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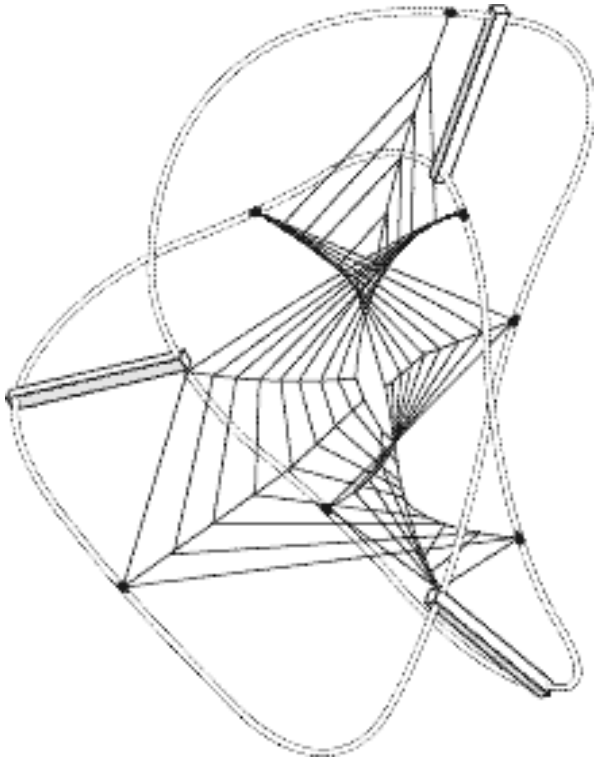
Gabriele Contessa
LSE



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IS DISSENT A CHALLENGE TO THE RATIONALITY OF SCIENCE?

KITCHER ON THE DIVISION OF COGNITIVE LABOUR*

Gabriele Contessa

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ABSTRACT

There is a spectrum of views about the significance of dissent in science. At one end of this spectrum are those students of science who tend to praise dissent as the main source of scientific development and believe that the more dissent there is, the better it is for science. At the other hand, are those who, like the social constructivists, claim that dissent in science is evidence in favour of the a-rationality of scientific decision-making. In this paper I will critically examine Philip Kitcher's model of the division of cognitive labour against this background. I take Kitcher's model to be an attempt to meet the social constructivists challenge, but I will argue, from this point of view, the model is not successful. Then, I consider some further philosophical difficulties to which the 'collectivistic' methodology which underlies Kitcehr's model may give raise. Furthermore, I claim that, rather than confronting directly social constructivism as Kitcher does, it is possible to put their challenge into perspective. However, in spite of its dubious merits in contrasting the social constructivist challenge, Kitcher's model turns out to be an interesting contribution to the debate. An interesting intuition underlies this model. This intuition is that the question of whether and how much dissent is beneficial to science is not a philosophical question but an economical one – it is the question of how the limited resources available in a given field are better allocated and this question can only be answered on a case-by-case basis.

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Comments to: g.contessa@lse.ac.uk

Is Dissent a Challenge to the Rationality of Science?

Kitcher on the Division of Cognitive Labour

I

Once upon a time a story used to be told both to legitimise scientific knowledge and to explain the successes of modern science. The story goes that there is a procedure, the Scientific Method, which univocally determines which theory is rational to choose among the available ones. According to this story, the fact that all scientists follow the same procedure is supposed to warrant both the rationality of their choices and the objective nature of scientific knowledge.[1] Unfortunately, this story is incompatible with some basic things we know about actual science: for instance, that scientists often dissent among themselves on the respective merits of different theories, models and strategies of research.

There is a spectrum of views on the significance of dissent in science. One end of this spectrum is occupied by those who praise dissent as the source of innovation in science. According to this view, of which Paul Feyerabend is often portrayed as the champion, the more theories are proposed and developed, the better. At the other end of the spectrum are those who claim that dissent is evidence that there is no principled way to choose between competing scientific theories.[2] Social constructivists perhaps represent the most radical version of this position. A social constructivist would claim that scientists disagree from each other only because they make their choices on the basis of their idiosyncratic inclinations, private interests, and individual values rather than on the basis of the Scientific Method, which they regard as a myth. Accordingly, the social constructivist explains dissent as an attempt by a marginal group of scientist to gain control of the resources in their field, attributes consensus in the scientific community to negotiations among the rival factions, and regards scientific knowledge as a social construction.

Against social constructivism, a view has emerged that aims to show that disagreement among scientists is not evidence for the a-rationality of scientific decision-making, and that the personal interests of the scientists are not necessarily detrimental to science ((Giere 1988); (Hull 1988a); (Hull 1988b); (Laudan and Laudan 1989); (Kitcher 1990); (Goldman and Shaked 1991)). Though this view aims at a particular strategy, it has been pursued via several, often unrelated, sometimes even mutually contrasting, strategies.[3]

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[1] See, for instance, (Hatfield 1998) and (Giere 1988).

[2] See, for instance, (Shapin 1982).

[3] See, for example, Alvin Goldman and Moshe Shaked (1991) assume that scientists can be represented as Bayesian agents, whereas Ronald Giere (1988) argues that there is empirical evidence that scientists cannot be considered Bayesian agents and, thus, adopts bounded rationality models.

In this paper, I restrict my attention to a model proposed by Philip Kitcher (1990, 1993) which I deem particularly interesting. In Section II, I present Kitcher's model and the two main theses in favour of which Kitcher argues. In Section III, I argue that Kitcher's model does not succeed in meeting the social constructivist challenge. In Section IV, I consider some further philosophical difficulties to which the 'collectivistic' methodology which underlies Kitcher's model may give rise. In Section V, I claim that, rather than confronting directly the social constructivist, it is possible to put their challenge into perspective. In Section VI, I claim that, in spite of its dubious merits in contrasting the social constructivist challenge, Kitcher's model turns out to be an interesting contribution to the debate. An important intuition underlies this model. The intuition is that the question of whether and how much dissent is beneficial to science is not a philosophical question but an economical one – it is the question of how the limited resources available in a given field are better allocated and this question can only be answered on a case-by-case basis.

II

In this section, the model proposed by Kitcher is presented and the two main theses he argues for are expounded. Let us consider a community of N scientists who have to choose between two competing theories (T_1 and T_2). Let us assume that there is a probability function $P(T_i)$ which assigns an objective probability value to the hypothesis that T_i is true given the available evidence. Let us further assume that there is a return function $r^*(n)$ which gives the probability that a theory, if true, will overcome all its problems, if n scientists work on it for a given amount of time.

According to the above story, the rationality of theory choice should be warranted by the fact that scientists unanimously choose in accordance with Scientific Method. Suppose that for our community Scientific Method amounts to the maxim: "Choose the theory which maximises $P(T_i)$" and suppose that $P(T_1) > P(T_2)$, the community will, then, reach the limit distribution $<N, 0>$ (where the notation indicates that N scientists pursue T_1, and no one pursues T_2). Let us call such a distribution the Individual Rationality-distribution (IR-distribution).

Suppose that the utility of adopting and developing the true theory is given by u and that the expected utility of a $<n, N – n>$ distribution is given by:

$$P(T_1)r^*(n)u + P(T_2)r^*(N – n)u.$$

Thus, an $<m, N – m>$ distribution is preferable to an $<n, N – n>$ distribution if and only if:

$$P(T_1)r^*(m)u + P(T_2)r^*(N – m)u > P(T_1)r^*(n)u + P(T_2)r^*(N – n)u.$$

A $<m°, N – m°>$ distribution is a Community Optimum-distribution (CO-distribution) if and only if, for every m:

$$P(T_1)r^*(m°)u + P(T_2)r^*(N – m°)u \geq P(T_1)r^*(m)u + P(T_2)r^*(N – m)u.$$

Once the scene is set, it is possible to present the two main theses the model is meant to show. The first thesis is that IR-distributions are not necessarily CO-distributions. There can be circumstances in which the CO-distribution is a genuine $<n, N – n>$ distribution (where $0 < n < N$). The second thesis is that a community of scientists guided by extra-epistemic goals can spontaneously attain a CO-distribution.

Since it seems highly plausible to assume that the probability of a theory overcoming its problems if no scientist works on it is 0, then a <N, 0> distribution is preferable to an <n, $N - n$> distribution if and only if:

$$P(T_1)r^*(N)u > P(T_1)r^*(n)u + P(T_2)r^*(N - n)u.$$ [4]

The received view of scientific rationality commits us to maintain that the previous inequality holds for every n (where $0 < n < N$), independently of the values of $P(T_1)$, $P(T_2)$ and independently of the form of the function $r^*(n)$. However, it is possible to specify sets of conditions under which this claim is false. Suppose, for example, that $r^*(n)$ takes the following form:

$$r^*(n) = \frac{\frac{(3n^2 - 2n^3)}{kN}}{k^2N^2} \quad \text{for } n < kN$$

$$r^*(n) = 1 \quad \text{for } n \geq kN.$$

If $0 < kN < N$, and the probability of T_2 is different from zero, then for every m where $kN \leq m < N$:

$$P(T_1)r^*(m)u + P(T_2)r^*(N - m)u > P(T_1)r^*(N)u.$$

It is, thus, possible to conclude that, in principle, a community which divides its cognitive labour, to use Kitcher's phrase, can be better off than a unanimous community. The cognitive labour in a community whose choices are solely informed by Scientific Method may be sub-optimally distributed.

This leads us to consider the second thesis maintained by Kitcher. He claims that it is not necessary to presuppose that the community is composed of purely epistemic agents (i.e. agents whose only purposes are impersonal epistemic purposes like the advancement of scientific knowledge) in order for it to attain a CO-distribution. Suppose that the scientists in the community are driven by the personal extra-epistemic goal of being singled out by the posterity as early champions of the true theory. More precisely, Kitcher assumes that a scientist will switch from T_1 to T_2 if and only if:

$$\frac{P(T_2)r^*((N-n)+1)}{(N-n)+1} > \frac{P(T_1)r^*(n)}{n}.$$

If the community is initially in a <m, $N - m$> distribution (where $kN < m < N$), there is a stable <$m°$, $N - m°$> distribution where:

$$m° = \frac{P(T_1)N}{P(T_1) + P(T_2)}.$$

Such a distribution is a CO-distribution, provided that $k < P(T_2)$.

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[4] Surprisingly, Kitcher does not consider the expected utility of the <N, 0> distribution to be a limit case of the expected utility of <n, $N - n$> distributions. According to Kitcher, the expected utility of a <N, 0> distribution is given by:

$$P(T_1)\, u^* - P(T_2)u^*.$$

There seem to be good reasons not to follow Kitcher on this point. First, the minus sign in the previous expression reveals the assumption that a utility loss stems from not pursuing any theory whose probability is different from zero. This suggests that we could be tacitly assuming more than we want to show - i.e. that theoretical pluralism is always good. Secondly, there is apparently no reason to maintain that the expected utility of N scientists pursuing a theory in a community composed by N scientists ($P(T_1)\, u^* - P(T_2)u^*$) is different from the expected utility of the same number of scientists pursuing the same theory in a community of (N+1) scientists (which is given by $P(T_1)r^*(N)u$).

III

To a certain extent, Kitcher's model manages to weaken the social constructivist challenge. If one accepts its assumptions, it is possible to show that any direct inference from disagreement (and in particular from dissent) to the a-rationality of theory choice is unwarranted. In principle, there are circumstances in which a unanimous community is a community which does not optimally divide its cognitive labour. Far from being a necessary condition for theory choice to be rational, the universal acceptance of Scientific Method could be detrimental to the scientific community.

In point of fact, however, the social constructivist challenge is not met by Kitcher's model. The achieved result is purely speculative. Even if we accept all the assumptions on which Kitcher's model is based, it is still undetermined whether the division of cognitive labour in real communities is optimal (or nearly optimal). It is possible that the conditions in which it is rational for a community to divide its labour force never obtain in actual communities. Moreover, even if these conditions sometimes obtain, we do not know if dissent actually arises when and only when they obtain. Even worse, it does not even seem possible to control whether these conditions obtain in the first place. First, it is not clear which form return functions may realistically have. It is reasonable to assume that their form is subject to some constraints. For example, it is plausible to suppose that the marginal utility of the contribution of a new scientist to the development of a theory decreases as the number of the scientists working on it increases. But assumptions of this kind only spring from vague pre-theoretical intuitions and, for the time being, they do not have any empirical underpinning. Secondly, there is no uncontroversial procedure to determine the objective probability that a theory is true given the available evidence. Thirdly, it is not even clear if talk of the measurement of the objective probability that a theory is true given the evidence makes any sense at all.

Similar considerations apply to the mechanism which is supposed to lead a community of self-interested scientists to achieve a CO-distribution spontaneously. This result seems to be only a theoretical curiosity. The mechanism proposed by Kitcher seems highly unrealistic and excessively delicate to tell us anything about the choice dynamics of a real community. Kitcher proposes an excessively poor representation of the complex of reasons and motives which lead scientists to disagree from each other. The supposition that the real reasons and motives which lead scientists to dissent will give rise to a CO-distribution seems to be completely unwarranted. Thus, even assuming that it was possible to determine the CO-distribution for a given real community, there seems to be no reason to believe that the scientists which belong to that community will attain that distribution by themselves.

As far as the champion of the rationality of science is concerned, the model proposed by Kitcher has a rather modest import. Apparently, the most interesting result is that it shows that, contrary to what the received view implies, consensus among scientists is not the hallmark of the rationality of science. Under certain circumstances, a dissenting community can be more rational than a unanimous community.

IV

Kitcher deems that there is a lacuna in the received view of scientific rationality. Even assuming that it is possible to individuate which theory is better supported by evidence, it does not necessarily follow that, from the point of view of the community, only the better supported theory is to be pursued. According to Kitcher, we should distinguish between *pursuit* and *belief*:

> Whereas it may be rational for each of the scientists to believe the theory that is better supported by the available evidence, it may not be rational for each of them to pursue that theory […] (Kitcher 1990: 8).

According to the received view of scientific rationality, individual scientists are the primary objects of rationality judgements: a rational scientist is a scientist who believes in and works on the theory which the Scientific Method indicates as the best available one. Kitcher breaks with this tradition. According to him, the primary objects of rationality judgements are scientific communities: a rational community is a community in a CO-distribution.[5] The rationality of the individual scientist's choices is assessed from the point of view of their effect on the community. A rational scientist is a scientist who belongs to a community in which the chances of pursuing the true theory are maximised.

The transition from individualistic methodology to a 'collectivistic' one, however, is not as smooth as Kitcher seems to believe. First of all, it is necessary to assume that groups are appropriate objects of rationality judgements. Rationality, instrumentally construed, can be loosely characterised as the (intentional) choice of means which are appropriate to achieve one's ends. Thus, the object of rationality judgement has to be able to make intentional choices and to have goals. Even supposing that these concepts can be applied when super-individual objects are concerned, it is still far from clear how they can be applied. For example, it is not clear how the goals of the scientific community are supposed to be set. From the point of view of methodological individualism, the goals of a group cannot be anything but those goals which are common to all (or most of) its members. The goal of a community is an epistemic one only if epistemic goals are among the aims of all its members. Scientists motivated solely by extra-epistemic goals cannot, thus, have an epistemic goal as their collective goal, unless some bizarre form of methodological holism is adopted.

Further, the fact that the community happens to be in a CO-distribution is a necessary but not sufficient condition for the community to be rational. The mere achievement of one's goals, by itself, is not rational. It is also necessary that the agent has *intentionally* chosen an appropriate course of action in order to attain them. Suppose, for example, that Mr White and Mr Black both intend to catch the same train in the morning. Mr White checks a recently updated timetable, makes a careful plan to be on that train on time and carries

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[5] It is important to notice that the very set-up of the model presupposes a departure from the individualistic methodology.

it out meticulously. Mr Black arrives at the train station in the morning and accidentally manages to get on the same train. Whereas we consider Mr White's conduct rational, Mr Black was simply lucky.

Analogous remarks apply to the two scientific communities considered by Kitcher. The community of purely epistemic agents and the community of self-interested scientists, though both in a CO-distribution, are not equally rational. The members of the former community have intentionally chosen to act as to achieve a CO-distribution; whereas the members of the latter just happen to find themselves in such a distribution. While the former community can be considered rational (at least, if we accept some form of collective intention), the latter is simply lucky.

This problem is related to a more general problem with giving functional explanations of a given practice.[6] If we hang on to a strictly instrumental conception of rationality it is not rational for someone to perform an action just because it brings about as an unforeseen by-product a desirable result. To consider a trite example, the Hopis perform a ritual rain dance because they believe it will cause rain to fall. As far as we know, this belief is false. However, an anthropologist could explain this practice by saying that the function of the Hopi rain dance is to bring about group cohesion. Is it possible to conclude that it is rational for a Hopi to perform the rain dance? Under a strictly instrumental conception of rationality, the performance of this practice cannot be considered rational. On the one hand, if we are right and the Hopi are wrong, they are choosing means which are inappropriate to achieve their goal. On the other hand, they may be unintentionally choosing means which are (allegedly) adequate to achieve a goal which the anthropologist deems important, but which was not their stated goal.

The situation in the case of the community of scientists which are moved by purely extra-epistemic aims is slightly different from the case of the Hopi rain dance. Kitcher is apparently trying to show that the fact that scientists pursue their extra-epistemic goals does exclude the possibility that the scientific community as a whole can achieve the epistemic goal which is usually attributed to it. It is important to notice that the rationality of the self-interested scientist is rescued only as far as she manages to choose so as to achieve her extra-epistemic goals. The fact that the community to which she belongs has thereby achieved a CO-distribution does not make either her or the community rational. It does not make her rational because it was not among her intended goals to act so as to maximise the utility of the community distribution. It does not make the community rational because a scientific community seems to be able to intentionally choose a course of action only in a derivative sense – i.e. only if the individual members of the community are intentionally choosing to pursue a certain course of action; and it is not the case that the members of the community of self-interested scientists are intentionally choosing so as to let the community achieve the epistemic goals.

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[6] Kitcher's model seems to be an attempt to give a functional explanation of disagreement and dissent in science. Kitcher's functionalist inclination clearly emerges in passages like the following: "Social structures within the scientific community can work to the advantage of the community epistemic projects by exploiting the personal motives of individuals" (Kitcher 1990, p.21).

The attempt to use the model proposed by Kitcher to rescue the rationality of scientific decision-making seems to be fraught with problems. No one of them seems insuperable, but their solutions seem to commit us to unattractive philosophical positions. We may, thus, want to look for a less compromising strategy to resist the social constructivist conclusion.

If put the argument into perspective, the social constructivist challenge could be less serious than it appeared at first. In the next section, I will try to outline an attempt to defuse the social constructivist attack, rather than confronting it directly. This attempt heavily relies on some intuitions which underlie the models presented by the authors I have mentioned in Section I, but it avoids their most doubtful assumptions.

V

The most obvious strategy to defuse the social constructivist challenge is to deny that Scientific Method is a necessary condition for scientific theory choice to be rational. This strategy, however, does not require a complex and dubious set-up as the one in Kitcher's model. It can be argued that, from the presence of dissent among scientists, it only follows that there is no such thing as a Scientific Method in the strict sense in which the expression is considered here. However, it is still reasonable to believe that scientists follow a number of rules of thumb and heuristic principles which are part of the tacit knowledge of practicing scientists, and which probably cannot be made completely explicit. Their tacit nature is not due to some arcane feature, but only to the fact that these rules are the result of a long apprenticeship during which scientists develop, among other things, the ability to assess the merits of competing theories. This case is analogous to the case of an expert violin-maker who will never be able to compile a list of explicit rules to choose the right wood to construct a violin. It is only through prolonged practice that one can learn how to do this. The 'scientific method' can be made explicit only at the price of formulating a set of shallow maxims like "Choose the theory which is better supported by the evidence."

The presence of disagreement in general and dissent in particular is due to the fact that these rules and principles leave some room for interpretation. Different scientists can reasonably disagree in their evaluation of the respective merits of competing theories. Consensus among them is eventually reached when one of the available theories fares better than the others under any interpretation of the set of rules. The fact that in the actual science consensus is almost invariably reached could be considered evidence that the discrepancies in the interpretations of the set of rules are not particularly radical.[7]

The social constructivist could now point out that the picture I have just presented is misleading, for it does not take into account that real scientists are not disinterested seekers after truth. Apparently, there are two kinds of interests that we should take into account: ideological and professional

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[7] The idea that scientists can legitimately differ in their choices without no one of them being irrational and the idea that consensus is reached when a dominant theory emerges is explicitly formulated in (Laudan and Laudan 1989). This idea is also present in (Giere 1988).

interests. A scientist is influenced by her ideological interests when she favours a theory because it promotes her extra-scientific beliefs (political, religious etc.). The influence of these kinds of interests, however, seems to be highly subject-specific. Intuitively, ideological interests seem to be more influential in sociology of labour (or sociology of science) than in particle physics. In general, it is really arduous to maintain that all scientists in all the research fields are solely influenced by their ideological beliefs. Even if some scientists working in some areas are particularly prone to be influenced by their ideological interests, it does not follow that all scientific decision-making can be reduced to an ideological struggle.

The second kind of interest seems much more pervasive. Like for other kinds of professions, personal success, fame and money seem to be important motives for many scientists. However, there seems to be no reason to believe that scientists have to be completely disinterested in this sense for them to be able to pursue the epistemic goals we usually attribute to science. Scientists know perfectly well that the best strategy to be successful in their profession is to do their best to pursue the epistemic goals of the community. A scientist whose only goal is to be successful in her profession has, therefore, to adopt epistemic goals as means to the achievement of her main personal goal. Unlike the case of the Hopi rain dance, in the case of science the goals of self-interested scientists and the epistemic goals which are usually attributed to the community as a whole are entangled. The self-interested scientist cannot pursue the ones without intentionally pursuing the others.[8]

If one adopts this perspective, the social constructivist challenge appears no longer alarming. Although dissent is evidence for the conclusion that there is no such thing as Scientific Method, the social constructivist conclusion that scientific decision-making is a-rational is unwarranted. The crucial move to defuse the social constructivist challenge consisted in abandoning the received view of scientific rationality. The rationality of scientific theory choice is not dependent ono *one* Scientific Method, but, rather, on a plurality methods (or rather a plurality of ways of applying the maxims of scientific common sense), no one of which is more rational than the others and no one of which deserves the honorific title of 'the Scientific Method.'

VI

Dissent does not seem to be evidence that theory choice in science is a-rational. Obviously, this does not imply that the view at the other end of the spectrum is to be embraced. Dissent has often played a positive role in the history of science. Many theories which are nowadays widely accepted are the product of the work of a group of dissenters. Moreover, from a certain point of view, dissent seems to be a positive phenomenon in itself. It is plausible, for example, to claim that the presence of competing theories motivates scientists to develop their theories better and to test the alternatives more rigorously. But we should not therefore conclude that dissent is an

[8] The idea that the professional goals of the scientist are closely connected with the goals of science as an epistemic activity underlies both Hull's model and Goldman and Shaked's one.

unqualifiedly positive phenomenon in itself and that the more theories are pursued, the better.

Indeed, dissent in science is such a diversified phenomenon that it is difficult to hold a univocal position about it. There are cases in which, after some time, the dissenters become the new hegemony (like in the case of the Copernican Revolution). In other cases, the dissenters constitute for a long time an active minority within the community, but, despite the efforts on both sides, neither faction ever clearly defeats the other (like in the case of Standard Quantum Mechanics and Bohmian Mechanics). In other cases, the dissenters isolate themselves from the rest of the community to the point that they cannot be considered any longer active scientists (this is, for example, the case of 'conservative dissenters' like Joseph Priestley). Sometimes, a theory supported by scientists who occupy marginal positions in the community is eventually accepted by the whole community (like in the case of the revolution in geology). Other times, influential figures are marginalised because of their embracing alternative theories (like in the case of Peter Deusberg and others who hold the theory that AIDS is not a viral disease).

An interesting intuition seems to underlie Kitcher's model. This intuition is that that how much dissent is beneficial to science, if any, is not a general question which can be answered by the philosopher of science. Rather, it is a contingent question to be decided on a case-by-case basis by those who allocate the resources in a given scientific community. These resources are inevitably limited and how much dissent is sustainable depends on many contingent factors. Therefore, dissent is a positive phenomenon only as far as the competing theories can be given a fair chance given the available resources. In principle, if our resources were unlimited, it would be possible and probably profitable to let a thousand flowers bloom. In practice, most of the times, it is possible and sensible to let just one or two carefully chosen flowers bloom.

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