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FINANCE AND FINANCIAL ECONOMICS

A Philosophy of Science Perspective

Melissa Vergara-Fernández and Boudewijn de Bruin

1. Introduction

Finance, from the perspective of the philosophy of science, is largely *terra incognita*. In contrast to social studies scholars, philosophers of science have been rather indifferent to the study of finance. An obvious starting point, from a philosophical perspective, is thus to ask what is distinctive about finance that has led to this indifference. One answer could be that the historical roots of economics lie in theorizing about the world and thereby understanding it, whereas those of finance lie in solving practical problems. Economics is a science; finance is workmanship.

But that cannot be the whole story. Economics and finance have too much shared history, at least recently. Take modern portfolio theory (MPT), with the mean-variance portfolio model, the efficient market hypothesis (EMH), and the capital asset pricing model (CAPM) as its main constituents. This accomplishment is taken by most historians of finance to be the watershed between "old finance" and "new finance" precisely because it established important areas in finance on a model-based footing, very much like the rest of economics. Or consider the institutions involved: the Cowles Commission, together with Carnegie Tech (later Carnegie Mellon University), the University of Chicago, and the Massachusetts Institute of Technology (MIT), shaped not only much of economics but also new finance (Bernstein 1993; MacKenzie 2006: Chapter 2).

This shared history of economics and finance is one of the reasons why financial economics deserves philosophical scrutiny. Furthermore, while financial economics is a young subdiscipline, it is of high importance: it studies the financial markets and their institutions. In fact, it actively shapes them and does so arguably to a greater extent than other scientific disciplines might shape their own object of study. We shall see that if we look at what drove the big innovations in financial economics, the connection with practice (industry, regulators, supervisory authorities, and so on) is clearly present. This is arguably the reason why the social studies literature has devoted so much attention to what is called the *performativity* of financial economics. We think this is an important issue. But we also hope to show that it is far from the only topic a philosopher of science should find interesting.

For obvious reasons, it is impossible to cover the entire field of financial economics in this chapter. We have thus opted for selecting some key elements of modern finance that we think should generate philosophical interest (see De Bruin et al. 2020 for a discussion of money and finance). We also offer a case study on a central result from finance, a model by Modigliani and Miller, that has attracted some earlier attention by philosophers, where we address the explanatoriness of unrealistic models.

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2. Key Elements of Finance

2.1. The Joint Hypothesis Problem

Two core elements of financial economics are the efficient market hypothesis (EMH) and the capital asset pricing model (CAPM). Taken together, they pose an important question about how theories are, and can be, tested, as well as about the assumptions necessary to do so. The EMH states that the price of any asset at any point in time fully reflects all available information (Fama 1970). The hypothesis can be seen as motivated by an equilibrium argument to the effect that rational traders, maximizing their expected utility on the basis of the best information available, would drive out *noise traders* (the minority of irrational traders) through arbitrage, if a market were not efficient. An illustration is the well-known *January effect*, according to which stock prices increase in the first week of the year. Following the logic of the EMH, this trend should disappear as soon as traders notice it, and indeed, there is evidence that the January effect and similar phenomena have disappeared once they became common knowledge (Thaler 1987).

The significance of the EMH is difficult to overstate. It tells us that there is no agent in the economy who can systematically "beat" the market. There is nothing they can know that asset prices do not already fully reflect. If the primary role of capital markets is the efficient allocation of capital stock, it is a desirable feature that prices fully reflect the available information, for only then can the allocation be efficient. The January effect, according to the EMH, will not happen systematically. The question is, then, whether the EMH can be tested empirically.

This is hard, as Fama (1970) observed. The definition of the EMH "is so general that it has no empirically testable implications" (Fama 1970: 384). In order for the EMH to be tested, the process of price formation must be specified in detail. In other words, "[w]e can't test whether the market does what it is supposed to do unless we specify what it is supposed to do" (Fama 2014: 1467).

At least in principle, the CAPM tells us what the market is supposed to do. It measures the market risk of an asset and prices that risk (on the concept of risk in economics, see Stefánsson, Chapter 3). A key idea behind it is that rational investors need not care about the *specific* risk attached to specific assets, as this risk can be diversified away by investing in a broader portfolio of assets. Instead, the CAPM states that the only risk to be priced is the one that cannot so be eliminated. It also states that, in equilibrium, there is a linear relationship between this undiversifiable risk and expected returns. A test of the EMH, therefore, is to determine whether the determinants of expected returns implied by the CAPM are indeed observed in actual returns. The problem, however, is that if actual returns do not conform to the predictions of the CAPM, we cannot tell whether it is due to the fact that the markets are inefficient or whether the CAPM is a poor model of market equilibrium. This is the *joint hypothesis problem*.

While this problem is not a textbook case of underdetermination, where competing theories are underdetermined by the data, rendering the test indecisive, it is clear that the testability of "the two pillars" or "Siamese twins" of asset pricing, as Fama (2014) calls them, is at stake. It might tempt philosophers to revisit the Duhem–Quine thesis and the problem of underdetermination altogether. The material, we venture, is certainly more interesting than similar problems in game theory (Hausman 2005; Karpus and Radzvilas, Chapter 7) and macroeconomics (Cross 1982) because, unlike game theorists, financial economists were keenly aware of the issue, describing it as a "problem" or a "research opportunity" (Campbell 2014).

2.2. Event Studies

How the profession has dealt with the lack of testability of two of its most substantive achievements is also worthy of philosophical attention. It led to the development of a new (and Nobel

Prize–winning) method: the event study. An event study examines the effects of a particular event (e.g., an oil spill disaster) on the price of certain assets (e.g., shares in oil companies). The first event study, by Fama et al. (1969), examined the effects of stock splits on share prices. The number of published event studies in economics may well run to over 10,000 to date (not only in finance but also in accounting). Unlike other widespread methods such as randomized controlled trials, to our knowledge no philosopher of science has so far looked at event studies.

Event studies are not only interesting because they are explicitly targeting a Duhem–Quine type of predicament. Their development also underscores our finance-as-a-practice view, with potential relevance to the philosophical discussion about non-evidentialism and the non-epistemic value of models. Here is our story. Modern finance would look different (or would have developed more slowly) if it had not been spurred by substantial funding from Merrill Lynch, for instance. In the 1960s, this bank contributed tens of thousands of dollars to establish a Center for Research in Security Prices (CRSP) at the University of Chicago, the aim of which was exactly what its name suggests. But as Fama (2017) writes in an anniversary article of the *Journal of Political Economy*, there was a worry that while the bank would benefit greatly from making available high-quality asset price data, mainstream economists would not sufficiently appreciate the value of the research conducted at the Center. The founder of the Center, James Lorie, therefore suggested that Fama conduct research on stock splits. And this led to the birth of the event study.

2.3. Performativity

MacKenzie (2006) uses the term performativity to refer to what happens when the practical use of some aspect of economics makes the relevant economic processes more similar to economic theory. A key example of performativity is the Black–Scholes option-pricing model (more appropriately also including the name of Robert Merton). An option is the right to buy (call option) or sell (put option) a particular underlying asset, typically a piece of stock, at a stated price within a specific time frame. The thought behind the Black–Scholes model, which has sufficient prima facie plausibility, is that one should be able to determine the "right" price of an option solely on the basis of relevant information concerning the underlying asset. Technical details aside, Black–Scholes accomplishes this by allowing one to determine the price of an option on the basis of a few variables, such as the underlying asset's price, its volatility, and the risk-free rate.

Yet, the model was not very accurate when actual option prices were compared with the prices predicted by the model. Only after the model was published (and Black–Scholes price sheets circulated among traders) did option prices converge to what should be expected on the basis of the model. This tempted social studies of science scholars to see Black–Scholes as a paradigmatic case of performativity.

We cannot give a full evaluation of the cogency of their argument here, but there is considerable opportunity for philosophical analysis. For instance, it may help to analyze the relation between theory (the Black–Scholes model) and practice (observed option pricing) as one between norms and behavior. A similar idea has been defended by Guala (2016). He suggested that option prices as determined by Black–Scholes can figure as coordination devices, just as people conform with certain traffic rules. This makes the traffic laws quite accurate as "descriptions" of observed behavior. So, when traders start using Black–Scholes as a guidebook, observed behavior will converge to theory.

Furthermore, relevant facts have not yet been taken into account in the performativity discussions. For instance, traders Haug and Taleb (2011) have argued that historical evidence of trading methods suggest that traders do not use Black–Scholes. Similarly, quoting extensively from Niederhoffer (1997), Phoa et al. (2007) detail how traders use an array of techniques to help decision-making, which is best compared with a mechanical engineer using astrology and necromancy alongside

Newtonian physics. Potentially more interesting than performativity may be, therefore, the observation that practitioners often use patently contradictory financial theories at the same time.

Use of the methods from analytic philosophy would allow us to define and study performativity with greater conceptual, and perhaps formal, precision. This would not just help us to better understand the logic of the Black–Scholes model but also allow us to assess the plausibility of claims for performativity or self-fulfillingness in other branches of finance, including agency theory and corporate governance (Marti and Gond 2018), as well as the EMH and the CAPM.

2.4. Benchmarks for Evaluation

Our finance-as-a-practice approach may suggest that concepts, theories, and models are used for a much larger variety of purposes than in other branches of economics. Philosophers have been concerned with the question of whether models are explanatory (Jhun, Chapter 23; Verreault-Julien, Chapter 22). In finance, these models may, however, play many different roles. Hindriks (2008, 2013) considers the 1958 Modigliani and Miller model, an early result in finance that is the topic of our case study. Hindriks uses this model to introduce what he calls "explanation by relaxation." The model states that the way a corporation is financed (that is, the proportion of debt versus equity) does not affect its market value. This was a highly surprising and decidedly unrealistic result, given that debt was long considered the preferable way to fund a firm, for instance, because it suggests greater trustworthiness and stability on the part of the firm's management. The validity of the result depends, however, on the assumption that there is no taxation. Hindriks therefore argues that the function of the model is rather to explain how taxes might matter: subsequent results did include differential taxation of stocks and bonds (relaxing the assumptions of the original model), and this shows how taxes matter.

But our finance-as-a-practice approach opens our eyes to another, complementary way in which these models are used in the finance industry: as a benchmark or normative guideline. Here is the idea. When a market fails to satisfy the conditions of the EMH, regulators may conceive of that as not so much the failure of theory but rather as a failure of the market. This is not a "saving the phenomena" approach where theory is maintained coûte que coûte. Rather, failure may have a moral or political connotation, such as when an inefficient market is seen as one in which certain participants are offered "unjust" opportunities for arbitration because of informational asymmetries. Or it may have a more instrumental type of connotation: the financial industry has, for instance, witnessed an array of technologies that make information provision increasingly smooth (thereby making the market increasingly efficient), ranging from the semaphore (really!) and the ticker tape over telegraph lines to the internet and high-frequency trading. The EMH here is a benchmark against which to evaluate information and communication opportunities. We can witness a similar benchmarking use of the CAPM (or more complex variants thereof). Such models underlie, for instance, arguments to adjudicate claims about the performance of fund managers. This is because the sole fact that a fund generated high returns does not mean it was managed well (just as losses do not mean it was managed badly), and an assessment should also take into account the level of risk the manager took. That is where the CAPM and its ilk come into play.

2.5. Ideology and Science

Some authors associate the EMH (or modern finance altogether) with a pro-business ideology of deregulation. Indeed, as we have already noted, Merrill Lynch contributed substantial funding to the development of financial economics at the University of Chicago in the 1960s. And in the 1940s, William Mellon had already offered an endowment to Carnegie Tech to establish a business school that would, more indirectly, play a role in shaping modern finance. Also, some forms of deregulation

are more easily defended using modern finance than old finance, and some representatives of modern finance do indeed favor deregulation.

However, to begin with, we should caution against what seems quite popular in some corners: to associate modern finance with regressive views of justice. Perhaps the biggest progressive development in finance, index funds as developed by Jack Bogle's Vanguard, can be seen as having contributed considerably to making financial services available to people with low or modest income and wealth. Yet, it is exactly this innovation that owes its existence to the EMH; the logic of index funds is that if markets are efficient, there is no hope for active investors to achieve higher risk-adjusted returns. Consequently, individuals would be wasting their money if they paid someone to manage their funds.

More fundamentally perhaps, it is unclear what the direction of causation is supposed to be between ideology and theory. Someone believing the EMH on the basis that it supports their ideology would be forming beliefs in a nonevidentialist way. Work on nonevidentialism or pragmatism in epistemology would help to elucidate this. Someone embracing a deregulation ideology solely on the basis of the EMH would, on the other hand, commit a naturalistic fallacy, or more plausibly their argument for deregulation would have to be construed as enthymematic, as it leaves unspecified the normative ideal that they think deregulation would help to realize.

Conceptual clarification is dearly needed here. We suggest the following as a default starting position: it is plausible to assume that the ultimate normative ideal that drives arguments for (or against) deregulation is that of a fair and cost-effective distribution of financial resources. Keep this normative ideal constant, and then see how policy recommendations change when theory changes. If one's empirical theory is 1960s modern finance, the advice may well be deregulation. Equally, if one's empirical theory is 21st-century behavioral finance, it may well be something like liberal paternalism. This does not mean, however, that modern finance and behavioral finance subscribe to or entail different ideologies; it would be equally odd to attribute different ideologies to 1960s and 21st-century food science, even though nutrition advice has radically changed in the last 50 years.

While we realize that we have only scratched the surface here and that our choice of topics is necessarily arbitrary, we hope that we have shown that finance and financial economics are interesting territory for philosophers to explore. It is now time to move to our case study.

3. Modigliani–Miller: Models and Epistemic Import

In 1958, Franco Modigliani and Merton Miller published the first of a series of papers in which they introduced a number of propositions about the cost of capital for firms, their dividend policy, and their debt/equity ratio. They demonstrated that the decisions of a firm about how to fund its activities, be it by issuing debt or by issuing equity, are irrelevant to the firm's market valuation (Modigliani and Miller 1958). The choice has no effect on the market's valuation of the firm. Their argument relied on the concept of arbitrage by noting that, if the value of the firm depended on the debt/equity ratio, there would be arbitrage opportunities for investors: they would have a "free lunch." They would be able to increase their returns without incurring higher costs. This is an unsustainable equilibrium.

The propositions are striking for at least two reasons: first, because many of the assumptions under which the result holds are unrealistic. As we mentioned earlier, taxation is assumed to play no role, though it is clear that differential taxation schemes for debt and equity do matter. Second, at the time, the propositions seemed to go against everything that was known about how firms could fund their activities. Equity still had a poor reputation by dint of the market crash of 1929 and the subsequent Great Depression: in contrast to debtholders, in the case of bankruptcy stockholders are not paid. Debt was therefore considered safer than equity. And yet, too much debt would make a corporation look risky in the eyes of potential investors and creditors. The intuition was therefore

that there had to be an optimum balance between the two forms of acquiring capital. By contrast, Modigliani and Miller argued that any combination of the two would have no effect on the firm's market valuation. Their contribution became a cornerstone of corporate finance and asset pricing.

3.1. Explanation by Relaxation

This kind of result is naturally of much interest to philosophers. What is the use of a model with patently false assumptions and implications? And how could it become a cornerstone in financial economics?

Hindriks (2008, 2013) took a stab at offering an answer with this model and argued for a specific method of explanation, namely, explanation by relaxation (see also Jhun, Chapter 23). He maintains that the model can be explanatory despite the unrealisticness of its assumptions and implications. If some of the assumptions are relaxed (for instance, to include differential tax treatment of debt and equity), the result obtained by Modigliani and Miller fails to hold. Hindriks therefore claims that this is explanatory because it allows us to see that taxes do matter; they are a factor in preventing the Modigliani–Miller result from holding. By relaxing assumptions and obtaining results that are consistent with reality, we are able to explain that the factors in the assumptions being relaxed do play a role.

Hindriks's motivation appears to be the worry that, under Hausman's (1992) philosophical account of explanation, economics models that are too idealized, such as Modigliani and Miller's, seem not to have any explanatory value. Hindriks considers it "implausible that, by their own lights, most of the models [economists] have proposed cannot even be used as the point of departure for potential explanations" (Ibid. 2013: 525). He seems to think that, if models cannot be the starting point of an explanation, then there is no use for them. With his method of explanation by relaxation, the Modigliani–Miller result is vindicated: it explains by relaxing assumptions.

A great deal of the philosophical literature on economic models has indeed focused on the explanatoriness of models. It has addressed models' capacity to make us learn (Claveau and Vergara-Fernández 2015; Grüne-Yanoff 2009), to explain (Aydinonat 2018; Reiss 2012), to yield understanding (Verreault-Julien 2017; Ylikoski and Aydinonat 2014), and to identify robust causal mechanisms to understand economic phenomena (Kuorikoski et al. 2010). But model building is not justified for its epistemic virtues alone. Indeed, some authors are skeptical of the epistemic benefits of models (Alexandrova and Northcott 2013). Some argue that models are only capable of serving a heuristic role as providing open formulae (Alexandrova 2008): mere causal hypotheses that have yet to be empirically verified in order to be used in explanations. Others have argued that they allow conceptual exploration (Hausman 1992).

3.2. The Search for Consistency

Rather than attempt to settle the matter here as to what the epistemic benefits of models are, we want to highlight that models may have other important purposes besides the strictly epistemic ones. Notwithstanding potential epistemic interests, the relevance of Modigliani and Miller's (1958) model is related to the values that motivated the contribution in the first place. The assessment of models through the lens of the values that motivate them offers important insights that would be overlooked if the focus remained on the actual epistemic payoff of models. There are two values in particular that help us understand the relevance the Modigliani–Miller model had for financial economics.

First, there was the motivation to bring consistency to the microeconomic and macroeconomic treatments of the problem of firm valuation. Indeed, the contribution of Modigliani and Miller was part of a long-term project that encompassed the question of the cost of capital and the implications for the firm's decision-making process and aggregate investment. It began in 1949 as the

Expectations and Business Fluctuations project, which Modigliani was asked to supervise upon joining the faculty of the University of Illinois. The research involved empirical and theoretical work on business expectations and their influence on aggregate economic behavior (Rancan 2020: Chapter 4). Modigliani and Miller's treatment of the problem would allow further theoretical developments in rational investment and financial policy under uncertainty.

The question Modigliani and Miller (1958) pose is the following: what is the cost of capital for a firm when the income stream of a particular investment is uncertain? They take particular issue with the way in which the problem had been treated by economic theorists. The traditional assumption had been that the cost of capital of a firm, regardless of whether it is financed through debt or equity, is simply the riskless rate of interest, as if it yielded sure streams. The question of how risk affected the cost of capital had been eschewed or treated in an ad hoc way, namely, by superimposing a risk discount on the results under sure streams. And while Modigliani and Miller admit that the ad hoc way had been helpful in addressing "the grosser aspects of capital accumulation and economic fluctuations" (Ibid. 1958: 262), in which Modigliani in particular was interested, they did claim that "at the macroeconomic level there are ample grounds for doubting that the rate of interest has as large and as direct an influence on the rate of investment as this analysis would lead us to believe" (Ibid.: 262–263). In the Keynesian model, on which Modigliani had worked, aggregate investment is written as a function of the riskless rate of interest. This same rate of interest appears, too, in the liquidity preference equation.

As a way to incorporate the uncertainty, their proposal was to tackle the question by finding a rational investment policy that maximizes the market value of the firm instead of one that maximizes profits. The problem, however, was that no theory of the effect of financial structure on market valuations was available, nor could these effects be inferred from available data (Ibid. 1958: 264). Prior to writing the article, Modigliani and Miller had dealt with the question from different angles: Modigliani from a purely analytical perspective and Miller from an empirical one, unsuccessfully trying to establish a correlation between debt/stock ratio and firm value.

Ultimately, more than giving an explanation of the way in which actual firms were funded in the 1950s, their result was meant to demonstrate that a problem that was generally seen to be solved "within" the firm (by the financial specialist and perhaps the managerial economist) with the use of only old ad hoc finance machinery, could be answered using the tools of the economic theorist.

3.3. Settling a Debate

To establish consistency between the micro and macro treatments is related to another value that seems to have driven the contribution of the two economists: the intent to settle a methodological debate that was taking place within Carnegie Tech, the institution with which they were affiliated. As mentioned earlier, in 1948, William Mellon offered a $6 million endowment to Carnegie Tech to establish the Graduate School of Industrial Administration (GSIA). The new leaders of the school, who included Herbert Simon, saw US business education at the time "as a wasteland of vocationalism that needed to be transformed into science-based professionalism" (Simon 1996: 139). Finance was mostly descriptive of financial instruments and institutions. The "chief book of finance," *The Financial Policy of Corporations*, first published in 1919 (Sewing 1919), focused on describing the workings of corporations and regulatory schemes, often with many historical asides, and had no mathematics beyond simple arithmetic. The GSIA aimed to establish a modern management science on the basis of a strong interdisciplinary and empirical approach to the study of firms' behavior (Rancan 2020: Chapter 4). The hiring of Modigliani and Miller at Carnegie was part of that interdisciplinary effort.

But there was resistance. Traditional scholars, in particular, whose approach to corporate finance was mostly description of the institutional setup were defiant. David Durand, for instance,

a well-known and respected representative of old finance, took issue with Modigliani and Miller because he thought that, although the arbitrage mechanism was at work, the real-world institutions posed restrictions for investors, which would not allow such arbitrage (MacKenzie 2006: Chapter 2). Scholars such as Durand were among the first to complain about modern finance lacking realisticness. Modigliani and Miller were thus keen to show that the "science-based professionalism" that Simon had in mind was to be found in modern economic theory.

The point Modigliani and Miller (1958) were precisely trying to make was the following: they were interested in arguing that an understanding of capital structure required an analytical framework that would allow an understanding of firm decisions as part of the larger economic setup. The analytical framework of microeconomics in terms of arbitrage and equilibrium suggested that the question of firm funding "was not really an issue."

> The paper was meant to upset my colleagues in finance by arguing that the core issue that received most attention in corporation finance, namely finding out what exactly is the optimum capital structure, was not really an issue. It didn't make any difference. To be sure, it might make a difference if there were taxes. If so, you would have to approach the problem precisely in that way and ask what is the effect of taxes and why do they make a difference.
>
> (Modigliani 2009: 126)

3.4. Epistemic and Non-epistemic Values

We read Modigliani's quote as underscoring our view that it can be useful to be sensitive to the fact that models do not necessarily have only epistemic goals. In particular, the relevance of a model might be better assessed by noting the values that drive its building in the first place. Previously, we argued that there were two values at play: an epistemic one, to establish consistency between theoretical frameworks, and a non-epistemic one, to settle an institutional battle within the GSIA to establish the methodological framework that would give its much sought-after scientific status.

Some might want to argue that the motivation of Modigliani and Miller was methodological and consequently still epistemic, namely, to demonstrate the superiority of the modern finance framework over old finance. But, if the ultimate purpose was to win an institutional battle at Carnegie Tech, does it still count as epistemic? We think the answer is not obviously affirmative. Some might also be tempted to dismiss Modigliani's reminiscence as spurious, subtracting plausibility from our claim. But our claim is not historical. It is sufficient for us to entertain his remark as plausible in order to suggest that the very motivations of model building need not always be purely epistemic.

Furthermore, similar concerns can be found elsewhere in economics. Robert Aumann's (1985) defense of the expected utility approach against Herbert Simon's (1947) satisficing, for instance, may also be amplified by strategic, non-epistemic reasons behind the model. The role of such reasons has been acknowledged in the literature on values in science (e.g., Diekmann and Peterson 2013; Douglas 2000, 2009; Baujard, Chapter 15; Reiss, Chapter 16). An apt metaphor employed by Elliott (2017) is that values "weave" themselves into scientific practice like the weaving of a tapestry, for instance, when it comes to choosing research topics and questions or the goals of scientific inquiry. Consideration of these aspects allows us to understand better why Modigliani and Miller's model in particular, as well as the others that followed, proved to be so influential in the discipline. A purely epistemic characterization in terms of the explanatoriness of the model leaves much unaccounted for.

More generally, an understanding of not only the epistemic contributions models make but also the values that drive their building offers us a more comprehensive understanding of the modeling

practice. Such an understanding allows for a better potential dialogue between philosophers and practitioners and a better assessment of the epistemic risks involved in doing science (e.g., Douglas 2011; Elliott and Richards 2017), and it contributes to the political philosophy of science urged by Douglas (2018).

4. Conclusion

In this chapter, we have made an attempt to highlight some features of financial economics that are of potential interest to philosophers of science. We have argued that the traditional key elements of finance (the efficient market hypothesis, the capital asset pricing model, and the Black–Scholes model) merit greater philosophical attention. They give rise to issues such as performativity and underdetermination that go to the core of the traditional philosophy of science. They also call for addressing questions that have to do with the non-epistemic value of models and the political or ideological values inherent in science, which have occupied philosophers more recently.

Perhaps some readers wish we had added other items to our survey. Others may have found our case study too quick to be immediately plausible. We are keenly aware of these and other potential limitations. Our ambition with this chapter was precisely to kindle interest in finance and financial economics from philosophers of science, hoping that the issues we treated here will be revisited.

Related Chapters

Baujard, A., Chapter 15 "Values in Welfare Economics"
Jhun, J., Chapter 23 "Modeling the Possible to Modeling the Actual"
Karpus, J., and Radzvilas, M., Chapter 7 "Game Theory and Rational Reasoning"
Reiss, J., Chapter 16 "Measurement and Value Judgments"
Stefánsson, H.O., Chapter 3 "The Economics and Philosophy of Risk"
Verreault-Julien, P., Chapter 22 "Explanation in Economics"

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