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Résumé

Abstract

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Le "free riding" soulève des problèmes méthodologiques particuliers. Son importance est indéniable mais souvent on ne l'observe pas en tant que phénomène réalisé. L'objet de ce papier est d'explorer la possibilité de le traiter comme un mécanisme causal ou sous-jacent. En tant que tel, il appartient au monde réel plutôt qu'à celui des modèles ou théories. Cependant, il est prédisposé à agir en combinaison avec d'autres mécanismes plutôt que seul. Cette caractéristique a une incidence sur des problèmes comme ceux du contenu empirique, de la testabilité ou de la pertinence. L'analyse est menée à la lumière de travaux récents dans le domaine de la philosophie des sciences et en tenant compte du développement des méthodes expérimentales en économie.

Mots-clés: méthodologie, free riding, action collective, mécanisme causal

JEL: B4, D7, H41

Free-riding is methodologically puzzling. It is at the same time important and often not observed as an actual phenomenon. The paper explores the possibility of treating free riding as an underlying or causal mechanism. As such, free riding is to be treated as part of the real world rather than of the world of models or theories. However, the free riding mechanism is particularly prone to operate not in isolation but together with other mechanisms. This feature has consequences on issues such as empirical content, testability or relevance. These questions are discussed in the light of some recent work in philosophy of science and with special attention given to the development of experimental methods in economics.

Keywords: methodology, free riding, collective action, causal mechanism

JEL: B4, D7, H41

1. INTRODUCTION

Free riding illustrates the need to wrestle with ideas which have come up recently in philosophy of science. I am thinking particularly of the growing recognition that some standard views or notions about science, though convincing on paper, are not doing justice to the complexity of the subject matters of most sciences, physics included. The actual world is more "open" or "messy" than is still often assumed, and thus it is often the case that science deals with it only indirectly.

In their own very different ways, the books of Roy Bhaskar (1975), Nancy Cartwright (1983, 1989) and Frederick Suppe (1989), among others, reflect this awareness. If there are laws (in some demanding sense, not as simple assertions of impossibility as are most of our laws in economics), most of these laws are to be found or defined at a level of reality which is not that of actual phenomena (see Cartwright, 1983). If properties of models are to be related to properties of real systems by mean of theoretical hypotheses, the real systems, again, are not part of the actual world but of a world in which a number of forces are supposed absent (see Suppe 1989). But the forces, interactions, mechanisms which are identified and studied by science in isolated or closed systems, such as the ones created artificially in laboratories, do not cease to operate at the level of the open, actual (non-experimental) world; for the purpose of applications to - and understanding of - this actual world, they must be assumed to operate there also (see Bhaskar, 1975).[1]

There are two reasons to think that these ideas may be particularly relevant in the case of free riding. Firstly, although free riding has different meanings (see, *e.g.*, McMillan, 1979; Sandler, 1992, pp. 16-17; Wolfelsperger, 1995, pp. 56-63; Breton, 1996, pp. 295-96), it is interesting mainly when its interpretation does not place it firmly in the world of manifest phenomena. This requires some explanation. Free riding occurs when some people legally do not pay, or not their share, for some good they draw utility from, but which, whether they pay or not, they cannot be prevented from consuming. The word "good" refers to anything which has a positive utility, the word "pay" to anything which has a cost. Technically there are two ways to free ride. A first one is when no authority, formal or informal, is endowed with the power to impose taxes or other contributions to users or potential users of the goods, and the goods have

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[1] My main excuse for venturing amateurishingly into as difficult a subject as contemporary philosophy of science is that keeping away altogether from it might be even more perilous, at least with regard to some of the methodological issues I, as an economist, am concerned with. In addition, I must stress that my objective is limited. I have no desire to interfere in philosophical discussions about, say, causality. As in previous work, my concern is economics, not philosophy, and my main motivation is unabashedly apologetic, based on the conviction that, given the subject matter, the approach characteristic of mainstream economics, although puzzling, is methodologically correct - or, perhaps more exactly, cannot be showed to be methodologically wrong.

the property of non-excludability (goods which have this property include pure public goods and commons). In this situation, free riders need not hide their preferences. Even when known to derive considerable utility from a good, nobody can legally force them to contribute to its provision. The second way is relevant only when people can be made to pay in proportion or in function of the utility they individually derive from a good; then free riding may require negating or understating that utility (and overcoming free riding may require recourse to a preference-eliciting mechanism). In this paper, I will consider only the first case.

Interpreted literally, as a manifest phenomenon of the actual world, free riding obtains only when there is a "ride", that is, when the good is actually produced. It refers to the unearned benefit drawn from the good by the free riders. Free riding in this sense hurts our sense of justice but does not raise as serious a problem as it does when its meaning is extended and expresses the fact that the good is not produced or is produced suboptimally. When some people contribute and others free ride (literally), the main problem raised by free riding may be the suboptimality of production rather than the exploitation of the contributors by the free riders.

Even more remarkable is the "free riding" referred to in the so-called "strong free riding hypothesis". That hypothesis is what many authors have in mind when they refer to free riding. It says that the fact that people are not willing to pay for a non-excludable good leads to the good not being produced at all (when the good is discontinuous, it may be sufficient that some people are unwilling to pay for that outcome to obtain). The damage may be particularly important; yet free riding, understood literally, as an actual phenomenon, has disappeared. Since there is no ride, nobody should be said to free ride.[2] This remark applies even more forcefully to some of the other ways in which free riding may vanish. For instance, if it is clear to all that a particular good cannot be produced because too many people would free ride if they were asked to contribute, the matter might not even be raised. Alternatively, people might be compelled to contribute by the mean of taxation; or there might be other institutional arrangements leading to the same result; or, following Mancur Olson (1965), contributions may be the result of selective incentives. In these examples, although free riding in a non-literal sense or as a non-actual reality may have far-reaching consequences, literally or actually there is none of it.

Altogether, the various cases discussed show that free-riding can be interpreted literally or not, as manifest or not, as an outcome or as a cause. They suggest also that

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[2] John Ledyard (1995) and Alain Wolfelsperger (1995) claim that this should prevent us in particular from using the word free riding in the context of the prisoner's dilemma. But see note 15 below.

the less literal and manifest and the more akin to a cause rather than to an outcome it is, the more important or interesting it is likely to be. This makes it a good object of reflection from the perspective of philosophical ideas which emphasise the non-actual side of reality.

A second reason to consider, perhaps mistakenly, that free riding is a good case is that it has been questioned in a way which is methodologically interesting. The term and whatever it refers to intervene massively in many parts of economics and other social sciences - *e.g.*, in public finance, theory of the state and politics, interest groups, theory of revolutions, economics of trade unions, international relations, altruism or philanthropy, resource conservation and environment, capital markets and theories of the firm, theory of religion - and political philosophy.[3] Todd Sandler even claims that the notion of free riding "has begun to permeate our everyday language and thought" (1992, p. 12). But, if this "our" refers to people in general, it is not certain that free riding has been taken in so widely or in depth. Erroneous neglect is pervasive. For example, politicians or journalists do not shy away from advising the members of large groups (currently, in France, doctors) to behave differently if they do not want some collective punishment imposed upon them. In this respect, free riding is akin to comparative advantage: observation of public opinion, confirmed by experience in the class , shows that the idea is both important and counterintuitive.

Given such success in the world of scientific or intellectual endeavours and such upsetting or novelty potential in the world of the public at large, one is inclined to consider free riding as being a major contribution of economic reasoning to social science or to thought *tout court*. But this view is definitely not shared by all commentators. Free riding, as an hypothesis, has been criticised by Earl Brubaker (1975), Leif Johansen (1977), Albert Hirschman (1982) and other well-known scholars. The most interesting and powerful critique, however, is the one formulated recently by two political scientists from Yale, Donald Green and Ian Shapiro (1994, 1995), as part of a more general critical examination of the "applications of rational choice theory in political science".

One must admit at the outset that Green and Shapiro's arguments seem directed at exactly the right target (polemically speaking). To use Lakatosian language, what they ask for is "corroborated excess empirical content", or "attested novel facts". This is a methodological demand which the programme they are concerned with does not seem particularly able to satisfy. Is it not an obviously legitimate requirement though? How

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[3] See, *e.g.*, Booth (1985), Grossman and Hart (1980), Hardin (1982), Iannaccone (1992), Keohane (1984), Ostrom (1990), Rawls (1971), Sandler (1992), Sugden (1982), Taylor (1987).

could an old Popperian like myself not agree with a question formulated as follows "Given that any proposed explanation might be wrong, what datum or data should persuade the researcher that this is so?" (1995, p. 266)? And how not to appreciate the way the two authors also formulate their criterion of success or admissibility: "nonobvious propositions that withstand empirical scrutiny"? When that criterion is applied to free riding and collective action dilemmas, one encounters, they claim, "arresting propositions that are not sustainable" (*e.g.*, "that changes in collective incentives have little effect on rates of participation in large groups"), "sustainable propositions that are not arresting (*e.g.*, "that rising selective incentives increases participation in collective action"), seldom applications "that are at once arresting and sustainable" (1995, p. 236). In their view, "exceedingly little has been learned" (1994, p. x) from - or "little empirically supported knowledge has grown" (id. p. 197) out of - that approach.

With the exception of one paragraph (1994, p.180) , Green and Shapiro are careful not to extend their critique of the applications of the rational choice approach in politics to its applications in economics. That the two authors believe their critique to be applicable to economics transpires clearly, however, in the paragraph just referred to. For example, they note in this paragraph the parallelism between their views and those of Richard Thaler and Alexander Rosenberg. In fact, the economic methodologist they are closer to is Mark Blaug. For these authors, as for Blaug, improved understanding, new insights, re-orientation of the discussion, etc. do not count if not validated empirically, or count only a trifle.[4]

This essay is not devoted to the writings of Green and Shapiro. I quote them because their analyses are very well defended and persuasive, and concern free riding and collective action, which is a subject matter common to both disciplines. But my main reason to start from their position is that it unintentionally illustrates how much and how quickly the philosophical landscape has changed. When responding to the objection that their underlying philosophy is naïve, Green and Shapiro (1994, pp. 180-83; 1995, pp. 255-61) deal with the objection as if it were exclusively stemming from

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[4] Green and Shapiro in particular underestimate the dynamics of argumentation or the heuristic effects of an analysis of collective action such as the one proposed in Olson's book (1965). Before 1965 the presumption was that all groups got organised. The questions were then, Why do not these particular groups succeed in organising themselves? What are the obstacles to their mobilisation? After 1965, the puzzle was: How the devil has this group or category managed to mobilise in spite of the free rider problem? A large literature on rationality and revolution for instance is entirely devoted to this problem. Similarly, since the seminal article of Sanford Grossman and Oliver Hart (1980) a question which has attracted a lot of interest in finance is: Why do take-overs work or even exist, in spite of the rationality of free riding on the part of the small shareholders of the attacked firm? Since I have very much insisted on this aspect of research in previous work (1976, 1994; Mingat *et al.*, 1985), I will not allude to it in the remainder of this essay.

the ideas of Kuhn and Lakatos. Inasmuch as the objection is based on the ideas of these authors, Green and Shapiro's response is not inappropriate, I find. But the main challenge to an empiricist position of the kind they adopt is not to be sought nowadays on that side of philosophy (paradigms, tenacity, protected hard core, etc.). It is to be found in the new or renewed awareness, referred to at the beginning of this introduction, that the *actual* world is "open" or "messy", and in the methodological consequences of this characteristic which I also referred to.[5]

Insistence on underlying forces, interactions or mechanisms is one of these consequences (it can also be the outcome of different considerations of course). In this paper, notably as a response to critical arguments such as the ones discussed above, but also as an effect of more intrinsic motivation, I explore the possibility of treating free riding as an underlying or causal mechanism.[6] I have made some effort to overcome my natural reluctance to define and have groped towards the beginning of a definition of mechanisms, in general as well as with special reference to free riding. Section 2 reflects in part this effort. The sections which follow are devoted to comments on two essential aspects of the tentative definition provided in Section 2. In Section 3, I discuss the relationship between real-world mechanisms, constructed models, and hypotheses or conjectures. I try to show that clarifying this relationship may be helpful to interpret some aspects of the literature on free riding. The fact that a mechanism such as free-riding is particularly prone to operate not in isolation but together with other

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[5] Because Shapiro did in a previous book (1990), I am surprised that Green and Shapiro do not cite at least Bhaskar. The reason may be related to the fact that Bhaskar himself and an economist like Tony Lawson (1994) who uses Bhaskar's philosophy see themselves and are seen by others as supplying arguments against the orthodox or mainstream in general, against neo-classical or mainstream economics in particular. Neither they nor Shapiro are apparently aware that Bhaskar's views about science (as exposed in his first book) can also be used, at least in part, to elucidate the difficult relationship between neo-classical or mainstream economic theory - or more generally the rational choice programme in social science - and empirical data.

[6] I have argued elsewhere (*e.g.*, 1994; see also Mingat *et al.*, 1985) that economics has always been based on the presupposition that more or less 'hidden' or deep-seated mechanisms - or interactions, processes, etc. - underlie and explain what we observe in the actual world. . Whether we consider Smith, Say, Ricardo, Marx, Marshall or, in this century, Keynes, Franco Modigliani and Robert Lucas, the aim of the analysis is of the same nature: some underlying mechanisms are essential, taking them into consideration will lead to unexpected results, and neglecting them will entail serious mistakes, in both cases at the level of the actual phenomena - which should be of some interest to all (even to those who care only about this level). In addition - this time to the non-instrumentalist subset of the profession only - these mechanisms or interactions are fascinating in themselves, independently of their implications in the actual world. A position such as Green and Shapiro's or Blaug's, is not incompatible in principle with the presupposition that mechanisms are essential but it may be incompatible in practice if the actual world is more messy than they suppose. We must certainly try to test all our hypotheses. At the same time, we must remain free to speculate on possible aspects of the world even when we cannot support empirically these speculations (this justifies the claim that mainstream economics is largely a critical science, as argued in my 1994 essay).

mechanisms is stressed in Section 4, and some of the problems that ensue discussed there and in Section 5. Section 6 consists of a brief conclusion.

2. TOWARD A DEFINITION OF THE FREE RIDING MECHANISM

What is a mechanism? I am not in the position to provide a general definition of that term. Jon Elster (1989) claims that social science should concentrate on "explanations by mechanisms". But, as noted by Paul Humphreys (1991), he does not really explain what that means, whilst the illustrations that he gives, in the form of a series of almost independent chapters, are no substitute to such explanation. Arthur Stinchcombe (1991) is even less helpful. It is not clear in particular whether he considers mechanisms as theories or as objects of theories. Many economists or social scientists (Blaug as well as Green and Shapiro, incidentally) stress the need to display or spell out "causal mechanisms" but remain vague about what these mechanisms are.[7]

The account of mechanisms which I have found to be the most precise and interesting so far is the one proposed by my illustrious namesake, Wesley Salmon (1984). According to him, "causal processes, causal interactions, and causal laws [*i.e.*, laws or regularities which govern causal processes and causal interactions] provide the mechanisms by which the world works; to understand *why* certain things happen, we need to see *how* they are produced by these mechanisms" (p. 132, his emphasis). As a non-philosopher, it is particularly difficult for me to gauge the implications of Salmon's views about causal processes and interactions, or more generally about the causal structure of the world (see, *e.g.*, Koertge, 1992). Although I have not found in what I have read an analysis of mechanisms which could replace Salmon's, I do not claim that there is none. Tentatively, thus, I will now use some of his analyses and examples, together with some of the examples provided by Elster, to formulate some rather simple ideas which might serve as ingredients for a more complete or general treatment.

In many cases, it seems, one can say that mechanisms are the sets (I hesitate to use the term "system" instead of "set") of causal processes (physical propagation, or, in social science, also mental processes) and interactions (notably between processes) which *connect* causes and effects. Thus, a "switching mechanism" - or perhaps a switching mechanism interacting with an electrical circuit (Salmon, 1984, p. 178) - connects one fact, "turning a switch", and another fact ,"the light goes on", one being the cause and the other the effect. The chief mechanism of transmission of the plague

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7 *E.g.*: "No market test is likely to discriminate between human capital and screening explanations, because the question is not whether schooling explains earnings but rather why it does. It would be difficult to find a better example of the difference between merely predicting a result and explaining it by a convincing causal mechanism" (Blaug, 1980, p. 216).

"is a flea that bites infected rats and then bites other organisms" (ibid., p. 271). Cognitive dissonance reduction is a psychological mechanism which makes people cease desiring what they cannot get, "as in the story of the fox and the sour grapes"; this mechanism, together with the news that a job has become unavailable, connects the fact that the job was desired and the fact that it is not desired anymore (Elster, 1989, p. 4).

In Elster's formulation here, one fact is the causal antecedent of the other, it has happened previously. But this is not always necessary, it seems. There is no explicit reference to causal antecedents, or things which happened previously, in the case of "internal causal mechanisms" accounting for the "nature" of a fact (Salmon, 1984, p. 275) - for instance the internal or underlying causal mechanisms (involving the behaviour of molecules in gases, Avrogado's law, different molecular weights, etc.) which account for the fact that humid air is less dense than dry air (ibid. p. 269). In that case, the underlying causal mechanisms "constitute" the fact or constitute the "causal structure" of the fact. Would a more general formulation of the sour grapes mechanism (say, simply, that it makes people underestimate the utility of unavailable goods) allow an interpretation along these lines? I do not know whether this would be acceptable. One might want to say that the knowledge of unavailability necessarily precedes on some time scale the underestimation of utility. But, as a more general principle, I think that we must allow that mechanisms also connect social or psychological facts so to say timelessly, in a setting of simple statics.

Where does this lead us to in the case of free riding? By analogy with some of the foregoing examples, I will define the free riding mechanism as being any set of processes and interactions in a group of individuals which, if operating freely and alone, will lead *many* of these individuals to decide, *because* of the non-excludability property of a good, not to contribute to its provision - or, in any case, not an amount at least equal to their marginal valuation of the good - even if the consequence is that the good will not be provided at all. In other words, the free riding mechanism connects a fact to which we give the status of a cause, the non-excludability of a good, with another fact, which we consider as its effect, *i.e.*, many individuals deciding not to contribute to the provision of the good, etc.

Some characteristics of this definition are related to problems which I discussed in the first section about the meaning of free riding. I comment some other characteristics now. First, I use the singular and refer to *the* free riding mechanism. One might prefer to use the plural and refer to several free riding mechanisms (I allude again to that question in the next section).

Second, although in general in this paper I may be a bit negligent about variations in the meanings of the terms I use, "free riding" in particular, I feel that the definition proposed here is relatively unambiguous and points rather clearly to a mechanism rather than to an outcome. As an outcome free riding may be unintentional and reflect circumstances such as ignorance, absence, or absent-mindedness. But this is excluded by the way the definition is formulated: people in the group do not merely not contribute (*i.e.*, objectively free ride); they decide not to contribute because - notably - of the non-excludability of the good. Let me note in passing that some so far unmentioned factors are certainly central to this decision. A clear case is the dispositions (psychological and otherwise) of the persons concerned. Whether, in particular in W. Salmon's framework, these factors should also be called causes, I do not know.

Third, I refer, rather vaguely, to "many" people. One reason is that I treat the free riding mechanism as an essentially social mechanism (itself involving individual psychological processes of course) rather than as a mainly psychological mechanism as many authors in fact do, I think. That the social character is compelling and should be stressed seems to me to come out particularly clearly from the experimental work and evidence I will refer to later on. Another reason is that, in the real world, this mechanism is most likely to involve stochastic processes. On this point, a relevant analogy is with the mechanism (or mechanisms) of the epidemics of plague as analysed, again, by W. Salmon (1984, pp. 271-73). In the case of both mechanisms, "many" is implied but not defined precisely, and the causal connection is not with the behaviour or destiny of given persons, except perhaps, implicitly, in the form of propensities ("many" persons also do not contract the plague, but the explanation of the epidemics does not require that we know why).

Fourth, I have attempted to define but not to elucidate the free riding mechanism. This is how it should be, I believe. Even if its methodological aspects are necessarily a concern of this paper, elucidating the free riding mechanism is a scientific, not a methodological endeavour. As we will see, substantial progress has been made by researchers in the fulfilment of this task. At the same time, the processes and interactions involved in the social mechanism of free riding come out as much more complex and subtle than believed initially.

Two characteristics deserve more extensive comments and will be the objects of the next sections. One concerns the implicit treatment of a mechanism as a part of reality, not as a theory or model; the other the fact, alluded to in the definition, that in

the real world the free riding mechanism is likely to operate not in isolation but together with many others.

3. MECHANISMS, MODELS, AND HYPOTHESES

Mechanisms are not theories or models. They are first of all part of the real world. I think that this is particularly clear in the analysis of W. Salmon (1984), but is supposed also, often implicitly, in most of the references to causal, generative, hidden, underlying or basic mechanisms that one finds in the literature. How, otherwise, could we say that the mechanism of something is mysterious, unknown, ill-understood, etc.? Now, this does not imply that mechanisms cannot also be included in models. Inspired by the so-called semantic conception of theories (see Giere, 1988; Hausman, 1981, 1992 ch. 5; Salmon, 1990), I interpret models as non-linguistic systems constructed to analyse particular aspects of reality.[8] As such, models *can* include anything found in reality and thus can also include mechanisms (though, I must stress, they do not have to).

According to the semantic conception, a model cannot be true or false. But its relationship with some aspects of reality is specified within a hypothesis or, to use Popper's word, a conjecture, and it is this hypothesis or conjecture which is true or false. Within the semantic conception, there are different ways to conceive this hypothesis or conjecture. I have argued in a previous paper (1990) that the semantic conception is mainly interesting or fruitful, for economics at least, when the relationship is not too close, that is, when sufficient distance is left between the model and the aspect of reality it is related to.

The distance is minimal when, as was the case at the beginning of the semantic conception (with Patrick Suppes in particular), the model is used to define a predicate, say K (*e.g.*, "a Keynesian economy") and when the hypothesis consists in pegging the predicate to some part of the real world, asserting that some part of the real world "is a K" (see, *e.g.*, Hausman, 1981; and, for a critique of this "exercise in set theory", Giere, 1988, page 286, n. 7).

The distance is larger in very different ways in the solutions proposed by Bas van Fraassen, Ronald Giere and Patrick Suppe - three major proponents of the semantic conception. Following van Fraassen, hypotheses would consist in specifying certain parts of models (the empirical substructures) "as candidates for the direct representation of observable phenomena" (1980, p. 64). In the same essay, I have argued that van

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[8] I am not concerned with the structuralist branch of the semantic conception as analysed in Hands (1985). For a particularly clear explication of the non-linguistic character of models in the semantic conception of theories (including what he calls "theoretical models"), see Giere (1988, chapter 3).

Fraassen's solution is unhelpful in economics, if only because all economic hypotheses built up as he suggests would immediately be found to be, as he would say, not "empirically adequate", that is, in Popperian language, refuted. For Giere (*e.g.*, 1985, p. 80), the form of the relation asserted by the hypothesis is as follows: "The designated real system is *similar* to the proposed model in specified *respects* and to specific *degrees*" (his emphasis). Degrees of approximation are eliminated by Suppe (1989) from the formulation of hypothesis but at the price of a reinterpretation of real ("physical") systems as "characterisations of how the phenomena would have behaved had the idealised conditions been met" (p. 65).

Combining Giere's interpretation of the relationship as "similitude in certain respects" with Suppe's interpretation of the real world objects of scientific conjectures as simpler systems than those found in the actual world gives us as large a distance as we may wish between models and "aspects of reality" - this, in principle, without driving us to be less concerned with truth and testability.

This expression "aspects of reality" is vague but essentially on purpose. Mechanisms are candidates to become one specification of its content. Mechanisms then are the objects of hypotheses, conjectures or, for that matter, theories;[9] and these hypotheses, conjectures or theories ascribe properties to mechanisms typically (*i.e.*, not necessarily) by asserting some relationship between these mechanisms and models. They (the hypotheses, etc.) are true or false. I avoid the words "description" and "representation", because (cf. the citation of Giere above) it is unclear that real systems such as mechanisms can always be described, or that models necessarily represent. One can, in particular, refer to some properties of a model - or of a mechanism in a model - and assert that the mechanism in the real world has the same properties, or the same properties in certain respects (this implies no isomorphism between the mechanism in the world and, if there is one, the mechanism in the model). To repeat, the assertion is true or false.

The distinction between mechanisms and models is useful to perceive or understand some aspects of the literature on free riding. I will concentrate on two. First, a point made both by James Buchanan (1968) and by Olson (1992), is that the analysis of free riding in the context of games with a small number of players, *e.g.*, in a two-person prisoners' dilemma, should be used to understand what happens not in situations

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[9] I do not really make distinctions between "hypothesis", "conjecture" and "theory". But, alternatively, a theory can be interpreted as including the description or specification of a model (or of a population of models) in addition to the assertion of the relationship of this model (or these models) with some part of reality (see Giere, 1988, pp. 84-85). In both cases, a theory, like a conjecture or an hypothesis, is true or false (Giere downplays the importance of that property, but I don't).

in which there are two or a few players but in situations in which there is a very large, almost infinite, number of players (*e.g.*, a "purely" competitive market). As a rule (outside the very peculiar circumstances depicted in the original story), the real-world relationship between any two persons having the payoffs supposed in the game - *i.e.*, being "in" a prisoner's dilemma with respect to payoffs - will be completely different (much more complex) than that supposed in the game. In particular (as a consequence) the dismal outcome predicted by the game will typically not obtain. Only when a large number of people are involved is the model of the two-person prisoner's dilemma useful as an analogue.[10]

This point deserves some attention because of the common but mistaken practice of defining a real-world situation as a prisoner's dilemma - or, for that matter, as a situation of "chicken", etc. - on the sole basis of the distribution of the payoffs, as if at least some of the general game-theoretic assumptions of non-cooperative games were not essential conditions for the identification of the situation. To use my own framework, the conjecture defended by Buchanan and by Olson as the only one that they (or anybody else should) make when referring to the prisoner's dilemma is that some specified properties of the real world *mechanism* of free riding in a large group are the same as some specified properties of the two-person prisoner's dilemma *model*. One of the properties of the model is that the collective good will not be produced at all. The conjecture can assert that this outcome will be also the outcome of the real-world mechanism - *i.e.*, it can consist of the "strong free riding hypothesis" referred to earlier - but, if found inadequate (*i.e.*, false), this could easily be attenuated (along the line suggested by Giere for instance) or dealt with at a later, empirical stage (see below).

A second aspect of the literature on free riding and collective action which the distinction between mechanisms and models may contribute to clarify is the relation between free riding and rationality. As teachers of microeconomics know, the method which consists in deriving the properties of market demand from a rigorous exposition of the so-called "theory of the consumer", a specification of the optimising rationality model, has many merits but also a serious drawback. It conveys the impression that the properties of market demand are dependent on the fate of that "theory of the consumer". Students must be informed that market demand and its properties are not dependent on the supposition that individuals are rational, especially in the form supposed in the

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[10] "In the original parable", Olson notes, the two prisoners are "denied communication with one another - and also the possibility of utilizing any of the manyfold possibilities for making mutually advantageous deals that human ingenuity and social institutions normally supply" (p. x). "In a sufficiently large or latent group... the incentive for strategic interaction - and even the incentive to bargain with other potential beneficiaries of the good - disappears" (p. xv).

"theory of the consumer".[11] As illustrated by its treatment in Green and Shapiro, free riding also is too often discussed simply as an *implication* of "rational choice theory" while it can stand or fall independently of that. An obvious motive not to contribute to the cost of a non-excludable good is what has been called "greed" in the literature. Another powerful motive identified by the literature is the fear of being a dupe, a "sucker". Now, neither greed (or, for that matter, self-interest), nor the fear or reluctance to be a dupe is dependent on rationality. One can be greedy and irrational (inconsistent, etc.) and the fear of being a dupe is even more likely to be pathologically irrational or the product of a non-consequentialist norm.

In other words, perfect, optimising, rationality is not a property to be sought in real-world mechanisms. Its habitat is the world of models. Why is it so used there? On the whole, the hypotheses which refer to models including optimising rationality as a characteristic are, in some sense or for some reason, "better" than the hypotheses, if any, which refer to models which do not (this statement is itself in part an empirical conjecture). One reason of this superiority is certainly technical. Models incorporating perfect optimising rationality are particularly convenient, neat, versatile, combinable, performing, etc. *as models* to be employed for the formulation of hypotheses or conjectures - the cost involved in assuming in models a perfect rationality which does not exist in the real world not being as important a counter-part to these benefits as it would be if models were interpreted, as they often are, as asserting something about this real world. Anyhow, and this is a second reason, rationality, albeit not of the kind found in the models (much more imperfect, intermittent, etc.) is not altogether absent from the real world. It *is* a tendency or personality trait of humans beings - as is probably also self-interestedness. The real-world existence of this double psychological tendency thus contributes to explain, together with technical convenience, that models in which *perfect*-rationality-cum-self-interest is a characteristic are in general performing instruments for the making of conjectures.

In fact, a major *research strategy* of economists is, and has always been, to ask themselves what would happen if humans were perfectly rational and self-interested, and this strategy has been productive, that is, has led to the discovery of interesting mechanisms. Free riding is a mechanism discovered in this way. Although clearly

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[11] In turn, the fruitfulness of the partial-equilibrium model of demand and supply, focusing on equilibrium, hides the existence in the mind of the economists who use it of a quasi-sociological, unformalised, theory of competitive price adjustments in markets (a theory which is more concerned with processes than with equilibrium). It is largely because of the existence of the latter that there is much confidence in or acceptance of the former. But that they present models in the shadow, so to say, of looser theories is rarely said by contemporary economists. For a development of this argument, see Salmon and Wolfelsperger (1991, pp. 4-6).

anticipated by Hume and Wicksell, possibly anticipated by Hegel, etc., it would not have been really noticed or would have remained unheeded if there had been no systematic exploration of the consequences of perfectly rational and self-interested behaviour in various specified theoretical contexts (public goods, etc.). In other words, free riding is a product of the rational choice approach as well as of the use of models. But now that it is here, we must not treat it as being necessarily an implication of that approach or a model.[12]

This last point suggests a more general one concerning the dynamic relationship between models and mechanisms. Simplifying outrageously, we can view rationality-cum-self-interest models as leading to the "discovery" of the free riding mechanism, the elucidation of this mechanism leading in turn to the construction of increasingly sophisticated models (game-theoretic models notably), whose use together with empirical work (see below) leads to substantial change in our perception of the mechanism and/or in the discovery of other mechanisms, involving for instance learning or norms, social and moral. Perhaps I should stress at this point that when I use the word "discovery" in the last sentence - and also in other parts of this essay - I do not intend thereby to deny that there is something artificial or arbitrary in the carving into particular mechanisms of whatever has been discovered.[13] In other words, there are always alternative ways to arrange our discoveries. This applies to the reality we refer to under the name of "free riding mechanism" as to any other. I do not think, though, that distinguishing between real-world mechanisms and the models which have led to their discovery becomes unimportant as a consequence.

4. FREE-RIDING AS A MENTALLY ISOLATED MECHANISM

Free riding, *by nature*, is particularly likely to be a mechanism which, in the actual world, interacts with other mechanisms and forces and thus needs to be referred to and analysed under a "method of isolation" (Mäki, 1992). Since the need to isolate matters in one's thought and discourse (the method of "analysis") is not confined to science but pervasive also in ordinary life, I will refer to it simply as "mental" isolation. Why "by nature"? Are not - in the actual, non-experimental world - interactions with other influences the fate of most if not all the aspects, forces or mechanisms science deals with, as several contemporary philosophers have stressed - and, before them, Mill

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[12] As a research strategy it can remain fruitful to stick to rationality in spite of apparently contradictory evidence, as has been done, for instance, in the context of agricultural development.

[13] A transcendental reality was discovered by Columbus, or some navigators before him, but that it is one America or continent is not transcendental but perfectly human and to a large extent arbitrary.

(as documented in particular by Hausman, 1981bis; and Cartwright, 1989)? What is there - in addition, so to say, to this common fate - which is specific to free riding?

Typically, free riding is also a problem to someone (this is in fact the way it is the most often referred to - "the free riding problem"). Solutions are sought to mitigate the problem. Inasmuch as solutions are found, this means that free riding has been combined with other more powerful devices, forces or mechanisms and that as a consequence it is no more an outcome or phenomenon manifest in the actual world, or at least much less manifest than if it had been left to operate freely. But, understanding the institutions, arrangements or other facts which allow the working of these counteractive mechanisms and thus constitute solutions to the free riding problem is difficult or impossible if we do not take into account the free riding mechanism.[14]

The subjects of Leviathan do not free ride but without free riding there would be no need for a Leviathan (to avoid functionalism, we should say that the free riding mechanism explains why individuals accept Leviathan). In similar fashion, there is not much scope for free riding under the arrangements studied by Elinor Ostrom (1990) in the context of common-pool resources, but the free riding mechanism is a major cause of these arrangements.[15]

The fact that free riding is a mentally isolated real-world mechanism raises, however, several serious issues. I will consider three - two in this section and the third in Section 5. First, there is the risk of vacuity. In physics, assertions concerning pure or mentally isolated mechanisms are true or false nonetheless. I agree on this point with Suppe (1989) as well as with Uskali Mäki (1994). In economics, however, if we put too many things in the category of disturbing forces or factors, there may be the risk that what remains to be said about our isolated mechanism verges on the tautological or the vacuous. I think that it is a merit of the distinction between real-world mechanisms and constructed models and of the relegation of pure rationality to the world of models (both discussed in the preceding section), that they allow for sufficient flesh to be left

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[14] If we gloss over our distinction between mechanisms and models, the same idea is expressed by Peter Ordeshook (1986) as follows: "*Often we might not find prisoners' dilemmas in a specific situation or institution because certain rules or traditions evolve to avoid them. But this absence of dilemmas does not make an understanding of their logic less relevant, because the only way to understand why such rules and traditions persist is to discern the dilemmas that arise without them* " (p. 235, his emphasis).

[15] In the context of a static *model*, especially one in which there is perfect rationality and foresight, a better formulation might be to say that it is the potential for free riding or the fear of free riding which explains, etc. In the real world, however, it is more plausible that free riding will be experienced rather than anticipated and thus more clearly the cause of the institutions which make it disappear (as an outcome). The beggar-my-neighbour policies of the interwar period, a form of free riding, are a major cause of post-war arrangements such as Bretton Woods or the IMF. Alain Wolfelsperger (1995) considers only models, but if he were not, a similar argument might apply to his objections against the use of the term "free riding" itself.

on the mentally isolated real-world mechanisms. In the case of free riding, as a result of experimentation (discussed below), this is not wishful thinking or a purely programmatic claim. The acid test relevant to this first issue is whether conjectures about a mentally isolated mechanism remain susceptible of being false: as we shall see shortly, the free riding mechanism meets this test.

A different issue, conditional on our answer to the foregoing question, is how do we know that a conjecture about the free riding mechanism is false. In other words, a second issue is: Assuming that a conjecture is susceptible of being false, is it testable? Now, I think that the answer to this question should take into account the major revolution in method which is currently taking place in some fields of economics, that is, laboratory experimentation. As a consequence of the extension of the use of experimentation in economics, we should now do what has always been done in some other sciences, such as physics, and distinguish sharply between testing conjectures - or more generally acquiring knowledge - about mentally isolated mechanisms, which takes place mainly in laboratories, and analysing the incidence of these mechanisms on phenomena situated in the actual (de-isolated, or messy) world. There are many parts of economics where this distinction may not be really workable as yet, and where as a consequence the two tasks are to a large extent confused, but I think that it is workable in the case of free riding. This allows me to separate the second issue - whether testing conjectures and acquiring knowledge about the mentally isolated real-world free riding mechanism is possible - from a third issue - what can be known about the influence of this mechanism in various actual-world (*i.e.*, mentally de-isolated) settings (the object of next section).

Testing conjectures *about* the free riding mechanism on non-experimental data is fraught with difficulty (as the discussion in and on Green and Shapiro's book shows particularly clearly).[16]

Admittedly, in one conspicuous case it seems relatively straightforward. This is when the conjecture at stake is that the individuals in the real-world mechanism behave *exactly* as they do in models built-up on the double assumption of perfect optimising rationality and narrow self-interest. If the public good is, say, an interest common to the members of a large group, this conjecture entails the prediction that nobody will contribute, that is, that there will be no mobilisation of the group - except if some "large" members decide to contribute alone (and thus be exploited by the "small", free-

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[16] I do not count as proper tests the retroductive achievements implicit in the understanding (discussed in the text) of the actual institutions and other arrangements whose *raison d'être* is to counter free-riding.

riding members), or if there are positive or negative "selective incentives" of some sort (see Olson, 1965; Hardin, 1982; Taylor, 1987; Sandler, 1992).

I am tempted to side with Green and Shapiro and agree that this particular conjecture about the free riding mechanism is clearly refuted by one salient fact, the *massive* participation of individuals in elections (the public good involved there is one's preferred side winning and/or the expression of support to democracy).

I am less sure about the other observed phenomena that they consider as equally straightforward (demonstrations, militancy, etc.). As noted by a number of authors, if the theoretical prediction is a corner solution (zero participation), all error or noise will be on the same side, which means that the predicted value of the observation (at least when output is a continuous function of inputs) is necessarily positive. At least fifty percent participation (in a large number of elections involving a large number of persons) is a solid phenomenon which cannot be accounted for in this way. In many other cases, however, this statistical consideration should be kept in mind, especially if it is combined with other difficulties. When some people contribute and many other do not, a decisive variable, the utility of the public good to the non-contributors, is unobservable and thus it is difficult to say whether their non-contribution is due to free-riding or simply (loosely speaking) to utility being inferior to cost. Since there are always some selective incentives around (many of them intangible), it is also difficult to say whether those who do contribute would have done so in the absence of these incentives. Testing the implication that the large members are exploited by the small members looked promising at the beginning (in the 1960s) but has become also uncertain in the course of time.

In any case, how important is the refutation of the "strong free riding" (*i.e.*, "perfect free riding") hypothesis ? Who ever believed that so extreme an hypothesis could be true (I resist the temptation to write "literally true")? These rhetorical questions are clearly related to economics being an inexact science, as explained in Hausman (1992). They were raised already, with regard to a similar kind of refutation, by D. McCloskey, whose amusing formulation I cannot resist paraphrasing as follows: Even if some econometrician told us, on the basis of some data, that the hypothesis of *perfect* free riding is rejected with 95 percent confidence, the question would be, Why in an *imperfect* world would it matter that *perfect* free riding is rejected (McCloskey, 1985, p. 157; the emphasis is the author's; the word replaced by "free riding" is "arbitrage")? We all know, by introspection or casual observation, that we and others around us sometimes but not always free ride when it is narrowly rational to do so. The

strong free riding hypothesis is too strong. The conjectures needed should fall short of so extreme a prediction.

This is why the bulk of empirical work on free riding, now that the possibility has been opened, should and does take place in the laboratory. Free riding has been the object of a great number of experiments (for a very good survey, see Ledyard, 1995). Within a relatively healthy relationship between theoretical and empirical work, many conjectures about free riding have been formed, tested, validated, rejected, etc. The essential point is that we now know a lot about free riding that we did not know some decades ago, and we are learning new aspects of it all the time (this does not mean that we know all there is to know of course). The mechanism of free riding is becoming progressively less opaque if not as yet nearly elucidated. Thanks to the isolation provided by experimentation, Buchanan's and Olson's already mentioned objections to the use of games such as the prisoner's dilemma in the setting of small-group relationships are put in a new perspective: it is possible to arrange experiments in such a way that many of the conditions (not only the payoffs, but also for instance the absence of communication) stipulated in a game are approximated. When this is done, it is not untypical to observe that about half the participants free ride and half of them do not. This can be considered as a refutation of the strong free riding hypothesis. However, a very important and robust result is that, when the game is repeated, free riding (as a proportion) increases. A possible explanation is that, in the free riding mechanism, the fear or reluctance to be a dupe dominates over greediness. To some extent, this in turn can be tested by proper arrangements (one's contributions made conditional on that of others, etc.). Another possibility is that participants are ignorant and learn. Again, this can be tested and a tentative result is that "kindness" survives the dissipation of "confusion" (Andreoni, 1995).

The literature on experiments is so fascinating that one is tempted to linger over their results. The foregoing sample is, I think, sufficient to justify my claim that the free riding mechanism can be isolated mentally without becoming vacuous and that we are learning fast about its content (we are progressively discovering, in the words of W. Salmon quoted in Section II, the "causal laws" which govern the "causal processes" and "causal interactions" making up the mechanism). That the material isolation is imperfect, that some results are unstable, that the particulars of the models on which the conjectures are based distort or reduce the significance of the results are difficulties which should be neither glossed over nor overestimated. Similar complications are found in other disciplines.

5. RE-EXPORTING KNOWLEDGE OF FREE-RIDING TO THE ACTUAL WORLD

This third issue raised by the analysis of free-riding as a mentally isolated mechanism is in a way the most delicate one. After all, as economists, we are not seeking knowledge about the free riding mechanism independently of the question of how this mechanism influences matters in the actual (de-isolated) world, in interaction or combination with other mechanisms. What are the characteristics of this interaction or combination is a question close to what I will call Mill's problem - *i.e.*, how do tendencies combine? - studied again by contemporary philosophy (see especially Cartwright, 1989). I have nothing particular to contribute to the discussion of this very difficult question. Instead, I would be inclined to argue that for three reasons, or in three ways (all related to the development of experimental economics just discussed), it can be in part evaded.

Let us assume that we have established the existence of and learned a lot about the mechanism of free riding when it is isolated. First, I think (following Bhaskar, 1975) that we are, metaphysically or ontologically, entitled to say, *without additional testing*, that the free riding mechanism also operates, in conjunction with other mechanisms, in the actual world. But, from our knowledge of the mechanism when isolated, we can probably say a little more, without entering into the details of Mill's problem. Free riding will tend to be more important in large groups than in small ones, when the stakes are high rather than a trifle, when people are mutually anonymous rather known or visible, etc. This should be assumed to carry over to non-experimental settings.[17] Similarly, a general result is that in most experiments, the order of magnitude of free riding, as measured against the optimum, turns out to be somewhere between, say, 30 and 70 per cent and that very often (as already noted) it is close to 50 per cent (see Ledyard). This kind of knowledge is enough, I submit, to consider as reasonable the unquantified assertion that free riding is also an important force in actual, non-experimental settings. And thus it is also enough, for instance, to contradict a number of claims made by Green and Shapiro. Although they are right to note that measuring the influence or incidence of free riding in the actual world is difficult or uncertain, this does not entail, as they claim or suggest, that the widespread assertion of a very substantial influence or incidence of free riding on group mobilisation, voter

[17] I put the stress here on features that seem sufficiently robust across experimental settings to be exportable also to the actual, non experimental world. This is related to what John Ferejohn and Debra Satz (1995) write about the universalism which should characterise mechanisms: "Explanations, as distinguished from descriptions, identify causal mechanisms that are held to apply across all relevantly similar contexts. Causal mechanisms are universalistic in that they can be represented independently of situations in which they are supposed to obtain" (p. 74). For a related discussion, see Cartwright (1994).

participation, production of public goods in general or institutional arrangements is groundless. To use a typically Popperian expression, experiments do provide us with *good reasons* to make that assertion.

My second reason is based on the observation that the situation that we discuss is not very different from those typically encountered in the other sciences. Mechanisms are studied in the laboratory and it is the task of *applied* science or technology (meteorology, etc.) to use the knowledge acquired there to improve the explanation and prediction of the phenomena of the actual world, in a instrumental perspective. Typically, "low-level" regularities, established, with methods that non-Popperians would call inductive, in close contact and interaction with non-experimental or technological data, are most helpful for the fulfilment of that task. But theoretical knowledge plays a role also. In economics, we are not used to view the separation between basic science and applied science in this way. We tend to think that basic science is purely theoretical - that is, unconcerned with empirical testing. I think that experimental economics, where it applies, will bring the discipline closer to the physical science in this respect. Basic research, not directly concerned with the actual world, will be much more related than it is now with the manipulation of data, albeit of a mainly experimental kind. Applied research, *i.e.*, research more directly concerned with actual world problems, will also rely on experiments. The difference between the two kinds of experiments will be related to differences in the problems raised at the two levels. As in physics or biology, we will have to accept that the relationship between the analyses and results of one branch and those of the other will not always be smooth or continuous (as stressed notably by Cartwright, 1983).

The third reason I have to be optimistic about the possibility of eschewing Mill's combination problem is that free riding can enter empirical work on non-experimental data in a more roundabout and less problematic way than the one suggested by this problem. Let me again assume that we have a relatively well corroborated prediction of the kind "under isolation, and in a wide range of circumstances (repeated interaction, one-shot encounters, small payoffs, large payoffs, etc.), free riding will typically range between 30 percent and 70 percent of the contributions that would have obtained if there had been no free riding". This might appear as perfectly useless for the purpose of empirical work on non-experimental data. But it is not: in exactly this form, it can be valuable as a relatively plausible or realistic (but untested) assumption in more encompassing theories or conjectures and it is these theories or conjectures which will be confronted to non-experimental data.

An example is Laurence Iannaccone's analysis of religions and sects (1992). It is sufficient for his purpose that, if nothing was done, some percentage of the membership of a sect would free ride, that is, not participate in all the ceremonies, and this would make these ceremonies less attractive for those who do attend (there are "positive returns to participatory crowding"); to avoid free riding of that kind, sects implement the "bizarre behavioral restrictions" for which they are best known (stigma, etc.). A number of empirical predictions are derived from the analysis and tested. In this case, as well as in similar ones, the theory itself makes free riding self-effacing as an actual phenomenon (*i.e.*, free riding is predicted not to be an outcome), but the hypothesis of its existence as a mentally isolated mechanism receives indirect empirical support from the fact that it served as a component of a theory which was itself tested successfully.

Similarly, the mere knowledge that mobilisation of a large group will always be difficult or uncertain is about enough for Olson (1982) to claim that at some point of time some interest groups (*i.e.*, categories) will be organised while others will remain "latent", and also that, in the course of time, there will be a tendency for an increasing number of groups to be organised. These claims then serve as ingredients or inputs in Olson's theory of growth and the theory itself gets tested (see Mueller, 1983). Whether this theory (or, for that matter, Iannaccone's) is corroborated is largely irrelevant to the methodological point that I am trying to make.

6. CONCLUDING REMARKS

Experimentation has played a large role in my arguments. It is clearly important in the case of free riding and it strengthens the case for mechanisms. I do not claim that either experiments or mechanisms are equally important in all areas of economics. Experimentation is generally interesting, however, also because it introduces in the debate between empiricism and its adversaries a useful complication. It opens the possibility that many hypotheses can be tested empirically, as the empiricists recommend, but only in artificial environments. With regard to the actual, non-experimental, world these hypotheses remain however disappointing to the empiricist. They are useful to understand and to explain but not (directly) to predict (they may though be useful as ingredients in other, predictive, hypotheses). Seen the other way round, the availability of experimentation means that hypotheses which were considered as untestable, and thus perhaps unscientific, become testable. The whole methodological debate in economics is very much conditioned by the assumption that the discipline is a non-experimental science (and not one benefiting from particularly

favourable circumstances like astronomy). That this assumption is becoming less warranted should be taken into account.

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