

the model should be able to predict which norm will be activated in a given scenario. We expressed our concerns that  $k$  could depend on other variables or parameters in the utility function. The same holds for  $N$ , the parameter representing the impact of the activated norm upon the choice of a strategy. To enhance the applicability of Bicchieri's approach, we need to thoroughly characterize the mapping from cues to activation and expectations, and assess the robustness of these relationships.

In this review we have attempted to illustrate a few aspects of the norm-based framework developed by Cristina Bicchieri. Many other interesting contributions of the book, like Bicchieri's analysis of the emergence and survival of dysfunctional norms, unfortunately remain beyond the scope of this short review. A stimulating work for scholars in social psychology, experimental economics and evolutionary game theory, this book motivates unexplored streams of research and provides an integrated and testable account of the role of norms in strategic interactions.

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*The Price of Truth: How Money Affects the Norms of Science*, David Resnik.  
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It is now commonplace to read about the dramatic changes in the funding of science that have occurred in the last quarter century. The headline story, which generally focuses on the United States, is one of a shift from public to private funding. While this can easily be overstated – even in the US, federal funding of total R&D has never actually decreased (at least in current dollars) and has remained by far the largest single source of finance for R&D in academic institutions – the greater importance of commercial funding is undeniable. In this context, fears about the effects of money on science have been steadily rising. Resnik's book seeks to examine these effects in detail and to propose possible solutions to the problems he identifies. In doing so, one of his main targets is the argument that science is an incorruptible pursuit of the truth. As he puts it (p. 5), it is "naïve to think that money has no influence on the behaviour of scientists."

In support of this claim, the book opens with a series of actual recent cases where monetary factors have interfered with the scientific process in a highly controversial way. Resnik also notes “a growing body of evidence supports a strong connection between the source of funding and the content of published results” (p. 7). But Resnik argues that it is money *per se*, or the scramble for funding, that is at issue and not private funding alone. He therefore devotes attention to government funding too, noting that “money has almost always had some effect on research” (p. 33) and is not merely a new phenomenon reflecting the greater importance of commercial sponsorship. In arguing thus, he seeks to plot a middle course between those who champion the effects of money on science, seeing the science itself as immune as regards the source of funding, and those who simply lament these effects and opine for a (largely mythical) past in which science and funding were kept separate due to federal largesse.

Resnik’s argument thus hinges on being able to argue that science can be, but is not necessarily, perverted by monetary considerations. This he does by arguing that science is not value free, but has its own norms and it is these that are transgressed when money becomes more important. His task is to spell out what these norms are, and then examine how money (or rather attaching greater value to money than to these norms) interferes with science. Throughout the book, his approach is analytical and solution-directed. As regards the latter, Resnik is clearly motivated by the need for his research to offer solutions to the problems he diagnoses. The concluding chapter, for instance, makes it clear that he has little patience for analyses that simply point out problems. But offering solutions takes us to the former (analytical) element: his goal is to spell out what the norms of science are, providing a comprehensive list for our (and policymakers’) consideration. This then facilitates the drafting and enforcement of policies that deal with the negative effects of money on the norms of science, which he takes to be “the most prudent and realistic response to this situation” (p. 33).

The book has many strengths, in particular its core commitment to proposing practicable solutions based on intelligent analysis of the problems of the effects of money. The extensive (and helpful) list in the conclusion, where he recapitulates the solutions suggested throughout the book, is testimony to this, as well as to the comprehensive range of issues covered, which are not just limited to the most obvious issue of personal conflicts of interest. Furthermore, some of his proposed policies are quite radical (e.g. prohibiting researchers from owning any stock in companies that sponsor their research) and/or novel/interesting (e.g. making obligatory the publication of negative clinical trial results).

Another strength of the book is the way in which it makes clear that asserting the impregnability of science to such corrupting effects is simply untenable. Resnik argues, and illustrates with numerous examples, that for

science to function, it has norms that must be observed, but as norms these are only ever motivating and aspirational ideals not modes of behaviour that we can simply rely upon as if they were automatic.

The book also incorporates a substantial discussion of the philosophical issues that underpin the more practical issues from which the book arises, and this too is (or at least should be – though see below) an important strength. It is all too easy to seek to discuss issues of the “corruption of science” in complete isolation from the philosophical issues that are implicit and crucial in such discussion, but this renders any such analysis simply a series of journalistic or anecdotal polemics and so entirely fruitless.

But the book also has several weaknesses, and it is upon these that this review will focus. First, and most importantly, the analytical approach Resnik takes has some important limitations and these are in evidence in the book. Such an approach presupposes a clearly defined problem that can in practice be tackled by teasing out and refining its distinct elements and addressing each in turn. It also thereby assumes that the different elements of the problem can be treated separately from each other, and also that they can be treated as objective phenomena that are not in any way responsive to the perspective of the person trying to tackle them, which is taken to capture the nature of the problem adequately. The sheer success of such an approach in many cases makes it an absurdity to argue that this approach is always mistaken. For instance, such an approach is clearly useful for the drafting of policies/rules that can be applied to concrete problems that arise. But since it can only examine the distinctions constitutive of the analyst’s current and shared understanding of the problem, the analytical approach is unable to provide assistance where this understanding itself is, respectively, inadequate (e.g. due to the emergence of a novel social reality not amenable to analysis in its conceptual terms) or contested.

Unfortunately, Resnik’s book displays these limitations of such an analytical work at numerous junctures. For instance, regarding intellectual property, Resnik argues that the “best way to promote the advancement of scientific and technical knowledge is to seek an appropriate balance between public and private control of information” (p. 136). Yet this tells us absolutely nothing, and is simply to restate the premise of intellectual property law itself (which is not itself put in question) while offering no guidance whatsoever regarding how this “delicate balance” should in fact be struck.

The most striking example of the problems with his approach, however, is in his initial discussion of the norms of science, which sets up the analysis for the rest of the book. Resnik argues cogently that there is no single over-arching goal of science but a Wittgensteinian family of goals. From this analysis, he then proceeds to list some scientific principles or norms that can be derived from these goals of science, divided into

two categories: the epistemological and the ethical. This list is presented with a short explanation of how each norm fulfils scientific goals and includes *inter alia*, as “ethical principles”, honesty, objectivity and credit and, as “epistemological principles”, testability, consistency, and empirical support.

As discussed above, however, this process of articulating the norms of science is of dubious assistance regarding real controversies about scientific conduct, and, crucially, the less useful the more heated the controversy. Such controversy presupposes not only that science is a normative enterprise, as Resnik correctly notes, but also that there are differing interpretations as to what its norms actually are. As such, enumerating these norms as Resnik does at the very least requires significantly more argument than he offers. This is clear, for instance, in the philosophically controversial inclusion of “testability” in his list. Admittedly Resnik acknowledges the continuing debate over this criterion, but he simply dismisses it through the briefest appeal to Popper and the assertion that “it still remains one the (sic) most important and influential principles in research” (p. 49).

But this is simply indicative of much more profound problems for Resnik, because even if, in a bid to rescue his definitive list, he were just to delete those norms that are philosophically controversial, it would remain the case that his exercise does little or nothing to assist our understanding of the norms of science for conclusive practical arbitration. Resnik’s analytical method can only produce uncontroversial norms insofar as it elicits analytical truisms. Yet these norms remain analytic truths, and thus uncontroversial, only as long as there is no disagreement as to their interpretation. Where there is such disagreement they cease to be analytic at all. As such, the norms Resnik lists can, at best, form the *context* for the debate regarding their substantive interpretation but they cannot themselves resolve the controversy. The utility of such an *ex ante*-compiled list of scientific norms is thus significantly curbed and just at the moment when it is supposed to be invoked. In short, what the norms of science are is precisely what is at issue in a controversy of this kind, so that no list of these norms can hope to offer definitive judgement without prejudging the argument.

This is a particularly important problem for Resnik’s argument as a whole, however, because he also argues that the importance of science being “objective” – a norm he takes to be particularly important and to be a major casualty of excessive monetary concern – arises from its capacity to act as “neutral party” and arbiter in democratic debate (p. 56). Yet lack of clarity and the possibility of incommensurable interpretations of scientific norms means that there is also scope for argument as to whether the supposedly “neutral party” of science is itself in fact neutral. Were Resnik to admit this possibility, however, preserving objectivity can no

longer be justified in the way that he does, and transgression of that norm by monetary considerations no longer seems such a problem. From here it is but a short step to the total collapse of his argument: maybe money does affect science, but so what? – a conclusion, by the way, that is neither welcomed nor accepted by this reviewer and so demands alternative arguments to rebut it.

This leads to the final point I would like to discuss, namely Resnik's discussion of objectivity in science, which focuses in particular on a critique of social constructionist accounts of science. Resnik's intention in this chapter is to argue that science should be, or should strive to be, objective. His argument is thus normative and not descriptive. He also distinguishes between two senses of the term "objectivity", namely the metaphysical inflection of "objective" as the mind-independent reality and the epistemological one of "objective" as the unbiased outcome of a rational and empirical investigation. Put together, his argument is thus for both a normative "realism" and a normative "rationalism", reflecting the two senses of "objectivity" respectively, and he argues for each of these in turn. In both cases, however, his arguments attack straw men of his constructionist opponents, while offering weak rhetorical questions in the place of argument and some astoundingly circular reasoning.

For instance, regarding "normative rationalism" (i.e. the thesis that science should strive for objectivity), he offers a substantial misreading or overstatement of the work of sociologists of scientific knowledge, such as Bloor, by claiming that they argue that "there is nothing special or unique about the way scientists form their (sic) beliefs" and that "science is not rational" (p. 61). For Resnik, these sociologists argue that:

Even when it appears that scientists accept a hypothesis based on the weight of the evidence in its favor, they are really accepting the hypothesis as a result of psychological, sociological, economic, cultural, or political factors. Scientists cannot even choose to be unbiased, because they will simply choose to favor one bias over another bias. (p. 62)

Without getting bogged down into the continuing conflicts between (some) philosophers of science and (some) sociologists of science, I think it can be stated that almost none of the latter would assent to these statements and, indeed, that they have gone to considerable lengths to make this clear. For instance, in David Bloor's case, it is his very insistence upon the singularity of science that leads him to argue for a scientific examination of science itself regarding the impact of such sociological (and psychological etc. . .) factors. But Bloor's views have also received considerable criticism from within sociology of science, so that he can hardly be held up as representative of all shades of a broadly social constructivist perspective. Indeed, for many the argument about the sociality of science is simply not concerned with bias in the way Resnik portrays it. On these terms,

Resnik is here, like so many, conflating two issues, namely the possibility of rational judgement and the socio-historical situatedness of the conceptual resources, technological practices and ways of life in the context of which all such judgements (including scientific ones) take place. Regarding the latter, then, it becomes clear that all science is indeed socially affected, thereby puncturing the pretensions of philosophical rationalists who defend an image of science as socially unmediated, but whether or not rational judgement is possible is an entirely separate issue.

In any case, Resnik's response to the (straw man) argument he presents is singularly lacking, and indeed one of the most flagrant circular arguments I have encountered for some time:

The reply to [the argument that objectivity is impossible] is that it is possible for science to strive to be unbiased because human beings conduct scientific research, and human beings are capable for striving for objectivity. (p. 63)

The reader is thus left entirely unconvinced that Resnik has answered any of the philosophical objections he rightly raises to treating science as if it can and does attain neutral objectivity.

The overall effect, though, is that, despite the attention Resnik pays to the philosophical issues underlying the issues of money and science, his refusal to admit a social dimension to science entails that he must treat the "norms of science" as if they are analytic and universal. This also denies him the possibility of dealing with some of the most important issues regarding the effects of money on science *qua* social (and so sociohistorically located) practice, such as the effect of private finance and intellectual property on the culture of science and on the very conceptual structures of scientific disciplines (the work of Daniel Kleinman (e.g. 2003) is an excellent example of this kind of analysis). Limited to treating the norms of science as timeless and rational truths, he can only discuss the way in which money leads to the actual transgression of scientific norms, whether intentional or unintentional. He cannot discuss the way in which private finance actually changes the nature of those norms themselves, so that what is judged to be acceptable itself shifts in particular ways that have normative consequences.

The book displays admirable passion for the preservation of standards in science and the crucial importance of just such normative consequences. It also displays understanding that where such problems arise, they must be tackled and not simply identified and moaned about. In this context, it is understandable that Resnik should feel an analytical exploration such as his could offer much welcome assistance to policymakers. Yet, while voices such as Resnik's defending the normativity of science are welcome, I fear that since the very meaning of these definitions of the norms of science is currently in flux – so that the presupposition of shared assumptions needed for analytical guidance to be useful is often lacking – such an analytical

approach may well add only more fine words to a debate that is already by no means short of loud, empty drums.

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D.D. Raphael, *The Impartial Spectator*. Oxford and New York: Oxford University Press, 2007. 143 pp.

D.D. Raphael is the dean of Adam Smith scholarship. His edited versions of *The Theory of Moral Sentiments* (TMS) and *Lectures on Jurisprudence* are now considered definitive, and his 1985 *Past Masters* volume, *Adam Smith*, the dominant introductory text for two decades, is a well-balanced work for generalists and specialists alike. In 1972, in a short essay titled “The Impartial Spectator,” He argued for a “genetic” approach to interpreting Smith: one must look at the development of Smith’s spectator theory over time, Raphael insisted, and not exclusively at the end result. Smith’s first book, *The Theory of Moral Sentiments* went through six editions in his lifetime. The second edition contained small but significant changes to his argument, including responses to criticism from David Hume and Sir Gilbert Elliot, and the sixth edition included both important and philosophically substantial alterations – Raphael calls them “drastic” (5). It contained an entire new section, Part VI, running fifty-three pages.<sup>1</sup>

Raphael’s newest book, *The Impartial Spectator*, shares both the title and methodology of his earlier essay but his argument is wider in scope and more detailed. In it, he argues that even some of the most important scholars have misrepresented Smith’s positions by quoting various editions to suit their interpretations. This is problematic because the philosophical differences between Smith’s early and late positions are significant (5).

The great achievement of Raphael’s 1976 edition of TMS is the comparative work. Every alteration, deletion, and addition amongst the six editions is documented and cross-referenced, both with TMS

<sup>1</sup> All references advert the Glasgow Edition.