Binswanger, Johannes

Conference Paper

Are Economists Rational? On the Rationality of the 'Rational Choice' Paradigm

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2010: Ökonomie der Familie - Session: Improving Productivity and Efficiency of Economic Science, No. A2-V3

Provided in Cooperation with:
Verein für Socialpolitik / German Economic Association

Suggested Citation: Binswanger, Johannes (2010) : Are Economists Rational? On the Rationality of the 'Rational Choice' Paradigm, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2010: Ökonomie der Familie - Session: Improving Productivity and Efficiency of Economic Science, No. A2-V3, Verein für Socialpolitik, Frankfurt a. M.

This Version is available at:
https://hdl.handle.net/10419/37414

Are Economists Rational? On the Rationality of the "Rational Choice" Paradigm*

PRELIMINARY

Johannes Binswanger†

February 25, 2010

Abstract

Today, an overwhelming part of academic analysis is based on the rational choice paradigm. This is true, in particular, for theoretical analyses; but it also holds for empirical analyses as far as they are based on a structural model. It also holds in spite of the existence of the behavioral economics paradigm. In this paper I ask whether the methodological restriction of rational choice is really in the interest of an efficient production of knowledge. Since rational choice is such a universal paradigm in economics, it holds, in fact, a monopoly. This would be desirable if there were a case for a natural monopoly. I argue, however, that this is unlikely to be the case and that more competition between different paradigms would increase the productivity of the economics profession. This increased competition will not simply occur by itself but requires a change in institutions. I discuss several ways, how increased competition between different methodologies could be implemented in practice. I also discuss how the restriction on the rational choice methodology may reflect a form of non-Bayesian judgment of economic researchers themselves.

Key words: Arbitrariness, Bayesian updating, causality, competition, methodology, parsimony, rational choice.

JEL classification: B40, B41, D01, D03

---

*I am grateful to Cédric Argenton, Alia Gizatulina, Katie Carman, Manuel Oechslin, Jens Prüfer, as well as to seminar participants at Tilburg University and the Swiss Society of Economists Abroad. All errors are my own.

†Department of Economics, CentER and Netspar, Tilburg University, P.O. Box 90153, 5000 LE Tilburg, The Netherlands; j.binswanger@uvt.nl.

1 Introduction

Rational choice is the dominating paradigm in economics. This is evident in the case of theoretical analysis, whether applied or conceptual. Beyond this, rational choice also plays a crucial if subtle role in empirical analysis. Outside the realm of the analysis of (natural or arranged) experiments, it is standard practice to back the estimated equations by a "model." In an overwhelming majority of cases, this model belongs to the paradigm of rational choice.

In this paper, I ask whether the dominance of the rational choice paradigm is really in the interest of an efficient accumulation of knowledge about economic issues. To make my analysis operational, I adopt the point of view of Keynes (1930) that the purpose of the economist profession is the accumulation of knowledge that is useful for solving practical economic problems.[1] This view is obviously normative. Adopting it means ignoring other objectives such as the beauty of a coherent framework that relies only on a few principles or axioms.

Once the accumulation of knowledge that is useful for solving practical economic problems is accepted as a goal, it should direct the actions of economists. In particular, it is in the interest of society that economists pursue this goal in a rational manner. This means that, given a certain amount of manpower and financial resources, economists should get out the most in terms of accumulating knowledge. In other words, we would not want to waste resources in the process of accumulating economic knowledge. Rather, we want to accumulate knowledge in an $\mathit{efficient}$ way.

Economists as a collective professional body seem to have come to the conclusion that the paradigm of rational choice allows for an efficient accumulation of knowledge and that all other conceivable paradigms are inferior to rational choice. As is to be discussed in this paper, there are two main reasons that are held responsible for the superiority of rational choice. First, it is believed that rational choice greatly reduces any arbitrariness in the explanation of economic phenomena, at least in comparison to other paradigms. Second, it is believed that rational choice is particularly parsimonious. Beyond this, the rational choice paradigm is also thought

[1] Keynes famously expressed his view as follows: "If economists could manage to get themselves thought of as humble, competent people on a level with dentists, that would be splendid" (p. 373).

to be particularly tractable. Furthermore, concerning macroeconomics, it is not subject to the Lucas critique.

In this paper, I will carefully analyze the arbitrariness and the parsimony doctrine. I will come to the conclusion that they cannot rationally justify the superiority of rational choice. Rather, I argue that a monopoly of rational choice will bring economists off the production possibility frontier of producing knowledge and that we could do better. The recipe I offer is one that should sound familiar to economists: There should be more competition between paradigms. However, economists also know that competition does not simply occur by itself. Rather, we need good institutions that promote competition. Therefore, we need to think about appropriate institutional changes that allow for a productive competition between paradigms.

All of the analysis in this paper is purely normative. It concerns what economists should do from the point of view of society. These normative goals should not be confounded with the positive goals that many economists may currently have in mind, consciously or not. Evidently, any junior economist has the goal of writing papers that have a chance of getting published in good international academic journals. This goal is perfectly rational from an individual point of view. However, the profession as a whole may not just have the goal of publishing in good international academic journals, or the bigger picture would be badly missing.

Evidence for the fact that only a very small minority of economists engage in (theoretical) research outside the paradigm of rational choice can easily be obtained by flipping through the pages of our leading journals. Candidate frameworks that fall outside the rational choice paradigm are: Behavioral economics (although I will qualify this later on); bounded rationality (as far as it is to be kept separate from behavioral economics); evolutionary economics, Post-Keynesian economics, Marxist economics, Austrian economics, and historical economics. At this point, the issue is not to judge whether any of these frameworks has contributed to the accumulation of economic knowledge. Rather, the point is to observe that an extremely small minority of articles published in leading economic journals fall into the realm of those paradigms. For instance, I counted the number of "behavioral" papers published in the $Quarterly\ Journal$

of Economics in 2008. I count 4 of 42 journals as clearly "behavioral."[2] Given that this journal is known for its "bias" towards behavioral economics, this number is surprisingly low. Similarly, flipping through the Papers and Proceedings volume of the *American Economic Review*, I count 2 sessions as "behavioral" out of 31.[3] Similar or lower ratios are found in other leading journals. My conclusion from this is that the rational choice paradigm holds a near-monopoly when it comes to economic theorizing.

This near-monopoly of rational choice would be perfectly fine if there were abundant signs that the rational choice paradigm is indeed a lot more promising than other paradigms in explaining *all* economic phenomena of interest. These signs would certainly have to come from empirical research. However, an unbiased look at the empirical literature does not reveal this picture. Undoubtedly, rational choice models provide a good description of behavior in *some* areas. However, there is no strong evidence that suggests that *any* economic behavior, including life cycle savings, daily shopping, job search, labor supply, financial investments, borrowing, housing, price setting, voting, is best explained by a pure rational choice model.[4]

Translating this observation into Bayesian language, there is no reason why we should have a very strong *prior* that rational choice explains all economic behavior of interest, given the available evidence. In a nutshell, this paper will argue that in the absence of a strong prior that rational choice is the most promising explanation of all economic behavior of interest, a near-monopoly of rational choice is not compatible with a rational strategy of knowledge accumulation. Rather, it may reflect a way of non-Bayesian judgment of economists themselves.

At this point, it is appropriate to spend a few words on behavioral economics. Clearly, behavioral economics is the most prevalent non-rational choice paradigm. Since behavioral economics is perceived as quite fashionable, it seems that, after all, rational choice does not really hold a monopoly status. However, almost all behavioral economics models are very tightly connected to a rational choice models. In particular, behavioral economics models such

[2]Details to be added.

[3]Details to be added.

[4]See Campbell (2006) on household portfolio choice; see Madrian and Shea (2001), and Beshears et al. (2007) on retirement saving; see Kashyap (1995) for evidence on firms' price adjustment behavior; see Caplan (2007) on voting.

as hyperbolic discounting, loss aversion, inequity aversion etc. have been constructed as "extensions" of a rational choice model. This means that, for special parameter values, the prototypical behavioral model coincides with a standard rational choice model. The hallmark of a rational choice model is the framework of optimizing under constraints. Individuals act as if they maximize their utility, given the relevant constraints. Behavioral economics is far away from stepping outside this framework. Rather, a behavioral model introduces a "distortion" into preferences. However, agents still behave as if maximizing their preferences under constraints.

The point of this observation is that there is a vast terrain of non-rational choice that is not explored by behavioral economics. The discussion of whether there is a good reason for this or not is the topic of this paper.

For many of us, it has become difficult to imagine what alternatives to rational choice there may be. One reason why rational choice (together with behavioral economics) holds a near-monopoly may be that there are simply no other frameworks that are accessible to rigorous mathematical modeling. This conclusion seems premature, however. There are many conceivable patterns of behavior that fall outside the realm of rational choice. A few examples may illustrate this. First, people may simple imitate the behavior of others that they perceive as successful. This imitation need not be the outcome of an optimization (corresponding to rational herding) but a simple one that even animals are capable to adopt. Second, people may follow simple rules of thumb that may not be the result of an optimization. Third, behavior may be influenced by unconsciously formed habits (for instance, think of shopping). Third, people may make decisions in areas where they are aware that they do not understand what they are doing. Clearly, this is inconsistent with rational choice but may have be the case for several financial mangers before the current crisis. Fourth, there may be compulsive behavior (e.g. when it comes to shopping) that is not well captured by an optimizing framework. It would be fairly easy to extend this list. Furthermore, it is reassuring that biologists, psychologists, and marketing scholars do have mathematical models of types of behavior that are clearly outside the realm of rational choice. The conclusion is thus that theorizing outside the realm of rational choice does indeed appear feasible.

This paper contributes to the literature on the methodology of rational choice, as well as

on the methodology of scientific research more generally. The seminal justification of rational choice as a leading framework in economics comes from Friedman (1953). In this essay, Friedman makes the famous argument that economic behavior should be understood as if being the outcome of an optimization. Sen (1977) argues that truly rational individuals would not behave according to the narrow framework of rational choice. The latter would rather describe the behavior of "rational fools." Kuhn (1962) provides a descriptive account how scientific activity is often driven by sociological processes rather than reflecting an activity of Bayesian learning. Sober (1992, 2001) and Gernert (2007) discuss how an exaggerated focus on parsimony may be counter-productive for scientific discovery. To my knowledge, this paper is the first one which discusses the rational choice paradigm from the point of view of an efficient production of knowledge. It thus adopts a logic that should come very natural to economists.

This paper is structured as follows. In Section 2, I discuss the argument that efficient scientific discovery requires guidance by a non-arbitrary benchmark paradigm. In Section 3, I discuss the argument that efficient scientific discovery requires the adoption of a parsimonious paradigm. In Section 4, I discuss how the institutional setup of our profession may be adjusted to increase competition between different paradigms and to make our scientific activities collectively more efficient. In Section 5, I conclude.

2 The Non-Arbitrariness Doctrine of Rational Choice

A first highly prominent justification of the rational choice paradigm is the claim that rational choice offers a clear non-arbitrary benchmark for theorizing about economic behavior and that it is this type of benchmark that is needed for making scientific progress. The premise is that there is only one way to act rationally while there are many ways to deviate from (full) rationality. Any particular deviation from rational choice must seem arbitrary and hence lack scientific discipline. Scientific discipline is needed, in turn, if we are to make scientific progress, i.e. accumulate knowledge about economic phenomena.[5]

[5]It is actually not straightforward to find *the* reference for this argument. Rather, this has become folk wisdom. The argument is regularly encountered in seminar debates and it is found in many textbooks.

The premise this argument is somewhat questionable. In most cases that are of interest in applied studies, rationality per se does not predict one particular type of behavior. Rather, auxiliary assumptions are required if we are to get specific predictions. For instance, rationality per se does not predict whether an increase in a subsidy on retirement saving will lead to higher or lower savings. The answer is ambiguous. However, assuming that preferences are time-separable and of the constant relative risk aversion type and that relative risk aversion is larger than one will lead to a definite answer. This notwithstanding, it is true that the set of predictions that are obtained in a rational framework is much smaller than the universe of predictions that can be obtained by stepping outside the realm of rational choice.

The essential question to be investigated in this section is the following:

Question 1 *Does a rational research strategy require guidance by a non-arbitrary benchmark?*

While this question could be analyzed by means of a formal model, I believe that it is particularly instructive to think about this issue by means of a story. The story I will tell represents, in fact, the verbal description of a kind of toy model. It would be straightforward to convert the below arguments into formulas.

The story I am about to tell is a story of two tribes living in an oasis. Think of the two tribes living under identical conditions but in two completely disconnected deserts. For the sake of convenience, let us choose some names and call the tribes Cons and Lib. Both tribes face an identical problem. They both have a belief that there may exist at least one other oasis with people living there. In particular, they imagine to engage in mutually beneficial trading with people from another oasis. However, they do not have any prior knowledge about where any other oasis may be located. The only way to find out is to set off for traveling. Put differently, the tribes face a *research* problem.

For simplicity, I make a number of auxiliary assumptions about each tribe's research problem.

- The desert landscape is uniform around the oases of the Cons and Libs. In particular, it is excluded that there are mountains to the north while there are no mountains to the south

etc.

- There are no existing roads or pistes.
- There is a large number of travelers available. The minimum size of a travel team is small compared to the number of travelers and is hence neglected.
- The solution of the research problem is the same of the Cons and the Libs.

I will relax these assumptions later. For the moment, they are just accepted in the way stated above.

The crucial twist of the story is that both tribes, Cons and Lib, approach their research/travel problem in a fundamentally different way. Tribe Cons requires all travel teams to head east. This may include everything from north-east to south-east. Some members of the Cons believe that heading east is best because the sun comes from east ("ex oriente lux"). However, most members of the Cons justify this traveling policy by the fact that east provides a clear benchmark. Any deviation from heading east would be arbitrary. And a successful research strategy cannot be based on arbitrariness. To give travelers the proper incentives, there is an implicit law that any team not heading east may not publish about whatever sign of the presence or absence of another oasis it has seen on its way.

Tribe Lib employs a fundamentally different traveling policy. Travel teams may head in any direction they like and report about any of their trips. Because of the prevailing liberal attitude, life among the Libs is sometimes a bit chaotic.

The important question is now which tribe's research strategy is more successful. It is conceivable that another oasis is located to the east. In this case, it will be quickly discovered by the Cons. In fact, the Cons may be faster in discovering this oasis than the Libs. In this case, we would say that the Cons' strategy was better adapted to their environment than the Libs' strategy.

The more important question is, however, which tribe's strategy is more promising from an $ex\ ante$ point of view, i.e. before they start traveling. The answer to this question relates to a form of Laplace's theorem (Geisser, 1984, Binswanger and Oechslin, 2009). This theorem

says that, in the absence of any informative prior about where another oasis may be located, it is rational to assume that its location is uniformly distributed around the own oasis. Suppose that there are a large number N of travel teams and a social planner would allocate the travel directions among the teams such as to maximize the discovery of another oasis. Under a uniform distribution, the planner will want to avoid any clustering of teams. Rather, he would spread them roughly equally.[6] The important thing to notice is that, for a sufficiently large N, the planner would never want to send all teams east.[7] This would be a strictly inferior strategy.

The implications of this story for the answer of Question 1 above are fairly straightforward. The claim that rational choice should be adopted because it provides a well-defined benchmark is comparable to the claim that all travel teams should head east because any deviation from heading east is arbitrary. Following such a conservative research strategy is not in the interest of an efficient accumulation of knowledge.

Another important implication is that, a priori, there is nothing wrong with going east, as long as not everyone is going east. Rational choice is a perfectly valid research strategy. However, it should not hold a monopoly. In fact, no single approach should have a monopoly in the absence of very strong signals that this approach is indeed the most promising. However, it seems that human beings have a tendency to go for clusters. In psychology, this is known as the bandwagon effect (Cohen and Rothschild, 1979; Noelle-Neumann, 1974).

For instance, before rational choice has become the dominating paradigm in German speaking countries, the dominating paradigm was the historical one (Spiegel, 1992). Scholars adhering to historical economics were as intolerant to rational choice as today's advocates of rational choice are with respect to historical economics. Why do researchers find it so difficult to accept that the efficient accumulation of knowledge requires to adopt a set of various strategies rather than one single strategy? The answer is probably that researchers themselves are human beings who do not always follow the strict prescriptions of Bayesian judgment![8]

[6]The optimal solution would depend on a number of details such as topography etc. There may not be a unique optimal solution. This is not crucial for the discussion here, however.

[7]This requires assuming that N is sufficiently large, as we have done.

[8]Examples of monopolizing paradigms abound in the history of science. See Gribbin, 2002.

So far, we have maintained the assumption that the number of available travelers N is large. Clearly, if N were to be small and if the optimal size of a travel team is large due to economies of scale, then it may not be suboptimal to have all available teams head east. Getting back to economics, it may indeed be true that a particular paradigm can be explored in a more productive way if it is adopted by many researchers, due to economies of scale. However, I believe that the size of the economist profession is much larger than the critical mass of people needed to benefit from economies of scale within one particular paradigm. In other words, I believe that the size of the economist profession is sufficiently large to invalidate the optimality of clustering due to economies of scale. This reinforces the conclusion that the clustering of economists in the rational choice paradigm may reflect a bandwagon effect rather than a collectively rational research strategy.

Another strong assumption of the above analysis is that there is no prior information available about the location of another oasis. How would the arguments change if there were signs that it is more likely that another oasis is located east rather than west, south or north? How should a planner allocate travel teams in this case? Surely, the planner should now send more teams east compared to the situation without an informative prior. However, as long as N is large enough and the rational prior probability that another oasis is located east does not come close to one, it is still rational to send some travel teams in other directions.

Getting back to economics, this means that it is efficient to have some economists adopting a non-rational choice paradigm as long as we do not have very strong signals that rational choice is extremely more likely than other approaches to explain all economic phenomena of interest. As discussed in the introduction, a non-partisan look at the empirical evidence does not justify a prior that rational choice is a lot more likely than other paradigms to explain all economic behavior of interest.

Some readers may feel that a view on the economist profession through the lens of the oasis story misses an important part of the picture. After all, research on economic phenomena is not only carried out by economists, but there are also psychologists, sociologists, political scientists, biologists etc. While economists may head "east" (i.e. adopt rational choice), other scientific disciplines have their researchers head in other directions. As a result, all academic disciplines

together may well cover the whole spectrum.

This view has some merit. However, it boils down to the view that economics is the academic discipline exploring the world through the lens of rational choice. Similarly, other disciplines are defined by their respective methodologies. In other words, economics is not defined any more by the object of inquiry, i.e. understanding economic phenomena, but rather by a method, i.e. rational choice.

There are indeed quite a number of psychologists, sociologists etc. exploring economic phenomena from the point of view of their paradigms. It is noteworthy, however, that their work is ignored by economists, to a good degree, while the work of economists is mainly ignored by them, in turn. This points to a problem of defining academic disciplines by methods rather than objects of inquiry. Researchers from different disciplines tend to ignore each other much more than researchers within a particular discipline. This may partly be due to reasons as simple as the fact that scholars within one department interact more with each other than scholars between departments. If academic disciplines are defined in terms of methods rather than content, many synergies arising from a common inquiry of a subject may be lost. This may be offset, to some degree, by gains in terms of synergies from a common adoption of methodologies. However, insofar as the ultimate goal of scientific inquiry is to understand phenomena rather than the development of methodologies, defining academic disciplines by content rather than by methods seems rather more natural.

Even if it were efficient to define academic disciplines in terms of methodologies rather than content-wise, there is no rational reason why different disciplines should meet with the typical contempt and hostility, rather than curiosity. Typically, economists are proud of the rational choice methodology to such a degree, that they feel that they can comfortably ignore what any other discipline has to say about economic behavior. Getting back to the oasis story, this would mean that those travel teams heading east would be proud of ignoring any reports from other teams not traveling east. In particular, the “east teams” would continue search east regardless of any evidence reported by the teams that have headed for other directions. This situation is clearly inefficient since some valuable information is not used productively.

In sum, the mutual ignorance of academic scholars from different disciplines investigat-

ing the same phenomena leads off the production possibility frontier for the accumulation of knowledge. Rather than by collective rationality, the mutual ignorance of adherents of different methodologies may be explained by the psychology of "us versus them". In particular, rational choice defines an in-group showing hostility towards non-rational choice out-groups (and vice versa).

Overall, the discussion of this section leads to the following proposition:

Proposition 1 *The accumulation of knowledge on the production possibility frontier does not require guidance by a non-arbitrary benchmark. In many cases, adoption of such a benchmark prevents an efficient accumulation of knowledge. In other words, there is no rational basis for the non-arbitrariness doctrine.*

3 The Parsimony Doctrine of Rational Choice

A second prominent justification of the rational choice paradigm is the claim that rational choice is a particularly parsimonious paradigm and that parsimony is what is needed for making scientific progress. In particular, parsimony is thought to be required since, with a sufficiently high number of explanatory elements we finally may always be able to explain everything. In this way, no scientific progress can be made.

Clearly, the behavioral motives that are consistent with rational choice are a strict subset of all potential behavioral motives. Thus, adopting the rational choice paradigm limits explanations of economic phenomena to a relatively small subset of the whole universe of potential explanations. It is in this sense that rational choice is to be understood as parsimonious. A rational choice model is parsimonious in this general methodological sense even if it may have many "parameters."

It is noteworthy that the parsimony doctrine, i.e. the view that a good scientific theory needs to be parsimonious, is much less strict than the non-arbitrariness doctrine. The non-arbitrariness doctrine eliminates competition between different paradigms in the interest of scientific progress. In this sense, it takes an extreme stand. The parsimony doctrine allows for

any theory that invokes only a "small" set of explanatory elements. For instance, let us identify psychoanalytic theory as a theory that explains human behavior by means of (suppressed) sexual drives. Then, psychoanalytic theory would pass the test of being a parsimonious framework. Similarly, Marxist dialectic theory of history is a parsimonious theory since it explains the evolution of history simply by referring to class struggle.

The central question in this section is the following:

Question 2 *Does a rational research strategy require parsimonious explanations of the phenomena of interest?*

It is noteworthy that the meaning of "parsimonious" is naturally vague. How many explanatory elements may a theory have to still count as parsimonious? I sidestep this issue here since it seems that there is little scope for general insights into this aspect of parsimony. Rather, the issue here is whether somehow minimizing the explanatory elements should be a leading principle of scientific theorizing. The discussion of this will again be rather informal. I make my arguments by means of a thought experiment. The thought experiment goes as follows.

Suppose that a group of medical researchers has come up with a new paradigm. According to this paradigm, every medical disorder is a result of a deficiency of the immune system. Would you like to get treatment from this group of "parsimonious" medical scientists?

To be sure, according to modern medical science, it is beyond reasonable doubt that the hypothesis underlying the above paradigm is wrong. For instance, disorders may be caused by a disfunctionality in the biochemistry of a cell (possibly at a genetic level) that is not induced by an infection. For this reason, you may not be very enthusiastic about getting a treatment from adherents of this new paradigm.

Suppose that you express your concern to one of the adherents of this paradigm. You are pointing out that according to empirical research, medical disorders may not only be caused by a deficiency of the immune system. If the medical researcher has read Milton Friedman's essay on methodology (Friedman, 1953), he will respond by saying that your concern is mistaken. After all, it is good enough if a disorder can be understood *as if* caused by a weakness in the immune system. As long as the immune model may make good predictions on average,

everything is all right. You may not find this very reassuring, however.

The point of this thought experiment is to show that parsimony does not have any universal value. To the contrary, if the parsimony principle is taken very strictly, it may be quite harmful. We do not wish to get a diagnosis from a doctor who considers it as an intellectual sport to limit the causes for a disorder to a low number. Similarly, most of us would be hesitant to send a person with a mental disorder who we care about to a psychiatrist when knowing that the psychiatrist restricts his explanation of a disorder to suppressed sexual drives. But what about sending a practical economic problem needing diagnosis and treatment to the economist profession?

I conclude from the above that a statement like "rational choice is superior to other paradigms in economics since it is (more) parsimonious" cannot offer a methodologically rational justification for rational choice.

I turn now to a more nuanced views of parsimony. Let us consider a set of patterns in the data, let us call it $\mathcal{A}$. This set of patterns is explained equally well by two different theories, A and B. Theory A explains the patterns by invoking $n \geq 1$ explanatory elements while Theory B invokes $n + m$ explanatory elements, where $m \geq 1$. The fact that both theories explain the data equally well may be thought to entail similar R^2 or other statistical measures of fit. For the sake of the argument, we assume that there are no statistical pitfalls involved, i.e. the two models are fully identified, there are no endogeneity problems etc.

Clearly, Theory A is more parsimonious than Theory B. If the purpose of a theory is indeed to (only) explain the set of patterns $\mathcal{A}$ in the data, it makes perfect sense to prefer Theory A to the less parsimonious Theory B. There is no reason to prefer a more complex theory to a simpler one if the simpler one does its job sufficiently well. No one can have a reasonable doubt about this. The question is, however, whether this reflects the situation in economics concerning rational choice. Is rational choice really an empirically successful paradigm and any non-rational choice paradigm explains just the same phenomena but in a less economical way? I do not think so. Before turning back to this issue, let us however continue the above argument.

Suppose now that there is not only a set of patterns $\mathcal{A}$ to be explained, but there are also sets

of patterns $\mathcal{B}, \mathcal{C}$. Suppose that, while Theories A and B are equally good at explaining $\mathcal{A}$ while Theory B is better at explaining $\mathcal{B}, \mathcal{C}$. Now, we are in fact looking at a broader frame and it is not clear any more whether the more parsimonious Theory A is still preferable to Theory B. It may be for some purposes, but this cannot be concluded in general. The point of this is that a theory that looks better within a narrow frame because it is more parsimonious with respect to that frame may turn inferior when the frame is broadened.

What are the implications for economics and the rational choice paradigm? According to the above, rational choice would potentially represent a preferable paradigm if it explained a set of economic data at least as well as another less parsimonious theory. For instance, rational choice may explain bargaining behavior in an economic experiment at least as well as another theory that allows for non-rational elements. If we were only interested in bargaining behavior, this may establish the claim that rational choice is a superior paradigm. However, if we broaden the frame to all economic phenomena of interest, there are many instances where rational choice models do not perform well and it is conceivable that other less parsimonious theories may do better.[9] Therefore, when broadening the frame to many economic phenomena of interest, it is hard to establish the claim that rational choice models are better because they are more parsimonious.

Causality

The above discussion naturally leads us to the deeper reason why parsimonious models may not be desirable even if they perform well within a sufficiently narrow frame of phenomena. This deeper reason relates to the concept of causality. Many phenomena may arise from the interaction of a large set of causal factors. This is particularly salient in the case of medicine. A certain symptom may arise from many different causes. After all, this is the reason why diagnosis is often so difficult to perform. A parsimonious epidemiological theory would restrict the number of potential causes to just a few. For instance, the physicist may restrict his search to causes that are compatible with a deficiency of the immune system. Alternatively, he may

[9] See references in footnote 4.

restrict the search for causes that are linked to inherited factors etc. However, whenever the set of causal factors of the parsimonious diagnosis scheme are substantially smaller than the universe of true potential causes, then the parsimonious scheme introduces the possibility for serious error. In fact, a parsimonious scheme may then just represent a harmful way of non-Bayesian judgment.

Economic data are typically observations that are comparable to symptoms in medicine. Typically, economists do not observe the underlying causes of economic behavior. For instance, we observe that someone accepts a job offer after an unemployment spell of three months. We do not directly observe why the person has made this particular choice. For instance, this may be the result of the design of unemployment insurance, it may be because the job was a particularly motivating one in terms of content, it may be better paid than alternative jobs, it may be that the individual started to panic and has taken the first available job, or ir may be that the person has a friend who recently took a job at the same firm etc.[10] A typical task of the economic researcher is to identify the underlying causal mechanism that generates patterns in observed data. In other words, the economist does something comparable to carry out a diagnosis given symptoms, in order to infer the causal structure of the phenomena of interest.

If a researcher adopts the rational choice paradigm, the only acceptable causes for explaining a pattern in the data are obtained by referring to preferences and constraints, assuming that the observed data are generated by maximizing preferences under the relevant constraints. Thus, there is no room for biases, poor judgment, imitation, compulsive action, random choices etc. To the degree that the surmised parsimonious causality pattern of rational choice is sufficiently at odds with the true potential causality pattern, there is a real danger that parsimony may lead to an erroneous picture. If economic researchers adopt the rational choice paradigm when diagnosing the causes of a policy problem, there may be a serious danger of coming up with the wrong treatment.

Broadening the frame to an extreme degree, the true causality pattern of any human behavior is likely to relate to the universe of brain activity. That, ultimately, behavior originates in

[10] Alternative examples: Housing, saving etc.

the brain is probably beyond reasonable doubt given the current stage of scientific evidence. Furthermore, it is beyond reasonable doubt, that the brain is an extremely complex structure and that it gives raise to a large variety of different motives of behavior. This is, after all, the deep reason why "psychology" is such a bewildering and elusive collection of phenomena, to the disgust of many economists. At a neuro-biological level, there is any reason to expect that human behavior is not the result of a narrow set of motives such as the ones identified by the rational choice paradigm. In other words, the true neuro-biological causal pathways of behavior are sufficiently complex to suggest that an a priori restriction to a parsimonious framework like rational choice in order to understand human behavior will introduce a high degree of diagnostic error. This, in turn, is at odds with an efficient accumulation of knowledge.

Overall, the discussion of this section leads to the following proposition:

Proposition 2 *Parsimony is not a universal requirement of a rational research strategy. In many cases, the strive for parsimony may be a source of error and prevent an efficient accumulation of knowledge.*

Why, then, do economists stress so much the need for parsimony? Rather than representing a feature of a collectively rational research strategy I view it as a form of non-Bayesian judgment. It may originate from a human desire for finding the "world formula."[11] This formula is supposed to explain the world by means of a small set of underlying principles. The dream of finding this small "magic" set of underlying principles is a very old one. In physics, it was given up at least about a century ago as it was discovered, that the underlying principle of matter would not just be atoms. Rather, atoms consist of electrons, protons and neutrons. In turn, these particles consist of a number of smaller particles. It seems that economists still hold the dream that the phenomena that interest them can be explained by a small number of fundamental principles: Preferences, constraints, and optimization.

Getting back to the discussion in the last subsection, there is nothing wrong with some economist teams pursuing a parsimonious approach in order to stimulate competition. However, it is not in the interest of an efficient accumulation of knowledge that the profession obstructs

[11] In the Faust drama of the German writer Goethe, Faust wants to know *was die Welt im Innersten zusammenhält.*

alternative attempts to understand economic phenomena that rely on a causality structure richer than just preferences, constraints, and optimization. This brings us to the topic of the final substantive section of this paper.

4 We Need more Competition

We like to preach the benefits of competition, as long as it does not concern ourselves. When it comes to our own business, we usually prefer monopolies. Economists are not so much different from business people in this respect. We economists praise the capitalist system because it fosters competition and competition is believed to foster discovery and hence progress. When it comes to economic research, things seem to be different. We explain to those who use an "excessive" amount of psychology in their models why their behavior is misguided and, in fact, harmful for scientific discovery. The supposed harmfulness arises from the fact that their theorizing apparently lacks scientific discipline. This, in turn, occurs because non-rational choice paradigms violate the non-arbitrariness and the parsimony doctrine. All this smacks somewhat of the talk of business people justifying a monopoly in their market. After all, many monopolies are claimed to serve the interest of society.

What can be done? I hope to have convinced the reader that the economics profession could produce knowledge more efficiently if we were to break the (near-) monopoly of rational choice and create an environment where different paradigms compete with each other. Note that this proposal is very different proposal from the one to abandon rational choice! The latter proposal would potentially be as foolish as banning research committed to non-rational choice.

There are two essential questions to be addressed at this point. The first question is about how competition between paradigm can be achieved in practice. The second question relates to the fact that in a "competition" there must, ultimately, be a potential winner. In other words, competition defines a game. An important question relates to what would be the rules of this game. In particular, how will it be decided who has won?

Concerning the first question, it has to be noted that competition can only work if it is backed by adequate institutions. Given the current situation, it is very hard to publish non-

rational choice research in prestigious international journals. However, publications in these journals are crucial for a successful career in academia. As a result, the incentives to adopt the non-rational choice paradigm are extremely low, especially for junior researchers.[12] Once the rational choice approach has been practiced during the PhD phase and the years of assistant-professorship it has become a habit for many. Given that breaking habits requires efforts and that the rewards for doing so are very low under current institutions, few do so.

It is therefore important to change the incentives. Here is a list of potential actions that may be taken.

- Editors of renowned journals may reserve a certain number of slots for papers that adopt alternative paradigms, provided that the papers adhere to the true standards of rational research (rather than the standards of "non-arbitrariness" and "parsimony".) The true leading standards of a rational research strategy are practical testability and a the logical coherence of arguments.[13] The quality of a paper must be judged according to these criteria.

- Grants from national research foundations and other sources may require the recipient to commit to a particular paradigm. In particular, several grants may be provided on the same topic but differing in terms of the required methodological approach. For instance, one may imagine different grants on the working of financial markets where one grant requires adoption of rational choice, another requires adoption of behavioral finance, a third one a historical approach etc. Gaining a grant may count for tenure in universities.

An important question is, of course, how to select the relevant paradigms. After all, competition just for the sake of having a variety of paradigms may not lead anywhere if the paradigms are simple invented because there is an incentive to come up with new paradigms. This may lead

[12]The astute reader may have noticed that the author of this paper believes that rational choice provides a good *partial* explanation of why so many economists adopt the rational choice paradigm. However, he also believes that many economists adopt the rational choice paradigm simply because they like to think about problems in the way their colleagues do. Thus, overall, he prefers a less parsimonious theory explaining the methodological choice of individual economists.

[13]After all, this paper tries to provide logically coherent arguments to make the point that non-arbitrariness and parsimony cannot be universal requirements for a rational research strategy.

to an industry of inventors of silly paradigms. One way to address this problem is that there is a "paradigm competition" where scholars can submit paradigm proposals. The proposals need to be justified by the standards of good logic. A jury of experts may then select a predefined number of proposals. The winning paradigms are then used for creating special sections in leading journals or for the definition of grant themes.

The mentioned measures have the disadvantage of all measures that are administered top-down. However, it is hard to see how a bottom-up approach may lead to a relatively quick change given that so much power is currently in the hands of adherents of rational choice. While a top-down approach may be need to initiate more competition between paradigms, it may not be used any more in the longer run, once the profession has found a new equilibrium with more diversity.

I turn now to the discussion of the "rules of the game." Competition requires that there is a score of who is doing better than others. How can we judge the success of different paradigms? At a general level, the answer to this question should be very familiar to economists. Ultimately, the success of a particular paradigm must be established empirically. A paradigm is more successful than another one if it explains more patterns in the data with more precision.

The natural sciences offer excellent examples how this competition works. For instance, the information contained in a cell that is passed to daughter cells during cell devision was originally thought to be contained in protein molecules (Gribbin, 2002). Then, a competing and $less\ parsimonious$ theory arose. It has hold that this information is contained in a different type of molecule (now identified as DNA). For quite some time, the issue was unsettled. During that time, an intense competition took place between the two paradigms. What were the rules of the game? How did the game manifest? It did manifest in that proponents of both paradigms collected data (in this case by conducting experiments) that were allowing for making inferences about the correctness of one or the other paradigm. In a nutshell, the rules of the game predominantly entailed data collection.

This directly bears on how competition should work between different economic paradigms. Proponents of different paradigms should engage in the gathering of data that allow to discriminate between the validity of different paradigms. Ideally, these data give some hint about the

underlying causal pathways giving raise to the phenomena of interest.

In economics, data collection has been outsourced all too often to government institutions or other bodies. Very often, economists complain about the data they use but use them anyway because of a lack of alternatives (Johnson et al., 2009). In our profession, gathering data is considered a job of low prestige since it means getting your hands dirty. It is considered much more prestigious to develop elaborate estimators that are used even when quality of the data may be quite questionable.

An effective competition between paradigms requires that the profession changes its mind with respect to data collection. Data should be collected by the very best researchers. Only high-quality data allow for inferring a reliable score about how different paradigms are faring. Since data collection is expensive, this requires shifting money from other uses to data collection. Again, it is conceivable to establish grants that are provided for the collection of data that allow for discriminating between different paradigms. A shift towards data collection also requires more research do be carried out on questions of measurement.

5 Conclusion

In this paper I have asked the question whether the fact that rational choice is a dominating paradigm in economics is in the interest of an efficient accumulation of knowledge. The reasons that are usually put forward for establishing the value of rational choice is that it offers a non-arbitrary benchmark guiding research and that it is particularly parsimonious. I have provided arguments that both justifications of rational choice do not stand up to scrutiny. Rather, in many situations a strive for non-arbitrariness and parsimony is likely to bring us off the production possibility frontier and prevents an efficient accumulation of knowledge.

The main conclusion of the analysis is that there should be more competition between different paradigms. This competition needs to be assured by appropriate institutions. Journals may offer reserved slots for particular paradigms. Grants may require the adoption of various paradigms. Finally, it is particularly important, that economists engage in the collection of data that allow to determine a success score for different paradigms. This may again be fostered by

specifically designed grants.

The purpose of this paper has been to stimulate discussion. Of course, many more thoughts should be spent on how to design appropriate institutions that encourage more competition between different paradigms in economics.

References

Beshears, John, Choi, James, Laibson, David, and Madrian, Brigitte (2008), "The Importance of Default Options for Retirement Saving Outcomes: Evidence from the United States," in: S. Kay and T. Sinha, eds., *Lessons from Pension Reform in the Americas*, Oxford: Oxford University Press, pp. 59-87.

Binswanger, Johannes, and Oechslin, Manuel (2009), "Beliefs, Political Institutions, and the Societal Learning Process," Working Paper.

Campbell, John (2006), "Household Finance," *Journal of Finance*, 61(4), pp. 1553-1604.

Caplan, Bryan (2007), *The Myth of the Rational Voter,* Princeton University Press: Princeton NJ.

Cohen L, Rothschild H, (1979), "The bandwagons of medicine," *Perspectives of Biological Medicine*, Summer, 531-538.

Friedman, Milton (1953), "The Methodology of Positive Economics," in: *Essays in Positive Economics*, Chicago University Press, 3-43.

Geisser, Seymour (1984), "On Prior Distributions for Binary Trails, *The American Statistician*, 38(4), 244-247.

Gernert, Dieter (2007), "Ockham's Razor and Its Improper Use," *Journal of Scientific Exploration*, 21(1), 135-140.

Gribbin, John (2002), *Science: A History*, London: Penguin Books.

Johnson, Simon, Larson, William, Papageorgiou, Chris, and Subramanian, Arvind (2009), "Is Newer Better? Penn World Table Revisions and Their Impact on Growth Estimates," NBER Working Paper 15455.

Kashyap, Anil K. (1995), Sticky Prices: New Evidence from Retail Catalogs," *Quarterly Journal of Economics*, 110, 245-274.

Keynes, John Maynard (1930), "Economic Possibilities for our Grandchildren," in: *Essays in Persuasion*, New York: Norton 1963, 358-373.

Kuhn, Thomas S. (1962), *The Structure of Scientific Revolutions*, Chicago: The University of Chicago Press.

Madrian, Brigitte C., Shea, Dennis F. (2001), "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior", *Quarterly Journal of Economics*, 116(4), 1149-1187.

Noelle-Neumann, Elisabeth (1974), "The spiral of silence: a theory of public opinion," *Journal of Communication*, Spring, 4352.

Sen, Amartya K. (1977), "Rational Fools: A Critique of the Behavioral Foundations of Economic Theory," *Philosophy and Public Affairs*, 6(4), 317-344.

Sober, Elliott (1990). "Let's Razor Ockham's Razor," in: Dudley Knowles, *Explanation and its Limits*, Cambridge: Cambridge University Press. pp. 7394.

Sober, Elliott (2002), "What is the Problem of Simplicity?" in: H. Keuzenkamp, M. McAleer, and A. Zellner (eds.), *Simplicity, Inference, and Econometric Modelling*, Cambridge University Press, 13-32.

Spiegel, Henry W. (1991), *The Growth of Economic Thought,* 3rd Edition, Durham and London: Duke University Press.