

# **Roads to Wisdom**

**Where Do Path-breaking Ideas in Economics Come from?**

**Some Inspirations from Nobel Laureates\***

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This paper has been prepared for the 2009 regional meeting of the Mont Pèlerin Society in Stockholm, Sweden. It draws on my new book (HORN 2009), which contains interviews with Paul Samuelson (who received the Nobel Prize in 1970 “for the scientific work through which he has developed static and dynamic economic theory and actively contributed to raising the level of analysis in economic science”), Kenneth Arrow (1972, “for pioneering contributions to general economic equilibrium theory and welfare theory”), James M. Buchanan, Robert Solow (1987, “for his contributions to the theory of economic growth”), Gary Becker, Douglass North (1993, “for having renewed research in economic history by applying economic theory and quantitative methods in order to explain economic and institutional change”), Reinhard Selten (1994, “for pioneering analysis of equilibria in the theory of non-cooperative games”), George Akerlof (2001, “for the analysis of markets with asymmetric information”), Vernon Smith, and Edmund Phelps (2006, “for his analysis of intertemporal tradeoffs in macroeconomic policy”). Beyond the interviews, the book contains critical introductions into the works of each of the ten Nobel Laureates, and a comprehensive analysis of the insights gained in the interviews, the gist of which I have the honor to present here. I owe special thanks for useful comments and support to Dr. Michael Wohlgemuth, Walter Eucken Institut, Freiburg, Germany, and to Dr. Adriana Neligan, Institut der deutschen Wirtschaft Köln, Berlin, Germany.

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## Introduction

The Mont Pèlerin Society (MPS) is particularly rich in “Nobelity”. Founded in 1947 upon the initiative of the Austrian economist and social philosopher Friedrich August von Hayek, this international network of classical liberal thinkers has enjoyed the privilege of counting eight Nobel Prize winners among its members:

- Hayek himself, who was awarded the Nobel Prize in 1974 together with for “pioneering work in the theory of money and economic fluctuations and for their penetrating analysis of the interdependence of economic, social and institutional phenomena”
- Milton Friedman (1976), “for his achievements in the fields of consumption analysis, monetary history and theory and for his demonstration of the complexity of stabilization policy”;
- George Stigler (1982), “for his seminal studies of industrial structures, functioning of markets and causes and effects of public regulation”;
- James M. Buchanan (1986), “for his development of the contractual and constitutional bases for the theory of economic and political decision-making”;
- Maurice Allais (1988), “for his pioneering contributions to the theory of markets and efficient utilization of resources”;
- Ronald Coase (1991), “for his discovery and clarification of the significance of transaction costs and property rights for the institutional structure and functioning of the economy”;
- Gary Becker (1992, “for having extended the domain of microeconomic analysis to a wide range of human behavior and interaction, including nonmarket behavior”;
- Vernon Smith (2002, “for having established laboratory experiments as a tool in empirical economic analysis, especially in the study of alternative market mechanisms”)<sup>1</sup>.

The MPS has benefited enormously from the impulses of these outstanding scholars who are united not only in their academic excellence, but also by their general commitment to liberal values. But what is it that allows these excellent scholars – such as “our” Nobel Laureates and others, with “Nobelity” simply chosen as a proxy for academic excellence – to discover their promising lines of thought and research? What is it that inspires and triggers their path-breaking ideas? What are those “Roads to Wisdom”?

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<sup>1</sup> For all citations, see [www.nobel.se](http://www.nobel.se).

The notion of “wisdom” may perhaps, at least at first sight, seem a little exaggerated in this context. The reason why I do find it rather appropriate, beyond the welcome pun in homage to Friedrich August von Hayek and his famous book “Roads to Serfdom” (HAYEK 1944), is that it actually means more than academic achievement, and even more than knowledge: it is a state of mind. It means love of learning, and it encompasses the judicious application of this knowledge in order to solve problems. As Confucius said, wisdom comes from reflection, imitation and experience. This process of reflection, imitation and experience being gathered is precisely what we will be concerned with here.

The first part of this paper will describe the micro and macro aspects of the “discovery procedure” framework in which the analysis will be couched. The second part is fun reading, it will present the micro observations that I have been able to make in the series of interviews conducted for my book, with respect to the triggers of academic excellence. In order to pass on to the macro level, the third, certainly more intricate part will then attempt to reach a working definition of progress in economic science. Finally, the fourth part will address the tricky question whether the economic mainstream can likely go astray, i.e. whether, within a science-as-market analogy, “market failure” is at all conceivable in the market of economic ideas. And finally, a conclusion will... conclude.

## **I. The academic discovery procedure**

As a rule, voluntary interaction in the market place is not only preferable because it is free of coercion, but also because it is dynamically productive: it allows people to use knowledge in the most productive, creative, and innovative way. There are several essays by Hayek that, especially when taken together, make this point: “Economics and Knowledge” (1937), “The Use of Knowledge in Society” (1945), and “Competition as a Discovery Procedure” (1968). In his 1945 paper, Hayek describes that “the economic problem of society is thus not merely a problem of how to allocate given resources – if given is taken to mean given to a single mind which deliberately solves the problem set by these data. It is rather a problem of how to secure the best use of resources known to any of the members of society, for ends whose relative importance only these individuals know. Or, to put it briefly, it is a problem of the utilization of knowledge which is not given to anyone in its totality” (HAYEK, 1945/80, 77/78). Given the “unavoidable imperfection of man’s knowledge”, there is a need for “a process by which knowledge is constantly communicated and acquired.” (HAYEK, 1945/80, 91) The verb still missing in this passage is “generated” – but this indeed follows from his 1968 paper where he presents competition as “a procedure for the discovery of such facts as, without resort to it, would not be known to anyone” (HAYEK, 1968/78/82, 179) and the results

of which are “unpredictable and on the whole different from those which anyone has, or could have, deliberately aimed at” (HAYEK, 1968/78/82, 180).

In these three important papers, Hayek principally refers to knowledge not as the sum, or aggregate stock, of knowledge, but to the more humble, individual “knowledge of the particular circumstances of time and place” (HAYEK 1945/80, 81), i.e. to the special or local knowledge that is necessarily distributed unevenly throughout society. He argues that decentralization is needed to make the best use of this dispersed local knowledge socially. In a competitive market, this task is being performed by the price system, which works as a mechanism for communicating information. This mechanism has been brought about through cultural evolution. “The price system is just one of those formations which man has learned to use ... after he had stumbled on it without understanding it. Through it not only a division of labour but also a coordinated utilization of resources based on an equally divided knowledge has become possible” (HAYEK 1945/80, 86).

Hayek’s discovery was the spontaneous process of anonymous social self-coordination through markets – and these are indeed absolute “marvels”, as Hayek puts it (HAYEK 1945/80, 86). While he concerns himself with the provision of goods and services, we can also apply his perspective to the division of knowledge in the process of social self-coordination with respect to the production of academic knowledge itself. This implies that we view academia as a marketplace of sorts. In such a framework, it is easily possible to switch from the *macro* level to the *micro* level. We can picture the individual scholar as a producer/investor who offers his theories on the marketplace of ideas. At the same time, he or she is a customer/consumer who is in demand of other people’s scientific output as well.

Hayek, in his endeavor to explain social processes, didn’t have to bother himself with the particular characteristics of the individual, dispersed bits and pieces of “local knowledge” that are being accumulated, spread, and expanded through the process of social interaction that he described. They are the source of everything, but it is not necessary to be aware of their detailed characteristics. In our present context, however, it *is* indeed interesting to find out what the particular individual ingredients are that have made scholars productive in the great social self-coordination process of science. The “*micro*” focus here is on why the producer produces what he produces, and why the customer seeks what he seeks. This is an interesting question because not only the spontaneous process of social self-coordination must fill us with awe. We may marvel just as much at the courses of individual lives, at the amazing coincidences of the *right* circumstances of *right* time and *right* place. Some of this may be due to sheer luck. But luck cannot be everything.

## II. Triggering some path-breaking ideas

A look at the specific itineraries may help us understand more generally what the deeper prerequisites for scientific originality and overall academic excellence may be, where it actually is that the unquenchable thirst for scientific inquiry originates, and where path-breaking ideas and innovation thus ultimately tend to come from. Here are my rather anecdotal observations, cast into a set of categories, all drawn from the interviews in my book. Without any specific order, these categories refer, amongst others, to essential character traits, inherited or acquired; to origins and backgrounds; to external influences; to historical events and the ensuing intellectual as well as political challenges; to crucial personal choices – and to the role of luck.

### *Missionary or technician*

Some Laureates have come to economics because they were immediately attracted by its questions – questions they felt an imperative urge to answer in order to improve the world. Those are the missionaries. People with this character trait usually have, as Joseph Schumpeter says, a “vision” (SCHUMPETER 1954, 41). Douglass C. North says that he is “still trying to save the world” (in HORN 2009, 167). Gary Becker talks about his youth referring to the emerging “desire to do something for society” (in HORN 2009, 137). James M. Buchanan, too, says elsewhere that “we have a moral obligation to think that we can constructively design and implement reform in social arrangements” (SZENBERG 1992, 100).

On the other hand, there are some born technicians, who easily possess themselves of the proper (mathematical) “technique”, as Schumpeter would say (SCHUMPETER 1954, 7), and who take a while to discover their more profound interest in “people and policies”: those Laureates have come to economics via their mathematical talent. To some extent, this is the case of Vernon Smith. With his mathematical talent, he started out studying physics and electrical engineering at Cal Tech, where he was confronted with the social sciences more or less by accident. It proved crucial for him to stumble over Paul Samuelson’s new textbook (SAMUELSON 1948), a textbook that was revolutionary in presenting the subject matter in mathematical terms: “Samuelson’s book indicated that economics was just physics” (in HORN 2009, 229). Ever since, he found that economics is *not* like physics, but the newly discovered field stuck nevertheless.

### *Chance and other temptations*

There may be a humility bias in how much one ascribes to chance, or luck, looking back onto one’s life – but it is probably simply true that the dice are not yet fully thrown at age 16

or 17 in most cases. Decisions made at such young age are systematically decisions under a lot of uncertainty, encompassing even uncertainty about one's own preferences which one is still to discover. Thus it must be taken seriously when most people claim that they have more or less stumbled into economics. Even for most "missionaries", economics wasn't a fully unequivocal choice before they saw it in class. In Vernon Smith's case, for example, it was chance exposure to economics at Cal Tech that proved decisive. Paul Samuelson says that he "came to Chicago only because of location" and that it "was an accident that I liked the subject. I had not even thought about economics in my high school years" (in HORN 2009, 46). Kenneth J. Arrow was lured by a scholarship. James M. Buchanan also embarked on economics only due to a scholarship that was available; Gary Becker was forced to take an economics class at high school, found the class terrible but the subject interesting – but then lost interest again. It was sheer luck to have him back "on track".

### *Bourgeoisie or not to be*

This brings me to the influence of family backgrounds. Edmund Phelps certainly hit the nail on the head when he once remarked in conversation that it is not straightforward – not at all impossible, but more difficult – to become an economist if one doesn't grow up in a somewhat bourgeois setting. Absent major crises that affect virtually everybody, economic issues do tend to subside into the fuzzy background of social life unless one lives in a context that brings with it some regular intellectual exposure to questions of this kind. Phelps grew up in precisely what he calls a "somewhat bourgeois setting", both parents being oriented towards business and coming from relatively wealthy backgrounds. Gary Becker's case is quite similar, his father having been a pretty well-to-do businessman. The same is true for Douglass North, whose father was a successful insurance manager, and even for Paul Samuelson, whose father had his own drugstore and whose family had accumulated a relative degree of affluence – but had to watch it dissipate when the Florida land bubble burst in the Twenties.

The situation was however altogether different for Vernon Smith. Neither of his parents had an education beyond 8<sup>th</sup> grade. His father provided work ethic and can-do knowledge, his mother was active in political and social affairs in the community. Financial problems forced the family to practice autarky living on a farm for a couple of years. The situation of James M. Buchanan was quite similar. His family lived on a farm throughout his youth; his father had gone through two years of University training ("and played football", in HORN 2009, 91). His mother, however, had been a school teacher. She was endowed with an exemplary work ethic and a voracious intellectual appetite. Both character traits, as it seems, have left an important and lasting impact on the son.

## *Family discussions*

Even without much schooling, parents can provide their children with intellectual appetite and with a motivation for achievement. Discussions at the dinner table, or other regular family gatherings, are extremely important – and it doesn't matter so much how high-powered the arguments are. What is crucial is that the awareness is raised – awareness about the importance of certain topics relating to economics and economic policy, to anything that touches social questions, and of course the appetite to learn more about them. This is an experience that most Laureates share.<sup>2</sup> Vernon L. Smith was fascinated to find out at College that the topics that had been debated at the dinner table were actually “things you could study, that it needn't be only a matter of opinion. You could actually base your opinions on analysis, on investigation, on some kind of understanding about how society and how the economy work” (in HORN 2009, 227). In the Buchanan household, discussions were more about politics – populist politics. Gary Becker reports that he had many discussions at home, especially with his sister – and some arguments with his father.

## *Worldviews*

Given worldviews also play a non-negligible role in instigating research. As Joseph Schumpeter wrote, worldviews, or ideologies, already enter the “pre-analytic cognitive act” or “vision” that “supplies the raw material for the analytic effort” (SCHUMPETER 1954, 42/43).<sup>3</sup> This latter analytic effort really begins “when we have conceived our vision of the set of phenomena that caught our interest, no matter whether this set lies in virgin soil or in land that had been cultivated before. The first task is to verbalize the vision or to conceptualize it in such a way that its elements take their places, with names attached to them that facilitate recognition and manipulation, in a more or less orderly schema or picture... Factual work and ‘theoretical’ work, in an endless relation of give and take, naturally testing one another and setting new tasks for each other, will eventually produce scientific models, the provisional joint products of their interaction with the surviving elements of the original vision, to which increasingly more rigorous standards of consistency and adequacy will be applied...” (SCHUMPETER 1954, 42). But the very first step is entirely colored by a worldview: “Analytic work begins with material provided by our vision of things, and this vision is ideological almost by definition. It embodies the picture of things as we see them, and wherever there is any possible motive for wishing to see them in a given rather than another light, the way in

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<sup>2</sup> This also explains why they did not necessarily start out with practical, applied problems. “The best work of Samuelson, Stigler, Solow, Nash, or Debreu was inspired by a desire to improve the way we think the world works.” DALEN 1997, 13.

<sup>3</sup> Schumpeter actually deplores the influence of what he also calls “ideology”. He fears that, “the more honest or naïve our vision is, the more dangerous is it to the eventual emergence of anything for which general validity can be claimed.” In SCHUMPETER 1954, 42/43.

which we see things can hardly be distinguished from the way in which we wish to see them.” (SCHUMPETER 1954, 42).

To some extent, a person’s worldview is usually shaped at home, in the family, actively and passively, perhaps also during the already mentioned dinner table conversations. This “initial endowment” may however fade away as new influences come in later in life. Interestingly, in the case of the ten Nobel Laureates that I have interviewed for my book, the initial ideological endowments were mostly socialist. There is a saying according to which he or she who, as a youngster, is not socialist, has no heart – and he or she who still remains a socialist in later years has no brain. There might be something to it... the evidence is quite striking. Vernon Smith comes from a socialist background; Gary Becker was a socialist, just like his father who, “although he was a pretty successful businessman, strongly supported interventionist-type candidates” (in HORN 2009, 146). Douglass North was an outright Marxist; Kenneth Arrow regarded himself as a socialist (“not a communist”). James M. Buchanan doesn’t make such a fine distinction. He came from a populist background, as already mentioned, but when he got interested in economics, his peer group lured him over to socialism. “I would have signed up immediately to the Communist party had a recruiter come along”, he says (in HORN 2009, 95).

As predicted by the popular saying, those initial ideological endowments indeed didn’t last in most cases. In James M. Buchanan’s case, it was studying economics at Chicago that turned him around. “And in a hurry. We were about thirty people in Knight’s course in price theory. Fifteen switched over completely within 6 to 8 weeks, and fifteen stayed exactly the way they were“ (in HORN 2009, 97). The same happened to Gary Becker who remembers that two things pulled him away from socialism, and with force: Milton Friedman and economics. “Studying economics, at the end of my sophomore year at Princeton, I remember I debated somebody about markets versus socialism. And I was on the market side. I had already shifted away from socialism. Entering Princeton, I was a socialist. Two years or so later, I was no longer a socialist. Three years later, I decided to go to Chicago. I still had an uneasy feeling. Even though I had the basic principles why I should lean in that direction, I still was missing the theory. I then got that at Chicago” (in HORN 2009, 147). When Douglass C. North got his first job at Seattle, he re-learned theory – and, as he says, this “was the last step in my getting rid of Marxism. As I re-learned theory, I became a very rigid neoclassical, Chicago type economist“ (in HORN 2009, 164).

### *Teachers make a difference*

As the Nobel Laureates went ahead with their schooling and academic training, they came across teachers and mentors who, in most cases, played a decisive role – in the sense

that these mentors managed to entice their intellectual appetite even more, opened up interesting new fields for them, gave them good advice, and urged them ultimately to stay in academia. The role of teachers is as important as it is psychologically interesting. Teachers provide the intellectual socialization, as Harriet Zuckerman points out: “the elite masters shape their apprentices and prepare them for elite status by inculcating and reinforcing in them not only cognitive substance and skills but the values, norms, self-images, and expectations that they take to be appropriate for this stratum in science.” (ZUCKERMAN 1977, 129). I am not able to go into much detail here, but I cannot resist the temptation to refer the reader to the works of the French philosopher René Girard in this context.<sup>4</sup> According to Girard’s theory of “mimetic desire”, one borrows one’s desires from others. The desire for a certain object – or field of research, in the present context – is always provoked by the desire that another person has for this same object. Girard explains that in most cases, people desire something or engage in something because they think that this brings them closer to the person(s) from which they have inherited this desire. In some way, it is in fact the model which is being desired and sought.<sup>5</sup> Girard has demonstrated this – very plausible – theory by looking at the psychological patterns that are being used in novels, ranging from the writings of Marcel Proust to Fyodor Dostoyevsky. The pattern of “mimetic desire” may also be at work in academic economics. Admiration and the ensuing imitation can be a strong incentive – and it usually pays: “apprentices of elite scientist are less likely to be lost in the crowd” (ZUCKERMAN 1977, 135).

As a matter of fact, the impact and the effect of admiration for outstanding academic teachers are indeed clear from Gary Becker’s experience with Milton Friedman at Chicago, for example. “He was by far the greatest living teacher I have ever had,” Becker says (in HORN 2009, 141). At college in Princeton, Jacob Viner had been decisive in orienting Becker towards Chicago. But Becker defended his intellectual autonomy some years later, deciding to move on to Columbia University. James M. Buchanan was in Frank Knight’s wake, the works of Knut Wicksell came to widen his scope. In Vernon Smith’s case, Wassily Leontief and Gottfried Haberler left their mark, and especially Edward Chamberlin.

### *History’s heavy footprint*

Great events leave great marks, on people and on theory. People get motivated by catastrophes and cataclysms to try and explain them, and theory changes correspondingly. All economic theory is thus historically bound, both in terms of the real-life events behind it and in terms of the trajectory of theory itself, as Joseph Schumpeter points out (SCHUMPETER 1954, 4). Looking back at the past century, it must be stated that there are only a couple of

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<sup>4</sup> I am indebted to Antoine Martin for drawing my attention to this.

<sup>5</sup> “L’élán vers l’objet est au fond élán vers le médiateur... l’hostilité apparente du médiateur, loin d’amoinrir le prestige de ce dernier, ne peut guère que l’accroître.” GIRARD 1961, 24.

events from an economic point of view that qualify as having had this kind of importance. The first, and foremost, was the Great Depression. Paul Samuelson doesn't hesitate (in 2007) when asked what has been the most serious economic catastrophe so far: the Great Depression. Phelps, Solow, Arrow and Smith join in. The Great Depression, triggered essentially by bad monetary policy, was a tragedy in many respects. It brought misery to the world. It began an era of anticapitalism, and it started the – soon to be excessive – rise of the welfare state with Franklin D. Roosevelt's legislations and New Deal programs that followed (SHLAES 2007). As Milton Friedman stresses elsewhere, instead of blaming the Great Depression on business and on capitalism, "the lesson that should have been learned was that government let them down. That it was mismanagement of the monetary system that produced it and not a failure of the market system" (in PARKER 2002, 50/51). Unfortunately, the same mistaken lesson seems to have been "learned" again in 2008, after the burst of the real estate bubble in the US and the following break-down of the American banking system. For the future, it will be interesting to observe whether the global financial meltdown will again leave its mark in the research programs of individual scholars – and whether it will again give rise to a major paradigm shift, as it did in the Thirties and Forties with the advent of Keynesian economics.

### *Lone wolves or tribal animals*

What is the most promising strategy in academia? Can you survive as a lone wolf – or do you have to be a group animal faithful to the tribe? How much cooperation and interaction is needed in order to be successful? My interviews show that "anything goes", as the famous saying by Paul Feyerabend has it (FEYERABEND 1970); all characters find their ways. Paul Samuelson, for example, certainly isn't a lone wolf. He has co-authored a lot, and much of his work – if not most of it – has been triggered by other people's papers. James M. Buchanan co-authored a lot, and derived from it as much inspiration as despair, as it seems. Vernon L. Smith clearly isn't a lone wolf, he has always worked out his experiments within groups. Reinhard Selten, however, the one German Nobel Laureate, not member of the MPS, denies that kind of disposition. "I'm not so dependent on external inspiration", he says (in HORN 2009, 194). And Douglass C. North is "sure I have learnt a lot from other people... But in terms of where my ideas come from, I have been very much a loner" (in HORN 2009, 166). This ties in with a study by Hendrik P. van Dalen that shows that "most path-breaking publications are written alone, contrary to the trend in academia where co-authored papers have become the rule" (DALEN 1997, 14). Given this currently quite predominant trend, those numbers may change with time. Co-authoring allows for intellectual exchange, and it supposedly even saves time (there are exceptions which perhaps confirm the rule), and it will thus continue to spread.

### *Puzzle solvers or system builders*

Robert Solow distinguishes between “puzzle solvers” – a notion which he borrowed from Thomas S. Kuhn (KUHN 1962/70, 35-42) – and “system builders”. Puzzle solvers go stepwise, they are humble, they are pragmatic, occasionally pedantic, they are somewhat eclectic and not overzealous. “You find something strange and not quite easy to understand, and then you try to get closer to understanding it” (in HORN 2009, 121). This “piecemeal” approach – to borrow another famous notion, this time coined by Karl Popper (POPPER 1966, 162/3) – is descriptive of researchers doing “normal science”, in Kuhnian terms. We may perhaps view these people as arbitrageurs in Israel Kirzner’s sense, meaning an alert (intellectual) entrepreneur<sup>6</sup>, providing better solutions for given problems, i.e. finding and filling the gaps in the existing body of theory, in order to enhance and strengthen the given paradigm. Obviously, Solow classifies himself amongst those “puzzle solvers”, just like Gary Becker, and perhaps Vernon Smith also would.

James M. Buchanan and Douglass C. North are harder to classify in this respect. In their personal research itineraries, they have also been puzzle solvers initially. As new questions came up, they tackled them, one after the other – but their effective thrust resulted in creating a new, distinctive paradigm of their own. In Kuhnian terms, they experienced a “scientific crisis”, given that the existing paradigm in economic theory couldn’t solve their foremost problems, and so they brought about a paradigm shift. However, the shift was not complete in the sense that the old paradigm – standard neoclassical economics – wasn’t fully supplanted. In modern theory, a variety of approaches seem to coexist. Maybe the time for full-fledged “scientific revolutions” is over in the field of economics. At any rate, people in this category can be viewed as rebellious “innovators” in the sense of the Schumpeterian entrepreneur, providing new solutions for new problems, i.e. creating new paradigms and provoking a potentially substantial degree of “creative destruction” as they move ahead (SCHUMPETER 1934). James M. Buchanan, for example, started out analyzing Public Debt, fully within the existing paradigm of economic theory, and then moved on toward Public Choice and Constitutional Economics. Douglass C. North began measuring history, but ultimately turned to the New Institutional Economics. Intentionally or not, these Laureates have built their systems and whole new paradigms.

### *The unlikely sources of inspiration*

Here again, Paul Feyerabend’s dictum applies: “anything goes”. Crucial ideas are a gift; they tend to come from the most unlikely inspirations, at the most unlikely moments, under the most unlikely circumstances. James M. Buchanan traces back his approach mainly

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<sup>6</sup> See KIRZNER 1973, 48, and also KIRZNER 1979, 130-1, 152.

to an old, dusty book he stumbled on by pure chance in the Chicago Library: Wicksell's "Finanztheoretische Untersuchungen" (Wicksell 1896). Vernon Smith owes his inspiration to a course taught by Edward Chamberlin, but the ensuing illuminating idea came later – without any announcement: "In the middle of the night, in 1956 at Purdue, I wake up and I have this idea" (in HORN 2009, 231). Gary Becker remembers puzzling about the "economics of marriage" while sitting in a hotel room all by himself. Douglass North tells the story how some serious criticism by a student prompted him to stay up all night drinking brandy and reflecting on the topic. Kenneth Arrow remembers how painfully long it took him to jump off the starting block, and in the end, his dissertation came "just like that". And Paul Samuelson acknowledges that many of his topics came up because somebody else had written something that he felt an irresistible urge to straighten out.

### *What fish in what pond*

The area of research also matters. Its choice is to a large extent predetermined by individual character. From the point of view of clever self-marketing, it is of course more promising to place oneself in a new field, or even to create one from scratch: Who would not prefer to be a big fish in a small pond rather than to be a small fish in a big pond? In order to prevail in academia, one should try to come up with a truly pioneering idea, a fruitful approach that one can expand. Breaking free from the mainstream is a risky investment; becoming the mainstream is a big reward.

To differing degrees, most Nobel Laureates interviewed in my book have been rebels against the established mainstream at some point, and this may bear some causal connection with their dissenting personal character – dissenting but not necessarily combative. Gary Becker rose against the narrow scope of economics; Vernon Smith against the black holes in general equilibrium theory; James M. Buchanan against the missing regard for the public sector and collective decision-making. In Reinhard Selten's case as well, his rebellious character predisposed him for swimming against and beyond the mainstream. As he admits, "more than anything else, the effect was that I have always mistrusted majority opinion. I have always had to think independently. ... I've never been swimming with the crowd" (in HORN 2009, 185).

### **III. Progress in Economic Science**

As my ultimate aim is to try and understand how progress in economic theory comes about, let us now turn our backs to these more anecdotal insights, shifting from the *micro* perspective back to the *macro* perspective, from the individual researchers and their

motivations to science as a social phenomenon. Preliminarily, it may be helpful to check how the Nobel Laureates themselves think of “progress”. Clearly, asking this question is to prepare the ground for an “is-ought problem” of sorts: looking back, one is easily tempted to conclude that the path behind has necessarily been a path of scientific progress. After all, one has learnt something, i.e. one has added something to one’s stock of knowledge, hasn’t one? So there “must” have been progress. Of course progress doesn’t mean that everybody thinks alike. Progress doesn’t imply scientific consensus.

As Paul Samuelson says elsewhere, what has changed in economics over the last century is that economists are now able to delineate quickly the contours of their disagreements (in SZENBERG 1992, 237). Anyway, the temptation toward overconfidence is as pervasive as it is understandable: otherwise, one would all too easily admit that one has wasted one’s time. But the problem with the notion of “progress” and its underlying strong positive connotation is that everybody just moves ahead. One actually cannot even help moving ahead. Moving ahead, however, may lead into good or bad directions. Therefore, James Buchanan has good reason to be extremely humble: “I don’t know what progress really means. .... There are whole realms of discourse out there that we cannot reach, by definition. There are always going to be limits beyond which we cannot go. Knowing that they are there, you can always hope to move a little closer – but that’s all” (in HORN 2009, 107).

Progress, in George Akerlof’s procedural view, means that “people will spring loose from the cognitive tradition, that they will try something different...” For him, this means specifically that they try “mathematical economics, for example. As a result of this pattern of progress, we can now consider phenomena in ways in which we couldn’t consider them before. ... In order to get new solutions, you have to free yourself from the older, from all previous non-mathematical tradition” (in HORN 2009, 217). And Robert M. Solow indeed sees such progress in economic theory. “What matters is whether errors get corrected”, he states and pragmatically defines progress as an accumulation of solutions to given problems: “You find something strange and not quite easy to understand, and then you try to get closer to understanding it. The accumulation of the solutions that you find is progress. Sometimes the right answer to a puzzle changes, because circumstances change, or institutions change, or attitudes change. Very likely, we will never run out of puzzles to solve...” But that, for him, doesn’t imply a random walk. “There tends to be a direction. The direction comes both internally and externally”, i.e. from real world problems and from the literature. The result, nowadays, is “not that there are fewer questions, but there are now more difficult, more refined questions” (all in HORN 2009, 120). We are still confused, but at a higher level. Gary Becker also sees “a lot of progress in economic theory ... for example human capital theory, family economics, Public Choice theory, and informational economics. I could go on quoting things that have constituted progress in economic theory. And I think that will continue” (in

HORN 2009, 149). Akerlof, in 2007, may however be a bit overoptimistic when he states that “in the last century, we made one major bit of progress. We explained how, if you get into a depression, you get out of it...Today, we would know what to do” (in HORN 2009, 215). Viewed from the position the world is in right now, this may seem questionable, to say the least.

To recognize progress after the fact is necessarily a biased endeavor. The problem is that – using the optimistic saying “All roads lead to Rome” – one doesn’t know exactly where Rome is.<sup>7</sup> Worse, one doesn’t even know *what* Rome is. If lucky, Rome will be the yet unknown end of one’s interactive journey of life. Does that which will be discovered in the academic discovery procedure automatically deserve the label “progress”? This is implausible. It therefore seems wiser to use Joseph Schumpeter’s value-free term, which is the “filiation of economic ideas”. In his gigantic “History of Economic Analysis”, Schumpeter writes that “scientific analysis is not simply a logically consistent process that starts with some primitive notions and then adds to the stock in a straight-line fashion. It is not simply progressive discovery of an objective reality – as is, for example, discovery in the basin of the Congo. Rather it is an incessant struggle with creations of our own and our predecessors’ minds and it ‘progresses,’ if at all, in a criss-cross fashion, not as logic, but as the impact of new ideas or observations or needs, and also as the bents and temperaments of new men, dictate” (SCHUMPETER 1954, 4). The “filiation of scientific ideas”, then, is a “process by which men’s efforts to understand economic phenomena produce, improve, and pull down analytic structures in an unending sequence” (SCHUMPETER 1954, 6). According to Thomas Kuhn, again, this can happen in two familiar ways: through normal “puzzle-solving” (KUHN 1962/70, 35-42), whereby little bits of knowledge are added to an already existing stock, or through scientific revolutions, which implies that the existing stock of knowledge may be overthrown as one paradigm is replaced by another (KUHN 1962/70, 92). This may however entail “Kuhn losses”, i.e. some phenomena that used to be successfully explained by the old paradigm may become an open question again with the new one (KUHN 1962/70, 99/100).

For the time being, let me pragmatically define progress in economic science as some forward move that indeed allows a better understanding of the world, as compared to a most recent status quo. But again, what is this progress about? What is it really that allows one to better understand the world? How much can we know? Is this all just about a worldview, a paradigm, or is this a factor so concrete as to envisage precise predictions? This amounts to the question whether or not one now adheres, or should adhere to a positivist point of view. As Bruce Caldwell writes in his brilliant Hayek biography, if progress in economics means that one understands more and more economic laws and law-like relations, in a positivistic

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<sup>7</sup> Talking about progress in economic science in an opinion piece for Frankfurter Allgemeine Zeitung, in 2006, I once quipped in the headline: “where is up?” This is exactly what the non-directedness of progress is all about. HORN 2006.

sense, then “improvements in empirical methods will allow ever more precise predictions to be made; theory change will involve the steady accumulation of a well-corroborated theory base; errant theories will be gradually but steadily falsified and eliminated; and as the findings of economic science become more widely accepted methodological debate across competing paradigms will wither away, as will, indeed the competing paradigms themselves” (Caldwell 2004, 373). None of these things have come about. As Hayek put it drastically in his Nobel Memorial Lecture, “as a profession we have made a mess of things”( HAYEK 1975/78/89, 3-7). This is the least we can say right now, in the midst of the global recession triggered by the financial melt-down of 2008.

Hayek had an ongoing controversy with the economic mainstream. His essential criticism is one that is as valid today as it was then. He accuses the economics profession of *scientism*, i.e. of a “propensity to imitate as closely as possible the procedures of the brilliantly successful physical sciences – an attempt which in our field may lead to outright error ... since it involves a mechanical and uncritical application of habits of thought to fields different from those in which they have been formed” (HAYEK 1952/79, p. 24 and HAYEK 1975/78/89, 3). One problem that has arisen with Keynesian economics is the focus on quantitative methods, in an attempt to resemble the exact physical sciences as much as possible. All modeling nowadays is done in such a way that a problem can be treated mathematically and tested empirically. Hayek warns about the ensuing incentive effects on economist, which do not at all warrant progress: “The correlation between aggregate demand and total employment, for instance, may only be approximate, but as it is the *only* one on which we have quantitative data, it is accepted as the only causal connection that counts. On this standard there may thus well exist better ‘scientific‘ evidence for a false theory, which will be accepted because it is more ‘scientific’, than for a valid explanation, which is rejected because there is no sufficient quantitative evidence for it” (HAYEK 1975/78/89, 3).

And beyond that, the mere approach of the economic mainstream, which tries to be as precise a science as physics, is misleading. The problem of the social sciences differs crucially from the physical sciences. In the social sciences, there is no clear, non-arbitrary distinction between what is exogenous and what is endogenous. The question in the social sciences is “not how far man’s picture of the external world fits the facts (as in the physical sciences), but how by his actions, determined by the views and concepts he possesses, man builds up another world of which the individual becomes a part... This is the field to which the social studies or the ‘moral sciences’ address themselves” (HAYEK 1952/79, 40). And also, therefore, they “have to deal with structures of *essential* complexity, i.e. with structures whose characteristic properties can be exhibited only by models made up of relatively large numbers of variables“ (HAYEK 1975/78/89, 4).

Sure enough, with the almost secular trend toward mathematization and the emergence of better statistical techniques, better data, and better computers, the technical possibilities in economics have improved a great deal. As Hayek writes, “the great strength of mathematics is that it enables us to describe abstract patterns which cannot be perceived by our senses, and to state the common properties of hierarchies or classes of patterns of a highly abstract character” (HAYEK (1964/67), p. 24). Also, mathematics has provided the profession with a mutual language. And this enables people, potentially, to better describe the current situation and to track down correlations observable in the past. But predictions still stand on shaky grounds, which means that straightforward economic laws have not been established as yet. As Terence Hutchison has it, “... economists have constantly used, and are constantly using, trends, tendencies, patterns or temporary constancies, as the basis for predictions, because in fact, they have not available any genuine, relevant, non-trivial laws” (HUTCHISON 1977, 21). I am inclined to side with Roger Backhouse when he concludes that “despite the immense effort, undreamed-of increases in computing power, and the development of vastly more sophisticated statistical techniques, econometrics has failed to produce the quantitative laws that many economists, at one time, believed it would” (BACKHOUSE 1997, 136).

Given this, we should bear Hayek’s warning in mind that many of the phenomena that economists study are, in fact, examples of complex phenomena. The problem is in the subjectivity of the data of the social sciences. As Hayek says, “the facts of the social sciences are merely opinions, views held by the people whose actions we study. They differ from the facts of the physical sciences in being beliefs or opinions held by particular people, beliefs which as such are our data, irrespective of whether they are true or false, and which, moreover, we cannot directly observe in the minds of the people but which we can recognize from what they do and say merely because we have ourselves a mind similar to theirs.” (HAYEK 1952/79, 47). Hence, social phenomena are distinct from physical phenomena, which in turn can be “described by relatively simple formulae” (HAYEK 1964/67, 26). Social life never lends itself to such simple descriptions. As Hayek explains, “the student of social phenomena cannot hope to know more than the types of elements from which his universe is made up” (HAYEK 1952/79, 73). This means that “economic theory is confined to describing kinds of patterns which will appear if certain general conditions are satisfied, but can rarely if ever derive from this knowledge any predictions of specific phenomena.” (HAYEK 1964/67, 35). Such exact predictions are simply not feasible – whatever the database and however sophisticated the technical equipment.

Progress in economic science then gets to mean something else, something more humble than in the traditional positivist approach. Progress can only mean that one produces new “explanations of principle” and new “pattern predictions”. Explanations of principle mean that one can explain the general principle by which something works, i.e. why and how it

works the way it does. Pattern predictions are empirically testable as well, and they give a qualitative prediction (up or down, for example), instead of a quantitative one. Complex phenomena should teach everybody humility, along Robert Solow's humorous lines which parallel Oscar Wilde's famous description of a fox hunt,<sup>8</sup> saying that perhaps economics was just an example of "the overeducated in pursuit of the unknowable" (SOLOW 1997, 100). Solow's own modesty, however, doesn't lead him to question the use of mathematical models; it just makes him ask for more responsible, encompassing modeling. This is more easily said than done. As John Maynard Keynes already cautioned, "good economists are scarce, because the gift for using 'vigilant observation' to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one" (MOGGRIDGE 1973, 297).

Caldwell goes on to describe that "a dominant research strategy in economics over the course of the twentieth century has been to provide models at varying levels of formality of what Karl Popper called situational analyses" (CALDWELL 2004, 388). Examples that easily come to mind are the economics of information, transaction cost economics, and game theory. All of these fields start from snapshots of given situations, deriving theories from there. And they do so not just as a plausible default solution, but perfectly on purpose. In my interview, Akerlof speaks of the "English garden" model of economic science, meaning that you "look at the world out there, and from those detailed stories, you can then construct a more general theory" (in HORN 2009, 209). The problem with this is that those general theories are not necessarily worth much. As the explanatory value of these models is increased, they also become more punctual, and therefore less falsifiable – and therefore they cannot qualify as "laws" in the positive sense. "Either the models that we develop are less directly falsifiable, or ... it is now a matter of methodological principle not to take falsifications seriously. This is an inevitable result when trying to model complex phenomena" (CALDWELL 2004, 396. See HAYEK 1964/67, 28/9).

As Hayek formulates, "a theory of essentially complex phenomena must refer to a large number of particular facts; and to derive a prediction from it, or to test it, we have to ascertain all these particular facts. Once we succeeded in this there should be no particular difficulty about deriving testable predictions – with the help of modern computers it should be easy enough to insert these data into the appropriate blanks of the theoretical formulae and to derive a prediction. The real difficulty, to the solution of which science has little to contribute, and which is sometimes indeed insoluble, consists in the ascertainment of the particular facts" (HAYEK 1975/78/89, 6-7). In order to keep a theory falsifiable, according to the Popperian criterion, predictions can then not be any more precise than pattern predictions. "Of course, compared with the precise predictions we have learnt to expect in the physical sciences, this

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<sup>8</sup> "The unspeakable in pursuit of the inedible." Quoted after Robert M. SOLOW 1997, 100.

sort of mere pattern predictions is a second best with which one does not like to have to be content. Yet, the danger of which I want to warn is precisely the belief that in order to have a claim to be accepted as scientific it is necessary to achieve more. This way ahead lies charlatanism, and worse. To act on the belief that we possess the knowledge and the power which enable us to shape the processes of society entirely to our liking, knowledge which in fact we do *not* possess, is likely to make us do much harm” (HAYEK 1975/78/89, 7. See also Hayek 1952/79, 95). This is true for politics – but also for economic science.

James M. Buchanan has warned that “we are human animals, and we are bound. There are whole realms of discourse out there that we cannot reach, by definition. There are always going to be limits beyond which we cannot go. Knowing that they are there, you can always hope to move a little closer – but that’s all” (in HORN 2009, 107). But not everyone is so humble. And therefore Caldwell quite rightly laments the long-lasting damaging effects that positivism, triggered by the Keynesian answers to the Great Depression challenges, has had on economic science: “Positivism in all its various guises fostered false hopes and permitted self-delusion. It misled economists into thinking that we can, and, indeed, that to be scientific we must, always improve the predictive adequacy of our theories... Our failure to recognize the limitations of economics has cast a long shadow over the discipline.” (CALDWELL 2004, 400).

The global financial breakdown that has hit the world in 2008 unfortunately demonstrates just how apt this analysis is. Many factors have contributed to this crisis which was actually much more of a government failure than a market failure; policy was far from innocent. An overgenerous social policy that operated through the real-estate market played a major role, just as much as the easy money that the Federal Reserve pumped into the American economy to keep people spending. All these policies were acts of public self-delusion. They all go back to this very “belief that we possess the knowledge and the power which enable us to shape the processes of society entirely to our liking” that Hayek denounced (HAYEK 1975/78/89, 7). In this, mainstream economics, without consciously realizing it and certainly without admitting it, also carries some rather heavy burden of guilt.

Modern finance theory, especially portfolio theory, has exploded over the recent decades; researchers have paved the way for more and more sophisticated financial tools. The problem with those tools was not only that those who ended up using them were no longer capable of understanding them. The problem was also that the underlying models concentrated on risks and no longer took the fundamental uncertainty related with all human interaction into account.<sup>9</sup> Nobel Laureate Myron Scholes sums up that the crisis “might have resulted from a)

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<sup>9</sup> It may be useful in this context to reconsider Frank Knight’s famous differentiation between risk and uncertainty, KNIGHT 1921.

bad management, b) imperfect incentive compensation contracts, c) bad models, d) bad inputs to models, e) a lack of understanding of the aggregation problem, and f) a false sense fostered by government entities that the world was a safer place with less risk – or a combination of all of them” (SCHOLÉS 2008). What he misses is that it was not only government that fostered this false sense. With its undeniable positivism and its blindness to underlying, fundamental uncertainty, financial theory did as well.

#### **IV. Can mainstream economics go astray?**

These reflections seem to lead to the conclusion that mainstream economics has indeed gone astray. But does such a statement make any sense? If we view academia, or science, as basically a market for ideas, then the question poses and imposes itself: Can a market go astray at all? Within the evolutionary “discovery procedure” framework, such a statement might at first sight seem self-defeating. In this specific market, in science-as-market, people within and without academia feed in their latest findings, submitting it to the judgment of their peers. What eventually survives in these markets are ideas, concepts, or approaches that are promising enough for other scholars to pick them up. Can one be sure, or must one be worried whether, in this selection process, only those ideas, concepts, or approaches will get picked up that really stand for progress? Is there, to use a notion coined by David Colander, “an ‘invisible hand of truth’ which guides the progression of science?” (COLANDER 1989) Can everybody be happy as “good Darwinians”, as Robert M. Solow puts it?<sup>10</sup>

No, and *loin de là*. Even trivial technical errors are still commonplace in published papers; intellectual effort comes at a cost which is usually weighed against benefit; interest-group behavior has some effect on research; ideology also plays a role in academic findings.<sup>11</sup> But all that doesn’t mean that the science-as-market doesn’t function. What is being traded on this market is just not necessarily the truth – but academic elegance, proofs of brilliance, a new language, etc. While there are many criticisms that one may address to academia as a business or institution, with its own peculiar rules, incentives, reward systems, and funding procedures, the base line is still the same: it is indeed basically a market that we are talking about.

The science-as-market parallel has been regularly used, but it has also been rejected at least as often. The reason why I am using it here is that, in accordance with Hayek’s idea of

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<sup>10</sup> “Why is the line between mathematical economists and general economic theorists growing fuzzier and vaguer? As a good Darwinian I believe that this is no accident. I suspect it is because a large and growing fraction of what is interesting and valuable in the economic theory of the last twenty years (longer if we go back to Jevons, Marshall, Wicksteed, Walras, Pareto, Edgeworth, Barone, Bickerdike, Wicksell, etc.) has been produced by theorists who do at least some of their theorizing in mathematics. Survival in the literature is a test of fitness, if an imperfect one.” SOLOW 1954, 373.

<sup>11</sup> See DEWALD/THURSBY/ANDERSON 1986; AULT/EKLUND 1987; GRUBEL/BOLAND 1986.

the “division of knowledge” in society through markets, it does permit a better understanding of the dynamics of science. As Hayek explains in “Economics and Knowledge” (HAYEK 1936/80), still using a static equilibrium framework, the division of knowledge in an ordinary market for goods and/or services is a process tending toward an equilibrium, namely a process by which “the spontaneous interaction of a number of people, each possessing only bits of knowledge, brings about a state of affairs in which prices correspond to costs, etc., and which could be brought about by deliberate direction only by somebody who possessed the combined knowledge of all those individuals.”<sup>12</sup> But how exactly does this happen? In “The Use of Knowledge in Society”, Hayek explains that this spontaneous process takes place through the price mechanism. “We must look at the price system as such a mechanism for communicating information if we want to understand its real function.” HAYEK 1945/48/80, 86). The reason is that as relative local scarcities change in a competitive market, relative prices change as well. Relative prices transmit the information about scarcity without the individual trader having to go out and measure global supply and demand – all he needs to do is look at the evolution of prices in the overall market. This is what Hayek famously calls “a marvel.” As for the generation of knowledge, beyond the simple transmission, this takes place through competition. Through competition, external shocks which modify relative local scarcities will provoke differential reactions by supply and demand trying to find better solutions than before – reactions that are impossible to predict. This is the very source of evolutionary dynamics. Therefore, Hayek frames competition as “a procedure for the discovery of such facts as, without resort to it, would not be known to anyone” and the results of which are “unpredictable and on the whole different from those which anyone has, or could have, deliberately aimed at” (HAYEK 1968/78/82, 179/80).

The generation of knowledge in the market for private goods thus takes place thanks to competitive exchange. Buyers and sellers trade goods for money, and information on scarcity is passed on between them and others in the form of relative prices. However, as Michael Polanyi writes, “the coordinating functions of the market are but a special case of coordination by mutual adjustment” (POLANYI 1962, 55). And hence, to draw a parallel that may seem daring at first sight, let me now look at the moral market. Here, what people “trade” is good behavior toward each other, as Adam Smith describes in his “Theory of Moral Sentiments” (SMITH 1759/1982). As Adam’s namesake Vernon Smith explains, everything is about gains from trade. “Whether it is goods or favors that are exchanged, they bestow gains from trade that humans seek relentlessly in all social transactions. Thus, Adam Smith’s single axiom, broadly interpreted ... is sufficient to characterize a major portion of the human social and cultural enterprise. It explains why human nature appears to be simultaneously self-regarding and other-regarding” (SMITH 1998, 3). The beauty is that, just as self-interested

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<sup>12</sup> This, for Hayek, is the central question of all social sciences. HAYEK 1936/80, 50/51.

behavior in the market for goods brings about the greatest good for all,<sup>13</sup> “by acting according to the dictates of our moral faculties, we necessarily pursue the most effectual means for promoting the happiness of mankind” (SMITH 1759/1982, 166).

Just as trade in goods only takes place when prices reflect scarcity correctly, i.e. when enough money is being paid for a given unit of a good, reciprocity in moral behavior implies that people treat each other equally, according to generally accepted norms. If reciprocity doesn't result, one of the two parties will usually walk away. Norms are the outcomes of behavioral interaction just like prices are the outcome of market exchange. Conversely, one can more or less observe relative norms, just like one can observe relative prices for goods, and these reveal something about the probability – or scarcity – of good moral behavior. The dynamic aspect is that as people interact in the moral market, new types of behavior may be tried. While the new knowledge that is being created in the market for goods refers to as yet undiscovered scarcities and potential solutions; the new knowledge generated through competitive interaction in the moral market concerns as yet untried possibilities and ways of dealing with each other.

Now let me try the same exercise with regard to the market for ideas, or scientific knowledge.<sup>14</sup> What is being traded here is ideas, “visions”, approaches, techniques. Ideas are being exchanged against recognition in academia, provoking some checking comments at first, some refutations, and ultimately leading possibly to approval and emulation by others on a broad scale.<sup>15</sup> Success in science simply means that one's contribution survives and even spreads. Here, reciprocity means that a good piece of academic work that is being submitted to the community one way or another does receive its due amount of praise, general acceptance and emulation. How much recognition it gets is then encapsulated in the degree to which it becomes part of the dominant doctrine in the field. The relative degree of recognition

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<sup>13</sup> “It is not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own interest. We address ourselves, not to their humanity but to their self-love, and never talk to them of our own necessities but of their advantages.” SMITH 1776, 26/7.

<sup>14</sup> George J. Stigler has a pertinent description of this market: “Those economists who seek to engage in research on the new ideas of the science - to refute or confirm or develop or displace them - are in a sense both buyers and sellers of new ideas. They seek to develop new ideas and persuade the science to accept them, but they also are following clues and promises and explorations in the current or preceding ideas of the science. It is very costly to enter this market: it takes a good deal of time and thought to explore a new idea far enough to discover its promise or its lack of promise. The history of economics, and I assume of every science, is strewn with costly errors.” STIGLER 1982/83, 59.

<sup>15</sup> Max Albert rejects the “trading” aspect because in his view, there is no “exchange between producers and customers, but the mutual recognition and checking between producers.” ALBERT 2004, 128. This argument, however, is not very convincing. Albert seems to imply that researchers cooperate and compete in order to cater to a customer on the other market side, perhaps corporations, government, or the general public. In my view, this is true only in exceptional cases. Most of the time, academia is a self-referential system. In the market for ideas, most readers/receivers are customers – and these customers happen to be producers as well. This is the reason why another argument of his, namely that “production decisions are not governed by the evaluation of final consumers”, also seems beside the point. ALBERT 2006, 25.

of a new idea is, so to speak, its relative “price”.<sup>16</sup> When the degree of recognition is high, a theory becomes “*monnaie courante*”. (The wording is, I must admit, of course not innocent.) Admittedly, this market parallel may seem a little bit of a conceptual stretch, but it is useful – even if the coordinating mechanism may perhaps not be entirely as powerful, precise, and transparent as the price system in the market for goods and services. I am preposterously tempted to think that James M. Buchanan denies himself a promising avenue for further reflection when he criticizes it as outright „improper, even metaphorically, to conceptualize scientists as ,trading”“ (BUCHANAN 2001,158).

While it is the relative price resulting in a global market that tells local sellers whether they should increase or decrease their production efforts, it is the relative degree of acceptance of a certain type of idea that gives an indication to a scientist whether to increase or decrease his effort, whether to pursue in some theoretical vein or not.<sup>17</sup> Connected with looking at academia this way is the implicit hope that Adam Smith’s “invisible hand” will come in to direct self-interested individual action towards a common effort that serves the advancement of scientific knowledge as a whole. When the social cooperation game is so structured and so embedded in adequate rules that individual goals and social concerns find themselves ultimately in harmony, then there can be hope that academic research goes the right way, and that it indeed generates valuable new knowledge. Under such conditions, science can be pictured as a truly positive evolutionary process. The new knowledge that will then be generated shows new, promising avenues in research, as yet undiscovered “holes” in an existing body of theory, or altogether new combinations of ideas.

Whether competition in academia really turns out to be such a “good” evolutionary process, i.e. whether private and public interests are aligned in such a way as to make the knowledge generation indeed socially desirable, is a question that Adam Smith might have answered positively. He did answer it in the affirmative with regard to the market for goods. With regard to the moral market, his approach was more positive than normative, but his general thrust was the same. His view of the “simple system of natural liberty” (SMITH 1776/1981, 687) is an overall optimistic one. As Constitutional economics and game theory have insistently shown, this outcome however does depend on the “rules of the game”, and these in turn underlie collective choice. This is where the beef is – or, as we say in German, where the dog is buried. As Viktor Vanberg explains, “market and science are games of competition. In both realms, the engine that drives the evolutionary invisible-hand process is the competing ambitions of reward-seeking agents. And in both realms, competition is ...

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<sup>16</sup> Philip Mirowski argues that „the act of formal acknowledgement of published work does not look much like a price system“. MIROWSKI 2004, 60.

<sup>17</sup> One may object that such an “intangible price” may not adequately reflect the value of a new idea, since “buyers” may need time to realize how promising it is. That is true, but this differs in nothing from the market for so-called “experience goods” and services. In all markets, economic, moral, or academic, entrepreneurial action requires endurance.

subject to rules that can be more or less suitable in channeling the participants' ambitions in socially productive directions" (VANBERG 2008, 9). The only important caveat may be that, in science perhaps even more than in the market for goods, we are incapable of gauging *ex ante* what those "socially productive directions" are. Where is Rome? An open competitive process is needed precisely in order to find this out.

This is where Hayek's notion of pattern predictions finds application. One may analyze the given rules under which academia operates and see whether some of them appear outright perverse. As a matter of fact, academic research is not only influenced by the institutional rules which guide the competition for recognition among peers, it is also affected by the underlying necessary competition for resources. My guess is, with Viktor Vanberg, that a "harmonious relation is by no means guaranteed." And "likewise, in the case of science, the ways in which research institutes, universities and professional associations assign positions, promotions, rewards and honors to their members will have an obvious effect on the ways in which scientists compete with each other" (VANBERG 2008, 19). If these incentive structures direct behavior in obvious ways that cannot possibly be judged functional, then a modification of the rules may appear imperative in order to keep the social enterprise of research in line with a potential "common good".

To corroborate the verdict that the market for economic research has gone astray in the sense that its overmathematization and formalization has distracted economists and made them close their eyes to relevant issues for all analysis of human interaction, to the detriment of the financial markets and the entire global economy, one must be able to point out an incentive mechanism that might be responsible for that – or else it is improper to make that point. If the reward systems, i.e. the incentive mechanisms and selection principles, look alright, if one therefore accepts as appropriate the basic principles that govern academia, then one has to accept its results – even though one might still have a preference for the market moving in some other direction.

The rules of the game, however, are themselves subject to an evolutionary process. Some of the rather "tangible" rules – especially as far as financial and organizational questions are concerned – are laid down by governments. They can be biased, they can be distortionary. Most of the "intangible" rules concerning peer-review processes and the like, however, have never been laid down by authority, but have sprung up in spontaneous interaction. They do change over time, but of course there is no guarantee that they will move in the "right" direction. If economists have adopted selection principles that are counterproductive, "both with regard to the outside reputation of their profession and with regard to its contribution to the growth of knowledge" (VANBERG 2008, 22), this is entirely their own fault. Spontaneity has never been a synonym of optimality.

The current state of economics is a clear case in point. Much of mainstream economics has developed in a direction that has made it incapable of facing the acute political challenges of an expanding financial market, with its implications in terms of appropriate pricing, regulation, and supervision. It has overlooked the danger of overexpansive monetary policy and government intervention generally. This growing blindness has been the result of a self-perpetuating development, fostered by the spontaneously evolved and freely chosen reward system, ultimately to the detriment of the profession. There is a host of studies concerning the reward system of science, and as a matter of fact, none of them is very comforting in this respect. The broadest, survey and interview based study is the classical one by Harriet Zuckerman, which extends over various fields and does seem to warrant generalization. As early as more than 30 years ago, Zuckerman finds strong evidence for the phenomenon of accumulation of advantage in the sense of a “spiraling of augmented achievements and rewards for individuals and a system of stratification that is sharply graded” (ZUCKERMAN 1977, 249) and also for an academic equivalent to the Matthew effect according to which the rich get richer and the poor get poorer. In academia, the Matthew effect “confers further authority and influence on those who are already influential and authoritative and brings further honors to those who have already been honored“ (ZUCKERMAN 1977, 249). In such a self-perpetuating and self-enclosing elite, real breakthroughs often have to come about *against* the odds of the traditional reward system.

## **Conclusion**

“There are many roads that lead to Rome.” Summing up, there is an impressive variety of itineraries. What the Nobel Laureates all share is an independent mind, intense curiosity, a natural focus on relevant problems, and helpful networks. As Henrik P. van Dalen has found, “economics is a young man’s game”. Most breakthroughs take place when the scientists are still in their twenties. He traces this back to typical character traits that eminent economists all share: “The average Nobel laureate is equipped with the following blessings: talent, an independent or an outsider’s mind, a love for risky projects, a nose for being at the right place at the right time, the gift to see fundamental problems and, last but not least, luck” (DALEN 1997, 16). This very much coincides with my findings. Most of the Nobel Laureates have shared an interest in political and economic questions with their families and thus had, as youngsters, some early mental practicing ground at home. Beyond their parents, they also benefited from teachers and other role models, especially at university. Most of them started out wanting to improve the world, especially under the shocking experience of the Great Depression. Some sort of slipped into economics through their mathematical talent but then, in a second step, also discovered their “missionary” tendencies.

What seems to matter a little less is the actual sociological background, contrary to the bourgeoisie hypothesis. The Nobel laureates I have interviewed all come from very different backgrounds, ranging from a poor life on a farm (James M. Buchanan) in Tennessee and life in a rather well-to-do family traveling all over the world (Douglass C. North). Working habits and impulses also differ greatly. There are some lone wolves out there and some tribal animals; some scholars who draw their inspiration from interaction and others more from introspection. Some depend more on the literature, some follow the trend of political history, and some very directly translate their personal issues into a research agenda.

Turning from the *micro* level, i.e. from the personal motivations that make for individual careers, to the *macro* level, i.e. towards the miraculous process of the generation of excellence and progress in science as a social phenomenon, one can also be optimistic. It seems that yes, economics as a whole – the whole being different from the mainstream – is making some progress, slowly but surely. Uncomfortably, the path of economic science tends toward a moving target. It is being directed by a vast array of incentives, some of which may be dysfunctional, some of which will be with mankind a long time even though they deserve to get discarded. The mainstream may go astray, and it takes time to bring it back on track. Theories are sometimes sticky. People get used to them. They are part of a generation's mental maps; it is difficult to imagine the world without them. But Hayek reminds us that “man has been impelled to scientific inquiry by wonder and by need” (HAYEK 1964/67, 22). And this will go on forever. This is a great source for optimism: wonder and need will keep us intellectually thirsty and striving for knowledge. The mainstream will never be safe from beneficial heterodox threats. The potential for improvement will be used. I will therefore join Viktor Vanberg in his outlook: “The principles of selection that prevail in research institutes and universities, the constraints that national rules and regulations define for scientific work, and conventions that come to prevail in professions such as economics may be dysfunctional, but man's ineradicable interest in knowing how the world around him works will be an incessant force that tends to select in favor of more informative theories, and stubborn reality will be an inescapable ultimate selector between conjectures that are compatible with the facts and those that are not. Globally and in the long run the capacity of markets to serve consumer interests and the capacity of science to advance the growth of knowledge appear to be quite robust” (VANBERG 2008, 22).

And yet, Hayek's warning should be heard: “It is high time, however, that we take our ignorance more seriously” (HAYEK 1964/67, 39). Rome definitely still needs to be discovered.

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