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The Invisible Hand and Science

In this paper I will discuss the idea of the invisible hand in the connection of its recent use in the philosophy of science. It has been invoked by some philosophers of science with a naturalistic bent as a part of their account of science. Some have made explicit references to the idea (Hull, 1988a) and others have only presupposed it (Giere, 1988; Goldman, 1991; Kitcher, 1993). I will argue that there are some problematic features in the way the idea of the invisible hand is used in these accounts.

I will first discuss some general properties of the invisible hand explanations and then present some motives for its use in the theory of science. Then I will show how one particular philosopher of science, David Hull, uses the idea. I will use Hull's account as a practising target and offer some comments and criticism in order to promote more disciplined use of this model of explanation in science studies.

The very idea of the invisible hand

The idea of the invisible hand has a history of a couple of centuries in political philosophy and in the social sciences. Its use is usually associated with the name of Adam Smith, who used the idea in his writings (see Rothschild, 1994). For example, Smith writes (quoted in Elster, 1978: 107–108):

> By preferring the support of domestic to that of foreign industry, [every individual] intends only his own security; and by directing that industry in such a manner as its produce may be of the greatest value, he intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention. Nor is it always the worse for society that it was no part of his intention. By pursuing his own interest he frequently promotes that of the society more effectually than when he really intends to promote it. I have never known much good done by those who affected to trade for the public good.

Adam Smith made the idea widely known and also gave it its name. The actual history of the idea is somewhat longer (see Elster, 1978: 106–108). After Smith there have been legions of more or less disciplined users of the idea (Nozick, 1974: 18–22; Elster, 1978: 106–111; Brennan & Pettit, 1993: 198–205).

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The invisible hand has also found its way to evolutionary biology by the way of Darwin's theory of natural selection (Sober, 1984: 189–193). The general reputation of the idea of the invisible hand has not always been very good, but it certainly has some appealing virtues that have brought it in to discussion again and again.

How should one characterise an invisible hand explanation? The first thing to note is that in an invisible hand process the outcome to be explained is produced as a result of a *decentralised* process. There are no explicit agreements or centralised decisions by the participating agents (Brennan & Pettit, 1993: 195–196). The second essential component is that the process is also *non-intentional*: The agents do not intend to produce the result. They are promoting their own objectives and the result to be explained is a *by-product* of this promoting. The idea is that the process should work even if the participating agents have no knowledge of the process. This is why the mechanism is called invisible (Ulmann-Margalit, 1978: 271).

One should note that the term invisible hand is only a name, not a description. The invisibility is not an essential requirement for a process to be considered an invisible hand process. The result in question need not be unknown to the agents participating in its production. Modern discussion about the invisible hand allows that the agents know the result of their actions. It is however required that the agents do not intend to produce that result. The result should be an unintended consequence of the action, that is a by-product of intentions to do something else (Brennan & Pettit, 1993: 196–197). Robert Nozick goes as far as considering the case where agents actually intend to bring about the result, but where the actual mechanism that produces the result is different from the one the agents believe it to be, as a case of the invisible hand (Nozick, 1994: 314).

There are also some requirements that the result of the process should meet in order to claim the right to be called a product of a *hand* that is invisible. The result should be a pattern or a structure that seems to be made or designed intentionally; it should be somebody's handiwork (Ulmann-Margalit, 1978: 268–270). This means that the product in question should be somewhat complex and it should not seem to be accidental. To be non-accidental, the result should be somewhat stable and recurring (Brennan & Pettit, 1993: 191–192).

There are two kinds of processes that can work behind agents participating and which should be clearly differentiated: the invisible hand and the hidden-hand. In the hidden-hand process there really are agents working behind the backs of the others. These explanations are familiar conspiracy theories that have nothing to do with the invisible hand (Nozick, 1994: 316). In an invisible hand process these conspiratory agents are missing and this is why an invisible hand explanation is so mysterious: how can something that seems to be a product of intentional design come to exist without an intentional maker? How is this spontaneous order created?

It is the *mechanism* of the invisible hand that is responsible for its explanatory force (when it has one). Without the mechanism the invisible hand account is only a statement of a surprising fact to be explained. It is by understanding this mechanism, that one can understand how the miracle of spontaneous order is produced. The mechanism works by combining the contributions of the participating entities in a way that the result to be explained follows. The mechanism may be composed of two kinds of processes. There are *filtering processes,* wherein some filter eliminates all entities that do not fit into a certain pattern. There are also *equilibrium processes* wherein each component part adjusts to local conditions and in this way changes the environment of the others close by, so that the sum total of these local adjustments realises a pattern (Nozick, 1994: 314; for a different terminology see Brennan & Pettit, 1993: 193–195). Very often the actual mechanism is a combination of both of these processes.

There is another supra-intentional

explanatory mechanism used in the social sciences that is worth mentioning, because it is a close relative of the invisible hand: *the invisible backhand*. If in the paradigm example of the invisible hand the pursuit of selfish interests produces a common good, then the invisible backhand is contrary to it. The pursuit of good intentions produces something the actors do not wish to have. From the methodological point of view both the invisible hand and the invisible backhand are of same value, both explain the order that individual actors have produced unintentionally (Elster, 1978: 106–110). The only difference is that the product of the invisible hand is valued positive and the product of the invisible backhand negative (Brennan & Pettit, 1993: 192, 204–205).

One should take care to notice that the idea of the invisible hand explanation can be used in two different ways. The first one might be called an ideological way and the second a methodological way. Both uses try to give invisible hand explanations, but their actual explanatory statuses are very different. The *ideological* use is familiar as a defence for *laissez-faire* capitalism. The existence and the proper working of the mechanism are taken for granted, without specifying the boundary conditions for its functioning. Usually even the description of the mechanism that does the explanatory work is left without details. There is also no empirical confirmation that the conditions for the working of the mechanism are actually fulfilled.

In contrast the *methodological* use starts from the fact that it is contingent if the mechanism is working. It is demanded that the existence of the mechanism is shown, not just assumed. This means that there is a demand for a detailed description of the proposed explanatory mechanism. It is also thought that it is necessary to make explicit the boundary conditions for the working of the mechanism proposed and to ascertain that they are actually fulfilled.

There is not much interesting to say about the invisible hand in general. The interest is clearly in its particular instances and especially in the particular mechanisms with which these instances work. From the general perspective it is not usually clear which explanations should be categorised as invisible hand explanations proper. For example, the invisible hand in the evolutionary theory does not include intentional agents, because the organisms participating do not intend to do anything at all. This means that the produced adaptations cannot be by-products of intentional action. Is this a reason to say that evolution by natural selection does not include the invisible hand? Maybe not, because it clearly meets the other requirements of the invisible hand and its explanatory mechanism can be used as a model for processes that really include intentional agents, as we will shortly see.

Why is the invisible hand needed in science?

The idea of the invisible hand in science has been used by many contemporary philosophers of science. They are not, however the first ones to use this idea in the context of science. For example, Michael Polanyi in an article published in 1962, describes science as an invisible hand process. However, Polanyi does not make explanatory use of the idea. He just uses the term to describe the self-co-ordination of independent performers of scientific work (Polanyi, 1969: 50–53).

The more recent employers of the idea include Ronald Giere (1988), Alvin Goldman (Goldman & Shaked, 1991), David Hull (1988a), and Philip Kitcher (1993; 1994), who certainly are the most notable figures in the recent naturalistic movement in the philosophy of science (for naturalistic movement see Callebaut, 1993). There are also other users of the idea (Quillian, 1994; Solomon, 1994a; 1994b), who are not as famous, and whose premises for the use of the idea are somewhat different. For most of these philosophers of science, the invisible hand is just an unarticulated presupposition that is not analysed further. It seems that only David Hull has explicitly invoked the idea of the invisible hand, calling it the visible hand of science (Hull, 1988a; Chapter 10). The fact is, that for example Giere, Goldman and Kitcher, discuss only the actions of individual scientists, they simply presuppose that a larger scientific process instils biases and mistakes that affect individual scientists. How this happens is not indicated. It is clear that also these thinkers must presuppose something like the invisible hand.

It is interesting that only philosophers have invoked this idea. Most representatives of other disciplines in science studies have thought that they can do without the invisible hand. Philosophers seem to have one item on their agenda that others in science studies do not seem to have: the defence of the objectivity or rationality of science. Why do philosophers, and philosophers alone have this item on their agenda? This is an interesting sociological question that cannot be addressed here.

But it is not a mystery why of all philosophers, the naturalistic philosophers of science have invoked the idea of invisible hand. They have been committed to offer an explanatory account of science, and because of their naturalism they are also bound to take into account some empirical facts about science. Why have these various writers thought that the help of the invisible hand is needed in science?

Firstly, the human cognitive limitations are seen to affect an individual scientist. The experimental studies of cognitive psychology have shown that humans and also scientists are prone to bad reasoning habits, judgmental errors and other cognitive defects. There is of course much discussion on how to interpret these results, but the fact is that the individual scientist is not an ideal reasoner (see Faust, 1984; Nisbett & Ross, 1980; Kahneman, Slovic & Tversky, 1982).

Even if we humans were without these defects, there are other cognitive limitations. Scientific literature is expanding and different specialities are born at such a rate and measure that there is no hope that an individual scientist can keep track of all the developments that affect his own speciality, not to mention the neighbouring specialities. Purely physical reasons are responsible for this: there is not enough time and human information processing capacity to handle all the relevant data.

Secondly, there are non-cognitive factors working in science. Sociological studies on both contemporary science and on the history of science show that scientist do not live up to their idealised public image. Scientists are certainly interested in other things than the truth. They have all kinds of non-cognitive commitments and interests. The interests vary from different kinds of purely extra-scientific interests to professional interests that are internal to science. Even if, as one might hopefully suggest, all scientists had a common interest in finding the truth, they have very different conceptions of how to get there. And of course the truth is not ever the only interest. There is of course much controversy about interests and their consequences in science, but their existence is not in question. One can say that scientists are *interested*, which contradicts the usual picture of disinterested scientists.

Thirdly, there is no such thing as the Scientific Method. Of course, the reference to the Scientific Method has always been a part of the scientific rhetoric, for example in demarcation controversies. But this is all that the Scientific Method is: a rhetorical construction. There is no algorithmical or mechanical procedure that can be used to produce certain or even plausible scientific facts. If science is rational, it is not because of some algorithm, be it deductive or probabilistic. This does not of course mean that there are no methods in the sciences. There are plenty. But they don't have any interesting common essence that could be named as the Scientific Method.

The lack of the Scientific Method means that there is no easy procedural solution to our problem: if the objectivity of science depends on the existence of the Method, we can give up the whole idea. After several centuries of searching, the Method remains still unformulated, and it is also quite a mystery that scientists have done so well without knowing the Method that has been said to explain their success. Therefore it becomes clear why a naturalistic philosopher of science cannot accept mystical explanations, such as a reference to the Scientific Method in their accounts of science.

Fourthly, the norm-oriented accounts, such as Robert Merton's famous four norms of science, do not work. The Mertonian norms might describe rhetorical resources that scientists use, and in this way they might not be fully without effect. But they do not explain why science works. The Mertonian account does not explain why scientists follow these norms or why they make rhetorical use of them. Neither does it explain why science is as successful in producing knowledge as it is. The norms might be a partial explanation, but it should be supplemented by something else.

To summarise, one can say that scientists are *humans without a great secret of success* (that is to say without the Scientific Method). So we might have to get rid of some our usual ideas about the nature of science. Are the ideas of objective knowledge and of the cognitive authority of science among these? The idea of the invisible hand is supposed to save us from throwing them away along with other things. It refers to a naturalistically acceptable process, in a way that a naturalistic philosopher of science can accept it. It would show that there is no mystery after all in the discrepancy between the results of science and the limitations and biases of the agents participating in its creation.

How is it thought to work in science?

I have claimed that the only writer among naturalistic philosophers of science who has tried to make explicit his use of the invisible hand is David Hull, best known as a philosopher of biology. But it seems that also the other philosophers mentioned must accept some kind of an invisible hand in their account of science. This is because they all accept the previously mentioned arguments on science. They also hold that the cognitive performance of science as a whole is better than the cognitive performance of the individuals participating it. Furthermore, they all defend scientific knowledge and the authority of science against the skeptical inferences drawn by some sociologists of scientific knowledge. Because they are all committed to give an explanatory account of science, the only acceptable choice seems to be the invisible hand.

How does David Hull think that the invisible hand works in the sciences? My presentation is selective and it omits many interesting parts of his theory, which are not essential from the point of view of this paper. (For more details reader may consult Hull's own book-lenght exposition of the theory, Hull, 1988a; also Hull, 1988b presents the main ideas).

It is evident that Hull is really using an invisible hand account. He writes:

> Although objective knowledge through bias and commitment sounds as paradoxical as bombs for peace, I agree that existence and ultimate rationality of science can be explained in terms of bias, jealousy, and irrationality. As it turns out, the least productive scientists tend to behave the most admirably, while those who make the greatest contributions just as frequently behave the most deplorably (Hull, 1988a: 32).

He does not simply say that science works even when scientists aren't what we think them to be. His claim is much stronger; he writes:

> The romantic view of scientists as dispassionate, disinterested seekers after truth for its own sake needs debunking, ... because there is some danger, though not much, that scientists might actually be tempted to put this romantic view into practice. If scientists at large adhered to the professed more of science, science

might be possible but I doubt it. (Hull, 1988b: 125).

If these quotations do not sound like paraphrasing our usual account of the Mandevillean fable of 'Private Vices, Public Benefits', then what does?

Hull's theory of science is based on his account of evolutionary theory. It might be said that what Charles Darwin borrowed from Adam Smith's account of society in his account of evolution (see Sober, 1984: 189–193) – the idea of the invisible hand – David Hull is bringing back to social science in a Darwinian package.

In Hull's account, scientists are seen as agents trying to maximise their conceptual inclusive fitness. They are trying to make other scientists accept their accounts as widely as possible. This is of course an analogue to biological evolution, where it is understood that organisms try to promote their genetic inclusive fitness. As in biological evolution, where organisms do not promote their genetic inclusive fitness intentionally, in science scientists are not promoting their conceptual inclusive fitness intentionally. Hull's theory is not about motives or goals that drive individual scientists. It is about the mechanism by which contributions of these individuals become to be objective knowledge, or at least scientific knowledge. But as a scientist succeeds in science, she is at the same time promoting her conceptual inclusive fitness (Hull, 1988b: 128).

According to Hull science functions the way it does because of its social organisation. The social organisation of science harnesses scientists's "base" motivations for more "loftier", nobler goals. This means that scientists need not sacrifice their individual interests for the greater good. What is good for an individual scientist, is also good for the science in general (Hull, 1988a: 31–32, 357).

The mechanism by which scientific knowledge is produced rests on the relations which exist between credit, use, and mutual testing. The driving force in science is the pursuit of credit and recognition by individual scientists and research groups. This striving is bounded by two factors: the need to use each others work and the possibility of empirical checking. The credit in question can be conferred in many ways, but the most important of these ways is the use that one scientist makes of another scientist's work, preferably with an explicit citation. Conversely, the worst thing that a scientist can do to another, is to ignore him or her totally (Hull, 1988a: 305–309).

Scientists are competing in getting their ideas accepted. To succeed in this they have to make their case as convincing as possible. One's ideas have to be better than those of his competitors and they should stand empirical tests. Scientist will accept only those ideas that they think can be used to support their own work. In this way the filtering mechanism can work in two ways. First individual scientists filter out those ideas that they think cannot work. This filter is of course fallible because individual scientists work with their different heuristics and there is no guarantee that those heuristics really work. The second filter is the Nature, or pragmatic success: the ideas are filtered by the success of their users. Only those scientists succeed who have accepted the ideas that really work. This is the way the second filter corrects the first one. Only the ideas that really work and their proponents stay in game as the time goes by (Hull, 1988b: 139–143).

Scientists want their work to be acknowledged as original, but they also want it to be accepted. To have it accepted, they must gain support from other scientists' work. Without the support of others, there is no way to get the work accepted. A scientist must take care to make her case convincing and a way to do this is to use other scientists as authorities. A peculiar trade off exists. One way to gain support is to show that one's work rests solidly on the preceding research, but the price of this is a decrease in apparent originality. But if one exaggerates one's originality one will not get one's work accepted. Scientist would like total credit and massive support, but they cannot have it both ways. They are forced to trade off credit for support. The credit is not generally given where it is due but where it can be useful (Hull, 1988a: 304–6, 357).

Those who are familiar with the recent sociology of scientific knowledge might notice that Hull's account has many similarities to the theory of the cycle of credibility by Latour and Woolgar (Latour & Woolgar, 1986: Chapter 5) As they show, the success in getting credit can be transformed in to resources for new research, which again makes possible new results and recognition for them. Without credit the pursuit of professional interests is impossible. In this way scientists are dependent on each other. They need each others support and acceptance, but because credit is given to originality, there is also competition. Also Kitcher (1993) and Goldman & Shaked (1991) use this same idea of a credibility cycle. This idea seems to be the most widely accepted part of Latour & Woolgar (1986; originally published 1979).

One should note that in Hull's account, the units that participate in this competition are not necessarily individual scientists. The research groups can be primary actors in this game of credibility. In fact, one of Hull's main ideas is the idea of the demic structure of science. Because few scientists have all the skills and knowledge needed to solve the problems they confront, they tend to band together to form research groups. These research groups are useful for the sharing of conceptual resources and in this way they make the conceptual evolution more effective. These research groups are considered analogous to the demes in biological evolution (Hull, 1988a: 433–437).

According to Hull, his model explains why the fabrication of results is more sanctioned in science than stealing. Stealing hurts only those whose work has been appropriated, but lying will hurt anyone who uses the work in question. Misassigned contributions are just as useful as works whose authorships are attributed correctly (Hull, 1988a: 311–319).

The idea of competing scientists and research groups is essential to the functioning of the invisible hand. Individual scientists and groups are not very good at exposing faults in their own reasoning and results. They are always to some extent prisoners of their own conceptual system. But competing groups are not limited in this way, and they have an interest in exposing these faults. This is because one way to promote your own ideas is to show the shortcomings of the ideas of the others. Their career-interests are not damaged if your views are shown to be mistaken. This way the essential self-correction in science does not depend on scientists being totally unbiased or having no career-interests, but on other scientists having different perspectives, and conflicting career-interests (Hull, 1988a: 22–23).

So, Hull is offering us an invisible hand account of science, where scientists produce objective knowledge as a by-product of their pursuit of professional self-interests. In Hull's application of the invisible hand there is an interesting twist: scientists, at least sometimes, claim that they are aiming at the truth. Anyway, this is not an argument against the use of the label "an invisible hand explanation": objective knowledge is produced by different mechanisms than those imagined by agents themselves (See Nozick, 1994: 314). If scientists think that the truth is a result of their intentions, they are committing an animistic fallacy: it does not follow from the fact that they intend to do something and when something happens, that this something happened because of their intentions.

So, what is the problem with the idea?

There are many great insights in Hull's theory, and I have personally great sympathy for some parts of it. For example, the idea of the truth as a by-product of scientific research is fascinating. But there is still something suspicious in the general picture. Especially, I find his use of the idea of the invisible hand very problematic. In the following I will try to articulate what is wrong with the use of the idea.

One problem in trying to articulate what is wrong with Hull's account, is that his own presentation of his ideas is rather vague. The book is rather long and the way in which its contents are organised is not the best possible. This means that I cannot be certain that my criticism is exactly based on his ideas or that he is committed to all the assumptions that I suppose he has made. But I hope that my criticisms at least can offer an useful mapping of the dangers inherent to the idea, a mapping that can be useful in forthcoming discussions on the philosophy of science. I believe that my points will also apply to the other recent naturalistic invisible hand accounts that I have mentioned (if they will ever be articulated by their authors). There might be some differences between these authors and Hull, for example in how great the explanatory gap between the individual cognitive performance and the cognitive performance of the scientific community is thought to be. There might also be some differences in the details of the invisible hand presupposed. But I would claim that the general picture must be rather similar, because there really are not many alternatives from which to choose.

In order to be able to evaluate Hull's use of the invisible hand, one has to think about the invisible hand in a more disciplined way than he does. There is a set of questions that should always be asked about the use of invisible hand. The first question concerns the quality and the nature of the results produced: are they really what one (or somebody else) wants them to be? Secondly, one should try to make explicit the presuppositions under which the proposed mechanism works. This includes two different questions: (i) what is the nature of the mechanism? and (ii) can the mechanism proposed really deliver the promised goods? The third question is an empirical one: are these presuppositions actually fulfilled? There is a difference between a possible mechanism and the actual mechanism. The last question is a comparative one: is the mechanism proposed the best mechanism available to produce the results?

As I claimed in the first section, there are in principle two different ways to use the idea of the invisible hand. These two differ in how they answer to the above mentioned questions. The *ideological* use takes the existence and proper working of the mechanism for granted. Usually this indicates that the details of this explanatory mechanism are left vague and that the ideological users are not interested in an empirical confirmation that the conditions for the working of the mechanism are actually fulfilled. They will typically also suppose without argument that the results are the ones desired, and that the invisible hand is the optimal (or the only) way to produce them. In contrast, the *methodological* user starts from the fact that the working of the mechanism is contingent. For him, the existence and the proper working of the mechanism are to be shown, not assumed. This of course presupposes that the presuppositions of the mechanism are articulated. For the methodological user, it is an empirical question whether the mechanism really is the optimal one. It is clear that only the methodological use of the idea is really explanatory. This means that because naturalists are committed to give an explanatory account of science, they should use the idea of invisible hand in a methodological way, if they are to use the idea at all.

Let us start our evaluation of the naturalist user of the invisible hand with the question concerning the results produced. What kind of results does one (or somebody else) want the invisible hand to produce? This is not an explanatory question, but it is still an important one. Firstly, when one labels something as an invisible hand process, one takes the evaluations of the agents participating (or our own evaluations) and compares them with the results produced. If the results are the ones desired, one might call the process an invisible hand process. Secondly, the fourth question in our set, the comparative one, presupposes that one has a measure to compare alternative mechanisms. Thirdly, if one is to defend the current practices in science with the idea of the invisible hand one needs some independent criteria to show that the current practices are really as good as they can get.

In this count Hull and his naturalistic allies seem to be rather weak. The naturalistic philosophy of science they promote has been criticised for its normative anaemia. It has been claimed that a purely descriptive account of science is not enough, and that there is plenty of room for both normative considerations and improvements of cognitive practices. Naturalistic accounts of science seem to take the current practices of scientists – the status quo – for granted. They seem to suppose that science is as good as it can get (see for example, Fuller, 1994).

The critics have claimed that ideals should be set higher than current practices. This criticism seems justified. The invisible hand variety of philosophical naturalism seems to invoke some kind of hyper-naturalism: it can even make the best of the seemingly non-optimal practices. For example, Hull seems to use the following heuristic: "if scientists behave in certain ways, then possibly there is something to be said for this behaviour". Even practices in science that seem to us dysfunctional are in reality perfectly functional, so science is at its best as it is. Here the invisible hand seems to work as an excuse for "vices" of scientists. This is dangerously close to the Panglossian adaptationism, that Hull will not accept in biology. And from the normative standpoint this is a very disappointing stance. Of course, it is possible that science is as good as it can get, but this needs arguments in its favour. A naturalist should be suspicious of this kind of a coincidence of the actual and the desirable. The question about the aims of science is a real value question that cannot be answered by a descriptive study of current science. In order to evaluate the workings of the invisible hand, one needs some standards, and taking status quo for granted is not the way to get them.

Let us next ask the question concerning the mechanism: What are the presuppositions that need to be fulfilled to mechanism for the work? The idea of the invisible hand easily suggests to us that things work fine by themselves. But the real secret of the invisible hand is always in its mechanism and this mechanism works only if its presuppositions are fulfilled.

I take that it is obvious to everybody that there are structural presuppositions for working of science both internally and externally. Hull seems to consider only the first kind of presuppositions. His supposition seems to be that science is a self-sufficient part of a reality that has no significant connection to the society outside it. Hull is a proponent of free and autonomous science (in this context one might talk about free scientific markets), and he also takes for granted that science really has this autonomy (at least within a certain measure). But clearly he should articulate the preconditions for this autonomy, because it seems to me that he thinks that this autonomy is also a presupposition for the working of the mechanism.

Maybe the impression of an autonomous science is only natural to Hull because he has mostly studied biological taxonomy and highly theoretical discussions in evolutionary biology, both of which are quite remote from societal interests. However, this conclusion is not supported in terms of science in general. This brings to mind a particular question: how are these non-autonomous fields doing? Hull claims that science works best when it is left alone, but what is his evidence? Some comparative data would be helpful. One should also remember that there are many ways in which the societal interests can affect science. Some are certainly detrimental to science, but some of them might even be good for science. My point is that one needs an articulation of presuppositions in order to say how much freedom is good for science or for society in general. There may also be great differences between different scientific fields.

A part of the problem is the notion of

interest that Hull uses. It is far too narrow. He supposes that interests that drive individual scientists cancel each other out, when science is working properly. He de-intentionalises scientific objectivity in order to defend it, but maybe he should also de-intentionalise the interests that threaten it! It may seem from the inside point of view that science is working freely and without societal interests, but actually the direction of science can be highly directed. This is possible because you don't have to use for example, military-minded scientists to produce militaristic science. All you need to do, is to fund research that can be used this way. Regard for scientists' self-understanding is not necessary. I believe that Hull might be committing this fallacy, because he does not take into account that science is highly dependent on resources coming from outside of it. The "real" market forces might have a quite distinctive effect on science. For example, if outside agents only filter the questions that scientists should address, this will not affect the proper working of the mechanism that Hull has proposed. One can have epistemic autonomy without self-regulating scientific markets. But as said, these external presuppositions of the mechanism are not articulated.

What about the structural presuppositions within science? Only after these presuppositions are clear, can one accept the mechanism proposed. And only articulated presuppositions can help one to delineate the boundaries within which the proposed mechanism can work. In this aspect Hull has a certain advantage over other models such as the ones proposed by Kitcher and Goldman. Their social epistemology seems to include only highly idealised models (analogous to those of microeconomics) concerning the choices of individual scientists and the way their co-operation and competition *can* bring about desirable results. The institutions that make these interactions, and the results they possibly produce are not raken to consideration. In contrast, Hull clearly states that it is the institutional structures that make the invisible hand work.

The problem with Hull is that he has not yet shown what these institutions are and how they make things work. It is not certain that his account gives the resources to do this. In this sense he is in the same boat with Kitcher and Goldman. Their mutual problem is their commitment to both methodological individualism and epistemic individualism. Here I will only discuss the former. It seems to me, that even the authors themselves admit that microeconomics and decision-theory are not rich enough approaches for the understanding of science and cognitive practices in general. So, why don't they use more "social" approaches in their accounts of science? It seems that the obstacle for this is the unarticulated commitment to methodological individualism. For example, Philip Kitcher (1994: 116) accepts the somewhat outdated version of methodological individualism as a premise for his individualistic preference. This commitment is not the only available choice. Recent discussions on the philosophy of social sciences indicate that one does not need to choose between atomistic individualism and holism that presupposes some supra-intentional entities as agents in the society (Pettit, 1993). In order to make the invisible hand account acceptable one needs also some structural elements, otherwise the mechanism will always stay somewhat mysterious. And in order to make room for these structural elements, one needs to give up unnecessary individualist commitments.

The third set of questions concerns the factual accuracy of the account. Are the presuppositions of the mechanism that Hull proposes fulfilled in modern science in general, and are they fulfilled in individual sciences? Hull certainly has shown that in theory science can work by the narrow self-interest of scientists. Does science actually work this way? Although Hull's book includes a very large selection of empirical material, he does not try to test his theory on this material. He uses his empirical material only as a heuristic aid. This again sounds like an ideological practice, as I have defined it. In Hull's defence, one might say that there are some difficult methodological problems in this kind of testing. For example, where to get the comparative data? But the criticism stands: one should at least try!

When trying to find out if science really works in the way Hull says it does, one might find out that actually there are some other mechanism working within it. One should also be ready to accept the fact that science might not work properly. The question should be answered empirically, not with a conservative or a revolutionary bias. This brings us naturally to the last set of questions. Are there possible alternative mechanisms and how effective are they? It is only after this kind of a comparison, that one is justified to claim the effectiveness and desirability of the invisible hand. Is it really so, that self-regulating scientific markets are the only, or at least the most optimal way, to promote the epistemic aims and the other aims of science?

When one does not yet have a theory that shows how science really works, one cannot say for certain whether there are possible alternative mechanisms. From a speculative point of view one could ask if the the performance of the invisible hand is made better if it is turned into a *hidden-hand mechanism*? If the process of science is really an invisible hand process, how will it be affected when one starts to regulate and manipulate this process from the outside? I think that social epistemology proposed by Steve Fuller and social empiricism by Miriam Solomon take this possibility seriously. (Fuller, 1993; Solomon, 1994a, 1994b) Surely there are many problems with this kind of a "bureaucratic alternative" for the invisible hand. Who will be the regulators? From which kind of a knowledge basis will they be making their decisions? What are the aims and means? But the most interesting question from the perspective of this paper is: will science work better without regulation (as Hull for example claims) or when regulated (as Fuller and Solomon claim)? One should notice that the aims of these regulators need not be non-epistemic, the advancement of epistemic objectives of science might just as well be their only interest. The bureaucratic alternative needs not to be an authoritarian or an anti-scientific alternative.

I have posed a set of questions which the naturalistic user of the idea of the invisible hand should ask. The indifference of naturalists to these kinds of questions indicates that their use of this idea has at least some ideological dimensions. This means that they have not been naturalists enough! My arguments should not be taken as definitive refutations of naturalism. Rather, the points I have made can be seen as constructive criticism. And the more general lesson is not that the invisible hand mechanism should not be used, but that it should be used with caution and methodological strictness. Otherwise it can be a very dangerous idea.

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