other authors, he increases the credibility of his own statements and, on the other hand, the citation process increases the reputation of the cited first finder (Stephan, 1996; Levin and Stephan, 1991). Indeed, this system is self reinforcing and stable because all participants have incentive to cite.

Two more remarks are also worth: first, applied research works differently. The main goal is to transform the finding in monopolistic rents (Dasgupta and David, 1987) and so it can be profitable not to publish the discovery before having obtained a patent. In applied research there may also be some practical difficulties to implement a finding simply seeing its application in the competitors' product. This gives to the first innovator a lag of time where he can act as monopolist *de facto* (Denicolo' and Franzoni, 1999).

The second remark is that, having no rewards, the second and the subsequent researchers receive nothing for their efforts: the used resources are wasted (problem of duplication of research effort: Dasgupta and David, 1987) and the risk in the research profession is very high (problem of scientist wage: Stephan, 1996).

The system of priority establishment, publication and citations assigns to each researcher a certain level of reputation. The aim of each researcher is to maximize his reputation, although, sometimes, the sole solution of the "puzzle" is a sufficient motivation. Reputation is the source of social recognition and also economic advantages. Moreover, it is also a fundamental determinant for carrying over new important researches.

# 4 The "Matthew effect"

Many authors studied the life-cycle productivity of researchers (Levin and Stephan, 1991; Combes and Linnemer, 1999; Arora, David, and Gambardella, 1997; among others) and have shown that only few people are very productive during their life. On the other hand, there is a very large majority of researchers that remain unproductive.

This is due not only to an unequally skills distribution, but also to the prevailing reward system. Reputed researchers obtain the possibility to be called in famous research centers. These centers are famous because in their past and in their present have collected the best researchers. They dispose of many resources given by public and private sources, because they have a high probability of success in new researches. In this environment, the enrolled scientists have the means to be more and more productive because they can dispose of the best technology, the best assistants and the best administrative organization. The contrary is generally true for those who have not been able to emerge. This is the so called "Matthew effect" as Christianized by R.K.

Merton[3].

Such a system is something of path dependent, starting-point dependent and self reinforcing. In fact the higher the reputation of the first works, the higher the possibility of having success in the subsequent ones.

However, empirical studies (Levin and Stephan, 1991) show that there is (on average) a decline in the productivity in the life-cycle, that is, scientists produce less as they age. A rationale for this phenomenon is that, once reached a certain reputation level and a certain social position, incentives to research are no more present.

A contrasting evidence comes from the Arora, David, and Gambardella, 1997, paper where the authors try to estimate a sort of science production function. The result is that for the high reputed scientist the elasticity between the output and employed resources approaches to the unity, while, on average, this elasticity is around 0,6. Even if this is a static analysis, it is possible to see the Matthew effect:

> "past performance, by affecting the scientific competence and professional reputation of the researchers [...], will be related to future performance. In addition to a direct competence-based effect, past performance may have two indirect effects on research output. First, units with better past records are more likely to be successful in getting research grants. Second, knowing this, they will invest in applying for larger grants. (Arora, David, and Gambardella, 1997)".

A practice adopted to limit the Matthew effect or to use the reputation of a star to advantage a whole research group, is that of calling a star in a research center and to allow as many collaborations as possible between the previous center members and the new comer. In such a way not only the proximity helps the group to acquire new methods or new ideas, but the common practice of co-authorship allows to improve the reputation indices of anyone and therefore of the research center at the whole. For this reason, there can be a competition between research centers to attract a star. The weapons of this war are not only the amount paid to the star, but, mostly,

[3] This name comes from the New Testament according to S. Mattew where there is this Christ sentence: "for unto everyone that hath shall be given, and he shall have abundance; but [...] from him hath not shall be taken away even that which hath" (Mattew 13:12 and 25:29)

the kind of organization present in the center, the administrative facilities, the endowment quality and so on (Rychen and Soubeyran, 1999).

# 5 Citation indices

In this section we consider more in deeply the role of citation system in assessing the researcher's reputation and how it is possible to build a reputation measure, an index based on citation. In particular, we focus on the effects of its use on the individual incentives[4].

When an article is published in a journal, it is supposed having passed the exam of the referees and so scientific community can suppose it adds something to the knowledge stock, it is coherent, logic, correct at least at a normal check. By now, this article is a part of knowledge and can be used for further developments. The majority of the criticisms is mostly on the employed hypothesis. This kind of criticisms is the soul of the scientific debate.

In any case, either to criticize or to apply a work of others, it is necessary to cite the author. This institution has been developed inside the scientific community to correctly reward the authors participation to the knowledge progress.

An index to measure the researcher's reputation and participation to the scientific debate can be based on the number of citations an author obtains. The sum of all these citations represents the simplest citation index. The rationale staying on the basis of such an index comes from two simple hypotheses:

a) the work, if cited, is sufficiently visible and induces a researcher to refer to it;

b) the work, if cited, has an influence, more generally an impact over the knowledge production (Callon, Courtial, and Penan, 1993).

Therefore, a citation index is a measure of the impact that an author and his activity have on the scientific community, and not directly as a measure of the quality of the author's work[5].

[4]A discussion of indices based on pubblications is presented in the Appendix.

[5]The quality of a publication can be taken into account by using the so called co-word and co-citation analysis, breafly discussed in appendix.

This method of evaluation seems simple and powerful because it takes into some account the relevance of a research activity and not only quantitative aspects (such as the number of pages published). However, several difficulties arise when one tries to build this index as well reported in MacRoberts and MacRoberts, 1996[6].

It can be worth to remark that the construction of these indices and the way to refer to the works or to the theories employing the name of the first author determine the way to order the authors names in a front of a paper. Engers, Gans, Grant, and King, 1999, show that the alphabetical ordering is an equilibrium of a game where the authors try to maximize their payoff in terms of reputation giving signals about their contribution (eventually in a strategic way)[7].

[6]Here are some of the critical issues arising in the citation index use.

i) Authors do not cite all their references and in many fields the references coverage is only 30% of the actual utilization of external sources.

ii) There is a bias on citation, because "*some influences were almost always credited correctly while others were either not credited or credited to someone else*" (*ibidem*). This is the case of the methodological article or the survey ones.

iii) Often a relevant share (for instance, 38% in the sample analyzed by MacRoberts) of citation is second source, that is the authors credit findings or ideas to other users rather than to the real discoverers.

iv) Informal influences are not cited: informal interactions are always present in the research centers or in the universities, where contacts between scientists are continuous.

v) Another problem is the role of citation in the strategy of authors: "*No longer can we naively assume that authors cite only noteworthy pieces in a positive manner. Authors are revealed to be advocates of their own points of view who utilize previous literature in a calculated attempt to self-justify (*Brooks, 1985*)*". In other words citing is a complex social-psychological behavior.

vi) Self-citation is often excessive (strategic behavior).

vii) There is a first author privilege because often citations data-bases contain only the name of the first author (this is the case of the International Citation Index).

viii) Citation rates vary with disciplines, nationality, time period and size and type of speciality;

ix) It is necessary to normalize the reputation index to take in account the audience size of a given discover.

[7]This paper shows also that this noncooperative equilibrium leads to a lower quality than the social optimal level and than would be achieved if coauthors were forced to use name ordering to signal relative contribution.

# 6 Importance of citation indices

Many institutions, as university departments, base the rewards or the career advancement on these indices. Tuckman and Leahey, 1975, established the value of an article in monetary terms for its author(s). This value derives from direct salary increments, promotion-related salary increments and career-related option effects. In the Seventies, the value of an article varied from around 12.000 dollars for an assistant professor to 7.000 dollars for a full professors. These figures are useful to demonstrate the high importance of reputation indices on researchers' careers and to highlight how this system of rewards affects mostly the younger researchers. Nevertheless, in general, all the reputation measures tend to be unfavorable for the younger, because they find more difficulties to impose themselves to the attention of the scientific community.

Given the importance of such indices, it is plausible that a researcher wants to maximize them rather than other variables, i.e., work quality, originality, relevance etc. Clearly the two strategies are not incompatible, because the former are a sort of proxy of the latter.

Since citation indices represent the impact of a finding, an agent maximizing them has different available actions. We limit our attention to the non pathological. First, he should choose a topic in the core of the debate, obviously within the limits of his formation and of his preferences. Second, he should send his paper to a review with a large audience to maximize the probability of being read. Third, he should participate to as many seminars as possible to present his work and to make it known and, maybe, activate himself to organize meetings. Fourth, choosing to work with a reputed colleague can help his success, and so on.

Obviously, all these actions presume that the work quality is sufficiently good. However, it is interesting to notice how this kind of indices enlarges the set of actions and activities relevant to improve the own reputation. While publication indices (see appendix) have mainly an effect on the research quality (whenever they give high weight to papers accepted by top journals), citation ones require some marketing actions, because what really matters is the impact dimension. Authors are stimulated to signal their activity to the scientific community producing an increasing information stream about the works-in-progress at a given time. This increased information can lead some authors to work together or to exchange ideas or solutions (as often happens in the workshops). Thus some positive externalities arise or strengthen.

Nevertheless, there is a trade-off between research and marketing activities, because the both are time spending. In other words, they are complementary factors of the reputation production function. There is an optimal mix that should be found. Note that, if we are interested in maximizing the number of new discoveries in the society, we will have to balance the externalities value with the time spent to produce them: each scientist devotes less time to research, but makes easier the others' task. Hence, since externalities increase the productivity of the research, their optimal level is, likely, positive. Nevertheless, since individuals maximize their reputation rather than the number of new findings, the actual quantity of spillovers may be too high, i.e., too much time is devoted to marketing activities.

# 7 Strategic behaviors in research

In this section we present different game theoretic settings that deal with the network externalities arising in the research world. The first model (*Clusterisation*) aims to explain the existence of fashions in the scientific production. The second (*Xenophobia*) and the third (*Willingness of separation*) consider the possibility of strong separation between stars and, say, common researchers. The last model (*The planet of the Gods*) assumes that stars produce positive externalities, while among common researchers the competition effect prevails, reducing the returns of working on a "crowded" topic.

## 7.1 Clusterisation

### 7.1.1 Some more specifications

Everyone can observe that some research topics are more analyzed and dealt than others. Moreover, we can see that the number of articles on certain topics fluctuates in the time. Not only topics and fields are subjected to these dynamics, but also the way to formalize, the used tools, the kind of hypotheses and assumptions vary. We can consider these fluctuations as fashions, exactly as in completely different domains.

Part of this variance can be explained by the paradigm argument of Kuhn, 1962. At a certain point, the scientific community agrees upon a method of work or an interpretation. Some basic ideas are defined and remain on the background. Many researchers and many articles deal with or use them. For a while, the knowledge evolution is continuous and smooth. From a certain point more and more authors begin to be unsatisfied towards the previous and recognized paradigm. An increasing number of critical articles appears and finally a new paradigm imposes itself.

Generally, only some research leaders with a high reputation can start the process. Actually, it can be difficult to get over the tradition and it is simpler for a star to attract the scientific community attention. After the star, some members of his *entourage* publish something of supporting, some reputed but not top researchers support this new view and finally, if the case, the mass of scholars accepts the new paradigm and write about it. This process can be more or less rapid, but usually it takes years[8].

In this section, we mainly analyse the behavior of the researchers' mass, when some paradigms or some topics are already defined by the stars, i.e. we deal with the phase after the seminal papers publication, when a topic or a view may become a true fashion. The movement of the mass makes a fashion to arise.

We claim that a determinant for the mass movement are the externalities that arise in a research world where any individual tries to maximize the impact of his contribution. Then we suppose that a kind of citation index is widely used to assess the researchers' reputation and that career chances, research opportunities and peer recognition depend on it.

We have mentioned that many actions are useful to enlarge the impact of a research on the scientific community. In particular, some "marketing" activities are valuable, such as workshop organization, on-line publication of the papers, or participation to several and different meetings. Such activities increase the natural positive externalities between researchers who work on the same topic, making, respectively, research and publication easier and more probable. Publication is more probable because a topic dealt by a large mass of researchers has also a large audience. Moreover, the possibility of co-authorship is increased, being co-authorship either a way to formalize a

[8] An example is given by the evolution of the firms strategic studies: from the late Sixties, the dominant paradigm was the "*Structure - Behavior - Performance*" of the Harvard School. In this view the environment would have a fundamental importance in determining the firms behavior and performance. In the same line and from the same school came out the "*Competitive Advantage*" of Michael Porter during the Eighties. After, quite suddenly a new paradigm has arisen, that of the "*Resource-based View of the Firm (RBV)*", where the attention has been put over the internal variable and resources of a firm (Tracogna, 1999).

cooperation or a good strategy to increase the individual scientific production.

Now, not only the *priority rule* pushes towards a rapid publication of the new results, but the goal of maximizing the impact makes necessary to inform the others in a more personalized and punctual way. First, drafts are sent to reputed researchers as well as to selected colleagues, in order to make them directly informed, rather than limiting only to the impersonal and quite untargeted media represented by the scientific journal, even if it has a large audience.

When researchers take into account the *Matthew effect* they have to acknowledge even more importance, firstly, to the informational activities and, secondly, to the cooperation. Being the career path-dependent, the present value of a paper embodies the value of the future opportunities that it will disclose. Therefore, the *Matthew effect* itself reinforces the externalities between researchers.

Dealing with a topic studied by many researchers, with a number of papers already published, allows a researcher to benefit of a huge amount of externalities[9], even if he has to specialize on a particular aspect, to limit the competition induced by the *priority rule*.

### 7.1.2 The model

We shall formalize the issue that fashions in research and scientists' clusterisation are a matter of externalities by considering a game with many players. We use and specialize a game studied by Konishi, Le Breton, and Weber, 1997b. Such a game is special in the sense that strategic interaction is summarized by making the players' payoff dependent only on the number

[9]This kind of externalities belongs to the class of network externalities, dealt by Farrell and Saloner, 1985 or, more recently, by Grilo, Shy, and Thisse, 1999. The former shows as in a context of incomplete information, the network externalities may slow down or stop the passage towards a new and more efficient standard. The latter analyzes the network externalities role in terms of price competition in a duopolistic world *à la* Hotelling. Here the network externalities if positive (conformity) but not too strong, lead to a fiercer price competition between differently located firms; if negative (vanity), they lead to a more relaxed price competition. In both cases positive network externalities bear a negative effect to the players: in the first case firms do not adopt the new and more efficient standard, while in the second the firms profit is lower. We anticipate that the same phenomenon arises in our model: with positive network externalities, in general, the alternatives chosen by researchers are not their most preferred.

of agents making the same choice[10,11].

Let us imagine that the leader researchers (the stars) have defined a set $X$, finite, of possible topics. There is a set $N$ composed by $n$ researchers with $n > |X|$, where $|X|$ is the cardinality of $X$.

We consider a no spillover game, that is a game where for every group of players choosing the same strategy, the payoff of every member of this group is independent of choices made by players outside the group.

Let us suppose that the individual payoff functions, for any strategy profile $\mathbf{x} = (x^1, ..., x^n)$, are $u^i(x, N_x(\mathbf{x}))$ where $x$ is the chosen alternative and $N_x(\mathbf{x})$ is the number of players choosing the same alternative.

We assume, as Konishi, Le Breton, and Weber, 1997b, that the following three conditions are satisfied.

- *Positive externalities (PE)*: for any two players $i, j \in N$, for any subset of players $S \subset N$ with $i \in S$ and $j \notin S$ and alternative $x \in X$, we have $u^i(x, S) \leqslant u^i(x, S \cup \{j\})$

- *Anonymity (AN)*: for any player $i \in N$, for any $S, T \subset N$ such that $i \in S \cap T$ and $|S| = |T|$, the equality $u^i(x, S) = u^i(x, T)$ holds for every alternative $x \in X$

- *Order preservation (OP)*: for any $i, j \in N$, for any $S, T \subset N$ such that $i \in S \cap T$ and $j \in S \cup T$, for any two alternatives $x, y \in X$, $u^i(x, S) \geqslant u^i(x, T)$ if and only if $u^i(x, S \cup \{j\}) \geqslant u^i(x, T \cup \{j\})$

*PE* means that the utility or the payoff increases with the dimension of the set composed by the researchers making the same choice. This assumption is key to obtain clusterisation. These positive externalities represent the advantage of working on a popular topic.

The second assumption, anonymity, reflects the fact that it doesn't matter who is the researcher working on the topic: what matters is only the number of researchers. *AN* seems to be correct in a context where stars are not present and where only "common" researchers play.

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[10] Refer to Chapter 1 for a more complete discussion on this game and on the property of anonymity.

[11] This paper *fait pendant* with another one written by the same authors (Konishi, Le Breton, and Weber, 1997a), where the negative externalities setting is analyzed. Here more stringent conditions are required to guarantee the equilibrium existence, because players tend to distribute themselves as far as possible one from the others.

The third assumption, order preservation, implies that joining the same researchers to two sets does not change the individual preferences. This specification leads towards a certain regularity of the payoff function.

Under *AN,* utility can be written as $u^i(x, S) = h^i(x, |S|)$ where $S$ is another way to indicate the set of players choosing the $x$ alternative.

The following two results can be shown:

**Proposition 1** *(Konishi, Le Breton, and Weber, 1997b). Suppose that X is finite and the payoff of each player satisfies PE, AN and OP. Then a no spillover game admits a Nash equilibrium.*

**Lemma 1** *(Konishi, Le Breton, and Weber, 1997b). Let the set of alternatives X be finite and assume that PE, AN and OP hold. Then for every* $i \in N$ *there exists a nonempty set* $X^i \subset X$ *such that*

*(i) for any* $x, y \in X^i$, $h^i(x, 1) \leqslant h^i(y, n)$,

*(ii) for any* $x \in X \backslash X^i$, *there exists* $y \in X^i$ *such that* $h^i(y, 1) \geqslant h^i(x, n)$.

*Moreover, there is a utility representation* $v^i : X^i \longrightarrow \Re$ *such that one of the following two statements is true:*

*(iii) for any* $x \in X^i$, *for any integer* $1 \leqslant k \leqslant n$, $v^i(x) = h^i(x, k)$

*(iv) for any* $x, y \in X^i$, *for any integers k,m such that* $1 \leqslant k, m \leqslant n$, $v^i(x) + k \geqslant v^i(y) + m$, *if and only if* $h^i(x, k) \geqslant h^i(y, m)$.

We can refer to $X^i$ as to the set of relevant strategies, while $X \backslash X^i$ is the set of irrelevant strategies for player $i$.

*Lemma 1* is necessary to prove the equilibrium existence and so it is logically preceding *Proposition 1.* I will mostly use *Lemma 1* to characterize the resulting Nash equilibria which surely exist by *Proposition 1.*

**Definition 1** *An identical strategy profile* $\mathbf{x} = (x, ..., x)$ *is such that all players choose the same alternative.*

**Corollary 1** *For all payoff functions defined by conditions PE, AN and OP such that condition (i) of the Lemma 1 holds with strict inequality, all identical strategy profiles* $\mathbf{x} = (x, ..., x)$ *are Nash equilibria.*

**Proof.** If for any $x, y \in X^i$ [and for any $i$], $h^i(y,1) < h^i(x,n)$, and if $\mathbf{x} = (x, ..., x)$ is the strategy played, than the relative payoffs are $h^i(x,n)$ for all players. Thus, none has incentive to switch to another strategy alone and $\mathbf{x}$ is a Nash equilibrium. ■

This is a first result for the general setting. It is possible that some equilibria of maximal concentration (all researchers deal with the same topic) arise. Due to the externality effect, any topic can be a Nash equilibrium where all players make the same choice, even if their personal preferences over the alternatives are heterogeneous and, in general, their (individually) most preferred alternative is different by that of equilibrium. This result is due to the *PE*, *AN* and *OP* conditions that are able to limit the differences between the researchers' tastes. In fact it is no possible for anyone dealing alone with a topic when all the others have chosen differently.

We assume now complete homogeneity of tastes.

**Assumption** Let be $h^i(\cdot,\cdot) = h(\cdot,\cdot)$ for all $i$.

This assumption obviously satisfies the existence conditions.

We can imagine the mass of "common" researchers whose objective is to maximize some reputation index, say the citation one. All these researchers should have the same topics valuation in terms of reputation, because the indices are something of completely objective, by definition. Moreover, the audience size is given for anyone as well as the reviews editorial lines.

**Proposition 2** *Let us suppose that $h^i(\cdot,\cdot) = h(\cdot,\cdot)$ for all $i$ and that such a payoff function satisfies the PE, AN and OP conditions. Then*

*(i) all identical strategy profiles are Nash equilibria.*

*(ii) some alternatives of $X$ are not chosen by anyone.*

*(iii) a strategy profile, such that at least two alternatives are chosen by the same numbers of players, is not a Nash equilibrium (Impossibility of uniform distribution).*

*(iv) a strategy profile, where two relevant alternatives $x, y$, such that $v(x) < v(y)$, are chosen by respectively $k$ and $m$ players with $k < m$, is not a Nash equilibrium.*

*(v) if $h(x, \cdot) = v(x)$ for all $x \in \overline{X}$ (set of relevant alternatives), then there is a unique Nash equilibrium generated by the strategy profile $\mathbf{x}^* = (x^*, ..., x^*)$, such that $v(x^*) > v(y)$ for all $y \in \overline{X}$.*

*(vi) if $h(\cdot, k_x) = k_x$ for all $x \in \overline{X}$ then all the homogeneous strategy profiles are Nash equilibria.*

**Proof.**

(i) A specification of the corollary proof.

(ii) Given the features of $X^i$ such as defined in *Lemma 1*, $\overline{X}$, that is the set of relevant alternatives in this context, is neither nonempty nor it coincides with $X$. Thus $X \backslash \overline{X}$ is nonempty.

(iii) Let us suppose to order all alternatives on the basis of their "direct" utility, that is in terms of $v(x)$, by the mean of a ordering operator $\sigma(\cdot)$. Denote $x_{\sigma(1)}$ the most preferred and $x_{\sigma(2)}, x_{\sigma(3)}, ..., x_{\sigma(|\overline{X}|)}$ the others. Suppose that $k$ players choose $x_{\sigma(i)}$ and $k$ players choose $x_{\sigma(j)}$ with $i < j$. Then all agents preferring $x_{\sigma(j)}$ get $v(x_{\sigma(j)}) + k < v(x_{\sigma(i)}) + k$. Indeed, they have incentive to switch towards $x_{\sigma(i)}$. Thus a strategy profile, such that any two alternatives are chosen by the same number of players, cannot be a Nash equilibrium. We can extend the argument to the case where more than two alternatives are chosen by the same number of researchers (Impossibility of uniform distribution).

(iv) Using the same structure of point (iii), it is straightforward to verify that the incentive to switch is even stronger.

(v) If $h(x, \cdot) = v(x)$ for all $x \in \overline{X}$, we are in a game where strategical interdependencies are not present. Thus every player maximizes his own payoff regardless the others' strategies. Since the set of relevant strategies is finite, there exists at lest one most preferred alternative. Whenever a unique best choice exists, all players, having the same preferences, will choose it. This situation clearly verifies the Nash equilibrium definition.

(vi) If $h(\cdot, k_x) = k_x$ for all $x \in \overline{X}$, then we are in the opposite case. Here only interaction matters. Given any strategy profile, it is always possible to switch towards the alternative chosen by the largest number of players. The only possible Nash equilibria are those arising from identical strategy profiles.

■

The main result of this proposition is the impossibility of a uniform distribution. Under the *PE, AN* and *OP*, a game with all the players endowed with the same payoff function, cannot bear a Nash equilibrium where the same number of players chooses each alternative. This is sufficient to conclude that in proximity of any Nash equilibria, all researchers are not distributed in a uniform way, but with some concentrations (clusterisations) around certain alternatives.

This phenomenon is linked to both the "direct" preference of an alternative and to the network externality effect. Moreover an equilibrium distribution (i.e. the players' distribution at a Nash equilibrium), is skewed to the left[12], when we represent it over the ordered alternatives, as shown in the proof of point (iii). Thus an equilibrium distribution, except those degenerated on a unique alternative, should be decreasing. To summarize, either all players choose the same alternative (identical strategy profile), or they choose different alternatives but such that their distribution is decreasing.

The second remark is the role of the network externalities on the features and on the number of the equilibria. If we consider only the case of point (iii) of *Lemma 1*, that is, if we imagine that the common payoff function is such that $h(x, \cdot) = v(x)$, then the unique equilibrium arising is the Pareto optimal. At this equilibrium there is no player that would be better choosing a different alternative (the others are not touched by such a deviation in this context).

In the general case, where also the network externality effect is present, the Pareto optimal equilibrium is only one of the possible. Without any form of coordination, suboptimal equilibria may arise. Thus the overall effect of positive externalities is that of limiting the chance to obtain a Pareto efficient outcome. This is similar to the results of Farrell and Saloner, 1985 and of Grilo, Shy, and Thisse, 1999, mentioned above.

In any case, inside the assumed setting, network externalities induce a clusterisation of the researchers over relatively few alternatives. Therefore, such externalities may be an important determinant of the fashions formation that we observe in science as well as the engine, operating at the micro level, of the science evolution through paradigms.

[12] We will put $x_{\sigma(1)}$ on the right extremum and $x_{\sigma(|\overline{X}|)}$ on the left extremum of the support line.

## 7.2 Xenophobia

We consider two groups of researchers, the "common" researchers and the stars. Empirical studies showed that only a little 6% of authors produces half of the articles (Lotka, 1926) and, moreover, it also produces the greatest share of the papers having the highest impact (Combes and Linnemer, 1999). We suppose that agents compete between them to maximize an index of reputation, say the index of citation. As usual, large positive network externalities exist.

Nevertheless, in many fields it is apparent that there is a separation in the scientific community: it is easier to benefit from the externalities or from the cooperation of members belonging to the same group. Indeed, stars prefer to meet and cooperate with other stars, to avoid the possibility that common researches act as free riders.

Now let us formalize these ideas in a simple way.

Consider a set of players $T = [0, \gamma] \cup ]\gamma, 1]$ where the former interval represents the common researchers (group 1) and the latter the stars (group 2).

The set of topics is binary or $E = \{e_1, e_2\}$ where $e_1 = (1, 0)$ and $e_2 = (0, 1)$ are the unit vectors of $\Re^2$.

The payoff functions are $u_1(e_i, s_1, s_2) = \alpha_1 s_{1i} + \beta_1 s_{2i}$ for the common researchers and $u_2(e_i, s_1, s_2) = \alpha_2 s_{1i} + \beta_2 s_{2i}$ for the stars, where $\alpha_i, \beta_i \in [0, 1]$ and $\alpha_1 > \beta_1$ and $\alpha_2 < \beta_2$. Finally, $s_{ji}$ is the measure of the subset of the group $j$ dealing with the topic $i$.

Such a formulation capture the idea that working on the same topic of others is more valuable and that such externalities are more easily enjoyable if they come from the same group.

The best reply function has the following form for both groups $j = \{1, 2\}$

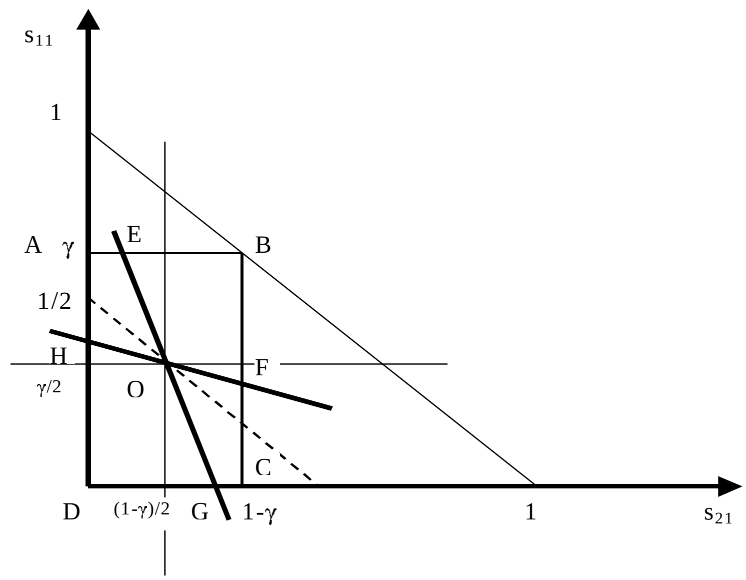
$$B_j(s_1, s_2) = \begin{cases} e_1 & \text{if } \alpha_j s_{11} + \beta_j s_{21} \geq \alpha_j s_{12} + \beta_j s_{22} \\ e_2 & \text{otherwise} \end{cases}$$

In other words, group $j$ prefers $e_1$ for all the pairs $(s_1, s_2)$ above the straight line

$$s_{11} = -\frac{\beta_j}{\alpha_j} s_{21} + \frac{\gamma(\alpha_j - \beta_j) + \beta_j}{2\alpha_j}$$

This is a family of lines centered on $(\frac{1-\gamma}{2}, \frac{\gamma}{2})$ and with negative slope belonging to the interval $[0, -\infty)$, given the possible values of $\alpha_j$ and $\beta_j$. It is clear that all the lines associated with the group 2 are more sloped, in absolute value, than those associated with the group 1.

Look at the following picture and notice that each point can be thought as a 4-tuple $(s_{11}, s_{12}, s_{21}, s_{22})$ where $s_{12} = \gamma - s_{11}$ and $s_{22} = (1-\gamma) - s_{21}$:




All the pairs $(s_1, s_2)$ in the area EOFB are such that both groups prefer the alternative $e_1$. Therefore the unique fixed point for this region is the point B. Indeed the best response to the point B is B.

Symmetrically, all the pairs $(s_1, s_2)$ in the area HOGD lead both groups to chose the alternative $e_2$ and the unique fixed point is D.

In the region HOEA, group 1 prefers $e_1$ but group 2 prefers $e_2$. The only possible fixed point is A and this represents the separation equilibrium. Nonetheless, such equilibrium exists only under some conditions that we wish discuss briefly below.

The last region is GOFC, where the unique fixed point is C that also exists under the same condition for A.

The existence of the fixed points C and A depends on the parameters. If $\gamma \geq \frac{1}{2}$, we need that $\gamma(\alpha_2 + \beta_2) \leq \beta_2$. This condition is verified when $\beta_2 >> \alpha_2$, i.e. when a star finds much more valuable working with other stars than with common researchers. Symmetrically, if $\gamma < \frac{1}{2}$, we need that $\alpha_1 >> \beta_1$ or that a common researcher finds much more valuable working with others common researches than with stars. Therefore, the separation equilibria exist if the stars' "xenophobia" is high enough, i.e. if they operate in such a way that the produced externalities do not benefit the common researchers.

Obviously, this conclusion is very weak, since the pooling equilibria always exist, independently of the degree of "xenophobia". However a sufficiently high degree of "xenophobia" may lead to segregation equilibria.

## 7.3 Willingness of separation

Quite usually, one may observe that there are communities of researchers that avoid the comparison with others, especially with the stars, and organize an autoreferential circle, impermeable enough, that allows them to make research and publish on topics that we may say reserved. Such a practice allows a researcher to get a certain reputation (an so a certain utility) recognized inside the circle, also if, in absolute value, the quality or the interest is low. The circle may also edit a journal that publishes its activities.

Such a system may survive if the stars do not write on the reserved topic and do not submit their papers to the circle's journal.

Let us formalize such ideas.

The set of players is $T = [0,\gamma]\cup]\gamma,1]$, with the same interpretation of the previous subsection, the alternatives are $E = \{e_1, e_2\}$ and the payoff function are $u_j(e_i, s_1, s_2) = \pi_{ji}(s_{11}, s_{22})P_i(s_{1i}, s_{2i})$ where the subscript $j$ is for the groups and the subscript $i$ for the alternatives. Let $\pi_{ji}(\cdot)$ represent the probability to publish on the journal specialized on the topic $i$, for the group $j$. Finally, $P_i(\cdot)$ represents the reputation of the journal $i$ (and so the reputation a researcher obtains by publishing on it). The journal reputation is a function of the number of common researchers and stars that publish on its pages.

We make the following specifications.

The probability to publish is always higher for the stars than for the common researchers, i.e. $\pi_{21} > \pi_{11}(s_{11})\ \forall s_{11}$ and $\pi_{22} > \pi_{12}(s_{22})\ \forall s_{22}$. For both groups, topic 1 is "easier" than topic 2. Formally: $\pi_{11}(s_{11}) > \pi_{12}(s_{22})$ $\forall s_{11}, s_{22}$, $\pi_{12}(1-\gamma) = 0$, $\pi_{21} > \pi_{22}$. However, a common researcher faces a probability to publish on topic 1 increasing with the number of members of group 1 that studies the same topic, i.e. $\pi_{11}(s_{11}, s_{22}) = \pi_{11}(s_{11})$ increases on $s_{11}$. On the contrary, the probability to publish on topic 2 decreases with the number of stars that deal with it, i.e. $\pi_{12}(s_{11}, s_{22}) = \pi_{12}(s_{22})$ decreases on $s_{22}$. We assume also $\pi_{2i}(s_{11}, s_{22}) = \pi_{2i}$ constant. To strengthen the group 2 scientists' advantage, let be $\pi_{11}(s_{11})/\pi_{12}(s_{22}) > \pi_{21}/\pi_{22}$ $\forall s_{11}, s_{22}$: the probability gap is higher for the common researchers than for the stars.

There exist two journals and each one publishes a different topic. The reputation of the journal, $P_i$, depends negatively on the number of common researchers and positively on the number of stars that publish on it, i.e. $P_i(s_{1i}, s_{2i})$ decreases on $s_{1i}$ and increases on $s_{2i}$. Moreover we assume $P_1(0,0) = P_2(0,0)$ and $P_i(s_{1i}, s_{2i}) > 0$ $\forall i, s_{1i}, s_{2i}$. The payoff of each researcher depends on the expected reputation he receives from publishing on a journal.

Let us now find the equilibrium set of the game.

First, we simply show that there exist no equilibria where both groups are distributed on the two topics. Indeed, the conditions would be

$$\begin{cases} \pi_{11}(s_{11})P_1(s_{11}, s_{21}) = \pi_{12}(s_{22})P_2(s_{12}, s_{22}) \\ \pi_{21}P_1(s_{11}, s_{21}) = \pi_{22}P_2(s_{12}, s_{22}) \end{cases}$$

Given the conditions on $\pi_{ji}$, the two equalities above can never be verified simultaneously. Neither it can be possible that one group is distributed on the two topics in equilibrium.

The unique possibilities are the separation equilibria. Only the case where all commons researchers write on topic 1 (the easiest) and all the stars write on topic 2 is possible. Indeed, the corresponding conditions

$$\begin{cases} \pi_{11}(\gamma)P_1(\gamma, 0) \geq \pi_{12}(1-\gamma)P_2(0, 1-\gamma) \\ \pi_{21}P_1(\gamma, 0) \leq \pi_{22}P_2(0, 1-\gamma) \end{cases}$$

are verified.

Thus the common researchers close themselves into a circle that deals only with topic 1, published on journal 1. Stars find not convenient to research on such a topic, even if easier, because of the low standing of the corresponding journal. In other words, the common researchers keep away the others, by lowering the reputation the stars can get.

## 7.4 The planet of the Gods

Here we present a setting where the payoff of the group 1 depends negatively on the number of its components that write on the same topic and positively on the number of the stars. This stays for the fact that an higher competition reduces the possibility of publishing, but the presence of stars on a topic increases its interest. On the other hand, smart researchers are interested only on their own distribution and do not care on the group 1 decisions: the stars influence but they are not touched by the common researchers' choices. We also imagine that the alternatives are ordered by their degree of difficulty and interest represented by the probability to discover something (a low probability stays for an higher interest of the discovery).

The formal setting is:

$T = [0,\gamma] \cup ]\gamma, 1]$ is the set of the players

$E = \{e_1, ..., e_n\}$ is the set of the alternatives or of the topics.

The payoff functions are $u_1(e_i, s_1, s_2) = \pi_{1i} P(s_{1i}, s_{2i})$ $\forall t \in [0,\gamma]$ and $u_2(e_i, s_1, s_2) = \pi_{2i} R(s_{2i})$ $\forall t \in ]\gamma, 1]$. We assume that $P_{s_{1i}} < 0$, $P_{s_{2i}} > 0$ and $R_{s_2} > 0$. The probabilities $\pi_{ji}$ are such that $\pi_{11} < ... < \pi_{1n}$ and $\pi_{21} < ... < \pi_{2n}$ and finally $\pi_{1i} < \pi_{2i}$ $\forall i \in \{1, ..., n\}$. Indeed, as in the previous example, group 2 has an advantage in terms of probability to discover something (because it has more talent).

Moreover, we assume that $\pi_{1i} P(0, s_{2i}) \geq \pi_{1j} P(s_{1j}, s_{2j})$ $\forall j, s_{1j}, s_{2j}$. This assumption means that for a common researcher is always preferable a topic where no other common researchers work.

Therefore the only equilibrium distribution is a distribution where all topics are covered. Indeed, the equilibrium condition is

$$\pi_{11} P(s_{11}, s_{21}) = ... = \pi_{1n} P(s_{1n}, s_{2n})$$

On the other hand, the group of stars is unaffected by group 1 and a necessary condition for an equilibrium is either $s_{2i} > s_{2j}$ for any $j > i$ or $s_{2i} = 0$.

Let us now specify the functions to find a closed form equilibrium.

Let $P(s_{1i}, s_{2i}) = \begin{cases} \Pi & \text{if } s_{1i}=0 \\ \alpha s_{2i} - \beta s_{1i} & \text{otherwise} \end{cases}$ and $R(s_{2i}) = s_{2i}$. Let $\Pi$ be large enough to satisfy the condition on $\pi_{1i} P(0, s_{2i})$. Moreover we impose that $\pi_{2i} = (1+\delta)\pi_{1i}$ with $\delta > 0$. We consider only the equilibria were the stars' group allocates itself over the first $m$ topics and, following the necessary condition, in a decreasing way. Notice that, contrary to the requirements of the theorem of existence, $P(\cdot, \cdot)$ is not always continuous. However, the considered discontinuity is very special and it does not prejudge the equilibrium existence.

The equilibrium condition, given the distribution of the stars, is

$$\pi_{11}(\alpha s_{21} - \beta s_{11}) = ... = \pi_{1m}(\alpha s_{2m} - \beta s_{1m}) = \pi_{1m+1}(-\beta s_{1m+1}) = \\ ... = \pi_{1n}(-\beta s_{1n})$$

Rearranging, for $i \in \{1, m-1\}$ the chain of equalities gives

$$s_{1i+1} = \frac{\pi_{1i}}{\pi_{1i+i}} s_{1i} - \frac{\alpha}{\beta}\left(\frac{\pi_{1i}}{\pi_{1i+i}} s_{2i} - s_{2i+1}\right)$$

For $i \in \{m+1, n\}$ we have:

$$s_{1i+1} = \frac{\pi_{1i}}{\pi_{1i+i}} s_{1i}$$

and for $i = m+1$ we have:

$$s_{1i} = \frac{\pi_{1m}}{\pi_{1i}} s_{1m} - \frac{\alpha}{\beta} \frac{\pi_{1m}}{\pi_{1i}} s_{2m}$$

Now we compute the distribution of the group 2, the stars.
The equilibrium condition is

$$\pi_{1i}(1+\delta)s_{2i} = \pi_{1i+1}(1+\delta)s_{2i+1} \text{ for all } i \in \{1, m-1\}$$

By the chain of equalities we obtain simply

$$s_{2i} = \frac{\pi_{11}}{\pi_{1i}} s_{21} \text{ for } i \in \{1, m\}$$

and

$$s_{2i} = 0 \text{ for } i \in \{m+1, n\}$$

This distribution is a part of an equilibrium if $s_{2i} \geq 0$ and if $\sum_{i=1}^{n} s_{2i} = 1-\gamma$. By simple computations, we obtain that

$$s_{21} = \frac{1-\gamma}{\pi_{11}A} > 0$$

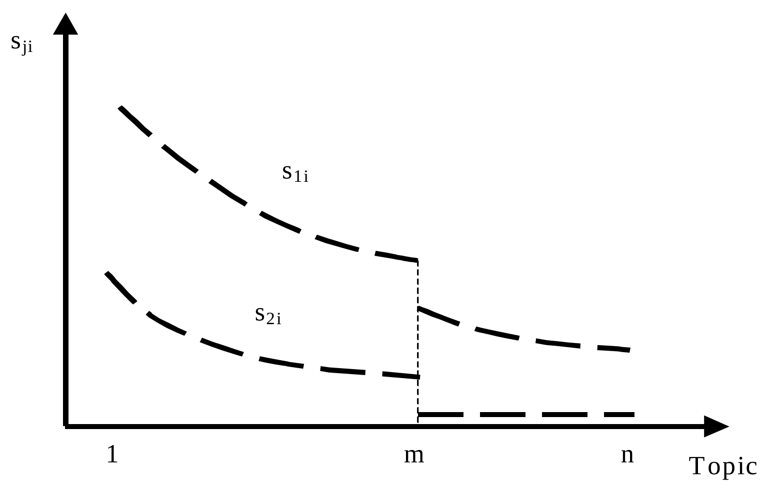
where $A = \sum_{i=1}^{m} \frac{1}{\pi_{1i}}$ and all conditions are verified.

Now, come back to the group 1 distribution.

Substituting for $s_{2i}$,summing all the different $s_{1i}$ and imposing the equality to $\gamma$, we obtain

$$s_{11} = \frac{\gamma + \frac{\alpha}{\beta}(1-\gamma)\frac{B}{A}}{\pi_{11}(A+B)} > 0$$

where $B = \sum_{i=m+1}^{n} \frac{1}{\pi_{1i}}$. We have only to check that $s_{1m+1} \geq 0$, i.e., the number of researchers in the first alternative without stars is positive: this is sufficient to guarantee the positivity of all the $s_{1i}$ for $i \in \{m+2, n\}$. Such condition is verified for $\gamma > \frac{\alpha}{\alpha+\beta}$ or if the dimension of the group 1 is large enough. To conclude, the equilibrium of this game can be represented as in the following picture.




The distribution of both groups over the alternatives is decreasing, with a higher concentration over the more difficult or interesting topics. This feature is completely explained by the stars' distribution effect that more than compensates the competition effect, summarized in the condition $P_{s_{1i}} < 0$.

# 8 Conclusions

In this paper we have shown that games involving anonymity with both a finite and an uncountably infinite number of players, well represent a setting of positive externalities as the research world is. We have also introduced limited anonymity to distinguish the different roles and influences of *stars* and of *common researchers*.

Whenever only positive externalities are considered, a clusterisation phenomenon, generalized or inside two distinct groups, arises. If we suppose that externalities produced by the own group are more effective, it is possible to show the existence of equilibria where common researchers and stars deal with different topics and never meet.

A similar finding arises when we focus not only on the externalities, but we consider the competition of the stars suffered by the common researchers. The latter, to avoid the competition, enclose themselves in a kind of lobby, dealing with special topics and publishing in low reputed journals specialized on their topics. Such low reputation keeps away the stars.

Adding competition between common researchers and taking into account only the positive externalities produced by the stars, clusterisation always exists, but the equilibrium distribution presents also few researchers choosing unpopular topics. Moreover, we get that the stars deal with difficult but interesting topics, given their preferences. The resulting externalities push also the common researchers to choose the interesting topics, despite their difficulty and the relatively low probability of success.

We have argued that the main rationale for the (strong) externalities existence is the need of "marketing" activities in order to expand the impact of a new research. We have shown that the *priority rule* and the *Matthew effect* strengthen the incentive to promote the own work and to open themselves to the current debate and to cooperation.

# 9 Appendix

## 9.1 Journals system

Given the relevance of publication as a device to determine the priority of a discovery, we spend some words about the system of (economic) journals. The first published journal in economics is German, the “Zeitschrift fur die Gesamte Staatswissenschaft” dated 1844. The first English-language are dated by the end of the 19th century, such as Quarterly Journal of Economics (1886), Economic Journal (1891) and Journal of Political Economy (1892). Today the large majority is American or European and its language is English, reflecting the fact that economics is an English language science ( Stigler, Stigler, and Friedland, 1995). Their number amounts of several hundreds, while 4300 reviews on social sciences was covered by the “Social Science Citation Index” in 1980 (Liebowitz and Palmer, 1984).

Each journal specializes on a field of studies an applies a specific editorial line (Stigler, Stigler, and Friedland, 1995). Its reputation is given by the level of its editorial board and of its referees that choose the articles to publish and by the fact that, very often, it is promoted by an university department with recognized stars (publishing on “their” journal). An element of the editorial policy is given by the personal preferences or persuasions of the editors. Here are a couple of interesting examples that show this and give a proof of the competition between journals:

> Davis Dewey, editor of the AER from 1911 to 1940, made that journal unreceptive to the growing technical rigor and formalization of economics, but the effect was a good deal stronger on the AER than on the profession. In fact Dewey subsidized the rise of Econometrica. Similarly Keynes's long reign at the Economic Journal probably discouraged its publication on econometric works, of which he was a skeptic, again a subsidy to Econometrica, and his policy also helped the Review of Economic Studies (*ibidem*).

Many recent articles have tried to rank a quite large sample of economic journals. Several different techniques are applied, from the simple interview system to a sample of researchers (Dusansky and Vernon, 1998), until sophisticated tools, based on citation of the published articles on all others journals, technique known as the “measure of the journal impact on the profession”

(Liebowitz and Palmer, 1984), or based on the ratio between the number of times journal A is cited by journal B relative to the citations of journal B by journal A (Stigler, Stigler, and Friedland, 1995). Often, different tools are used simultaneously to limit some inevitable biases. All these studies lead to a ranking that is quite similar. Some differences may arise for the medium and bottom ranked. Incidentally, we wish to mention that similar methods are also employed to rank economics departments and the results are equivalent, even if much more debated (Dusansky and Vernon, 1998; Feinberg, 1998; Griliches and Einav, 1998; Kalaitzidakis, Theofanis, and Stangos, 1999).

A last remark comes from Laband and Piette, 1994 who published an econometric work on the JPE. They start by the critics made towards the editor of the JPE about a supposed favoritism for papers of the Chicago University members (promoter of the JPE). Their conclusion based on empirical evidence (relative to 28 top economic journals) is that favoritism is no present, and "although journal editors occasionally publish subpar papers authored by colleagues and former graduate students, on balance their use of professional connections enables them to identify and capture high-impact papers for publication. This implies that a practice interpreted as favoritism by many scholars in fact serves to enhance efficiency in the market for scientific knowledge".

## 9.2 Indices based on publications

Publication indices are built using different techniques. The easiest way to expose this topic is to follow the Combes and Linnemer, 1999, paper. In such a way the reader can find tree examples of these tools.

Let us now analyze them, chosen to evaluate the productivity of a sample of french economists. This indices are built by the combination of the following sets: {Egal, Dix, Blue}, {N,P}, {1,1/n} and normalized by the length of the career of each researcher (starting from his first publication). *Egal* means that each review has the same weight, *Dix* means that each review is labeled by a weight from 1 to 10, *Blue* means that only 8 famous reviews[13] are taken in account (as in Dusansky and Vernon, 1998). In the second set, N is for the number of articles, P is for the total number of pages; finally,

[13]These reviews are: AER (100), Econometrica (51), JET (23), J. Polit. Econ. (36), Quater. J. Econ.(28), Review of Ec. Studies (38), Rev.of Econ. and Stat. (24), Int. Ec. Rev. (9)

in the third set, 1 is for no sharing in co-authored articles while 1/n means that each of the n authors is attributed by a 1/n of the publication.

Clearly the number of possible combinations of this three sets is 12. By the mean of the correlation matrix between them, the authors chosen Egal_N_1, Dix_P_1/n and Blue_P_1/n. This is because they found that the highest correlation is between indices embodying the same element of the first set, whereas significantly little correlation was found between any two embodying different elements of the same set. Therefore, any element in {Egal, Dix, Blue} elicits specific and quite independent features of the researchers' productivity.

The choice of the component {N,P} and {1,1/n} depends upon other considerations like simplicity, possibility of comparison with other preceding analysis or traditional evaluation criteria (length, number of authors).

The authors show that the top ranked ten researchers are essentially the same, with some little differences in the index based on *Egal* (i.e. where only the number of published pages matters). Not only: it is apparent from these ranks that, with some significant exceptions, such as Grandmont, the more productive are also those publishing in the more reputed reviews. If we accept that the more reputed reviews publish only high quality articles, we can conclude that the most productive researchers are also those who produce the highest quality.

This result seems in contrast with the analysis of correlation. Nevertheless, such inconsistency can be simply explained by the fact that the correlations are calculated over the entire sample while here we consider just the first ten ranked researchers.

The stars can be unanimously defined, despite the used tool, while the problem is which index is relevant to sort the medium and bottom ranked researchers. Moreover this result (based only on the french researchers) is consistent, at least in its quantitative terms with the *Lotka law*, based on US data.

I would remark three considerations about the variables taken into account here:

1. all these indices are normalized by the career length, in such a way that they represent a sort of average productivity during the activity life. Since we observe, even with some contradictions, that researchers have a diminishing productivity with the going on of the age, these indices tend to favor the young.

2. taking into account only the number of publications introduces a bias in favor of those researchers that publish more notes or brief articles; however, taking into account the number of pages can not be correct because an article length may vary by 5 pages only in response of editing criteria without any additional work requirement: "*cinq pages supplémentaires dans un article déjà long contiennent parfois moins d'idées nouvelles qu'une note de même longueur*" (*ibidem*)

3. the choice of counting only for 1/n a n-authored article suppose that to write it each co-author spends exactly 1/n of the effort required to write an article alone: "*il peut sembler qu'une telle pondération pénalize trop le travail en équipe. Écrire un article a deux, par exemple, ne nécessite certainment pas deux fois moins de travail que de faire seul*" (*ibidem*).

Let us remark few points. The first index is the simplest: every article, even if written with co-authors, counts for 1, independently of the review of publication. It is evident that it does not consider the quality of the writing. The second appears more fair, in the sense that it accounts for the number of pages, the weight of the review and the numbers of authors. The same reasoning is worth also for the third, but here only the publications on the blue ribbon reviews matter: this hurts those authors that publish on very specialized reviews, even if their work is extremely relevant on their domain.

If the relevant index was Egal_N_1, then any researcher would have maximal incentive to write *n'importe quoi* in *n'importe quelle* review. The only fact that matters is the number of publications. A way to limit this issue is to circumscribe the set of reviews taken into account, but this would need a form of evaluation. Moreover, there is the maximum incentive to cooperate and eventually to be free rider while on the other hand there are no incentives to contrast free riding. Thus, this index seems to induce an inflation of publications and to have a limited ranking efficiency.

Using the Dix_P_1/n or the Blue_P_1/n indices, authors are strongly encouraged to publish on top ranked reviews: an article published on AER weights as 10 articles published on *Rivista Internazionale di Scienze Economiche e Commerciali,* if one use the Dix_P_1/n, and infinitely more, if one use the Blue_P_1/n.

Finally, it is worth to highlight some problems that in general arise with the reputation indices based on publication. First, there is not a general agreement on the weights to give to each review, also if for the top reviews, is obviously simpler to obtain a homogeneous evaluation (see the appendix devoted to the discussion about the journal system). It is also necessary to consider the career of each researcher to avoid discriminations towards the young and it is necessary to identify an attribution criterion for the multi-author works. This last problem is maybe the most debated. It involves the theme of research portfolio diversification, exploitation of stars' reputation but also efficient allocation of effort, complementariness of skills, increased easiness in joint researches due to information technology (Bird, 1997). All these issues push towards the use of a set of indices rather than a single one as in the paper of Combes and Linnemer. In any case a good remark comes from Griliches and Einav, 1998: they criticize the way to rank US economics departments, as done by Dusansky and Vernon, 1998, but the argument can be easily extended to the individual researchers. Even if two articles are published on AER, their relevance can be very different: a paper can remain in the core of the science for many years while another may have limited impact (see also Berg and Wagner-Dobler, 1996). A strong support to this thesis can be found in Callon, Courtial, and Penan, 1993, where, by the mean of some evidence, they found that the distribution of citations (and so the impact of an article) follows the *Lotka low*: "*les articles ayant un fort impact contituent une faible minorité*" and "*un tout petit nombre d'articles attire la majorité des citations*" (*ibidem*).

## 9.3 Co-citation and co-word analysis

Publication and citation indices tend to emphasize the value of the more productive researchers. This is in particular the feature of the former. The latter, trying to assess the impact dimension of a work, indirectly and approximately can individuate the effective contribution to the science. Nevertheless, sometimes not very prolific authors write cornerstone papers ( Berg and Wagner-Dobler, 1996). By the mean of the evaluation tools we presented until now, the relevance of these low productive scientists can be misrepresented. To limit this bias, two tools are defined: co-citation analysis and co-word analysis aim to evaluate the content and the fondant-contribution feature of a work.

Co-citation analysis simply counts the number of times a couple of citation is repeated in a sample of articles about the same subject. The rationale is simple. Since a single citation can have any meaning (acknowledgment, criticism, politeness...) and any author can cite the same source in very different topics, if we find the same couple of citations in different works, then we can infer that these two contributions have a complementary content. In fact, in this case, different authors consider that the two sources, for some reasons, are linked and it is necessary to cite both. Thus we have obtained an information about the nature of the two references. At this point we can represent the whole network of co-citations. In the middle we shall find the more cited couples, maybe with some reciprocal relations, while externally we will find the citing articles. Following the notion of paradigm proposed by Kuhn, 1962, we can conclude that the papers belonging to the more co-cited couples are those seminal, those paradigmatic, while the papers citing these couples are those that contribute to develop such a paradigm. As observed by Kuhn, we can trace the science evolution through the evolution of the clusters formed by the core/paradigmatic articles and the cloud of citing ones (Callon, Courtial, and Penan, 1993). Some refinements of such a method are individuated in Small and Sweeney, 1985aSmall and Sweeney, 1985b.

The second tool able to evaluate the contents is the co-word analysis. It has no a direct meaning in terms of research activity evaluation, because its aim is to observe the links between concepts or basic ideas through an analysis of co-occurrence of some significant words. These can be picked out from titles, abstracts or simply from the keywords that authors themselves highlight. The logic is similar to that explained for the co-citation tool. While a word taken alone has no meaning to define the content of an article, if this word co-occurs with another one or with few others, then we can infer something more about the contribution. Moreover, if we find the same patterns of co-occurrences throughout a wide set of paper, we can infer that each paper has the same topic. At this point we are able to build the dynamic of a topic in the time or in the space - i.e. in the journals or in the countries (Bhattacharya and Basu, 1998; Callon, Courtial, and Penan, 1993).

Using this system it is possible to follow the evolution of the interest and the validity of a certain topic or paradigm. This is an important result for the evaluation of the research activity: while some papers remain seminal for long periods of time, others are subjected to a more or less rapid decline in the scientific community's consideration. Then, we need a rule of depreciation, to compute the present value of a contribution. Once defined the appropriate co-word pattern, we will possess an objective parameter to measure the current relevance of an article.

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