Allusions to evolution:

Edifying evolutionary biology rather than economic theory[1]

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Abstract

The 'selection arguments' advanced by Alchian (1950) and Friedman (1953) were clearly meant to boost confidence in neoclassical economics. But whereas Friedman didn't show the slightest reservation, Alchian warned economists "not to push their luck too far" in relying on the argument. Alchian's warning spurred some economists (such as Nelson and Winter) to engage with evolutionary theorizing in a more profound way, if only to find out under which sort of circumstances the "constrained maximization" framework of neoclassical economics is able to predict the outcomes of evolutionary processes. But this is not the path that subsequent generations of the Chicago School of Economics took. Friedman's reassuring reliance on the argument won the day. Instead of opening up economics to evolutionary theorizing, the second generation (with Tullock, Becker and Hirshleifer as important exponents) pleaded mainly for the reverse opening up of evolutionary biology to the "constrained maximization" framework of economic theory. To be sure there were suggestions that evolutionary biology (sociobiology, in particular) can enrich economic theory, especially by identifying what basic preferences people have. But the few attempts made in this direction once again amounted to demonstrations of the usefulness of "constrained maximization" in evolutionary biology.

[1] I want to thank Eric L. Charnov for providing helpful information (in private correspondence) about the history of optimality models in evolutionary biology (and in behavioral ecology in particular).

> The danger is that dabbling in sociobiology may prove to be more attractive to many economists than the use of sociobiological findings to improve our economics.
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> (Coase 1978, 245)

I. Introduction

In some of their papers, the second generation Chicago economists Gordon Tullock, Gary S. Becker and Jack Hirshleifer explicitly forge a link with evolutionary biology. This paper discusses these papers. I am interested mainly in two issues. One is whether these papers are in any way related to the so-called selection arguments that Alchian (1950) and Friedman (1953), as key exponents of the first generation of the Chicago School of economics, advanced earlier (Vromen 1995). Do the papers build upon the selection arguments, for example, or are they completely unrelated? The second issue is what specific forms the links forged with evolutionary biology take in the papers. In particular, what purposes are these links meant to serve? Are they meant to contribute to an improved economic theory, to an improved biological theory, or to both of them?

II. Alchian's (1950) and Friedman (1953)'s selection argument: From leaves around a tree to businessmen and back

A lot has been written about Friedman's (1953) selection argument. But for the purpose of this paper we can be very short. The crucial passage reads "… given natural selection, acceptance of the hypothesis [*viz.* the maximization-of-returns hypothesis] can be based largely on the judgment that it summarizes appropriately the conditions for survival." (Friedman 1953, 22). Friedman argues that economists can confide in their maximization-of-returns hypothesis because it accurately describes the behavior of surviving firms.[2] Unlike Alchian, who, as we shall see below, warned economists not to be over-confident about the applicability of the hypothesis on selectionist grounds, Friedman apparently did not see any reason for economists not to base their confidence in the hypothesis on such grounds. Friedman's main reason for forwarding his selection argument clearly was to restore or boost confidence in the maximization-of-returns hypothesis (or, more broadly, marginal analysis). As we shall see in the next sections, second-generation Chicago economists such as Tullock and Becker take the validity of this argument for granted not so much as a justification for their continued use of marginalist analysis in economics, but in their excursions into evolutionary biology. After all, Friedman's argument seems to imply that maximization-hypotheses accurately describes of predicts the outcomes of selection processes not only in the economic realm but in any realm. In fact, in developing his argument that the maximization-of-returns hypothesis accurately describes the outcome of economic market selection, Friedman discusses the potential usefulness of a maximization-hypothesis to predict behavior in the biological realm. What is interesting is that Friedman himself recoiled from urging biologists (and broader, evolutionary theorists) to accept maximization hypotheses.

Based on a study of earlier drafts of Friedman's (1953) essay, Hammond (2004) convincingly argues that Alchian and Friedman arrived at the idea to provide selection argument independently from one another. It is clear that even though Alchian (1950) predates Frieman (1953), earlier drafts of Friedman, written before Friedman read (earlier drafts of) Alchian's paper, already contained roughly the same formulation of the selection argument as the one that finally made it to print. What is less clear is whether or not there has been a reverse influence of Friedman on Alchian. Based on a letter that Alchian sent to Friedman in 1949 (that accompanied the revision of his 1950 paper, that Friedman – in his position of *JPE* editor - demanded from Alchian), Philip Mirowski suggests that Friedman used his power as *JPE* editor to make Alchian rewrite his paper more clearly in line with Friedman's own unflinching defense of neoclassical economics. Whether or not Alchian's earlier version was more critical of neoclassical economics than

[2] Vromen (2008) contains a more elaborate discussion of Friedman's selection argument, of the argument's place in Friedman's methodology and of recent insights about the argument's validity.

the published (1950) version, we cannot tell on the available evidence. We would at any rate need to see earlier drafts of Alchian's paper to find out what changes he made in subsequent versions. What might support Mirowski's reading, however, is that Alchian's (alleged) friend and RAND colleague Stephen Enke took the main message of Alchian's (1950) allusions to market selection to be rather different than what Friedman wanted to read into it: rather than providing marginal analysis with a more secure theoretical foundation, marginal analysis should be superseded by what Enke (1951) calls 'viability analysis'. Instead of continuing with marginal analyses, economic analysis should try to find out what sort of firm behavior is viable under various conditions. Enke's main reason for pleading for a replacement of marginal by viability analysis is that he is not confident that marginal analysis and viability analysis lead to the same predictions under a wide set of conditions. As we will see below, Alchian himself indeed was far from being perfectly confident about that too.

Alchian's stance towards marginal analysis is somewhat ambivalent even in the final published (1950) version. In the marginalism controversy he clearly sides with the marginalists. The counterevidence amassed by the anti-marginalists, Alchian argues, need not and does not invalidate marginal analysis. Empirical investigations via questionnaire methods are incapable of evaluating the validity of marginal productivity analysis. If there is one thing Alchian sets out to do in the paper, it is to show that "… individual motivation and foresight, while sufficient, are not necessary" (ibid, 217) for marginal analysis to be useful in making predictions about industry behavior. Marginal analysis can be useful even if businessmen do not pursue profits, or (in the, also for Alchian more likely situation) if businessmen do try to attain maximum profits but lack the foresight that would guarantee success in such attempts. As Alchian himself puts it in a footnote, "This approach reveals how the "facts" of Lester's dispute with Machlup can be handled with standard economic tools." (ibid, 217)

Thus Alchian is squarely in the marginalist's camp. But there are also statements suggesting that economists should not lean back and rest content with marginal analysis. He argues for example, that "Most conventional economic tools and concepts are still useful, although in a vastly different analytical framework – one which is closely akin to the theory of biological evolution" (Alchian 1950, 219-20) and "The formalization of this approach awaits the marriage of the theory of stochastic processes and economics" (ibid, 221). Apparently there is still some work to do for economists. Far from telling economists that they can marvel at and acquiesce in the undiminished predictive powers of marginal analysis, Alchian here seems to envisage the development of a new analytical framework or approach of which marginal analysis is part at most.

Even though Alchian does not exhibit a more thorough-going understanding of evolutionary biology than Friedman, he at least engages in an attempt to think things through in more depth and detail. Unlike Friedman, who simply does not go into this, Alchian asks himself what would or could be plausible counterparts of key biological concepts in economics. His answer: "The economic counterparts of genetic heredity, mutations, and natural selection are imitation, innovation, and positive profits." (ibid, 220) can be said to present the point of departure for Nelson and Winter's (1982) attempt to develop an explicit evolutionary alternative for 'orthodox economics'. Alchian furthermore displays a more profound understanding than Friedman that evolutionary theorizing entails population-thinking (Mayr 1982, Metcalfe 1989). Contrary to Friedman, who argues that every surviving firm cannot be but maximizing its returns, Alchian more cautiously argues that the only thing that can be predicted is that there is a tendency in the behavior of firms in some industry to go in this direction: "The prediction will not assert that every – or, indeed, any – firm necessarily changes its characteristics. It asserts, instead, that the characteristics of the new *set* of firms, or possibly a set of new firms, will change." (ibid, 216) Finally, and most importantly, Alchian shows a greater awareness than Friedman of the fact that evolutionary processes produce the outcomes that marginal analysis predicts only if certain conditions are met. Alchian notes correctly, for example, that "… unfortunately" (ibid, 219) trial-and-error processes (in firm behavior) need not converge on a "profit maximizing" equilibrium. In particular, if the environment changes, the comparability of the actual results of the trials is destroyed. Alchian also rightly draws attention to the fact that "what really counts is the various actions actually tried" (Alchian 1950, 220) and there is no guarantee that the best (or optimum) action is among the set of various actions actually tried. For reasons like this, Alchian concludes: “The economist may be pushing his luck too far in arguing that actions in response to changes in environment and changes in satisfaction with the existing states of affairs will converge as a result of adaptation or adoption toward the optimum action that should have been selected, if foresight had been perfect.” (ibid, 220).

Al these finesses and subtleties were lost on Friedman. It seems Friedman did not really make an effort to get the similarities and dissimilarities between biology and economics right. As remarked earlier, the only thing Friedman seemed to be interested in was to restore and boost confidence in maximization hypotheses and marginal analyses in economics. But unlike later followers of Friedman’s selection argument, who thought that Friedman’s selection argument could be generalized so that maximization hypotheses could be applied also to predict outcomes of selection processes in the biological realm, Friedman recoiled from advocating maximization hypotheses in the biological realm. This is remarkable, because in his essay Friedman does argue that maximization hypotheses can accurately predict surviving behavior also in the biological realm.

In preparing the ground for his argument that only those businessmen survive market selection that behave as if they are maximizing returns, Friedman discusses not just the example of the expert billiard player, but also (before that) the “largely parallel” example of trees having a greater density of leaves on the sunny south side than on other sides (Friedman 1953, 19-20).[3] This typical behavior of leaves, Friedman argues, can be predicted on the basis of the hypothesis that leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives. The main point Friedman wants to make here of course is once again that the objection that leaves do not really seek in any meaningful sense, do not deliberate where to be positioned, and lack the competences to calculate the “optimum” position, does not invalidate the hypothesis. Properly understood, Friedman’s hypothesis does not imply that leaves actually do these

[3] Whether these two examples are really largely parallel remains to be seen. In the example of the expert billiard player, Friedman simply takes for granted that the playing of the expert billiard player is perfect (or approximates perfection). No allusion is made to some sort of selection process (such as tough competition) responsible for this.

things (they only behave as if they did these things). Thus the objection is misdirected and irrelevant. What the hypothesis does imply is clearly consistent with experience and *this* is what is relevant.

So far so good. But the interesting thing is that Friedman does not go so far as to suggest that his contrived 'maximization-of-sunlight' hypothesis should be seriously entertained. Why not? Friedman does argue that the "largely parallel" 'maximization-of-returns hypothesis should be seriously entertained. The arguments that he brings to the fore to support the "maximization-of-returns" hypothesis seem to carry over immediately to the "maximization-of-sunlight" hypothesis. We have already seen that Friedman does not regard the objection that businessmen lack the information or competence to really calculate maximum profits as a sound counter-argument. It is clear that leaves lack such a competence. But if such a lack (on the part of businessmen) does not undermine the 'maximization-of-returns' hypothesis, then the fact that leaves do not have such as competence does not undermine the 'maximization-of-sunlight' hypothesis either. The same reasoning applies to the incapability of leaves to seek and to deliberate, as Friedman himself argues. This too does not undermine the 'maximization-of-sunlight' hypothesis. Furthermore, in his selection argument Friedman argues that confidence in the 'maximization-of-returns' hypothesis is based on the fact that there is a selection process going on in markets and that the hypothesis accurately predicts the behavior of surviving firms. It is clear that Friedman believes that similar selection processes see to it that leaves behave as if they maximize sunlight: "… the result achieved by purely passive adaptation to external circumstances is the same as the result that would be achieved by deliberate accommodation to them" (ibid, 20).[4] Thus virtually all the arguments that Friedman gathers in support of the 'maximization-of-returns' hypothesis seem to apply with equal force in support of the 'maximization-of-sunlight' hypothesis.

[4] In footnote 14 on page 19, Friedman rightly observes that this example is similar to and in much the same spirit ("…though independent in origin") as an example given in Alchian (1950). Alchian discusses the example to underscore one of his main points, namely that what looks like the result of motivational individual adapting sometimes actually is the result of environmental adoption.

What prevents Friedman from concluding that the “maximization-of-sunlight” hypothesis is as valid and useful as the ‘maximization-of-returns’ hypothesis is that there is an alternative hypothesis (that ‘explains’ and predicts the differential density of leaves around trees as a result achieved by purely passive adaptation to external circumstances; let’s call this an adaptationist hypothesis)[5] available that is to be preferred on both theoretical and empirical grounds:

> This alternative hypothesis is more attractive than the constructed hypothesis not because its “assumptions” are more “realistic” but rather because it is part of a more general theory that applies to a wider variety of phenomena, of which the position of leaves around a tree is a special case, has more implications capable of being contradicted, and has failed to be contradicted under a wider variety of circumstances. The direct evidence for the growth of leaves is in this way strengthened by the indirect evidence from the other phenomena to which the more general theory applies
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> (Friedman 1953, 20)

Apparently Friedman believes that the alternative adaptationist hypothesis is part of a more general selection (or evolutionary) theory than the ‘maximization-of-sunlight’ hypothesis and that this more general theory has more implications capable of being contradicted. Friedman does not tell us why the ‘maximization-of-sunlight’ hypothesis could not be part of an equally (or even more) general ‘maximization’ theory than selection (or evolutionary) theory that is equally (or even more) capable of being contradicted and that will fail to be contradicted under an equally wide (or even wider) variety of circumstances. Why couldn’t the economic maximization framework be applied beyond the economic realm? That would make for a very general theory indeed. It seems that the real argument for Friedman not to propose this is a theoretically

[5] Friedman does not display a thorough understanding of evolutionary theory in these passages. It is true that environmental factors (such as the exposure to sunlight) determine the position and differential density of leaves around trees. But this is not a selection, but a developmental process. It might be the case that natural selection has favored trees in which the positioning of leaves responds to the relative availability of sunlight. But how the leaves are positioned for each individual tree is a matter of how the tree, given the tree’s genetic material, responds to environmental circumstances in its developmental growth process.

conservative one: general selection theory has already proven its worth and value in the respects mentioned and it is because of this that it is more attractive than Friedman's constructed hypothesis.

This impression, that it is for theoretically conservative reasons that Friedman recoils from proposing to use maximization hypotheses across the board, is strengthened by the "… even more important body of evidence" (ibid, 22) that Friedman brings to the fore to support the 'maximization-of-returns' hypothesis immediately after his exposition of his selection argument. There too we find references to the generality of the hypothesis (this time the 'maximization-of-returns' hypothesis) as a major theoretical virtue of the hypothesis and to direct and indirect evidence for the repeated failure of the hypothesis' implications to be contradicted (as empirical support). What Friedman here adds to the story, however, is that "… the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect testimony to its worth" (ibid, 23). So the fact that the 'maximization-of-returns' hypothesis *de facto* is widely accepted in the economics discipline and has been the only game in town for quite a while is taken as evidence for the acceptability of the hypothesis in economics.

Whether this is a sound argument or not,[6] it is this argument, it seems, that is decisive for Friedman why biologists should stick to their well-entrenched adaptationist hypotheses.[7] Apart from this argument, it is hard to see why biologists (or evolutionary theorists in general) should not entertain maximization hypotheses such as the 'maximization-of-sunlight' hypothesis seriously. Just think of what Friedman could have argued were it not for this theoretical conservative stand. He could have argued that since maximization hypotheses accurately describe (or predict) the outcomes of selection processes, maximization hypotheses can also be usefully deployed outside what is traditionally

[6] Note that thus formulated, Friedman's argument here comes close to (if not is identical to) the notorious naturalistic fallacy.

[7] As Steven Medema points out in his contribution to this volume, Friedman might also have taken over Frank Knight's view that the applicability of economic theory is quite limited. Friedman's 1976 edition of *Price Theory* mentions Becker's work on the economics of the household and human capital only in passing.

considered to be the economic domain, in particular in the biological domain. But he could also have argued for the opposite claim that adaptationist hypotheses can also be usefully deployed outside what is traditionally considered to be the biological domain, in particular in the economic domain. This is not what Friedman argues. The uses of adaptationist hypotheses and maximization hypotheses should be kept to their own traditional domains. Thus Friedman arguably shows some respect for what practitioners in other disciplines have done and achieved in their own domain.

Friedman recoils from urging biologists (and broader, evolutionary theorists) to accept maximization hypotheses instead of their familiar adaptationist hypotheses. As we shall see, the second generation proved considerably less reticent when it comes to promoting maximization hypotheses in evolutionary biology.

III. Gordon Tullock's excursions into evolutionary biology

Gordon Tullock sees absolutely no reason not to apply maximization hypotheses in studying the behavior of non-human animals. On the contrary, his version of *bioeconomics* is one in which the use of the constrained maximization framework of standard economic theory in evolutionary biology is demonstrated and actively promoted Vromen 2007).[8] Occasional rhetoric allusions to bioeconomics being a two-way transfer of ideas, concepts and techniques between economics and biology notwithstanding, if his own work is to exemplify bioeconomics, then bioeconomics essentially is a one-way transfer of modeling techniques from economics to biology. There is nothing really in Tullock's writings suggesting that economics could learn something from (or could be enriched by) biology.[9] As Tullock remarks about his own "biological" articles, "indeed, it

[8] Gordon Tullock was one of the Founders (Jack Hirshleifer also was one of the Founders) of the International Society for Bioeconomics. The Society sponsors the *The Journal of Bioeconomics*, which was launched in 1999. As Landa and Ghiselin (1999) officially declare in the first issue of the Journal, bioeconomics encourages a mutually beneficial two-way transfer of ideas, concepts and techniques between biology and economics.

[9] See, e.g., "Unfortunately, I cannot claim that reading the book will add anything more to the usual economists' tool kit." (Tullock 1994, 4)

could be argued that I have never left economics, that all of my "biological" articles are simply economics articles in which I have rather unusual sets of entities maximizing a rather unusual utility function" (Tullock 1979, 2).

Looking back on his own early excursions into biology, Tullock (1999) points out that Malthus's views on population growth and the (natural and artificial) checks on it had a great impact on the evolutionary thinking of both Darwin and Wallace. In fact, Malthus's gloomy views had a greater, more lasting impact on biology than it had on economics. Tullock suggests that there are good reasons for this. Non-human animals lack the sophisticated cognitive capacities that humans exercise to reverse the trend that the rate of growth in resources lags behind the natural rate of growth of the human population: "non-humans do not make technological progress. They do not practice artificial birth control" (Tullock 1999, 14). In other words, the economic 'constrained maximization' framework might be more suitable to describe and predict non-human behavior than it is to describe and predict human (economic) behavior, not despite, but rather precisely because of the fact that non-humans are less smart than humans.

After observing that it is no coincidence that in the first six decades of the twentieth century biology and economics proceeded more or less independently, Tullock argues that it could not remain unnoticed that "… structurally the two fields had much in common. In both cases the basic problem was maximizing subject to a constraints" (Tullock 1999, 15).[10] Economists were earlier than biologists to discover that this is the basic problem in their field. It took biologists more time to find this out. Actually, it took some economists to point out that 'constrained maximization' is the basic problem also in biology before this dawned on biologists. And one of these economists was (not surprisingly) Tullock himself.

One of Tullock's earliest excursions into biology, Tullock (1971a), is an illustration of standard economic analysis' fruitfulness for the study of biological phenomena (in this specific case the consumption of the eucosmid moth *Ernarmonia conicolana* by coal tits).

[10] I guess the "…a" before "… constraints" here is a typo.

Tullock shows that the existing explanation of the coal tits' consumption behaviour in biology can be greatly simplified by treating coal tits as careful optimising shoppers.[11] This analogical treatment of the coal tits' consumption behaviour is warranted, Tullock argues, because it makes sense to assume that coal tits have inherited reasonably efficient patterns of behaviour. After all, if their ancestors did not evolve such patterns, they would not have survived and have left offspring. This clearly echoes Friedman's assurance that maximization hypotheses accurately describe the behavior of individuals that survive selection processes. There is nothing that economists learn from this transfer of their constrained maximization framework to biology, it seems, or it should be that economic analysis can also be fruitfully applied in the biological realm. Economic analysis itself, whether it is applied in the (traditional) economic realm or elsewhere, is not enriched or improved by it.[12]

Roughly the same picture surfaces in Tullock (1994), aptly called *The Economics of Non-Human Societies*. Although this little book is published a few decades after Tullock's earliest excursions into biology appeared, it actually is based on an unpublished book manuscript (called *Coordination without Command: The Organization of Insect Societies*; "… which was an effort to use economic tools to analyze the internal social structure of ants and termites and a few other species"; Tullock 1994, vii) that Tullock put together in the seventies of last century. Here too Tullock engages in economic analyses of the nests of ants (and of other social insects). To Tullock, this means first and foremost that *preference functions* are ascribed to animals: "I assume that the animals, plants, ameboid single-cells of the sponges, and the "individual" cells of the slime molds all have something which is functionally equivalent to the preference function that we find in human beings" (ibid, 72). Tullock points out that in non-human societies cooperation is induced by a preference function and environmental coordination. It is to be noted that Tullock does not postulate the existence of markets in non-human societies. It is only in human societies that we have both hierarchies and the market, he argues; in

[11] Note that Tullock is not arguing here that application of standard economic theory's constrained maximization framework in biology necessarily leads to different results or insights. The main gain envisioned is increased simplicity in explanation.
[12] Tullock (1979) argues that there is not much for economists to learn about human society from sociobiological studies of animal societies.

most non-human societies we have neither of them. But Tullock does argue that in human and non-human societies alike, central command is not necessary to get cooperation off the ground.[13]

Tullock (1999) reports that he contributed more or less accidentally to biology. Tullock recalls that while reading Lack (1966) on birds, he expected to find, but didn't find, an exposition in terms of demand and supply curves. It was obvious to Tullock not only that Lack's account of the birds' behavior lent itself to an exposition in terms of demand and supply curves, but also that such an exposition would make Lack's account much simpler and clearer. Indeed, this was so obvious to Tullock that he had to be persuaded to work it out in a paper for a renowned biological journal.[14] It was not his deepest paper, Tullock wryly remarks, but he was prepared to render this service to biologists: "Biologists were looking around for something like this, and I provided it" (Tullock 1999, 16). Tullock (1999) still shows amazement about the success the paper had in terms of citations and quotations biologists subsequently made to the paper.[15]

Does Tullock really believe that there was no one before him to notice the usefulness of the 'economic' constrained maximization framework for studying animal behavior? Presumably he does, but the belief is clearly false. Levins (1968) and MacArthur (1965) pre-dated Tullock (1971), for example. Biologists did not have to wait for Tullock to show them "the way". What is more, some of the papers that made the connection earlier have been quoted more often than Tullock's own 1971 paper. And there surely are many papers published after Tullock (1971) that exploited the constrained maximization framework in studying animal behavior and that are quoted much more often than Tullock (1971a).[16] In Parker's (2006) recent overview of the history of behavioral

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[13] Tullock adds: "I do not want my fellow members of the Mont Pelerin Society to feel that I have forgotten the possibility of coordinating activity by market arrangements" (Tullock 1994, 23).

[14] Note how similar this narrative is to Alchian's (1982) narrative about the history of his own 1950 paper.

[15] But note also that at other places Tullock reveals his disappointment about the fact that biologists did not seem to be interested in some of his other work ("Unfortunately, no biologist, not even Wilson, was interested", Tullock 1994, viii).

[16] As Eric L. Charnov writes: "his [Tullock 1971a] paper, known to me while still in grad school, (though published in the worlds top ecol/evol journal) has only been cited ~60 times; influential optimal foraging

ecology, Tullock is not even mentioned once (and for that matter, Becker or Hirshleifer are not mentioned either).[17] Thus Tullock seems to overrate the impact his own work had on biology. Presumably, Tullock simply is unaware of all the other attempts to get the constrained maximization framework accepted in the biology profession. Tullock clearly did not read very extensively and deeply into the biological literature. And this in turn suggests that he was not really interested in acquiring a profound understanding of biology. What he was really interested in, it seems, is in demonstrating the vast potential of the constrained maximization framework for studying behavior not only within but also outside the economic realm.

IV. Gary S. Becker's Rotten Kid

There is no doubt that Becker subscribes to Tullock's view that 'the economic toolkit', which for Becker mainly implies the assumption of constrained maximization for individual behavior, of equilibrium for social, aggregate behavior and of stable preferences, is pre-eminently suited to describe and predict outcomes of evolutionary processes. In fact, Becker (1962) was evidently inspired by and written in the same spirit as Friedman's (1953) selection argument. The great achievement of this argument, Becker argues, is that it shows that the decisions of irrational firms are limited by a budget constraint. Firms that continually fail to make positive profits will eventually "run out of gas". This is why market responses tend to be rational even in the face of irrational firm behavior.

In Becker (1976), however, Becker seems to want to go beyond this. There he argues that evolution should mean more to economists than just "… an occasional appendage" to the main body of economic analysis (Becker 1976, 818). There is more to learn for economists from evolution than that maximizers tend to be selected both in economic and

papers from the same era[+/- 5 yrs] are at ~500-1500 citations, and owe nothing to Tullock even for inspiration" (personal communication).

[17] Becker and Hirshleifer are not mentioned either. By contrast, the work of John Nash and of Peter Hammerstein is referred to.

biological systems (ibid). Evolution does not merely support economic analysis, it can also, and more importantly, add something to economic analysis. Becker argues that economics would gain from combining the techniques in population genetics, entomology, and other biological foundations of sociobiology with the analytical techniques of economists (ibid, 826). More specifically, what economics could gain from taking the techniques from the biological foundations of sociobiology into account is that it could help economists with specifying what tastes or preferences people have. Economists generally take preferences as given; sociobiology holds out the promise of explaining not only what preferences people have, but also why they have them.

Apparently, Becker is looking for how economists can incorporate insights into how preferences evolved genetically into their theories and analyses. Several statements in Becker (1976) indicate that Becker takes sociobiology (and the analytical techniques developed in the subdisciplines in biology underlying sociobiology) to be the main source or supplier of these insights. But upon closer inspection it turns out that this is not the case. The official rhetoric in Becker (1976) is one of economics and sociobiology mutually informing and enriching each other. But what is actually argued for is that Becker's own economic model of altruism is superior to and improves on existing sociobiologist's 'group selectionist' accounts of altruism. Rather than pleading for a two-way transfer of techniques and insights from biology to economics, the main message is that biologists have something to learn from economic analysis rather than the other way around.

Let us have a closer look at the central argument in Becker's paper. What Becker sets out to show is that an altruistic individual (call him 'Big Daddy') can attain higher personal fitness than an (otherwise equally able) egoistic individual, despite the fact that the altruistic individual is willing to increase the fitness of another individual (call him 'Rotten Kid) at the expense of his own personal fitness.[18] Becker shows that the direct fitness' disadvantages of Big Daddy's altruism may be more than compensated for by the

[18] 'Rotten Kid' is named after Becker's (1974) 'Rotten-Kid Theorem'. The persona of 'Big Daddy' was introduced in Hirshleifer's (1976) commentary on Becker (1976).

beneficial indirect effects on the behavior of Rotten Kid (ibid, 820). If Rotten Kid knows about Big Daddy's altruism (and can correctly anticipate Big Daddy's reactions), then this knowledge might entice Rotten Kid to behave differently than how Kid would behave if it had an egoistic Daddy (or if it lacked this knowledge). If indeed Rotten Kid is induced to behave differently (more 'cooperatively') then Big Daddy, even if he forgoes some of his own personal fitness to raise the personal fitness of Rotten Kid, can end up having a fitness that exceeds that of an (equally able) egoistic individual.

Thus Becker's (1974) so-called *Rotten-Kid Theorem* is a crucial part of the argument here: egoistic Rotten Kid behaves as if it too were altruistic because it is in his own interest to do so. Egoistic Rotten Kid figures out that it is eventually better off if it does not settle immediately for the seemingly best possible outcome but instead leaves something to altruistic Big Daddy. For it correctly anticipates that this initial loss is more than offset by Big Daddy by transferring part of the income left to him by Rotten Kid to Rotten Kid. This is why Rotten Kid behaves *as if* it, like Big Daddy, too were altruistic. It seems that Rotten Kid, like Big Daddy, is concerned about social income (or about 'group fitness'), whereas all it is concerned with is its own interest (its own personal fitness). It is the special configuration of *social interaction* that makes Rotten Kid behave as if it were altruistic.

What exactly is the contribution of sociobiology to this argument? Nothing at all! As Becker himself remarks, Becker's model is a purely economic one (Becker 1976, 818, 825), uncontaminated by alien elements from other disciplines. Instead of adopting one the sociobiological models or explanations of altruism, Becker develops his own economic model. Becker presents sociobiological accounts of the evolution of altruism not as supplements or complements to economic analysis, but as unnecessary (ibid, 818) and incomplete (ibid, 819) accounts that are to be superseded by his own superior, more parsimonious and fuller economic model.

In fact, as already noted and as Becker himself does not conceal, Becker's economic model of altruism is a variant of Becker (1974). Becker (1974) is an attempt to use simple tools of economic theory to analyze social interactions in general and intra-family behavior in particular. The only differences with the model presented in Becker (1974) are that the arguments in the utility functions posited in Becker (1974) – consumption, of the acting agent and possibly also of another agent – are substituted by fitness and that "shadow prices" are posited instead of market prices. Becker argues that, suitably reinterpreted, all the results obtained in Becker (1974) (such as the result that "… the existence of a head economizes on the amount of true love required in a family", Becker 1974, 1091) carry over to the evolution of altruism: "The important point is that all the earlier results on the consumption of goods apply equally to this analysis of fitness" (Becker 1976, 823).

Does Becker seriously think that his economic model of altruism tells us something about how our altruistic impulses and preferences (if any) actually evolved way back in history? This is hard to tell. Becker's model seems to presuppose that individuals are endowed with quite sophisticated cognitive capacities. How else could Rotten Kid correctly anticipate what Big Daddy is going to do? As we have just seen, it is a crucial part in Becker's argument that Rotten Kid is capable of doing this. Is it reasonable to assume that during the time that our basic preferences evolved, our ancestors were already equipped with such cognitive capacities? Perhaps it is telling that Becker does not go into this.

Becker wants to contribute to solving what Wilson called "the central theoretical problem of sociobiology [is]: how can altruism, which by definition reduces personal fitness, possibly evolve by natural selection?" (Wilson 1975, 12; quoted in Becker 1976, 818). And, indeed, Becker treats this problem not as an empirical but as a theoretical problem that calls for a theoretical solution that has to meet certain desiderata (or that has to display certain theoretical virtues). If possible, reference to dubious notions such as 'group selection' are to be avoided, for example. One of the things Becker argues is that sociobiological models of 'group' selection are unnecessary. He wants to point out that economic models of individual rationality (constrained maximization) in elementary situations of physical and social interaction suffice to explain altruism. Furthermore, if possible the theoretical solution should be general. Becker argues that his model is more general than kinship models because the individuals in his models need not have genes in common with one another. Becker's own analysis extends beyond kin toward unrelated neighbors and coworkers. It is on these *theoretical grounds* that Becker believes his economic model is superior to existing sociobiological models. Whether his model also gives a more realistic representation of how altruism actually evolved is an altogether different matter.

Thus Becker does not seem to be very interested in how altruism actually evolved. He does not seem to be very interested in what actually is done in sociobiology either.[19] Becker argues that the well-known kin selection model in sociobiology is a variant of a wider set of group selection models (Becker 1976, 818). If Becker had read the relevant literature more extensively and more carefully, he would have noticed that evolutionary biologists treat kin selection and group selection as perfect opposites of each other! Kin selection models are seen and were also originally presented by Hamilton as explanations of how altruism and cooperation could evolve without group selection. The idea is that individuals can enhance their 'inclusive fitness' by helping their genetic relatives (where the degree of 'sacrifice' should be proportional to the degree of genetic relatedness). There is no group selection going on here in any meaningful sense.[20] Indeed, kin selection models were among the models that prompted Richard Dawkins (1976) to single out the gene as the unit of selection in Darwinian biological evolution.

Or take the very notion of altruism itself. Becker criticizes Wilson for confusing two notions of altruism (Becker 1976, 824). The one notion is that altruism by definition reduces personal fitness, the other is that an act is altruistic when a person (or animal) increase the fitness of another at the expense of his own fitness. Becker claims that he has shown that the latter notion does not imply the former: "Using the latter definition, I have shown that altruism may actually increase personal fitness because of its effect on the

[19] Only the usual suspects (Trivers 1974 and Wilson 1975) are referred to in the References.
[20] This was at least how it was generally conceived of by biologists by the time Becker wrote his paper. Recently, Sober and Wilson (1998) have argued that there is group selection in kin selection models, but this argument is contested in the community of evolutionary scholars.

behavior of others" (ibid, 824). Becker is right that he has shown that the personal fitness of an individual may increase (via indirect beneficial effects) even if the individual increases the fitness of another at the expense of his own personal fitness. But Becker is wrong in asserting that he uses the latter definition of 'altruism' in showing this. Note that the second (latter) definition is purely in terms of behavior and its actual fitness consequences. It is completely silent on what motivates the person to engage in an altruistic act. This is the form of (evolutionary) altruism evolutionary biologists are interested in. By contrast, in his own economic model of altruism Becker defines an altruist in terms of a particular sort of motivation (or 'willingness'): "By definition, an altruist is willing to reduce his own consumption in order to increase the consumption of others" (ibid, 818). In Becker's model, not acts or behavior are altruistic (or not), but persons, or more precisely, the preferences of persons (on which they are assumed to act). This is a form of (psychological) altruism that most social scientists are interested in. If Becker had taken the trouble to look carefully into sociobiology, if he had noticed the difference between Wilson's second definition of altruism and his own, then he should have seen that in his own model Rotten Kid not merely behaves as if it were altruistic, as Becker himself argues, but behaves genuinely altruistically (on Wilson's second definition). If the behavior of Big Daddy is truly altruistic on the ground that he forgoes personal fitness and increases Rotten Kid's personal fitness (and despite the fact that, ultimately, his personal fitness increases), then Rotten Kid's behavior is equally truly altruistic. Rotten Kid also forgoes personal fitness and increases Big Daddy's personal fitness.

That Becker failed to see the latter can be called a serious omission. As one of Becker's compatriots, Jack Hirshleifer, never got tired of reminding us, behavior never is the outcome of preferences alone; behavior is always the outcome of preferences in combination with constraints (or opportunities). Indeed, this seems to be one of the main lessons of the Rotten-Kid Theorem: given the right circumstances, Rotten Kid can be induced to behave altruistically (or as if it were altruistic, depending on what definition one adopts). Just as selfish Rotten Kid can be induced to behave as if it were altruistic, altruistic Big Daddy can in principle be induced to behave as if he were selfish (i.e., increasing his own personal fitness and decreasing personal fitness of Rotten Kid). But before we turn to Hirshleifer, let us first take stock.

Despite his statements to the effect that *both* economics and sociobiology are to benefit from the combination of their respective analytical frameworks, what Becker really argues for is that sociobiological analyses of altruism can benefit from taking the 'constrained maximization' framework from economics on board. What is sold as a mutually beneficial two-way transfer of analytical frameworks turns out to be a one-way transfer of the analytical framework from economics to sociobiology from which only sociobiology is to profit. Becker does not really or seriously take recourse to existing sociobiological studies or analyses in order to find out what arguments to posit in utility functions. In fact, Becker does not show a real interest in what (socio)biology could offer to economists at all. Instead, what he is mostly interested in is in showing how standard economic analysis can improve on sociobiological analyses of the evolution of altruism.

V. Jack Hirshleifer's interests in evolutionary biology

Things are surely different with Hirshleifer. Unlike Tullock and Becker, who did not take a great interest in evolutionary biology and whose understanding of evolutionary biology was accordingly poor, Jack Hirshleifer undeniably displayed a greater interest in evolutionary biology and also had a deeper understanding of it. A cursory glance at Hirshleifer's lengthy and insightful early papers (Hirshleifer 1977, 1982) suffices to see that Hirshleifer read much more of evolutionary biology than Tullock and Becker presumably ever did. There is a much richer and better-informed discussion of relevant literature in evolutionary biology than one can find in the writings of Tullock's and Becker's writings. Although the selection biological papers and books listed in the 'References' section in Hirshleifer's papers is once again skewed towards the 'adaptationist' and sociobiological camp in evolutionary biology (such as the work of Alexander, Hamilton, Maynard Smith, Charnov, Lotka, Trivers and Ghiselin; there is no single reference to Gould and Lewontin 1979, for example),[21] one can also find sympathetic discussions of (and references to) works such as Nelson and Winter (1982) and Boyd and Richerson (1985).

Hirshleifer displays a serious interest, in a much more profound way and in much greater detail than Tullock and Becker ever did, in what credible economic analogs and counterparts could be of key concepts and notions in (evolutionary) biology. In Hirshleifer (1977), one of the issues Hirshleifer is struggling with is whether organisms or genes in biological systems should be viewed as the proper analogs of the fundamental acting unit in economic systems, the individual. If genes are the proper analogs, then firms in economic systems are perhaps the proper analogs of organisms in the biological system. Issues like these have also been occupying economists who, like Richard Nelson and Sidney Winter (and, more recently, Geoff Hodgson), tried to find out to what extent processes of economic evolution can be seen as analogous to processes of biological evolution.

There is also a lot Hirshleifer has in common with Tullock and especially with Becker, however. Just like Tullock and Becker does Hirshleifer more or less take for granted that a generalized version of the thrust of Friedman's selection argument holds true:

> What happens in the biological realm is that, given a sufficiently long run, *natural selection under Malthusian competition* allows survival only of the most successful among the possible strategies. So the result ends up almost *as if* baboons or rats or crabgrass plants were consciously optimizing.
>
> Hirshleifer 1987, 171-172)

Like Tullock and Becker, but unlike Friedman himself, Hirshleifer has not the slightest reservation or hesitation to use the constrained maximization framework of economic theory to describe the behavior of surviving organisms. What biologists can learn from

[21] Only in Hirshleifer (1985) we find a reference to Lewontin (1979) and a few statements that assuming that surviving organisms behave *as if* they maximize fitness is somewhat controversial in evolutionary biology.

economic theory in particular, Hirshleifer consistently argues, is that behavior never is fully determined by an organism's 'preferences' (or instincts, drives or dispositions) alone, but always also by the prevailing constraints and opportunities.

What Hirshleifer additionally shares with Tullock and Becker is nicely illustrated in Hirshleifer (1978). Hirshleifer (1978) elaborates on Ghiselin's (1978) proposal to regard biology as natural economy, the social sciences (and economics in particular) as political economy and to regard both as branches of general economy. Hirshleifer agrees with Ghiselin that the existence of institutions such as law, government and property in political economy marks a crucial difference with natural economy (in which there are no such institutions). Note that this was one of the reasons for Tullock to argue that there is not much that economists can learn from biologists. Hirshleifer also agrees with Ghiselin, however, that the underlying realities of natural economy always shine through in political economy. What Hirshleifer seems to have in mind when talking of the underlying realities of natural economy are the preferences (such as innate benevolence of malevolence) that are adaptive rather than arbitrary. Note that this is reminiscent of Becker's (1976) avowal that what economists can learn from (socio)biology is what preferences have (or have had) survival value.

We also saw, however, that Becker does not really make an attempt to learn something from (socio)biology. Instead, Becker argues that in their attempts to show how altruism could have evolved, (socio)biologists can learn something from economic models of socially interacting constrained maximizing individuals. How serious is Hirshleifer in arguing that economic models, and the model of 'economic man' in particular, can and should be enriched and broadened by evolutionary biology? The 'programmatic contention' (as Hirshleifer himself calls it) here is that "… preference patterns, despite seemingly arbitrary elements, have survived because they are mainly adaptive to environmental conditions" (Hirshleifer 1977a, 18).[22] Hirshleifer consistently argued that

[22] See also "where standard economics takes the satisfaction of preferences as the primitive objective or "utility function" of the acting individuals, biological theory suggests that what seems like mere preference or taste evolves out of the objective dictates of reproductive survival" (Hirshleifer 1977a, 50).

insights about the evolution of our own species help to go beyond the narrow conception of "economic man" (the economic conception of man and of human behavior should be enriched). But who are to provide these insights? How much can and should economics rely on biology to find out what preference patterns are adaptive to environmental conditions?

Once again closer inspection reveals that economic rather than biological analysis is supposed to do the job also for Hirshleifer. In fact, what Hirshleifer himself brings to the fore to shed more light on what preferences (or, more broadly, what proximate causes) are (or have been) adaptive is owes a lot to, and is written in very much the same spirit as, Becker (1976).[23] Hirshleifer's own contributions on this front started with a comment of Becker (1976). The main point in his comment (Hirshleifer 1976) is that Becker's argument that the fitness of altruistic Big Daddy can exceed that of an equally able non-altruistic father crucially depends on Big Daddy having the last word. If Rotten Kid and Big Daddy are to make their decisions simultaneously, or if Rotten Kid has the last word, the argument falters. Especially if Rotten Kid has the last word, Big Daddy's fitness will be lower than that of an equally able non-altruistic father.

One of Hirshleifer's more influential papers in this area of research,[24] Hirshleifer (1987), can be seen as a follow-up and generalization of Becker (1976) and Hirshleifer (1976). What Hirshleifer wants to show here is that particular forms of 'irrational' emotional behavior exist because they somewhat paradoxically enable the individuals exhibiting the forms of behavior to outcompete those that display rational behavior: "The economist

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Philosopher John Dupré (2001) fears the 'imperialist' combination of neoclassical economics with evolutionary psychology (and with evolutionary cognitive science), whereas philosopher (and economist) Don Ross (2005) welcomes it.

[23] Unlike Becker, Hirshleifer never presented a clear thesis or research program of his own (or it should be the economics of conflict and of conflict resolution as his own field). Instead, what we find in Hirshleifer's writings is a host of suggestions waiting to be worked out.

[24] This is part of what might be called Hirshleifer's tragdy of life (as a professional economist): Hirshleifer (1987) clearly pre-dates Robert Frank's (1988) idea of emotions as commitment devices (as Frank 1988 observes), but it was Frank and not Hirschman who got the credits for it. A similar thing seems to have happened with Axelrod's (1984) account of the evolution of cooperation: here too Hirshleifer's earlier work on the evolution of various strategies of reciprocity (cf. Hirshleifer 1982) seems to have pre-dated Axelrod (1984), but Axelrod got all the credits. See Demsetz (2005) for a short but informative overview of the work of Hirshleifer.

must go beyond the assumption of "economic man" precisely because of the economic advantage of *not* behaving like economic man – an advantage that presumably explains why the world is not populated solely by economic men" (Hirshleifer 1987, 322).[25] This phrasing might suggest that standard economic analysis, with its assumption of "economic man", is ill-suited to explain how people that do not behave like economic man can attain economic advantage. But Hirshleifer is clearly of a different opinion: following up on his Hirshleifer (1976) he puts standard economic analysis to creative use to show how this is possible. "My discussion follows a lead by Becker (1976) , who demonstrated how "altruism" can, in effect, force cooperation upon a completely selfish partner (the "Rotten-Kid Theorem"). I shall try to show more generally here how, and up to what limits, positive or negative emotions can serve a constructive role as guarantors of threats or promises in social interactions" (Hirshleifer 1987, 308).[26]

Hirshleifer (1987) starts with discussing the well-known problem that making promises or threats only seemingly offer an easy escape from concluding that individuals get stuck in mutually harmful inefficient defection or cheating equilibria in mixed-motive games. The problem is that talk is cheap; when the time arrives to fulfill (or live up to) promises and threats it is no longer in the interest of those that made the promises and threats to fulfill the promises and threats. Knowing this, the other party (or player) will not find the promises and threats credible… unless, that is, the promises and threats are backed up by something that make promises and threats credible.

Hirshleifer goes on to argue that emotions can play this role; emotions can serve as guarantors of threats and promises. If a person is benevolent towards a particular other person, for example, and if having this emotion makes the person cooperate, then this could back up the person's promise sufficiently. Hirshleifer discusses benevolence and malevolence as action-independent emotions and anger and gratitude as action-dependent

[25] See also "The thrust of the argument here has been that certain patterns of emotional payoffs to interpersonal cooperative opportunities can make retention of a capacity for emotion materially profitable" (Hirshleifer 1987, 321).

[26] Hirshleifer's discussion draws our attention to the interesting possibility that standard economic theory's constrained maximization framework might do a better job in describing the outcomes of evolutionary processes than in explaining behaviour. See Vromen (2003) for further discussion.

emotions. The underlying idea with the latter is that people become so angry about an action undertaken by another person that the angry person loses control over his actions. The anger could make the person act ‘irrationally’; against what rationally speaking would be in his best ‘objective’ interest to do. The general point Hirshleifer wants to make is that people acting on their emotions might be better off in terms of material gains actually reaped (i.e. the personal income they actually enjoy) than people lacking these emotions, not despite but precisely because of their ‘irrational’ emotions.

How precisely this could work is illustrated visually by Hirshleifer with the aid of standard indifference curve analysis. Although the analysis itself is familiar and standard among economists, the use to which Hirshleifer puts it is creative.[27] Instead of putting different goods and services that some consumer might want to have on the axes, the axes now stand for the incomes that the benevolent person and some other (possibly receiving) person might enjoy. A benevolent person will have the usual convex difference curves, whereas the opposite malevolent person will have concave difference curves. Next to the indifference curves, Hirshleifer also plots (135 degree) income transfer lines for benevolent persons and (45 degree) income deprivation lines on the assumption that benevolent/malevolent persons can choose to transfer/deprive part of his income on a 1:1 basis. Finally, a productive opportunity curve is plotted.

With the aid of this analytical apparatus, Hirshleifer is able to derive several results. One of them is once again a demonstration of the Rotten-Kid Theorem. The idea is that the game is played sequentially. Self-interested Rotten Kid is the first mover (“First”) and benevolent Big Daddy is the second mover (“Second”). Rotten Kid first can choose what point on (or within) the productive opportunity curve to pick. After this, Big Daddy decides what part of his personal income to transfer to Rotten Kid (based on his indifference curves). With all of these assumptions in place, Hirshleifer is able to show that if Rotten Kid is farsighted (i.e. is able to anticipate the subsequent income transferring behavior of Big Daddy correctly), Rotten Kid will choose another point on the productive opportunity curve than if Big Daddy weren’t (and were known by Rotten

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[27] This creative use of indifference curve analysis is foreshadowed in Hirshleifer (1976).

Kid) benevolent. As Hirshleifer himself observes, the important part of his result is not that far-sighted, selfish Rotten Kid can be induced (enticed) by benevolent (or ‘hard altruistic’) Big Daddy to act as if it too were altruistic (‘soft altruism’) so that it is better off. The important part is rather that Big Daddy is better off as well (than he would be if he were not benevolent or altruistic; the implicit assumption is that if Big Daddy were a self-interested economic man, point M would be the end result – which is worse for both than A).

Thus the crux of the matter here is the same as it is in Becker (1976) and in Frank (1988): ‘irrational’ emotions can only have survival value if the person acting on his emotions (such as altruistic Big Daddy) somehow is able to reliably *signal* his emotions and if the other individual(s) (such as egoistic Rotten Kid) is able to notice the signal (and what it stands for). Only then will the other individual(s) act in such a way that the fitness of the person having the emotions is enhanced rather than diminished.

To cut things short, what Hirshleifer contributes to the nascent field of bioeconomics is not so different from what Tullock and Becker contribute to it: demonstrations that the constrained maximization framework of standard economic theory can be fruitfully deployed to study processes of biological evolution and their outcomes. Hirshleifer’s writings undoubtedly displays a more profound understanding of work done in evolutionary biology than Tullock’s and Becker’s writings. In his informed overviews (Hirshleifer 1977, 1982), he introduced the economics profession to what was going on in substantial parts of biological literature in an accessible way. But despite declamations to the effect that connecting biology and economics implies (and should imply) a two-way transfer of ideas and concepts between biology and economics from which both biology and economics are to benefit, in the end his own contributions entail a one-way transfer of the constrained maximization framework from economics to biology from which biological analyses of selection processes are to benefit.

VI. The self-sufficiency of economic analysis and economic(s) imperialism

Becker (1976) and Hirshleifer (1987) suggest that economic analysis is in a way *self-sufficient*: economic analysis does not need any input from other disciplines. The constrained maximization framework of economic analysis is suited not only to study current behavior on the basis of given preferences, but also to identify what these 'given' preferences are! Economic analysis can tell us what preferences have (or had) survival value as well as what behavior people display given these preferences. Earlier economists such as Robbins might have thought that economics depends on psychology for identifying preferences. Sociobiologists and evolutionary psychologists might think that it is rather evolutionary biology that economists have to rely on for this purpose. But if we are to believe Becker and Hirshleifer, the fact of the matter is that economic analysis can do all these things on its own. Economics can render this service to itself. There is no need, it seems, for economists to take recourse to any other discipline.

Or so it seems. Note that neither Becker nor Hirshleifer deploys economic analysis to discover what (basic) preferences survived (or possibly could have survived) selection processes. Instead, they deploy economic analysis to show that already identified (or suggested) preferences such as altruism and benevolence could have survived selection processes. In other words, they deploy economic analysis to find out whether an evolutionary rationale can be provided for preferences for which they (or others) have independent reason to believe (or assume) that they exist and exert influence on behavior.

Becker and Hirshleifer do not engage in a grand attempt to identify all the basic preferences people have. They do not show a great interest in embarking on such an ambitious project. They seem to show even less interest in *revising* existing economic analysis on the basis of a full specification of all the arguments in the utility functions. In this, they differ markedly from economists who have started doing this. I am thinking here in particular of so-called *social preferences* models (cf. Güth and Yaari 1992, Fehr and Schmidt 1999, Bolton and Ockenfels 2000).

What Becker (1976) and Hirshleifer (1987) seem to be really interested in, it seems, is rather in showing the usefulness and fruitfulness of the constrained maximization framework of economic analysis in shedding light on how altruistic (and other non-self-interested) preferences and the like could have evolved. Like Tullock, they are in the business of promoting the use of the constrained maximization framework outside the traditional boundaries of economics as a discipline. It is no coincidence that Tullock (McKenzie and Tullock 1975, Tullock 1987) and Hirshleifer (1985, 1987b) have been actively advancing *economic imperialism* (or economics imperialism; Mäki) and that Becker is usually seen as economic imperialism's leading protagonist.[28] All three believe that economics (constrained maximization at the individual level and equilibrium at the social level of analysis) provides "the universal grammar of social science" (Hirshleifer 1985, 52). And indeed, as we have seen in this paper, they believe the scope of economics is not limited to social science, but extends to evolutionary biology: "Economic imperialism"- the use of economic analytical models to study all forms of social relations rather than only the market interactions of "rational" decision makers – is similarly [similar to the image of "economic man"] entirely consonant with the evolutionary approach." (Hirshleifer 1982, 52).[29]

In calling Tullock, Becker and Hirshleifer economic imperialists some clarification might be in order. It is not necessarily the case that those inroads into other disciplines are aggressive, hostile intrusions, meant to conquer the disciplines. Such inroads can be made with the best intentions. They may be inspired by the (in itself) noble intention to contribute to the growth of knowledge in the other disciplines. It is not necessarily the case either that there is disdain for what practitioners in other disciplines have accomplished in their own domain either. But typically it is the case that there is not much understanding and appreciation of what is going on (and of what has been going on) in those other disciplines. No serious attempts are made to acquaint oneself with the history and tradition of the other disciplines, to get a feel of the specific sort of problems

[28] Interestingly, though, Tullock's, Becker's and Hirshleifer's inroads into evolutionary biology are not even mentioned in Lazear (2000).

[29] Following Ghiselin (1978), Hirshleifer also argues that economics and biology should be seen as one (or united) rather than as two competing imperialisms: "In short, these two colliding imperialisms can say, with the comic-strip character Pogo, "We have seen the enemy, and he is us!"" (Hirshleifer 1985, 65).

that occupy the practitioners of the other disciplines and of the specific *couleur local* in the other disciplines' culture.

This is exactly what seems to have happened with Tullock's, Becker's and Hirshleifer's inroads into evolutionary biology. If the relevant criterion is whether or not they contributed to the spread of the constrained maximization framework in evolutionary biology, their inroads were not very successful. To be sure, the constrained maximization framework has been used and applied more often in evolutionary biology (and especially in sub-branches such as behavioral ecology) after Tullock, Becker and Hirshleifer wrote their papers. But it is highly questionable that their papers contributed much to this. As already indicated earlier, there were already papers using the constrained maximization framework before Tullock, Becker and Hirshleifer wrote theirs.[30] It is telling that (with the possible exception of Hirshleifer) they were largely unaware of that. Attempts to estimate impact factors (such as in Parker 2006) suggest that other papers were far more instrumental in having the constrained maximization framework spread in evolutionary biology than Tullock's, Becker's and Hirshleifer's. As Eric L. Charnov has suggested to me, part of the reason for this might well be that that the three did not try to find out what specific sorts of problems evolutionary biologists were struggling with (or were interested in). By contrast, Charnov surmises, economist Colin W. Clark (cf. Mangel and Clark 1988) succeeded in attracting the attention and interest of behavioral ecologists because he did try to link up with the biologist's interests. This, of course, only further strengthens the idea that the three were simply not very interested in what was actually going on in evolutionary biology.

VII. Conclusions

Both Alchian's (1950) and Friedman's (1953) selection arguments were clearly meant to defend marginal analysis against (what both Alchian and Friedman took to be) misguided

[30] It is always good (and for some economists sobering) to keep in mind also that optimality principles and optimality analysis did not originate from within economics (Schoemaker 1982, 1991).

anti-marginalist critiques. Both argued that even if businessmen do not literally do the things that marginalist analysis seemingly ascribe to them, a selection mechanism in markets akin to natural selection in evolutionary biology would see to it that the predictions of marginal analysis tend to hold. But whereas Alchian's text contains various reservations and qualifications and also suggestions as to how processes of economic evolution, analogous to processes of biological evolution, could be studied further, Friedman simply boldly declares that the maximization-of-returns hypothesis summarizes appropriately the conditions for survival. Friedman's version won the day in Chicago. Apparently without reckoning with the possibility that they might be "… pushing their luck too far" (Alchian 1950, 220), the second generation Chicago-school economists Tullock, Becker and Hirshleifer simply take for granted that constrained maximization hypotheses accurately describe the behavior of individuals surviving selection processes.

In fact, the second generation Chicago economists took Friedman's selection argument further than Friedman himself ever wanted to take it. In preparing the ground for his selection argument, Friedman discusses the parallel example of the positioning of leaves around trees. Friedman argues that the hypothesis that leaves maximize exposure to sunlight would go a good job in predicting the positioning of leaves around trees. What prevents Friedman from seriously proposing maximization hypothesis like this one in evolutionary biology is the fact that there is already a well-entrenched and successful program of putting forward adaptationist hypotheses in evolutionary biology. Thus Friedman showed some respect for traditional disciplinary boundaries and for established traditions of theorizing in other disciplines. This is different with the second generation Chicago school economists. Tullock, Becker and Hirshleifer were considerably less reticent in crossing disciplinary boundaries. What they have in common is their belief in the usefulness and fruitfulness of applying the constrained maximization framework of standard economic theory in studying processes of biological evolution and their outcomes.

Officially, the credo of the bioeconomics of Tullock, Becker and Hirshleifer is to promote and contribute to a mutually beneficial two-way transfer of ideas, concepts and approaches between biology and economics. Becker and especially Hirshleifer suggest that evolutionary biology can enrich economics by providing a fuller view of human motivation than the narrow-minded model of economic man. But here too Becker and Hirshleifer seem to be more interested in showing that the constrained maximization framework of economists can complement, if not supersede existing (socio)biological accounts of the evolution of non-self-interested motives and preferences than in looking what economists can learn from existing (socio)biological accounts. Thus Tullock, Becker and Hirshleifer type bioeconomics turns out to be a one-way transfer of the constrained maximization framework of economic theory to evolutionary biology from which evolutionary biology is to benefit. This economic imperialist project does not seem to have been particularly successful. The constrained maximization framework did gain more ground in evolutionary biology in the last few decades, but the papers of Tullock, Becker and Hirshleifer did not seem to have contributed much to this.

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