The Scottish Verdict: Donald MacKenzie's "An Engine, Not a Camera"

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READING BETWEEN THE BLURBS

The challenge presented by this excellent book can be seen in the blurbs on its dust jacket. Three distinguished scholars of science and economics offer their assessments: Steven Shapin describes the book as an "exploration of the conditions in which finance economists help to make the world they seek to describe and predict." Karin Knorr Cetina says it is an "account of the development of finance theory and the ways in which it is intertwined with financial markets." And Michel Callon says MacKenzie "shows how economic theory has succeeded in shaping" modern financial markets. Each of the blurbs express something of MacKenzie's central idea, the notion of *performativity*. Economics, MacKenzie says, "does not always stand outside the economy, analyzing it as an external thing; sometimes it is an intrinsic part of economic processes. Let us call the claim that economics plays the latter role *the performativity of economics*" (16).

Sitting above the other endorsements on the back cover, however, are the words of an economist. And not just any economist, either. Paul Samuelson enthusiastically praises the book, saying that "finance theory has revolutionized the arts of canny investing. MacKenzie knows this exciting story, and he tells it well." So, full marks for the author's grasp of the details and his gift for clear, absorbing exposition. But Samuelson is perfectly silent on the theoretical point of the book. The question of performativity seems of little interest to him. David Warsh's recent review[1] suggests

[1] http://www.economicprincipals.com/issues/06.09.24.html

a similar reading. He likens the theory in the book to the sound of a foreign accent: initially distracting, perhaps, but something one can easily get used to – and then ignore.

So much the worse for the economists, we might be tempted to say. I think this would be a mistake, for two reasons. First, a clear lesson of MacKenzie's narrative is that the intellectual and practical achievements of modern finance theory are very substantial. Moreover, the disagreements and debates described in the book show that the main protagonists reflected on the substance and implications of their project as it developed. MacKenzie never patronizes his subjects, and the reader should not be tempted to, either. Second, even the sociological endorsements suggest the notion of performativity might be trickier than it looks. They say finance theory *shapes* markets, or is *intertwined* with them; economists *help* make the world they describe. To *shape* is not to *determine*. To *help* is not to *dictate*. Whatever the performative relation is, it is clearly not a matter of simple, one-directional cause and effect.

From a descriptive point of view, the book is a complete success: it gives the reader a terrific insight into the modern history of the emergence of finance as a discipline, and closely narrates four crucial points of contact between the theory of finance and the world of actual financial markets from the late 1960s to the late 1990s. But can we say that finance theory has "helped to create the world it posited, for example, a world that has been altered to conform better to the theory's initially unrealistic assumptions?" (24). MacKenzie wants to argue that we can, and thus that his theoretical vocabulary helps understand what is happening in the story: he wants performativity to be a language, not an accent.

FROM SCIENCE STUDIES TO FINANCE STUDIES

MacKenzie's background is in science studies. In the 1970s, David Bloor's (1976) "strong program" argued for "epistemological symmetry" in explanations of the acceptance or rejection of scientific theories. Roughly speaking, the idea was that social explanations of the acceptance or rejection of scientific theories need not take a position on the truth or falsity of those theories. Sociological processes or social factors were not not to be invoked just to explain the acceptance or persistence of seemingly false beliefs, but seemingly true ones as well. The obvious contrast is with the simplest realist view that the truth or accuracy of a theory is enough to explain why scientists believe in it (with appropriate caveats about the current state of knowledge and the possibility of new research). Recalling the main tenets of the strong program makes it clear why the so many researchers trained in science studies have been attracted in recent years to economic sociology. The *cri de cœur* of the strong program was that sociology was not there just to explain the mistakes or irrational episodes in the his-

tory of science. Strong-programmers criticized their Mertonian precursors for being too willing to accept this lesser role in relation to the official theorists and historians of scientific progress. In exactly the same way, the new economic sociology began by rejecting Parsons' (and before him, Pareto's) division between economics as the science of rational action and sociology as the science of the irrational residuum. No wonder it caught the attention of people like MacKenzie.

The strong-programmers were followed (and challenged) by the actor-network theory (ANT) of Bruno Latour (1987) and Michel Callon (1986). This school takes the more radical view that there is no point in even making a distinction between the social and natural when it comes to explaining scientific practice. They speak instead in terms of networks of "actants," a category including not just people but also things to which agency may be attributed, such as devices, technologies or bits of software. The focus is on the process by which a heterogenous collection of such actants can become organized into a more or less successful knowledge-producing network. The questions of most interest to ANT researchers are typically "what" and "how" questions: what are the actants in the network, how are they made to fit together, what are the results, how are they produced? This approach encourages very close study of the role of material devices, artifacts, algorithms and so on in the production and transformation of knowledge. Though not a convert to Latour's project, this last feature is strongly evident in MacKenzie's book.

The strong-programmers and the Latoureans disagree about a great deal. A recurrent criticism of both approaches from philosophers is that, although they may appear to have radical epistemological or ontological implications, on closer inspection their claims are either flatly indefensible or easily reconciled with a more straightforward philosophy of science or ontology. Work in science studies can indeed be unclear about the content of its claims about scientific truth or its correspondence to reality. At the same time, the caricature of strong-programmers (ridiculed as "postmodernist" or "irrationalist") often seen in the work of some rock-kicking scientific realists does little to convey the importance of science studies and less to recommend the fair-mindedness of its critics.[2]

An Engine, Not a Camera does contain one or two bits of loose talk on the topic of truth and reality, and its vocabulary in this respect derives more from the Latourean strain of thought than the Strong Progammers. But in its substance the book does not directly concern itself with the philosophical questions. In fact, because the topic is

[2] In retrospect, much of the early philosophical criticism of the strong program sounds like just the sort of Durkheimian response to the desecration of a sacred object that Bloor himself anticipated in Chapter 3 of *Knowledge and Social Imagery*. Re-reading that book today also makes clear, however, that Bloor did not engage with some of the more sophisticated versions of scientific realism (or empiricism) that were developed in the 1970s.

finance and financial markets, some standard battle lines in the "science wars" are beside the point. Given the case at hand, any claims about the performative relationship between theory and reality will refer to a link between two different kinds of social practice rather than – as in the case of physics, say – to the relationship between a social practice and some bit of the non-human world. Both economics as a body of knowledge and economies themselves are socially constructed. They are produced by the joint action of human beings, so what else could they be? The Chicago Board of Exchange is not like Halley's Comet, capable of going on as before in the absence of the social actions that bring it to life. In this sense, both economics and the economy are necessarily "performed" or "enacted" by people (together with other actants), just like any other social practice.[3] This means that the bar for showing that theory is "performed" through practice is lower than in the case of the physical sciences.

The worry here is that the bar might thus be *too* low. The kind of performativity that is necessarily true just in virtue of being a social practice is not very interesting. At the other end of the scale is what MacKenzie calls the "crude claim that any arbitrary formula for option prices, if proposed by sufficiently authoritative people, could have 'made itself true' by being adopted" (20). This much stronger version of performativity is obviously false. The question is whether there is any interesting middle ground which we don't already understand, and which the concept of performativity helps elucidate.

LEAD US NOT INTO TEMPTATION, AGAIN

I want to be clear about the concept of performativity because it is tempting to misinterpret it. Specifically, the label will have a strong rhetorical appeal to many sociologists because it suggests some kind of a debunking about what is going on behind the curtain, the exposure of a trick, showing that the emperor has no clothes. The implication is that economic theories are successful because they can get themselves enacted, and not that they get enacted because they are successful. Although MacKenzie disavows this interpretation early on (20), the book leaves a fairly wide space for ambiguity. This is where we came in: an audience of sociologists will tend to hear the argument as a kind of debunking of economic practice, or as an example of how

[3] This does not quite resolve things. Proponents of ANT often make strong claims about both the intentionality of non-human actants in the networks they study, and the nature of the division between subjects and objects of knowledge. But we can easily consider material objects or technological devices as crucial to the practice under study without believing they have intentionality. Here MacKenzie pragmatically bypasses these issues: he pays close attention to the material aspects of finance theory's ideas (such as the effect of Fischer Black's "sheets" for option pricing) without seeing the need to make any metaphysical claims about them. The only difficulty, as I discuss below, is that his use of this language encourages the reader to read such claims into the argument.

finance is better explained by a sociological approach than by its own theoretical resources. Meanwhile, if pushed by an audience of economists, MacKenzie can honestly say that he never explicitly makes these strong claims.

The self-image of economics as a discipline adds further enticement. Economics often pretends to the authority of the natural sciences, sometimes claiming the title of the physics of the social sciences.[4] The organization of contemporary economics as a powerful quasi-profession (Fourcade 2006) helps its adherents talk as though its point of view was inexorable and even beyond question. We have all heard – or perhaps been the victim of – aggressive browbeatings about "basic principles of Econ 101" or the "fundamental facts of economics." The implication of this kind of boundary-maintenance is that credible criticism of economic policy or theory can only come from those with a Ph.D in the field. But internally economics remains quite heterogeneous. When apostates arise within the fold, the temptation is to insist these lost sheep are not really economists after all, or that they have forgotten any economics they once knew.[5]

This is just the sort of attitude that the strong-program iconoclasts wanted to dismantle in the case of natural science. The dogma under attack was, as Stephen Shapin once put it, the idea that reality imprints itself on the content of science with "unmediated compulsory force" (Shapin 1982: 163). This project produced a vice of its own: a tendency for science studies to oversell its own findings. It proved hard to resist the subversive *frisson* experienced when insinuating that physical reality *per se* was in some strong sense dependent on our conceptions of it. Distaste for one extreme sometimes led some – intentionally or otherwise – to write as though they denied reality any role in determining the content of our beliefs about the natural world.

We can leave aside the question of whether the "unmediated force" view was in fact widespread amongst philosophers of science at the time. In part because of the rise of the Latoureans, the differences between contemporary strong-programmers and philosophers with a moderately realist view of science are perhaps fewer and less

[4] Paul Krugman (1995: xi) reports the following squib from Jagdish Bhagwati: "If you are a good economist, a virtuous economist …you are reborn as a physicist. But if you are an evil, wicked economist, you are reborn as a sociologist."

[5] It is interesting for outsiders to see different how (and which) intellectual tendencies tend to get expelled in different policy contexts, such as debates on corporate regulation, the housing market, the rationality of stock prices, immigration policy, the minimum wage and so on. A lucky few get to play policeman and pariah at different times in their careers. As seen in the previous note, in the 1990s Paul Krugman was a tireless defender of the discipline of economics against assorted pretenders to its expertise. These days he is subject instead to charges of political shrillness, though of course his credibility remains difficult to challenge and his political views are shared by many economists. Joseph Stiglitz is perhaps a better case, given that had the bad taste to win the Nobel Prize before writing a series of popular books containing views on free trade which most economists would repudiate.

significant than they might seem. The critical attack by strong-programmers on Popperian falsificationism and the Lakatosian program for "rational reconstruction" of the history of science was very effective, but its impact on succeeding versions of scientific realism was perhaps rather more constructive than is commonly acknowledged. Contemporary scientific realists are more careful than they once were about the relationship between science as a social practice and the truth of our beliefs about the natural world. But the same is also true of the strong-programmers themselves: they are much less inclined than before to gesture towards strongly relativist claims about truth.[6]

The science studies brigade has now arrived in force on the borders of economic sociology, and so much the better for both fields. But the last thing that we need is to rehash the same tired mistakes or polemics all over again regarding the content of claims about the constructed or performative quality of economic theory and practice.

KINDS AND CASES OF PERFORMATIVITY

MacKenzie distinguishes three kinds of performativity: "generic," "effective" and "Barnesian" (together with the latter's negative complement, "counterperformativity"). Generic performativity means the active use of some bit of theory not just by economists but also by economic agents, policy makers and the like. Effective performativity requires that the use of theory not just be window-dressing: it must "make a difference" (18) in practice. Finally "Barnesian" performativity (named for Barry Barnes) requires that the use of economics actively alter processes "in ways that bear on their conformity to the aspect of economics in question" (19). That is, the model or theory must bring participants into line with its picture of the world. In that case the model helps make itself true, in the sense that before the its public appearance the system did not behave in accordance with the model's predictions, whereas subsequently it does. Naturally, it is also possible that a model might undermine the real-world viability of the process it describes. That would be a "counterperformative" effect.

MacKenzie argues that performativity can be thought of as part of "a more general phenomenon: the incorporation of economics into the infrastructures of markets" (19). He examines four cases from the field of finance theory where ideas developed (mostly) by academics might have had performative effects on the structure and practice of financial markets. These are the Modigligani-Miller "irrelevance" propo-

[6] For example, here is Steven Shapin making this point about Boyle's Law: "Should a sociologist say that Boyle's law was wholly a social construct, and should she mean that the state of affairs in nature was socially constructed, and that the law was not, therefore, a reliable generalization, she would be both mistaken and unjustified" (Shapin 2001: 1791). David Bloor's (1999) attack on Latour's project is an extended example.

sitions for capital theory; portfolio selection theory and the closely related Capital Asset Pricing Model (CAPM); random walk models and the efficient market hypothesis (EMH); and the Black-Scholes-Merton (BSM) formula for option pricing. Taken together, these ideas form the core of modern finance theory, and they contributed to a revolution in financial markets that began in the late 1960s.

What sort of effects did these ideas have? It turns out that the cases are very mixed. The Modigliani-Miller propositions helped launch modern finance theory but did not have strong or immediate practical consequences. In retrospect, they could be seen as providing intellectual support for less negative attitudes towards debt-financing, and as foreshadowing somewhat the financialization of corporate governance which took place in the 1980s.

The CAPM's effects were also ambiguous. The model specified a parameter, beta, which captures the sensitivity of returns on a stock to fluctuations in the market as a whole. The CAPM defines the expected value of a stock as depending, in part, on the expected return of the market as a whole, and both of these quantities are unobservable in practice. (The unobservability of these numbers was at the heart of Herbert Simon's early critique of finance theory.) The best one could do in practice was to work backwards from observed values to estimate the model's parameters, using a stock index to be a proxy for expected market return, for instance. This practice raises significant questions about the validity and interpretability of the model, in particular whether true uncertainties about the market are being tacitly assumed away. In any event, when operationalized in this way the CAPM agrees only fairly well with the data, and the use of the model in practice has not improved its fit with the data. Thus, the performative consequences of both these innovations fall well short of the Barnesian threshold, though this is not to say that they were unimportant. In particular, the CAPM provided a way to evaluate the performance of investment portfolios (and their managers). The CAPM's beta parameter took on something of a life of its own in the 1970s and became part of the vocabulary of investing.

The EMH's effects were more direct. It dealt a lethal blow to the legitimacy of the older generation of professional stock advisers. The wisdom of the "chartists," who purported to predict the future path of stock movements on the basis of the shape of existing trends, was brutally undermined. To those convinced by even weak versions of the EMH, chartists looked like Roman priests interpreting the auspices by killing a chicken and looking at the entrails. A classic piece of business journalism from the 1970s, *The Money Game* by "Adam Smith" (aka George Goodman), contains a portrait of the eclipse of the "chart men" that almost makes the reader wince in sympathy. The other main competitor to EMH believers, the "fundamentalists," fared somewhat better. This was thanks in part to their more plausible arguments that the fundamentals of firm structure and market position mattered to long-term profitability. But the enormous success of their spiritual leader, Warren Buffett, added to their credibility

as well. A new generation of financial analysts succeeded the older school, with the EMH providing the theory for why instruments like index funds were rational investments. Prior to the EMH, the idea of buying and holding a weighted portfolio of every stock in an index seemed to defeat the whole purpose of playing the stock market and getting professional advice.

More importantly (from the point of view of performativity), the EMH allowed researchers and investors to systematically identify market anomalies. Using the CAPM as a baseline, a series of studies investigated the existence of investment opportunities that offered excess risk-adjusted returns. Oddities like the "small-firm effect," the "turn-of-the-year" effect, and the "momentum effect" were discovered, each of which seemed on its face to violate the EMH. MacKenzie provides a very good discussion of the tricky question of how to interpret these anomalies. They pose potential problems for both the EMH and the performativity thesis. For those involved in the EMH debate, anomalies admitted to several interpretations. Most simply, they were just statistical errors or the product of data dredging. Alternatively, they really existed in the market but, once identified, the action of arbitrageurs closed them up. Interestingly, this is the most satisfying explanation for *both* EMH advocates *and* sociological performativists. Sensible advocates of the EMH can claim that the actual practice of markets is of course subject to a certain amount of imperfection, but that once identified (either publicly in scholarly papers or privately by players in the market) any anomalies will be eliminated. From a performative perspective, these cases can be seen as the EMH providing the framework to improve its own predictions in a satisfying process of "identification, exploitation and attenuation" of anomalies.

Some anomalies were more difficult to account for because they persisted even when identified. EMH advocates responded by arguing that the anomalies are explained by deficiencies in the CAPM specification used to calculate the baseline criteria for excess returns in the first place. But anomalies are also problematic for performativists, and again for the very same reason: they persist in the face of a dominantly enacted theory that says they shouldn't exist. One possible response is to say that they might be an instance of counterperformativity. Here MacKenzie has only one partial example, the index inclusion anomaly. This is the phenomenon of a stock rising in price when it is included in a leading market index like the S&P 500. (This ought not to happen because mere inclusion in an index should convey no new information about a stock.) But most persistent anomalies do not fit the counterperformative picture because they are not examples of the application of the model actively undermining the model itself. Rather, they are examples of phenomena that the model cannot explain, which is quite a different thing.

Finally, there are the BSM equations for option pricing. This is one of finance theory's crown jewels and also MacKenzie's best case for performativity. If the EMH provided an overall vision of how the market should work, then Black, Scholes and Mer-

ton provided a technique that could be put to work within the market itself. MacKenzie documents the development of the theory behind option pricing and its subsequent application in practice. He argues that the appearance and application of the BSM formula had three main effects. First, the model's power and elegance legitimated the business of options trading: "it undermined the long-standing cultural association between options and gambling" (158). The fact that the basic equations were published work rather than private methods eased their acceptance. Second, Fischer Black sold elegantly-constructed sheets containing Black-Scholes values for options (and associated information) that traders could use on the floor while doing their work. They simplified the process of making trades, though traders using them were occasionally mocked as not being man enough to work without them. Third, MacKenzie argues that the model was put to use in "spreading," the identification of pairs of options on the same underlying stock where one member of the pair was underpriced with respect to the other. Here MacKenzie sees Barnesian performativity in action, because the method that traders used to identify the discrepancies in option prices was the same, in essence, as the one academic researchers used to assess the accuracy of the model itself:

> The most thorough tests of fit were conducted by Mark Rubinstein (1985) …[In essence] Rubinstein checked whether the graph of implied volatility against strike price was a flat line, as it should be on the model. There was thus a homology between the econometric testing of the Black-Scholes-Merton model and the trading-floor use of the model in "spreading." When spreaders used the model …it would be precisely deviations from that flat line that they would have identified and that their activities would have tended to "arbitrage away." It seems, therefore, that the model may have been helped to pass its central econometric test …by the market activities of those who used it (165).

This is MacKenzie's strongest example of Barnesian performativity, "a direct performative loop between 'theory' and 'reality'" (166). The mechanism here is of great interest because it is not what we typically mean when we say that economic theory has the capacity to make itself true by successfully implanting itself in our minds. Critics such as Margaret Radin have worried, for example, that the spread of the rhetoric of commodification makes people forget that their motives and actions are not all that well-described by the self-interested vocabulary they use. And experiments in social-psychology and behavioral economics have found that exposure to the lessons of undergraduate economics makes people more selfish (or rational, if you prefer), and more likely to behave like *homo economicus*. Nothing like this is happening in the case of option pricing. The setting is already a market, and the self-interested motiva-

tions of traders do not change a bit. Rather, the model is put to use prospectively in essentially the same way that a researcher would go about testing it retrospectively. It is adopted in practice in a way that mirrors its assumptions and prescriptions. This correspondence is what causes the gap between theory and practice, between economics and reality, to narrow. Moreover, the narrowing happens from the side of practice: by employing the formula to identify and exploit profit opportunities, market actors moved observed prices closer to what the model predicted should be observed.

ENACTED THEORY AND THE PROBLEM OF SUCCESS

The overall theme of the book is that finance theory is performative, but at most points of detail the evidence is ambiguous. One worry is this: when some version of performativity is used to label uncontroversially sociological processes, the reader suspects that these are already understood under different names. At other times, what MacKenzie describes cuts much closer to the economics, and the reader wonders whether the native story – the model's own account of its success, if you like – is a sufficient explanation instead.

Common-or-garden institution building

On the sociological side, the processes visible in what MacKenzie would classify as cases of "generic" or "effective" performativity seem to be fairly well-understood, though perhaps they have not been investigated quite so well before in the realm of financial markets.[7] For example, his description of the institution-building and collective-action efforts of Leo Melamed and his associates in Chicago is a superb study of collective action in the service of market creation. And although MacKenzie distinguishes his "social studies of finance" approach from regular economic sociology, he points out himself that John Meriwether and his associates at Salomon Brothers behaved as classic embedded actors. They drew on network ties and rich local knowledge about institutional structure to determine whether arbitrage opportunities identified by BSM-like methods were worth pursuing in practice. "'Mathematics was helpful,' says Meriwhether, but the kind of understanding of the institutional structure of the market that comes only from experience was – precisely as the Granovetterian tradition would predict – 'more important'" (216). He also shows how several prominent economists – Milton Friedman, Burton Malkiel, and others – worked hard to politi-

[7] It is worth noting that MacKenzie does not make all that much conceptual use of generic or effective performativity in the course of the book. They play much less of a role than one might expect from reading the first chapter, and he tends not to say, for example, that such-and-such is an instance of effective performativity.

cally legitimate the new financial instruments. Their goal was to make them palatable to regulatory agencies and legal professionals.

Now, in a loose sense all this effort was in the service of trying to enact a theory. The various players wanted to remove the stigma from certain kinds of financial instrument, and see legal, respectable markets in them come into being. It was important to their efforts that BSM and other theoretical ideas provided evidence that their project was intellectually viable. But it was also a straightforward exercise in institution-building and political legitimation, and these do not seem to me to be performative in a strong sense. Even for the BSM case, where evidence for performativity is strongest, MacKenzie describes a range of "associated processes" (near 167) at work around the same time as the model took hold. While not strictly performative, they did have the effect of bringing financial markets closer to instantiating a "Black-Scholes world," by creating the conditions whereby the model's assumptions did not seem so unrealistic. These included easier credit for stock purchases, vastly increased speeds of communications and computing power, the abolition of fixed commissions to brokers, the diffusion of options trading across sectors and exchanges, and the rise of wire-based markets such as NASDAQ, with its Autoquote software.

This is all evidence that a distinctively financialized conception of trading was on the rise from the 1970s onwards. But none of these processes involves the kind of elegant homology between theoretical validation and practical application that MacKenzie identifies as the key to BSM's Barnesian moment.[8] The further we move away from the traders working with the formulae on the floor, the more things start to look like political programs and institution-building projects that we understand fairly well. Vigorous efforts to realize some desired end, coupled with an insistence on the inevitable triumph of that end, characterize any strong ideological program.

At best, then, the Barnesian performativity of Black-Scholes was only one element amongst many explaining the success of the revolution in finance. Of course, just as advocates of the strong program in the sociology of science are not under any obligation to argue that scientific knowledge is *purely* social (just that it always has a social component), MacKenzie nowhere claims that the performativity of finance is *purely* Barnesian (just that it may have a Barnesian component). Nevertheless, the obviously important role of these other powerful sociological and political processes should lead us to ask whether the BSM's forumla's Barnesian performativity was so important to

[8] As the book makes clear, after the BSM equation's convergence with observed prices, some important discrepancies opened up such that the original formulation no longer predicted well. But the long-term importance of BSM was that, first, it was an exemplar "of a general methodology for pricing a derivative" which led to many refinements and extensions, and second, that it gave "a clear and systematic account of the economic process determining those prices …It affected how market participants and regulators thought about options" (20).

the successful institutionalization of options trading that MacKenzie describes. That the Barnesian variety is *logically* the strongest of the three types of performativity is no guarantee that it is the most central in practice, either in terms of its prevalence or the force of its effects.

Why BSM worked

On the economics side, there is the question of the reason for the formula's success. To see why this is an issue, it is worth returning to MacKenzie's early disclaimer that not just *any* apparently authoritative equation for pricing options could have taken hold and been enacted. That "crude claim" is obviously false:

> Imagine, for example, that as a result of a mistake in their algebra Black and Scholes had produced a formula for the value of a call option that was half or double their actual formula …, that no-one noticed, and that the formula was then used widely to price options. It would not have been a stable outcome: the sellers or buyers of options would have incurred systematic losses, and attractive arbitrage opportunities would have been created (20).

This example makes it clear that Barnesian performativity, of the sort MacKenzie wants, is not simply the forcible imposition or successful propagation of some set of beliefs about the world. Barnes himself, as quoted by MacKenzie, "conceived of a society as a distribution of self-referring knowledge substantially confirmed by the practice it sustains" (Barnes 1988: 166). On a weak reading, all social institutions worthy of the name exemplify this proposition: the mark of any institution – from religion to law to marriage to property – is that its everyday enactment by people chronically reproduces, confirms and thereby entrenches the expectations of those who bring it to life. But MacKenzie wants a stronger reading than this, because he argues there is a mechanism by which the application of a model can bring reality into line with itself.

What remains to be explained is *why* this mechanism worked as well as it did. As we have seen, MacKenzie argues that there was a "homology" between the testing of the model and its practical use (165). But this homology, even in conjunction with all the active institution-building described above, is not sufficient to explain the success of the formula. If an erroneous version of the formula had been implemented and tested, the same homology would have been present but traders would in the meantime have been systematically losing money. Either the fit would have been very poor and gotten worse, the formula would soon have been abandoned, or it would have been or fixed. We want to say that one of the reasons BSM thrived was

that it *worked* – it amounted to a significant discovery about the way options should be priced. This discovery allowed traders to act in a new way, a way that raised their chances of making money. In the process their actions also changed the way that these markets worked. Precisely because this powerful technique was developed, certain features of the market which presumably existed prior to the spread of BSM – such as undiscovered opportunities for spreads – tended to disappear. The game changed.

This does not mean that the BSM formula was the best or only tool which might have been developed and applied. (Indeed, subsequent parts of the book show how BSM was subsequently surpassed in practice, or "no longer enacted," in MacKenzie's terms.) But it does mean that the technique worked not simply because it was enacted, but because it was a genuine discovery about the institutions and rules of the financial markets that could be practically applied within them. This aspect of the success of the formula – the fact that it actually worked, or worked well enough – is underplayed if we speak only of the theory being "enacted" or "performed." Moreover, despite the early acknowledgment that the formula had to be *right* (or near enough) in order to prosper, this aspect of it does not feature in MacKenzie's subsequent discussion. When he comes to ask, "Why Black-Scholes-Merton?" he instead emphasizes some of the formula's other virtues, such as its explicitly theoretical (rather than econometric) grounding, the intuitive appeal of the concept of volatility and the fact that the formula was publicly available (162-163). These all mattered, but would have been of little use if the method itself was basically flawed.

At this point, it might be tempting to conclude that the reason BSM worked was simply because it was the correct way to price options. Therefore there would be no need for a concept of performativity – at least, not one that was expected to do any explanatory work. But this would be a mistake. For one thing, the evolution of futures markets since the 1970s shows that while BSM was an exemplar it was not something that was discovered to be true and remained so once-and-for-all. Recall that MacKenzie documents how the fit of the model improved but then declined as a "volatility skew" appeared and remained in the price data.

HOW TO THINK ABOUT PERFORMATIVITY

We can approach the issue more constructively. We know that the appearance and diffusion of options markets was not in any way inevitable. Even economists (most of them, anyway) know that well-functioning market institutions, especially for things like derivatives, do not spring up out of the earth. But the existing resources of economic sociology are well able to illuminate the political and organizational maneuvering required to get such things set up. On the other side, we have the power of finance theory to explain why, once things got rolling, they went like they did. The economic

theory tells a story about why, given a certain set of institutional arrangements, we should expect things to be priced in such-and-such a way. The finance theorists will want to say that, given the institution-building, this story explains the practical success of the formulae.

Performativity is in the middle, and this is its promise and ambiguity as an idea. On the one hand, it implies that the theoretical resources underpinning option theory *were* enough to get it to take root. So much so, in fact, that once it began to be enacted it contained a self-sustaining mechanism to make itself more and more true. Yet, on the other hand, the very need for a concept like "performativity" seems to suggest that the model can *not* account for its success on its own terms: the mechanism for its success was somehow not the one advertised. So we need the sociological vocabulary to really explain what happened. The notion of performativity suggests that we know something that the traders do not, that we have a richer perspective on why BSM and methods like it took hold.

The means to resolve this ambiguity (and to reconcile the internal and external perspectives on the performativity of theory) is right at the heart of the old strong program itself, in its treatment of norms and institutions. The strong program contains two lines of attack on this problem. They are complementary, but draw proximately on different intellectual traditions.

Rules as forms of life

The first, owing mostly to David Bloor, is Wittgensteinian in inspiration. Wittgenstein's ideas about rules and rule-following are read in a distinctively sociological way to produce a theory of the relationship between rules, knowledge and institutions (see especially Bloor 1983 and 1997). My concern here is with innovation in games, and its effect on the game itself. Think of those occasional developments in a sport that go beyond tweaking what is already known about how to play. Instead, a non-obvious gap in the rules or social organization of the game is discovered and exploited in such a way that allows someone to win more effectively. At that point, there may be a debate over whether the innovation should be legal, or whether it counts as playing the game at all.[9] The new move might be banned, or it might give rise to a different game altogether (leaving the original one as before). Or the innovation might be incorporated into the original game, significantly changing it in the process.

That the analogy extends to innovations in the market is obvious. Innovations that are both radical and successful are sometimes called "game-changing" for this

[9] Think of the case of the schoolboy William Webb Ellis, who "picked up the ball and ran with it" during a game of football, and who was judged not to have innovated in soccer but to have created a different game altogether: rugby.

very reason. Bloor discusses some observations of Wittgenstein on just this point in *Knowledge and Social Imagery*, the classic statement of the strong program. Wittgenstein invites us to consider a game being played:

> Let us suppose, however, that the game is such that whoever begins can always win by a particular simple trick. But this has not been realized; …Now someone draws our attention to it;–and it stops being a game. …That means, I want to say, it can also be taken like this: the other man did not *draw our attention* to anything; he taught us a different game in place of our own. –But how can the new game have made the old one obsolete? –We now see something different, and can no longer naïvely go on playing. (Wittgenstein 1983 III-77, p.203)

Bloor (1976: 140) brings this illustration up in the context of a discussion about the nature of logical inference. What I want to focus on is the idea of someone showing us the "simple trick" that changes things. Think of the financial markets as a hugely complicated game with sophisticated rules. Economic life in general is structured by a deeply institutionalized set of formal and informal rules and regulations that help specify how things look in particular circumstances. This is especially true in the world of stock markets, and by extension in markets for options and other derivatives. They sit on top of an enormously complex structure of conventional knowledge and practice. These rules define a field of play where some actions are judged out of order. But it is obvious that they do not determine the detail of what happens in practice. Real games are fluid and dynamic. Participants may have a strategy, but they do not know in advance what is going to happen. As Eric Leifer (1988: 865) observed, this is why game theory is a theory of games that do not need to be played.

Even in ordinary, quite simple games, it can be very difficult to discover some "simple trick" or strategy that allows you to win while still permitting you to say you are playing the game. The problem is immensely more difficult when complex games are considered. An important simplifying feature of markets that makes them different from other kinds of social games (such as the ones played in politics, for instance, or in the world of cultural taste) is that winnings and losses are clearly quantified and denominated in money. The well-defined nature of the outcomes makes the prospect of discovering a winning, replicable strategy somewhat more tractable.[10] The Barnesian performativity that MacKenzie documents in the case of BSM is, I suggest, a version of the game-changing trick that Wittgenstein discusses. It is not simple to discover, but it is simple in practice. Moreover, it changes the game being played, even if

[10] The absence of a quantifiable measure of success is one of the things that makes playing – and analyzing – games of status so much more complicated. See, e.g., Bourdieu (1984).

the players stay the same: "We now see something different, and can no longer naïvely go on playing."

Interpreting strong performativity in this way has three advantages. First, it removes potentially misleading metaphysical associations introduced by too-heavy a reliance on a vocabulary contrasting "theory" and "reality" or the transubstantiation of one into the other. But it is true to the original inspiration of the idea that rules structure the play we observe. Second, it reminds that the market itself is a complex, rule-governed social practice, and thus is properly the subject an analysis of the constitution and operation of those rules, and that this will be a sociological as well as an economic project. But third, it retains the intuition that the Black-Scholes-Merton formula was a genuine *discovery* about features of one such complex practice. The performative aspect of the formula is analogous to the game-changing consequences of deep innovation in other games. Thinking in these terms also renders irrelevant questions about whether the formula was "right" *sub specie aeternitatis*. BSM was not a discovery about nature, it was an innovation in a social practice which became incorporated into that practice, and thus changed it.

Convention and self-fulfilling prophecies

If Wittgenstein is not to your taste, a second line of thought on this topic has its roots in theories of rational choice and has been developed within the strong program mainly by Barry Barnes (1983, 1988). Barnes' work in this area can be thought of as a reworking and extension of Robert K. Merton's (1957) concept of a self-fulfilling prophecy. Merton's insight was to see how some beliefs about the future could make themselves come as a consequence of people acting on their expectations. The canonical example is that of a bank failure happening because too many people demand to withdraw their deposits, because they believe the bank is about to fail. If people believe there will be a run on the bank, then in the absence of any other countervailing effort to change their minds there will indeed be a run on the bank. A limitation of Merton's view is that he confines the idea of self-fulfilling (and self-negating) prophecies to "a *false* definition of the situation evoking a new behavior which makes the originally false conception come true" (Merton 1957: 421). The problem with this, noted first by Krishna (1971) and later developed in detail by Barnes, is that there is no reason to think that *only* false beliefs can have this character, or that it is a characteristic of self-fulfilling prophecies that the initial belief is false in some meaningful sense. In fact, for a very large class of social action the truth or falsity of the belief is beside the point when it comes to understanding its causal effects. As Krishna (1971: 1104-1105) comments,

> [Merton] seems to assume that there *is* a true or false definition of the situation concerning social reality apart from what men think or believe

> about it and that it can be independently known or determined without reference to this thought or belief …Where "beliefs" or, rather, the way consciousness conceives of a situation, forms and integral part of the situation itself, it is difficult to think of the truth of the situation *indpendently* of the way it is conceived to be.

To put it another way, Krishna's point was that a bank *not* failing is also ultimately a self-fulfilling consequence of people's belief that it is sound.[11] What Barnes did was to take this idea and develop it at length, seeing society as a huge distribution of self-referring knowledge. He connected the idea of a self-fulfilling prophecy to the analysis of institutions as conventions originated by Thomas Schelling's early work on focal points (Schelling 1960, 1978) and developed most fully in David Lewis's work on convention (Lewis 1969). Schelling explored ways in which individuals might come to agreement or understanding in the absence of direct communication, by way of reasonable inferences by each party about how the other might be thinking about the situation. By extension, Lewis clearly defined the concept of a convention, demonstrated how conventions might emerge, and showed how the concept could explicate the intuitive observation that languages were conventional systems.

Barnes' concept of society as a "monumental, sublime, self-fulfilling prophecy" (1988: 52) is an effort to generalize this idea. His view of society as a self-fulfilling, self-reinforcing, self-confirming system of knowledge and belief ultimately rests on the analysis of the emergence, stabilization and reproduction of conventions provided by Schelling and Lewis. This intellectual genealogy – going back to early work in the theory of rational choice and debates in analytic philosophy over the reductive analysis of linguistic conventions – might come as a surprise to critics determined to see strong-programmers as cartoon figures devoted to "postmodern" relativism and irrationality.[12]

Taken together, I think, the Wittgensteinian and conventionalist lines of argument do a lot to clarify how we should think about performativity. They make it easier to

[11] "Merton may reply that by calling the definition 'unreal,' he merely meant that the *actual* financial situation of the bank was such that a disbelief in its ability to meet its financial commitments was unwarranted. After all, the actual situation consists of the ration of assets to liabilities and the relative liquidity of the assets which one holds …But if all these by themselves are not able to save the bank from a financial crash without the belief in its financial viability on the part of a large number of its clients, then it is misleading to define the financial situation apart from the belief that men hold in the situation …Social phenomena, in fact, may be graded by the extent to which the belief or rather consciousness entertained about the situation is a constitutive element in it" (Krishna 1971: 1105).

[12] The work of Michael Chwe (2001) independently extends the Schelling/Lewis approach to convention and culture in a way that Barnes ought to approve of, and also shows many unexpected connections between the analysis of common knowledge and the sociology of culture.

see how both the economic and the sociological perspective on financial markets are related to one another, and they also clarify the intellectual roots of the concept itself in a way that should make it palatable to anyone who gets twitchy at the prospect of a sociological analysis of the conditions of scientific knowledge. This interpretation is perfectly consistent with MacKenzie's presentation of his argument. Indeed, one only has to follow up on some of the key references he provides to see where the argument is coming from. But if you are not familiar with the development of the social theory of the strong program, it is easy to read through *An Engine, Not a Camera* without a good sense of what stands behind the label of "Barnesian performativity." That makes me worry – I think justifiably, based on the history of polemic in the science wars – that neither MacKenzie's supporters nor his critics will be willing to follow up on things in this way. It would be a shame if that were the fate of the book.

CONCLUSION

To his credit, MacKenzie does take the easy route of writing as though the language of performativity was known in advance to be right. There are only one or two instances where he speaks as though the performativity thesis has been firmly established, saying that "Black-Scholes-Merton option-pricing theory was enacted at the Chicago Board Options Exchange" (179), or remarking that "what is now performed in Chicago is no longer classic option-pricing theory" (202). Much more often, he says that "there will often be an element of conjecture" (18) to such a claim, or that there is "a lack of conclusive evidence" (194) in crucial cases, or that the sociological processes "cannot be distinguished" (237) from the strictly economic ones, or that "there is no way of being certain" (256) that the practical adoption of theoretical models led to and improvement in the observed fit of those models. Nevertheless, MacKenzie clearly *does* believe that some nontrivial version of the performativity thesis is true. Although a definitive demonstration may be impossible to produce, he thinks a preponderance of evidence strongly supports the idea. But he is too honest to only play the role of an advocate presenting arguments to the jury. The book makes a strong case, but when acting as the judge of his own evidence MacKenzie remains more skeptical. At crucial points he seems to lean – appropriately enough – toward the "Scottish verdict" of Not Proven.[13]

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[13] http://en.wikipedia.org/wiki/Not_proven

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