REJECTED

Leading Economists Ponder the Publication Process

Edited by
George B. Shepherd

Foreword by
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Born in Argentina, Graciela Chichilnisky received no undergraduate education, but has obtained two Ph.D. degrees: in mathematics and economics from UC Berkeley. One of the profession's rare female technical theorists, Chichilnisky's more than 100 published articles and three books have investigated pure mathematics, including topics such as topology and nonlinear analysis; mathematical economics and social choice theory; and applied issues in development and international economics. An important contribution is her introduction and formalization of the idea that economic development occurs by satisfaction of a population's basic needs. In addition, she frequently advises the United Nations, the World Bank, OPEC, and the OECD.

This is in response to your letter of April 3rd, 1992, requesting stories about journal rejections--if any. Yours is a worthwhile venture. There seems to be room for improvement in our journals, and perhaps your book will be of help.

But I must warn you--and your reader--that the subject is sufficiently important and painful to an author that what one writes cannot be taken too seriously. Like the other authors of your volume I have had papers rejected by journals in several areas, for most of my career, including currently. I had rejections in pure mathematics and in applied mathematics, in economic theory, game theory and in mathematical economics, in financial economics, international trade and development economics, in environmental economics and the economics of networks. Perhaps I have more papers rejected now than ever, but this may be because I am writing more.

The record of rejections is somewhat numbing because I have by now published more than one hundred scientific papers. Most of these have been rejected by main journals, yet many have subsequently been published in top journals, have become influential and their ideas widely quoted and used. One single paper took nine years of refereeing and rejections at the same journal to be published. Think of the anguish and the paper spent on this process. I want to believe that I am giving the editors and the referees a good run for their money.

Leading Economist Ponder the Publication Process
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Mathematical journals, particularly those in pure mathematics, are generally better than economic journals. They do a better job of reading the papers and they are not put-off or angered by advanced techniques. My rate of rejections from top mathematical journals has been almost nil. But an amusing recent story, recorded below, blemishes this record.

In my experience, the more innovative and interesting the paper, the more likely it is to be rejected. Economics editors seldom read papers, and referees do not read them carefully either. People who do not publish very much sometimes become editors, and they can be unsympathetic to others who publish more than they do. It is also my impression that one's most direct competitors--or those people who for some reason feel competitive--are most likely to reject a paper or to inhibit its publication. "Inhibiting its publication" may be too polite an expression; on occasions it means they publish the paper or some of its results under their own names. It can border on plagiarism.

Being a woman does not help. The *American Economic Review* published in December 1991 a piece which confirms a certain bias against women's submissions, in combination with another symptom of bias, the professional status of the institutions where they work. Some people feel that women do not have the right to new or powerful ideas; it is almost a question of denied property rights. Men are supposed to be the intellectual leaders and women the followers. Even in these revisionistic times, when it helps sometimes to be a woman, it does not help when it comes to intellectual leadership. Women are supposed to be good cooks, but the great chefs are men, so to speak. This plays havoc with creative and powerful papers written by women and reviewed by men. Just about all editors and referees are men. Reflecting on this phenomena, I have tried using my initials instead of my first name, but it is no good. Apparently there are not too many mathematicians or economists with the last name Chichilnisky.

I digress with an unrelated but connected story. I recall an interview with a Nobel laureate and much admired economist, many years ago. This took place just when I had completed my Ph.D. in Economics from the University of California at Berkeley (a second one, my first Ph.D. was in Mathematics, also at Berkeley), was a lecturer at Harvard, had a few publications in pure mathematics and in economics, and was looking for a tenure track job in the East Coast. After inquiring somewhat politely about my life and the life of my son, and finding out that my son was successful in school and happy and well adjusted at home, the eminent economist then asked me: "What are you: single, divorced or what?" The temptation for the obvious answer was strong, but I managed to answer, politely, that I was divorced. To this he retorted "Are you trying to tell me that men are not needed for anything?" This left me without an answer at the time, but it helped me many years later in understanding the position of women in economics.

This situation is sobering. The worst part is one learns how to get a paper accepted for publication if one were willing to lower one's standards. The recipe is simple: it should be written in one day, with one idea or less, it should fit well in the literature, and in particular in the literature with the names of the editor or the potential referees in it. One can almost guarantee the acceptance of a piece by following such an undesirable formula. But what a way to earn tenure. What a way to earn a living. And what a way to advance science.

Let me give some examples. A paper that took nine years of refereeing and rejections in the same journal just appeared in the *Journal of Economic Theory* in April 1993 under the catchy title "Existence of Competitive Equilibrium in

Sobolev Spaces without Bounds on Short Sales" (with G. Heal). On its first published page (p. 364) it records that it was submitted in February 1984, with a final revision in January 1992. In the nine year process it took to appear, it was rejected many times, and always sent back because the referee's comments although sometimes helpful, were not correct overall. During this process, one of its referees, now a member of the editorial board of this journal, published part of its results under his own name elsewhere. During these nine years, the literature on equilibrium in infinite dimensional spaces took fire and almost burned out, and in the last round of refereeing in 1991, I was told that the results had by then little novelty value because of the referee's results, which had now been published somewhere else. I took exception to this treatment and asked the editor, Professor Karl Shell, to reconsider the issue. It seemed wrong that a referee could delay the publication of a paper while using the material for his own publications. Furthermore, there were important aspects of the results which were still new, to which he agreed, so the paper was finally published in 1993. I also complained personally to the referee about his behavior, and to my surprise I perceived a certain amusement and even sympathy on his part.

Until recently I believed that my initial publications in economics would be the most difficult-- and that things would improve after some point. I was right, but only on the first point. After publishing successfully a few papers in pure mathematics in journals such as the *Transactions of the American Mathematical Society* (1972) and the *Journal of Mathematical Analysis and Applications* (1977), I turned my attentions to economics.

My first research in economics involved the formulation of the Bariloche Model, an empirical study of sustainable development in five continents which we did in Bariloche, Argentina in 1972-74. Here I introduced the concept of economic development based on the satisfaction of "basic needs." I wrote several papers on basic needs, which were rejected by several economic journals as uninteresting. I could not prove that they were wrong, but anyway persisted partly because Professor Tjalling Koopmans, then at Yale University and a widely admired and respected economist and Nobel laureate, invited me to the International Institute of Applied Systems Analysis in Laxenburg, Austria, to lecture on this work and was very complimentary and supportive. A 1975 IIASA paper by a member of the audience, William Nordhaus, entitled "Economic Modeling from the Bottom up" records enthusiastically my October 1974 presentation and its introduction of the concept of basic needs. The 1974 book on the Bariloche report *Catastrophe or New Society* has been published by now in English, French, German, Arabic and Japanese. My paper "Economic Development and Efficiency Criteria in the Satisfaction of Basic Needs", which introduced the concept of basic needs formally, was rejected by several journals.

My paper appeared finally in 1977 in *Applied Mathematical Modeling*, because of the novelty of its mathematical formulation, and an empirical piece "Development Patterns and the International Order" appeared the same year in a special issue of the *Journal of International Affairs* to which Wassily Leontief, a distinguished economist and Nobel laureate, was also a contributor. Since then I published many related pieces. My "basic needs" concept and the Bariloche Model became famous and widely used in international organizations such as the United Nations and the World Bank. Excellent economists and friends such as Professors Partha Dasgupta and Amartya Sen have used this concept and closely related ones in their work. Our approach in Bariloche had a wide impact on the thinking about sustainable economic development. The now fashionable 1987

Brundtland report *Our Common Future* anchors its formulation of sustainable development on the concept of basic needs. But my work was initially rejected from publication by the main economic journals to which I submitted it. It is almost never referenced by economists, not even by friends who work in this area. Despite the distinguished economists who know and recognize my contribution, credit for introducing the concept of basic needs has eluded me for many years, and I feel lonely because I have been separated from my brainchild.

Another of my first papers in economics, "Basic Goods, Commodity Transfers and the International Economic Order" was rejected several times, but finally accepted by Professor Lance Taylor who was then the editor of the *Journal of Development Economics.* This led to a somewhat bewildering situation which had a significant impact on my life, something that I could not have possibly anticipated at the time. The paper is on international trade. I thought and still think that the result is natural, and confirms my observations of the reality of trade and development. It was submitted first to the *Quarterly Journal of Economics* in 1978, and rejected on the grounds that I was "barking up the wrong tree" (sic), and then to the *American Economic Review* in 1979 which rejected it. It finally appeared in the *Journal of Development Economics* in 1980. The editor of the American Economic Review had rejected the paper on the grounds that the results were well known.

Instead, the paper was surprising to many. It led to a 1983 issue of the *Journal of Development Economics* with ten articles which debated and extended results, some in favor and some against. The results are now well established as correct, and almost classic. They appear in standard undergraduate textbooks in international trade, such as that of Krugman and Obstfeld.

Obviously the referees of the *American Economic Review* and the *Quarterly Journal of Economics* were both wrong, but how was I to know this? I was new in economics, and had to follow my own inexperienced judgment.

My results were not known, nor was I barking up the wrong tree. These results of mine contradicted thirty years of common wisdom about one of the most classic problems in economics, first studied by John Stuart Mill, and subsequently by J. M. Keynes and W. Leontief, among others. The common wisdom, reproduced in my colleague Robert Mundell's book on international trade, emanates from Paul Samuelson's dictum in the 1950's that one can only have transfer paradoxes when the market is unstable. Therefore it is not worth worrying about this phenomena, despite its illustrious pedigree. My paper showed this dictum to be wrong, an unexpected outcome after all those years.

The "transfer paradox" occurs when a country donates some of its endowment to another country and a new competitive equilibrium emerges at which the donor is better off and the receiver worse off. Samuelson's insight was that this could not happen when markets are stable, that is when prices rise with the excess demand. Therefore, he argued, the transfer paradox was a curiosum, not worth investing time on.

Professor Samuelson was right, but only when the world has exactly two countries, a somewhat restricting assumption. I proved that when the world has three or more countries, the transfer paradox occurs roughly speaking 50% of the time; furthermore, it occurs in stable markets, markets where prices rise when there is excess demand and fall when there is excess supply. The results require a bit of mathematics, but not much more than the implicit function theorem.

More importantly, the results had somewhat unsettling implications for the then fashionable view in the United Nations about how to improve the position of lower income countries. The general policy recommendation was to give aid or "transfers"; I showed that this would in general help the donor more than the receiver. My analysis focused on a world economy where different countries had very different income levels. It anticipated the future: during the 1980's aid dropped to less than half the UN target of 0.7% of GDP in OECD countries.

I was then running a large research project, a follow-up to the Bariloche Model, for the Under Secretary General for Economic and Social Affairs of the United Nations, M. J. Ripert, and for an ex-Under Secretary General, M. P. Seynes, two excellent international civil servants. Both received letters and calls indicating that my results on transfers were not only completely wrong but also "dangerous" (sic).

Several colleagues, including some at Columbia, published papers against my 1980 results on transfers, and more generally against my North-South model of international trade. One of these articles ends with the recommendation that my results, being so seriously and obviously wrong, should be "buried quietly". It has been difficult to separate this death wish for my work with other possibilities, as the whole life of the Department of Economics at Columbia University became tainted by this issue for many years. It still is.

As time went on, the tide of my results on transfers turned in my favor, and more and more economists found the results surprising but correct and useful. One of my colleagues apparently decided that if he could not beat them he would join them. A few years after my 1980 publication, after years of publicly and vigorously challenging my results on transfers, he changed his mind: he published in a main journal a paper that duplicates my results on transfers, and without reference to either my previous paper or to his earlier repudiation of it. It is still a mystery to me how this well known sequence of events culminated with the acceptance of his paper by this major journal.

My paper "Terms of Trade and Domestic Distribution: Export Led Growth With Abundant Labor", was written in 1978 while I was teaching at Harvard's Institute for International Development: it became eventually my North-South model, and perhaps one of the best pieces I have written. It also led to a bewildering sequence of events which nobody could have anticipated. As you can by now surmise, it was initially rejected by several journals, including the *Journal of Political Economy*, as variously uninteresting, already well-known and, simultaneously, wrong. It was eventually published by Professor Lance Taylor, then the editor, in the *Journal of Development Economics*, in 1981. It has a new model of international trade which I developed for the United Nations, close to that of Heckscher-Ohlin, but in which inter-alia, the endowments of capital and labor vary with prices.

The article establishes a negative correlation between the level of exports of a labor intensive good and its prices and export revenues (across equilibria). This occurs whenever the country has abundant labor and rather different technologies in the industrial and basic sectors ("dual technologies"). The results apply to stable markets. Abundance of labor means a rapid increase in the supply of labor as wages increase, a characterization which is closer to Sir Arthur Lewis's than it is to Heckscher-Ohlin's, but firmly wedged between the two. Both abundant labor and dual technologies are typical characterizations of developing countries. The article indicates that emphasizing export led growth based on the exports of labor intensive commodities (cash crops or raw materials) is not such a good idea for developing countries. This is an observation to which the experiences of Latin America and Africa over the past twenty years lend support. The successful experience of the newly industrializing countries in Asia, Hong Kong, Taiwan, Korea, Singapore and Thailand, all of which moved away swiftly from exporting labor intensive products and raw materials, and climbed the "factor intensities" ladder, also lends support to these results. But the paper was viewed by some as contradicting the Heckscher-Ohlin theory of comparative advantages--which it does not--and more importantly, as proposing policies different from what was then fashionable. Most of a 1984 issue of the *Journal of Development Economics* (vol. 15, Nos 1, 2, and 3) was dedicated to comments on this work, eleven papers in total. This was flattering attention for a piece that was initially considered uninteresting by the editors who rejected it. The literature in the *Journal of Development Economics* included among others, two papers, one by F. Lysy of the World Bank called "Graciela Chichilnisky's model of North-South Trade" (1985) and another by J. Gunning titled "Export-led Growth with Abundant Labor: A defense of Orthodoxy" (1984), a title which still puzzles me.

All this added fuel to my recent victory on the transfers debate. My Columbia colleagues Professors J. Bhagwati and R. Findlay then said that my results on export-led growth "contains mistakes that an undergraduate would not make", and they both published several papers and review articles condemning it as obviously wrong, and also dangerous to the orthodoxy of international trade. It did not make the atmosphere very comfortable at my home institution, particularly when I never found anything meriting comment on their works, so this became a very one-sided situation. As an aside, I should mention that Professor Findlay had been a strong supporter previously, having championed my promotion to tenure in 1979, a year after I joined Columbia.

Both Kenneth Arrow, a superb economist Nobel Laureate and a friend, and Amartya Sen, an excellent economist, published reviews of this article in a 1981 United Nations publication "Evaluation of a UNITAR Model on Technology Distribution and North-South Relations," in which they find my work right and "powerful," and give it approval and support as an "exemplary application of general equilibrium theory," comparing it with Ricardo and Keynes. The results suggest that countries should concentrate on strengthening their domestic markets and exporting skill-intensive goods rather than goods based on cheap labor. Because they were established in a conventional two-good, two country general equilibrium model of the world economy having a unique and stable equilibrium, the results had more weight than if they were offered in a model with market imperfections. More recently, Christopher Bliss published a glowing report (in the *Journal of International Comparative Economics*, 1993) of my North-South trade model in connection with *Oil in the International Economy* (Oxford, 1991, co-authored by Geoffrey Heal). His review, which is very detailed, states: "I can think of no book which I could better recommend to a young student possibly interested in economics. The combination of pure theory and empirical description, and their powerful integration, would serve as a model of what economics is like and what it can do. The throwing of light on the unexpected fact, or the interpretation of known facts in an entirely new light will challenge any reader."

After 1985, the *Journal of Development Economics* changed editors, and the new editor, I am told, rejected the publication of many other papers which developed further these ideas, on the grounds that "enough space has been dedicated in the *Journal of Development Economics* to Chichilnisky's work". The editor of the *Journal of International Economics* rejected several positive papers on my North-South model, and on one occasion accepted a paper on the condition that he himself should rewrite the introduction. This transformed the paper from a positive comment into a negative one. In effect, the journal's rejection policies shaped the practical fate of this work, which could have had major implications for development policies in Latin America and Africa, regions which have absorbed the conventional wisdom and devoted themselves, with painful results, to the continuing increase of exports of labor intensive products and raw materials.

But the story does not end there. Life twists and turns. Now, it turns out, the issue re-emerges in the context of the over-exploitation of environmental resources. It is all the same: we over-consume resources worldwide because developing countries export too much of them, at prices which are below social costs. We all agree now that environmental resources should be treated more conservatively in the world economy, the point that I was making in my 1981 article. Three recent articles of mine "North-South Trade and the Global Environment" *American Economic Review*, September 1994, "North-South trade and the Dynamics of Renewable Resources" in *Structural Change and Economic Dynamics*, December 1993, and "Global Environmental Risks" (with G. Heal) in the Fall 1993 issue of the *Journal of Economic Perspectives*, take this up, one of them, within my 1981 North-South model and another with a dynamic version of the same. The variable endowments are explained here by the dynamics of renewable resource. This shows that the comparative "abundance" of resources in developing countries is often not real, but simply a result of missing property rights. It develops a theory of trade where trade between countries is explained not by the relative abundance of factors, as in Heckscher-Ohlin's model, but rather by the differences in property rights for environmental resources across regions.

Just about the same time that my transfer paradox and my model of North-South trade were a subject of flattering attention, the editor of *Econometrica* rejected another article of mine on the grounds that it was not interesting, and that, in addition, it was mathematically incorrect. This was only one of several articles which the same editor rejected in *Econometrica*; all my other submissions were rejected too, and all roughly on the same grounds. This particular article, "Social Choice and the Topology of Spaces and Preferences", was subsequently published in 1980 in the journal *Advances in Mathematics*, edited by professor G. C. Rota of the Department of Mathematics of MIT, because of its novel and advanced mathematical results. This article redefined Arrow's axioms of social choice and provided a simple geometric interpretation of the impossibility of social choice. The problem was cast in terms of the topology of a space of foliations, a rather complex mathematical object, but in geometric terms it had an appealing interpretation in terms of the topology of spheres. It remains one of my best papers, and led to a large number of other results in game theory as well as in the theory of aggregation and of competitive markets. Despite its reliance on algebraic topology, which is not a familiar tool in the economist's kit, it led to a large number of articles in main economics journals focusing on these results and expanding them in several directions.

In 1981, Professor Jerry Green, then editor of the *Quarterly Journal of Economics* and currently a provost at Harvard University, and always a fair and sympathetic colleague, suggested I write a piece for that Journal. I did and it was accepted; it appeared under the title "Social Aggregation Rules and Continuity" in 1982. Since then the *Quarterly Journal of Economics* published several pieces by a number of other authors commenting and extending my results and a second article by me, "On Strategic Control" appeared in February 1993. This work has led to a new vision of social choice and game theory which is still developing as I write this letter. It has also led to new results on algebraic topology which have appeared and are appearing in pure mathematical journals.

Observing the ramifications of the results to these different areas, in 1983 Professor Frank Hahn, then of Cambridge University, an excellent economist, friend and co-author, solicited a book on the subject of "Games and Social Choices" for the Monographs Series of the Econometric Society which he edited at the time. In 1984, I produced a 200 page monograph adhering to his dictum that it should be "short and deep, like Professor Gerard Debreu's monograph on the Theory of Value." Gerard Debreu, another Nobel laureate, was my Ph.D. adviser in economics. When I submitted the completed manuscript, however, it was rejected by Frank Hahn acting together with the new editor, Jean Michel Grandmont, "for my own good" (sic), on the grounds that the book was too condensed to be clear, and furthermore, that it had trivial errors or typos in elementary parts. However, no list of these supposed elementary errors was provided, then or ever.

Indeed, there were no such mistakes. For many years I tried to obtain a list of these "small errors" from F. Hahn and from the new editor. Both appear to be somewhat embarrassed by their handling of my book, which was never printed, and have suggested that I should resubmit it. The current economics editor at the Cambridge University Press, Scott Parris, has been pressing on this point lately. They have a point, because I signed a contract which must be honored. In a candid moment last year when discussing this matter in front of Professor Kenneth Arrow, Professor Hahn explained that he had sent my manuscript to a mathematician at Cambridge University, Professor Cassels, a numbers theorist who did not understand immediately the mathematics as he thought he should, and was put off by this fact. For Professor Cassels, I was told, economics involves only trivial mathematics, so this could not be economics. I explained patiently to Frank Hahn that number theorists do not know and typically are not expected to understand topology, so it is not appropriate to use them as referees for a book using topology, and that, furthermore, my work in economics did not fit well with Professor Cassels' vision of trivial mathematics, a divergence which could be viewed as a compliment to my work.

More recently, in what could be called poetic injustice, I have been criticized for not having published a book on this subject so far, because "it would have been a service to the profession to introduce these new techniques which I developed", and as it was, "I was keeping this field all to myself." Well, I tried. My 200 page manuscript has been circulated to selected students and colleagues, and as soon as I get a bit of time, it will get published. It will be, I try to convince myself, a better book after all this time.

There are reasons for this hope. Last year on the basis of these results and several which followed it, published variously in the *Journal of Mathematical Economics*, the *Journal of Economic Theory*, the *Review of Economic Studies*, the *Quarterly Journal of Economics, Social Choice and Welfare*, in *Mathematics of Operations Research, American Economic Review* and in *Economic Letters*, I was able to prove the equivalence of the problems of social choice, the existence of a competitive equilibrium, and the existence of the core of an economy. It turns out that, in a well defined sense, the three are the same problem.

This is a rather surprising result which, as Paul Milgrom has indicated, builds on more than 10 years of work, advancing step by painful step in the understanding of these central issues of resource allocation: the structure of market equilibrium, the "core" of an economy, and the structure of social choices. The paper "Markets, Arbitrage and Social Choices" where the equivalence result between market equilibrium and social choices is obtained was rejected for publication but is still being considered for publication by Professor Andreu Mas Collel, in *Econometrica*, where it was submitted two and a half years ago. The reader may sympathize when I say that I would not want to taint the outcome of this submission. The paper was rejected already once by referees roughly on the grounds that it is not an interesting problem. I sent Mas Collel a revised version asking him to reconsider it and responding to all the points made by the referees two years ago, and I am still waiting with baited breath for an answer, or even an acknowledgment of the resubmission.

The story of this paper has an interesting, almost perverse, twist. The mathematics behind it are deep, and I wrote them in another paper "Intersecting Families of Sets": there is a theorem there that gives necessary and sufficient conditions for the non empty intersection of families of sets, convex or not, acyclic or not, connected or not. My theorem implies immediately Brouwer's fixed point theorem, Helly's theorem, the Knaster Kuratowski Marzukiewicz (KKM) Theorem, and Leray's classic theorem of the cohomology of acyclic nerves, but it is not implied by any of these results. It requires somewhat advanced algebraic topology to establish it, and other techniques in topology that are rather unusual and I have introduced myself. For this reason I thought the paper had to be published in a pure mathematics journal, and sent it to Professor Graham Segal, then at Oxford and now at Cambridge University, an excellent topologist, who liked the paper very much and accepted it subject to revisions in the British mathematical journal *Topology*. But when I sent in the suggested revisions the editor had changed, and the new editor rejected it. This was rather unusual, something I had never heard of before, because the paper had been previously accepted for publication. I found out after some inquiries that the new editor had decided to obtain a new opinion, in addition to those of the previous referees who were pure mathematicians and who thought very highly of the paper. This time from and economist--even though the paper is pure mathematics, and even though the journal is read only by mathematicians. This was apparently because my stationery indicates that I am at Columbia's Economics Department. I am told that the economist, who probably could not read the mathematics, "stated boldly that the paper was of no economic interest whatsoever."

But this story has a very happy ending. In March of 1993, after many years of trying to publish this piece, I sent "Intersecting Families of Sets" to Professor Richard Palais who is an editor of the most established, widely read journal of pure mathematics in the United States, the *Bulletin of the American Mathematical Society*. Professor Palais liked the paper very much and wanted to publish it immediately; it has appeared in the October 1993 issue of the *Bulletin*. In addition, a version of this paper with its economic implications has also appeared in the *American Economic Review*, May 1994, entitled: "Social Diversity, Arbitrage and Gains from Trade: A Unified Perspective on Resource Allocation." Several derived economic results will appear in *Economic Theory*, January 1995, under the title "Limited Arbitrage Is Necessary and Sufficient for the Existence of a Competitive Equilibrium,"[1] in an International Economic Association book on the theory of social choice, edited by K. Arrow, A. Sen and K. Suzumura, and in *Economic Letters*, under the title: "Limited Arbitrage Is Necessary and Sufficient for the Existence of a Competitive Equilibrium and the Core and It Limits Voting Cycles".

A recent rejection by the *Journal of Industrial Economics* summarizes the issue well. I submitted a paper on the economics of financial networks and the formation of coalitions of users to support the critical mass of users required for the network to have a viable economic existence, in other words, meeting fixed as well a variable costs. I know this problem well, because I dealt with it effectively in the business area, where I ran a company which produced financial services based on telecommunications networks technologies. It is essential to solve the critical mass problem for survival, and this in turn requires careful marketing targeted on the formation of coalitions of users. It is a problem of major practical as well as theoretical importance, and the paper and its results clearly generated a lot of interest in various seminars and presentations, and I am continually requested to send copies of the unpublished manuscript. The paper was rejected in 1992 by Professor Richard Gilbert, the editor of a special issue of the journal, "because of the extent to which it pushes forward the frontier of knowledge in networks. In addition, coalition theory is not a standard tool of analysis for most readership of the *Journal of Industrial Economics*" (sic).

I resubmitted the paper to the *Journal of Public Economics*, and in the letter of submission I alerted the editor, Professor Tony Atkinson that "because this work is innovative and uses unusual tools of analysis, I would like to request that you do not send this paper to be refereed by people who are standard references in networks; they are known to erect barriers to entry into what they consider their own field." Professor Atkinson took this seriously, but rejected the paper with a kind remark: that it did not deal with certain aspects which are of interest to his readers, aspects which, as a matter of fact, are well covered in the last part of the paper. His letter included an invitation to resubmit.

Another recent rejection of interest is by an editor of the *Journal of Political Economy*. This was a joint paper which coalesces the treatment of individual and collective risk in a general equilibrium framework by an appropriate use of mutual insurance contracts and Arrow securities. The editor's 1992 rejection letter could be considered a classic. He rejects the paper because even though it is interesting and correct, it does not relate to other papers published in the *Journal of Political Economy*. Under this editor's innovative criterion, by backwards induction, the *Journal of Political Economy* should not exist today. Upon reflection, perhaps his position is that it shouldn't.

I admit, however, that there has been a change in the tone of my rejections. My papers used to be rejected because they were obviously wrong , or obviously well known, or entirely devoid of interest. Or all the three simultaneously. Now a recent 1993 rejection by Professor Guy Laroque in *Econometrica* sets a new tone. My paper on financial innovation and endogenous uncertainty (with a co-author), is rejected because although it is original and the subject very interesting indeed,

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[1]As I proofread this letter I note that one of the referees for my paper, who lobbied hard and unsuccessfully for its rejection, has recently written, presented and submitted for publication two co-authored papers where he duplicates without attribution some of my results in the paper that he refereed.

and although no doubts are offered about its correctness, it is "very poorly written". As a matter of fact, I am invited to resubmit other interesting papers in this area. This is more soothing, and Professor Laroque is of course an excellent and cooperative colleague. But the final result is exactly the same: the paper does not see light of day, at least in any interesting time scale.

There is a certain amount of truth in a recent comment which appeared in the *American Economic Review* in 1992 signed by forty five economists, including several Nobel laureates, indicating that "Economists today enforce a monopoly of method or core assumptions, often defended on no better ground than that it constitutes the mainstream. Economists will advocate free competition but will not practice it in the marketplace of ideas."* Journals are used on occasion to implement and maintain intellectual monopolies through the definition and enforcement of mainstream economics. One wonders the extent to which this is natural or even unavoidable.

My general feeling is that a certain amount of monopolistic behavior will always be present, because intellectual production has information as an important component. Technologies where information plays an important role, as pointed out by Kenneth Arrow and others, typically lead to increasing returns to scale. To a certain extent, intellectual production has some of the characteristics of what we call in economics "natural monopolies." The same is true about reputation, which depends on communication as well as information, again an area where fixed costs and increasing returns are typically present.

Natural monopolies are naturally regulated. It suffices to reflect upon the insurance and telephone industries, as well as cable networks. Economists have a well developed theory for the regulation of natural monopolies. It is possible, therefore, that we may contemplate similar regulation for intellectual production. This may be the analytical content of the 1992 *American Economic Review* comment which I already quoted.

But it is possible that under these conditions much improvement could be achieved if we used simple codes of professional and ethical conduct. Why shouldn't codes of professional ethics emerge to do a job equivalent to the regulation of natural monopolies? Perhaps they should.

Business accepts that truth in advertising is beneficial to all. Physicians and lawyers accept this principle and self-regulate, more or less successfully. So do other scientists, to a certain extent. The American Mathematical Society has published a code of Professional Ethics this year.[2] The time may have come to give professional ethics in economics a serious thought. Perhaps your work in compiling the evidence on journal rejections will suggest the need for a better developed code of professional conduct and ethics in economics.

Have I learned from these experiences? Yes, a few lessons. One is that one should trust one's own judgment almost exclusively. This is rather difficult, because one can be wrong, but there are no real alternatives.

A second lesson is that it is often a mistake to be born a woman, and this should be avoided whenever possible. It has some advantages: sex and the wonderful joy of having children. But on the whole it is a mistake.

A third lesson is that the kindest and most intelligent people can make random and damaging errors. One cannot lose one's friends simply because they

*Ed.'s note.: *American Economic Review*, 82(2), May 1992, p. xxv.

[2]In the *Notices of the American Mathematical Society*, April 1994, Vol. 41, No. 4, pp. 288-289.

reject one's best articles or because they publish one's results as their own. Life could otherwise be very lonely.

A fourth lesson is to learn to live with and even love one's scars. Edith Piaf used to sing a song on this, called "Je ne regrette rien." Nice song.

A final lesson is that it is very difficult to be an editor and a referee. Having acted on the editorial or advisory boards of the *Review of Economic Studies*, *Advances in Applied Mathematics*, the *Journal of Development Economics*, *Social Choice and Welfare*, *Economic Letters*, the *Journal of International Trade and Economic Development*, the *Journal of International Comparative Economics* and *Metroeconomica*, and as a referee for just about all the journals which I have mentioned so far in this piece, I learned to sympathize with the other side. I have received very valuable comments from editors and referees which improved my work, and I try my very best to do the same unto others. It certainly is difficult not to find more interesting an article which builds on one's own work, and it is very difficult to understand new ideas, no matter how beautifully written. The job is hard, and it takes much of one's scarce time.

While rereading this letter I am struck by the remaining puzzle: why do we go on with the indignities of this process? Why do we continue to do new and original research which is difficult to publish? The result may be beautiful but the process is painful, like dancing on a bed of knives.

At a certain point one feels one would become more acceptable socially if one stopped producing new, and perhaps unsettling, ideas. Rewriting or extending the best work of others, or one's best pieces, in more accessible or polished ways could be easier, more rewarding, and more acceptable. Some of the most intelligent people I know have succumbed to the discreet charms of the intellectual bourgeoisie. One is almost encouraged to take a senior, more genteel, role; to cease to rattle one's brains and struggle against the unknown. Then: why?

I have no answer to this. Perhaps one should reflect upon the mythical answer given by dancers, who lead notoriously difficult lives, to this question: "Got to dance."

Best Regards,
Graciela Chichilnisky