Invisible Hands and the Success of Science*

K. Brad Wray†‡
Department of Philosophy, University of British Columbia

David Hull accounts for the success of science in terms of an invisible hand mechanism, arguing that it is difficult to reconcile scientists' self-interestedness or their desire for recognition with traditional philosophical explanations for the success of science. I argue that we have less reason to invoke an invisible hand mechanism to explain the success of science than Hull implies, and that many of the practices and institutions constitutive of science are intentionally designed by scientists with an eye to realizing the very goals that Hull believes need to be explained by reference to an invisible hand mechanism. Thus, I reduce the scope of Hull's invisible hand explanation and supplement it by appealing to a hidden hand explanation.

1. Introduction. Several philosophers of science have recently developed invisible hand explanations for the success of science.[1] In an invisible hand

*Received July 1999; revised September 1999.

†Send requests for reprints to the author, Department of Philosophy, University of British Columbia, 1866 Main Mall, E-370, Vancouver, British Columbia, V6T 1Z1.

‡David Hull's detailed and probing comments on an earlier draft helped me improve the paper substantially. Lori Nash read numerous drafts and provided much helpful feedback. I also received helpful comments from the following people: Marc Ereshefsky, Tracy Glenn, Mark Migotti, Bob Ware, Jay Odenbaugh, and Francis Remedios. Eugene Beaulieu provided valuable information about the uses of invisible hand explanations in economics. I also thank the referees for their reports. Finally, I thank the Canadian Society for History and Philosophy of Science for the opportunity to present the paper at the annual meetings in Sherbrooke, in June 1999, and the 11th International Congress of Logic, Methodology and Philosophy of Science for the opportunity to present the paper at their meetings in Cracow, in August 1999.

1. Miriam Solomon argues that Giere, Goldman, Hull, Kitcher, Kornblith, herself, and Thagard have all developed invisible hand explanations for the success of science. Each maintains that "*psychological and social factors ('biasing factors'), appearances to the contrary, bring about desirable epistemic goals under regularly attainable conditions*" (Solomon 1994b, 2).

Philosophy of Science, 67 (March 2000) pp. 163–175. 0031-8248/2000/6701-0009$2.00

explanation a particular outcome is described as an unintended consequence of the intentional behavior of a number of individuals. The individuals have one end in mind, and act accordingly; but their concerted efforts give rise to a consequence that was no part of their intentions.[2] David Hull (1988, 1989, 1997) has developed the most detailed account of the success of science in terms of an invisible hand mechanism. Hull does not deny that scientists aim for knowledge of the world. But, he argues that if we were left to explain the success of science in terms of scientists' intentions to know the world, we would be unable to give a plausible explanation for it.

In this paper, I want to critically analyze Hull's invisible hand explanation. I argue that Hull fails to show that scientists' selfish motives are their dominant motives, and thus exaggerates the need for an invisible hand explanation. Further, I argue that many of the practices and institutions constitutive of science are intentionally designed by scientists with an eye to realizing the very goals that Hull believes need to be explained by reference to an invisible hand mechanism. Thus, I both reduce the scope of Hull's invisible hand explanation and supplement it by appealing to a hidden hand explanation. As Robert Nozick explains, whereas an "invisible-hand explanation explains what looks to be the product of someone's intentional design, as not being brought about by anyone's intentions", a hidden hand explanation "explains what looks to be merely a disconnected set of facts . . . as the product of an individual's or group's intentional design(s)" (Nozick 1974, 19). Those who designed the institutions constitutive of science, I argue, contribute significantly to the success of science.

2. Hull's Account. Hull aims to explain the success of science. In particular, he seeks to explain why science, the institution, is so effective at giving us knowledge of the world.[3] Philosophers of science have traditionally as-

2. Adam Smith provided the classic example of such an explanation, arguing that consumers, when not restrained by trade regulations, generally prefer to support domestic trade, rather than foreign trade, and are thus "led by an invisible hand to promote an end which is no part of [their] intentions" (Smith [1776]1970, 400). Consumers desire "to find out the most advantageous employment for whatever capital [they] command" (397). The result, which is no part of the intentions of individual consumers, is a marketplace "which is most advantageous to the society" (397). (For a detailed analysis of the nature and variety of invisible hand explanations, see Ullmann-Margalit 1978 and Ylikoski 1995.)

3. Even Steven Shapin claims that "science remains whatever it is—certainly the most reliable body of natural knowledge we have got—whether the stories we are told about its historical development and social relations are accurate or inaccurate" (Shapin 1996, 165). Petri Ylikoski (1995, 40) takes issue with the fact that Hull merely assumes that science, as it is currently practiced, is effective without providing an argument to that effect. I will not challenge Hull's assumption here.

sumed that the success of science can be attributed to the fact that scientists aim for knowledge of the world. This is what I will refer to as the "direct explanation"; the success of science is the result of scientists directly pursuing the very goal that science, the institution, aims for.[4] Indeed, Hull doesn't deny that there is evidence supporting the direct explanation. As he explains, "most scientists enjoy research. It is their life. They feel especially fortunate to be paid to do what they want to do more than anything else in the world" (Hull 1988, 305). And, "scientists view themselves as pursuing objective knowledge of the empirical world" (Hull 1989, 254; 1997, S122).

But, Hull believes that the direct explanation as the sole explanation for scientists behaving the way they do is untenable for two reasons. First, the direct explanation for the success of science requires us to regard scientists as significantly different from other people, in particular, more altruistic than others. As Hull notes, "the disinterested search for truth . . . is altruistic. It costs the agent massive amounts of time, labor and . . . money, and it affords benefits to anyone who can use the knowledge produced" (Hull 1988, 287).[5] Hull believes that "whatever is true of people in general had better apply to scientists as well" (304).[6] After all, scientists are people too. And because it would be untenable to claim that others would routinely act altruistically, Hull believes that it is untenable to maintain that scientists would. Second, according to Hull, scientists "readily acknowledge that they crave for recognition" (309). Hull argues that it is difficult to reconcile either scientists' self-interestedness or their desire for recognition with the direct explanation.[7]

4. The term "direct explanation" was suggested to me by Lori Nash.

5. Whereas Hull emphasizes the similarities between scientific knowledge and commodities in an economic marketplace, in a recent discussion of the Marketplace of Ideas, Alvin Goldman (1999; see esp. 194-209) identifies a number of significant dissimilarities between knowledge and commodities.

6. Like the sociologists who have studied science, Hull does not believe that scientists are more altruistic than other people, at least not significantly so (Hull 1989, 246). Hull, though, believes that many sociologists of science, in particular, the Strong Programmers, have exaggerated the influence that external factors have on scientists. As Hull explains, "perhaps scientists on occasion are influenced by the factors alleged by the externalists, but, to the extent that they are, they are not behaving scientifically" (Hull 1988, 2).

7. Whereas Hull tries to emphasize the similarities between scientists and the population at large, Richard Rorty (1991) suggests that scientists deserve to be recognized for their differences. Like Hull, Rorty does not believe that scientists are unique for their *epistemic* virtues. Instead, Rorty suggests that "natural scientists have frequently been conspicuous exemplars of certain moral virtues. Scientists are deservedly famous for sticking to persuasion rather than force, for (relative) incorruptibility, for patience and reasonableness" (Rorty 1991, 61). Hull, though, provides some evidence for questioning

In fact, Hull argues that scientists are primarily motivated by recognition. And "the sort of credit that really matters is *use*. Individual scientists want credit for their contributions" (309). Further, as Hull explains, "science is so structured that scientists must, to further their own research, use the work of other scientists" (4). When scientists use each other's work, both parties stand to gain. First, the cited scientist gets what she desires, "the recognition of a new scientific achievement" (Hull 1989, 252). Second, the citing scientist benefits from the use she can make of the other's achievement in her own work (252). Ideally, the citing scientist will be able herself, subsequently, to produce a new scientific achievement worthy of recognition. In fact, Hull argues that it is the desire for credit and the need to rely on the work of others that leads scientists to cooperate to the extent that they do (Hull 1988, 311). Hull believes that unless scientists' desire to gain knowledge of the world is supported by recognition for their contributions, science would not be as successful as it is.

Hull argues that the success of science "rests fundamentally on the relations which exist . . . between credit, use, support, and mutual testing" (Hull 1988, 281). Given the way that science is currently organized, "scientists need not sacrifice their individual interests for the larger good" (304). In fact, Hull claims that science is "so organized that individual and group interests tend to *support* each other" (305; emphasis added). "Scientists want credit and . . . science is so structured that this desire leads to increased knowledge of the empirical world" (357). And scientists act as they do, generally complying with the norms of science, because it is in their interest to do so (320).

Further, Hull argues that, given the structure of science, "factionalism, social cohesion, and professional interests need not frustrate the traditional goals of knowledge-acquisition" (26). In fact, Hull goes so far as to say that "some of the behavior that appears to be most improper actually facilitates the manifest goals of science" (32). As Hull explains, "certain behavior which most of us are likely to find admirable is not very effective in getting one's views adopted by others, while other sorts of behavior that we tend to decry promote recognition" (371).[8] Hull also believes that it is not necessary to eliminate the biases of each individual scientist. The goal of science can be effectively realized provided "different scientists have different biases" (22).

Hull's account of the success of science is an invisible hand explanation. Though scientists are primarily moved by the self-interested desire for

---

even this. See, for example, his comparison of the four founders of behavioral psychology (Hull 1988, 368–371).

8. Similarly, Solomon believes that biases play a constructive role in science, ensuring that competing theories are developed (Solomon 1992, 446).

credit, the result of their pursuit of this goal is scientific success, a consequence that would be realized to a lesser degree were we to rely exclusively on scientists' desire to realize this goal.

Hull recognizes that a compelling invisible hand explanation for the success of science requires that "the 'individual' goals of scientists and the good of science [be] sufficiently independent so that they can work at cross-purposes" (Hull 1997, S123). Without such independence, the success of science would be no mystery, and an invisible hand explanation would be unnecessary. I will refer to this demand as "the independence requirement." If the independence requirement is not satisfied, then the success of science would be an obvious outcome of the individual efforts of the many scientists seeking to realize that goal.

In an effort to demonstrate that the independence requirement is satisfied, Hull argues that scientists are not as altruistic as they profess to be. Hull believes that despite the fact that scientists *claim* to have higher goals, like the pursuit of knowledge, these are not what generally motivate them to act as they do. He argues that the presence of priority disputes attests against scientists' professed higher goals. Specifically, he claims that "if scientists were interested exclusively, or even primarily, in the good of science, then priority disputes would be rare or nonexistent, but they are the most frequent source of discord in science" (S122). Consequently, he believes that his invisible hand explanation for the success of science is vindicated despite the fact that scientists claim to have higher goals that coincide with the goals of science.

Further, Hull argues that the joy of discovery, the desire for credit, and the search for truth can run at cross purposes, though he believes that given the current structure of science, generally they do not (Hull 1988, 306). Thus, the goals of individual scientists and the goal of science are sufficiently independent to warrant invoking an invisible hand explanation. As he explains, "if the disparity between contributions and credit become too great, the system will break down" (311). If scientists frequently didn't get credit for their discoveries, then science would not be as effective as it currently is. Hence, were the institutions of science organized differently, science may not be as effective at realizing its goals as it currently is.

Two remarks are in order to keep the nature and scope of Hull's claim in perspective. First, Hull does not deny that scientists aim for knowledge of the world. He claims that "invisible-hand explanations . . . do not require that self-interest be the *only* motivation involved" (Hull 1997, S125). Hull's reason for insisting on an invisible hand explanation for the success of science is that the altruistic intentions of scientists are insufficient to explain the success of science. Second, Hull does not deny that the institutions constitutive of science are partly the product of intentional design

(S125). But, again, he does not believe that the altruistic intentions of those designing the institutions are sufficient to account for the success of science. Thus, what is at issue here is the degree to which the success of science can be attributed to people's altruistic intentions. And Hull insists that such intentions are insufficient to do the work required of them.

3. Reducing the Scope of the Invisible Hand of Science. In the remainder of this paper, I want to raise a number of criticisms against Hull's invisible hand explanation for the success of science. In this section, I argue that Hull does not provide sufficient evidence to prove that scientists' selfish motives are their dominant motives. My intention is not to defend the direct explanation for the success of science, but to challenge the extent to which an invisible hand explanation is needed. Scientists' altruistic intentions seem to be contributing more to the success of science than Hull implies.[9]

As Hull claims, the fact that scientists engage in priority disputes suggests that they do have self-interested motives. This I do not deny. But the presence of priority disputes does not show that scientists are *primarily* self-interested. Unless the self-interested behavior that scientists exhibit in priority disputes is typical, we do not have reason to believe that scientists' primary motives are self-interested motives.

And, there is compelling evidence to suggest that the behavior scientists exhibit while involved in priority disputes is atypical of scientists. Many scientists are not apt to find themselves involved in a priority dispute because many scientists publish very little. As Hull notes, for example, "of the 238 chemists receiving doctorates between 1955 and 1961 . . . sampled [in a study by Reskin (1977)], 7.5 percent published nothing during the first decade after receiving their degree, and 11 percent published only one article"; "in any given year, 60% of chemists publish nothing whatsoever"; "during the two-year period studied by Ladd and Lipset (1977), almost a third of the physicists teaching in American universities and colleges published nothing"; and Garvey (1979) found that "only 10 percent of psychologists published at least one article a year" (Hull 1988, 359).

Now, Hull may claim that these statistics are irrelevant to understanding the character of the people who "really" make science. I disagree. Like Robert Merton, I believe that "the great men of science, the undeniable geniuses" are not indispensable (Merton 1973b, 366). Merton argues that the principal value of such scientists is that they are "functionally equiv-

9. I think Hull is mistaken to regard the pursuit of knowledge as altruistic. One can share their knowledge with others without themselves losing it. I will, nonetheless, continue throughout this paper to refer to scientists' pursuit of knowledge as altruistic, as Hull does.

alent to a sizeable number of other scientists" (368).[10] Further, even those scientists who publish very little do make important contributions to the development of scientific knowledge. Most significantly, the large majority of scientists perform many of the tasks that the results of those who publish rely on.[11] And, they will, in the course of performing their duties, offer an alternative perspective, and thus ensure that the hypotheses under consideration have been subjected to critical scrutiny.[12] Consequently, I believe that the self-interested behavior scientists exhibit when they are involved in priority disputes is not only rare, but atypical for scientists.[13]

Further, it seems that priority disputes are becoming less frequent, and hence the behavior exhibited in such disputes is becoming even more atypical of scientists. In a study of 264 multiple discoveries, discoveries that could have led to priority disputes, Merton found that whereas 92 percent of the multiple discoveries he examined that occurred before 1700 led to a priority dispute, "the figure drops to 72 percent in the eighteenth century . . . and reaches a low of 33 percent in the first half of this century" (365).[14]

In addition, Merton provides us with both reasons for rejecting accounts of priority disputes like Hull's, that appeal to the self-interests of scientists, and an alternative explanation for the behavior of those involved in such disputes. Merton argues that explanations of such disputes that attribute them to "propensities toward egotism" are untenable for two reasons (Merton 1973a, 290). First, "these controversies often involve men of ordinarily modest disposition who act in seemingly self-assertive ways only when they come to defend their rights to intellectual property" (291). Second, "very often . . . the discoverers or inventors, take no part in arguing their claims to priority," and the dispute is pursued by others

10. Stephen Cole (1992) provides additional evidence for the dispensability of "the great men of science." Contrary to a hypothesis defended earlier by him and his brother, Cole found that "scientific advance is a function of the number of people entering science" (225).

11. To appreciate the collective nature of scientific inquiry, consider the article cited by John Hardwig that "approximately 50 physicists worked perhaps 50 man/years collecting the data for the experiment" which had 99 authors (Hardwig 1985, 357).

12. On the importance of having criticism from a variety of perspectives as a means to ensuring that the hypotheses we come to accept are the epistemically superior hypotheses, see Longino 1990 and Wray 1999.

13. As one referee noted, the fact that there were so few priority disputes in the period in systematics examined by Hull (1988) provides additional evidence for my claim, that the self-interested behavior scientists exhibit when involved in such disputes is not only rare but atypical.

14. Interestingly, Merton has found that the *shorter* the time interval between multiple discoveries, the *less likely* it is to lead to a priority dispute: "of those made within a year of each other, just about half were subject to a contest over priority; of those more than 20 years apart, four in every five were contested" (Merton 1973b, 365).

who "stand to gain little or nothing from successfully prosecuting the claims of their candidate" (291-292). This, Merton argues, suggests that priority disputes are not a consequence of the egotistical motives of scientists, but rather the result of scientists' indignation at "the violation of a social norm." As Merton explains, scientists get outraged because "they want to see 'fair play' " (292).[15]

Thus, given that the self-interested behavior scientists exhibit during priority disputes is atypical, it seems that we have less reason to invoke an invisible hand mechanism to explain the success of science than Hull implies. The fact that scientists aim for knowledge of the world, even if that is not all they aim for, should figure more prominently in an adequate explanation for the success of science than Hull suggests.

4. Supplementing the Invisible Hand of Science. In this section I argue that an adequate explanation for the success of science will require us to go beyond the resources of both Hull's invisible hand explanation and the traditional direct explanation. I will supplement these resources by appealing to a hidden hand explanation. Whereas an invisible hand explanation explains what looks to be the product of someone's intentional design as not being brought about by anyone's intentions, a hidden hand explanation explains what looks to be merely disconnected facts as the product of an individual's or group's intentions.[16]

The success of science is the result of the intentions of, not only those scientists working in labs, but also those scientists who have designed the institutions constitutive of science. In an effort to illustrate this I want to briefly examine some of the practices *intentionally* employed by the Royal Society. The Royal Society, aware of the need to block the influence of narrow national or religious interests on scientific knowledge and practice, admitted people from all ranks to its meetings. As Thomas Sprat explains,

> they have freely admitted Men of different Religions, Countries, and Professions of Life. This they were oblig'd to do, or else they would come far short of their largeness of their own Declaration. For they openly profess, not to lay the Foundation of an *English*, *Scotch*, *Irish*, *Popish*, or *Protestant* Philosophy; but a Philosophy of *Mankind.* (Sprat [1667]1959, 63)

15. Merton also argues that the desire for recognition serves scientists' "inner need for assurance that [their] work really matters, that [they have] measured up to the hard standards maintained by at least some members of the community of scientists" (Merton 1973d, 339).

16. Though Hull used the terms "invisible hand" and "hidden hand" interchangeably in the past, his intention is to offer an invisible hand explanation (Hull 1997, S119).

Indeed, the Royal Society was far less egalitarian than this passage suggests. As Sprat admits, "though the *Society* entertains very many men of *particular Proffessions*; yet the farr greater Number are *Gentlemen*, free, and unconfin'd" (Sprat 1959, 67). But the founders of the Royal Society were motivated to adopt a policy of including men from different professions in an effort to ensure that their society, as a whole, was less partial. By including people with diverse backgrounds, the Royal Society sought to ensure that the adverse effects of each scientist's biases were apt to be detected.[17]

For similar reasons, the Royal Society conducted experiments in the company of many people, seeking to exploit the epistemic advantages of an impartial audience. As Sprat explains,

> it is so farr from being a blemish; that it is rather the excellency of this Institution, that *men of various studies* are introduc'd. For so there will be always many sincere witnesses standing by, whom self-love will not persuade to report falsely, nor heat of invention carry to swallow a deceit too soon; as having themselves no hand in the making of the Experiment, but onely in the *Inspection*. (73)

Scientists design their institutions knowing very well the vices scientists, and people in general, are prone to. The members of the Royal Society, for example, knew that self-love persuades people to report falsely. And, measures were taken to compensate for such behavior.

The practices employed by the Royal Society are not isolated incidents of planning in science. Merton supplies a list of practices that scientists have appealed to in an effort to safeguard priority claims, and thus ensure that their peers made public their findings so that others could benefit from them. "Complex ideas [are] quickly published in abstracts" (Merton 1973a, 315; 1973b, 364); "there is the long-standing practice of depositing sealed and dated manuscripts with scientific academies"; and "scientific journals often print the date on which the manuscript of a published article was received" (1973a, 316).

Hence, even though, at the micro-level, in laboratories, scientists' self-interests sometimes determine how they behave, there is evidence to suggest that the practices and institutions that mediate scientists' interactions with each other are designed and sustained with the intention of realizing the goals of science. I want to emphasize that I am not claiming that all the practices and institutions that are beneficial for realizing the goals of

17. One of the principal goals of the various feminist critiques of science has been to ensure that we are even less partial, and do not merely lay a foundation of a "science of mankind", but rather the foundation of a "science of *humankind*". In this regard, see the various essays in Keller and Longino 1996.

science are the result of intentional design. But many are, and this is significant in understanding why science is so successful. Such institutions play a more than negligible role in the success of science.

5. Accounting for the Success of Science. I attribute the success of science to three factors. First, I grant that a thorough explanation for the success of science will require an appeal to an invisible hand. The fact that there is an efficient division of labor, and not all scientists in the same field are working on the same problem, cannot be wholly attributed to the intentions of those who designed the institutions constitutive of science. Rather, scientists are likely influenced in choosing their areas of research by a range of factors that have little bearing on the pursuit of knowledge. But, the fact that scientists are influenced by such factors need not be an impediment for ensuring that there is an efficient division of cognitive labor.[18] Second, as I argued in Section 3, because most (if not all) scientists have the pursuit of knowledge as one of their goals, it is not surprising that collectively they are able to realize this goal. Third, as I argued in Section 4, many of the institutions of science are intentionally designed to ensure that science is effective.[19] I believe that these three forces, even if each is only partially effective, collectively ensure that science is effective at giving us knowledge of the world.

18. In this regard, see Kitcher 1993. Further, the concept of an invisible hand also figures importantly in Adam Smith's discussion of the division of labor. It is worth briefly examining this section, not only because it is an invisible hand explanation, but also because he explicitly discusses science. As Smith explains, "the division of labour, from which so many advantages are derived, is not originally the effect of any human wisdom, which foresees and intends that general opulence to which it gives occasion" (Smith 1970, 12). The unintended result is general opulence. But, when people specialize, they have no intention to increase the economic well-being of society. Rather, their intentions are more modest, and more selfish. As Smith explains, "it is not from the benevolence of the butcher, the brewer, or the baker that we expect our dinner, but from their regard to their own interest" (13). These people serve our interests so well, providing us with competitively-priced quality goods and services, because their own livelihoods depend upon it.

Smith believes that even science ("philosophy") has benefited from the division of labor. "In the progress of society, philosophy or speculation becomes, like every other employment, the principal or sole trade and occupation of a particular class of citizens" (10). As a result, "each individual becomes more expert in his own peculiar branch, more work is done upon the whole, and the quality of science is considerably increased by it" (10). Ironically, this passage, where Smith offers an invisible hand explanation for the success of science, has been neglected or overlooked by philosophers appealing to either Smith or invisible hand explanations.

19. One of the referees for *Philosophy of Science* suggested that Adam Smith also recognized the importance of institutional design. In particular, Smith believed that a competitive market place needed to be consciously nurtured.

Significantly, my hidden hand explanation enables me to explain why science can be successful even though scientists often behave in ways that most of us regard as improper. The institutions and practices are designed to achieve particular results, specifically, the attainment of knowledge of the world. And those seeking to devise practices and institutions to ensure that science is successful are as interested in ensuring that hypotheses are not rejected because their adherents engage in infighting or have difficult personalities, as the Royal Society was in ensuring that hypotheses are not rejected because of their adherents' nationality or religion. As a result, a great range of behavior is tolerated from scientists by other scientists, including the sort of behavior that scientists exhibit when they engage in infighting, behavior that we nonetheless regard as improper. Further, it is worth noting, contrary to what Hull seems to imply, that the sorts of behavior that we tend to decry which *can* promote recognition are just as apt to lead scientists to adopt the epistemically inferior position as they are to lead scientists to adopt the epistemically superior position.

My hidden hand explanation also provides a means for accounting for the objectivity of science. Philosophers of science are confronted with a number of considerations that seem to threaten the objectivity of science. "Experimental studies of cognitive psychology have shown that humans and also scientists are prone to bad reasoning habits, judgmental errors and other cognitive defects" (Ylikoski 1995, 35); scientists "have all kinds of noncognitive commitments and interests" (35); and it is now widely acknowledged that there is no such thing as "the scientific method" that governs the behavior of all practicing scientists (35). The institutions constitutive of science, though, are designed and continue to be refined, with an eye to ensuring that the sorts of factors that lead scientists to be partial do not have an adverse influence on science. Thus, it is the hidden hand of science that ensures that science has a legitimate claim to objectivity.[20]

6. Concluding Remarks. In summary, I have challenged Hull's invisible hand explanation for the success of science on two accounts. First, I argued that he has not provided compelling reasons for believing that scientists are primarily motivated by self-interest. Second, because the practices and institutions that mediate the interactions of scientists were designed with the intention of extending our knowledge of the world, an invisible hand explanation for the success of science seems inadequate. Neither the traditional direct explanation nor Hull's invisible hand explanation for the success of science is complete. Instead, I have suggested that we supplement these explanations with a hidden hand explanation, attrib-

20. For a more complete discussion of the aspects of scientific inquiry that contribute to its objectivity, see Wray 1999.

uting some significant measure of the success of science to the fact that the constitutive institutions and practices were designed with the goal of extending our knowledge.

REFERENCES

Barnes, Barry, D. Bloor, and J. Henry (1996), *Scientific Knowledge: A Sociological Analysis*. Chicago: University of Chicago Press.

Bechtel, William (1991), "Review of David Hull, *Science as a Process*", *Philosophy of Science* 58: 138–139.

Cole, Stephen (1992), *Making Science*. Cambridge, MA: Harvard University Press.

Goldman, Alvin (1995), "Social Epistemology, Interests, and Truth," *Philosophical Topics* 23: 171–187.

———. (1999), *Knowledge in a Social World*. Oxford: Oxford University Press.

Hands, D. Wade (1995), "Social Epistemology Meets the Invisible Hand: Kitcher on the Advancement of Science", *Dialogue* XXXIV: 605–621.

———. (1997), "Caveat Emptor: Economics and Contemporary Philosophy of Science", *Philosophy of Science* 64 (Proceedings): S107–S116.

Harding, Sandra (1991), *Whose Science? Whose Knowledge?* Ithaca: Cornell University Press.

Hardwig, John (1985), "Epistemic Dependence", *The Journal of Philosophy* 82: 335–349.

Hull, David (1988), *Science as a Process*. Chicago: University of Chicago Press.

———. (1989), "Altruism in Science: A Sociobiological Model of Cooperative Behavior Among Scientists", in *The Metaphysics of Evolution*. Albany: SUNY Press, 243–262.

———. (1997), "What's Wrong With Invisible-Hand Explanations?", *Philosophy of Science* 64 (Proceedings): S117–S126.

Keller, Evelyn F. and H. Longino (eds.) (1996), *Feminism and Science*. Oxford: Oxford University Press.

Kitcher, Philip (1993), *The Advancement of Science*. Oxford: Oxford University Press.

Kyburg, Henry (1993), "Review of David Hull, *Science as Process*", *Noûs* XXVII: 107–109.

Latour, Bruno and S. Woolgar (1986), *Laboratory Life*, 2nd ed. Princeton: Princeton University Press.

Longino, Helen (1990), *Science as Social Knowledge*. Princeton: Princeton University Press.

Merton, Robert (1973a), "Priorities in Scientific Discovery", in *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press, 286–324.

———. (1973b), "Singletons and Multiple Discoveries in Science", in *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press, 343–370.

———. (1973c), "Institutionalized Patterns of Evaluation in Science", in *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press, 460–496.

———. (1973d), "Behavior Patterns of Scientists", in *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press, 325–342.

Nozick, Robert (1974), *Anarchy, State, and Utopia*. New York: Basic Books.

Rawling, Alison (1994), "The AIDS Virus Dispute: Awarding Priority for the Discovery of the Human Immunodeficiency Virus (HIV)", *Science, Technology, and Human Values* 19: 342–360.

Rorty, Richard (1991), "Is Natural Science a Natural Kind?", in *Objectivity, Relativism, and Truth*. Cambridge: Cambridge University Press, 46–62.

Shapin, Steven (1996), *The Scientific Revolution*. Chicago: University of Chicago Press.

Smith, Adam ([1776]1970), *The Wealth of Nations*. London: J. M. Dent and Sons.

Solomon, Miriam (1992), "Scientific Rationality and Human Reasoning", *Philosophy of Science* 59: 439–455.

———. (1994a), "Social Empiricism", *Noûs* 28: 325–343.

———. (1994b), "Is There an Invisible Hand of Reason?", unpublished manuscript.

Sprat, Thomas ([1667]1959), *History of the Royal Society*. J. Cope and H.W. Jones (eds.). London: Routledge and Kegan Paul.
Ullmann-Margalit, Edna (1978), "Invisible-Hand Explanations", *Synthese* 39: 263–291.
Wray, K. Brad (1999), "A Defense of Longino's Social Epistemology", *Philosophy of Science* 66 (Proceedings): S538–S552.
Ylikoski, Petri (1995), "The Invisible Hand and Science", *Science Studies* 8: 32–43.