Theorizing Efficient Markets: A Sociology of Financial Ideas

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Abstract

In this paper, I use the case of financial economics to show how an innovative idea can shape a research agenda. I focus on why the efficient-market hypothesis, crystallized in Eugene Fama’s research, acquired core theoretical status in the discipline of financial economics, whereas the Capital Asset Pricing Model, championed by, among others, Fischer Black, did not. I draw attention both to differences in the networks propagating these models, in particular differences in cohesion and coherence, and to differences in the methodologies underlying the models. I argue that Fama’s use of “data-dredging” techniques and frequentist statistics increased the coherence of the intellectual circle around him, turning the analysis of efficiency into a collective project oriented towards the discovery of objective properties. By contrast, Black’s adoption of more subjectivist methods exacerbated the individualistic tendencies of his approach and his network, increasing the incoherence of his research group.

Keywords: Financial economics; Cultural production; Social networks.

Many regard financial economics as one of the most innovative social scientific fields of the post-WWII period, and the efficient-market hypothesis (EMH) ranks possibly among the field’s most notable—and controversial—achievements (Bernstein 1992, 2009; Spotton and Rowley 1998). A new guard of theorists, eager to subvert the status quo of the discipline (Jovanovic 2008; Mehrling 2005; Poitras and Jovanovic 2010), embraced EMH soon after Kendall and Samuelson, in the late 1950s/early 1960s, gave it a first formulation: among them, Eugene Fama is the most renowned. Led by Fama, financial economists used EMH to build a relationship with the larger field of economics on more equal foundations, freeing their discipline from an earlier association with practical concerns and vocational training that undermined its seriousness as an academic discipline (Fourcade and Khurana 2013; Whitley 1986).
That the theory of efficiency gave scientific legitimacy to the field of financial economics may seem a point of historical interest only, given that the recent rise to prominence of behavioral economics, and the 2008 financial crisis, have cast serious doubts on the rationality of financial markets. But EMH continues to occupy a special place in the development of finance (Dimson and Mussavian 1998). The 2013 Nobel Prize Committee for economics, for one, recognized as much by celebrating Eugene Fama, along with Robert Shiller and Lars Peter Hansen. It would not be an exaggeration to say that EMH is more than theory, and certainly more than mere hypothesis; it is a worldview, the “null hypothesis” against which the data are tested, and rarely the subject of rigorous economic testing itself (Fourcade 2009: 95). Therefore understanding how EMH came to prominence can yield important insights to a sociology of intellectual innovation.

The sociological significance of EMH can be better appreciated if one contrasts its fate with that of a second innovative idea discussed in several accounts of the revolutionary nature of financial economics: the Capital Asset Pricing Model, or CAPM (MacKenzie and Millo 2003; MacKenzie 2006; Mehrling 2005). CAPM provided a theoretical rationale for, and gave a mathematical formulation to, the long-held intuition that the riskiness of an asset influences its expected return. CAPM was thus a model of how to price assets in the face of uncertainty. Naturally, the practical implications of CAPM were far-reaching, so that “its theoretical significance is equaled if not surpassed by its widespread use in business and finance” (Bernstein 1992: 201). Perhaps more so than efficiency, CAPM constituted a central part of the framework “for managing the transition from traditional craft practice to the modern scientific methods that would transform investment management over the ensuing 30 years” (Mehrling 2005: 5).

Yet, paradoxically, spurred by dissatisfaction with its empirical shortcomings (Roll 1977), finance theorists grew more aware over time of the theoretical limitations of CAPM. Ultimately, they relegated CAPM to the status of one among many competing (and, allegedly, more empirically accurate) methods of portfolio selection. Market practitioners continue using it to make quick, back-of-the-envelope calculations; academic scholars, however, have largely abandoned it as a theoretical framework (but see Dempsey 2013). While recognized as a “centerpiece of modern financial economics”, CAPM is now mainly discussed in terms of its historical significance, practical implications and empirical shortcomings.
It is often the case with complex theories that flawed or incomplete data, or gaps in the auxiliary assumptions necessary to operationalize key constructs, can be blamed for failing to bear out their central predictions (MacKenzie 2006: 23). Rarely, then, are theories dismissed on empirical grounds alone. The puzzling aspect of CAPM's rise and decline therefore is that, in spite of CAPM never being falsified in any definitive manner independently of efficiency, the empirical burden was put on the former, not the latter, even prompting the futile protests of the model's most steadfast proponents—Fischer Black and Jack Treynor (Black 1993; Treynor 1987). In other words, CAPM's failure was ultimately epistemological: CAPM was not perceived as a theoretical alternative to efficiency; rather it became an auxiliary model to be discarded once its predictions clashed with EMH. As such, CAPM was sacrificed on the altar of EMH.

What accounts for the contrasting fortunes of these two models? Given social scientists' increasingly sophisticated understanding of the role institutions play in producing economic knowledge, and specifically the way institutions turn certain cultural understandings of the economy into taken-for-granted facts, one way to answer this question would be to focus on broad processes, such as the political and ideological context of the period in which these ideas were generated (Fourcade and Khurana 2013; Fourcade 2009; Horn, Mirowski and Stapleford 2011; Mirowski 1991; Whitley 1986). Others would invite emphasis on the role financial markets play in the diffusion of each innovation (MacKenzie and Millo 2003; MacKenzie, Muniesa and Siu 2007). But while an understanding of the context in which financial economics developed in the second half of the twentieth century is crucial, it cannot explain why two ideas like EMH and CAPM that are so intimately connected to each other, developed so differently. Similarly, without a comparative analysis of the practices through which each idea acquired intellectual prestige and resonance, their different epistemological status remains unexplained (Collins 1998; Frickel and Gross 2005). Attending to this question, I submit, requires investigation of the socio-cultural processes and practices through which economic knowledge is codified and disseminated.

I focus on two main dynamics. First, drawing from new work in the sociology of ideas and intellectuals, I suggest, and show empirically, that the failure of CAPM was a network failure: CAPM's proponents did not create a genealogy of innovators (Collins and Guillén 2012; Collins 1998; Frickel and Gross 2005). Second, I submit that EMH
became the banner of a coherent group, whereas CAPM did not, because of the different cultural frameworks built around each model (Bortolini 2012), a central component of each being different “modes of statistical analysis” (Camic and Xie 1994: 77; Hacking 1984). That is, Fama and his collaborators used standard statistical methods conveying “mechanical objectivity” (Porter 1996) to frame efficiency as real, and discoverable. By contrast, Black and (some of) his colleagues understood CAPM as an ideal state of the world, not yet fully come into existence, but one that scholars could nevertheless theorize about using quantitative analysis as an “engine of discovery”. Through statistics, scholars also hoped to find ways to make the world more congruent with CAPM (Lehmann 2004; MacKenzie 2006; Porter 1996). But this methodology and research approach only exacerbated the incoherence of the CAPM network: it encouraged an individualistic intellectual style that hindered the creation of a research group sufficiently invested in CAPM to defend it from criticism (Parker and Hackett 2012).

I begin with two brief sketches of EMH and CAPM, highlighting their main features as well as commenting on their reception. I then present network citation and prestige data to characterize more precisely the intellectual context in which the two ideas developed. In a subsequent section, I present my second argument about the cultural framing of the ideas. Finally, as I lay out what implications this analysis has for the study of economic culture in my conclusions, I suggest one explanation for the difference in intellectual style between CAPM and EMH: CAPM scholars looked to the market as a source of knowledge; EMH analysis, by contrast, looked to academia. Therefore, in the vein of micro- and meso-level theorizing of economic culture (Bandelj 2012; Spillman and Strand 2013; Spillman 2012; Wherry 2014; Zelizer 2010), I call for increased attention to the types of boundaries producers of economic culture attempt to erect (Gieryn 1983; Lamont 2009), and specifically how they negotiate the boundary between the academic system and the market.

*Efficiency: from hypothesis to theory*

Though its roots are in philosophy, the concept of efficiency has been central to economics for quite some time. In its traditional connotation, efficiency refers primarily to issues of performance measurement (Alexander 2008; Dimson and Mussavian 1998).
But when it is imported into financial economics, efficiency takes a different and more specific meaning, which highlights a property of financial markets: namely, that they incorporate all relevant information into prices. Efficiency, that is, describes markets as perfect (or tending towards perfection) in processing information (Fama 1970, 1991).

The hypothesis that financial markets are informationally efficient, in turn, builds on the discovery of the statistical property of asset prices that prices are independent over time: the price of a stock is serially independent, meaning that it fluctuates “randomly” and therefore impedes systematic prediction of its future values (Bernstein 1992; Fama 1965a). Pioneering studies of the Parisian stock market in the late 1800s first argued for the existence of this “random walk” property, but efficiency crystallized into a theory with scientific aspirations only in the 1950s, and then took off in the wake of a major technological innovation: the compilation of computer-readable datasets recording stock market prices over a relatively long period of time, an innovation housed at Chicago in the new Center for Research in Security Prices (MacKenzie 2006). Analysis of these datasets produced a foundational study of stock market returns (Fisher and Lorie 1968), which provided empirical evidence of stocks commanding a higher premium than bonds. This finding paved the way for the re-legitimation of stock markets, at a time when the memory of the 1929 crash was still vivid. Similar studies by Fama (1965b) and Jensen (1968) further crystallized the view of financial markets as dynamic, innovative, and desirable.

To be sure, interest in random walks had not been confined to Chicago. Over at MIT, Samuelson had retraced some of the historical literature in order to understand the economic implications of random walk models. Many consider Samuelson the true progenitor of this approach (Bodie, Kane and Marcus 2011). In 1964, Paul Cootner, also at MIT, assembled a reader on random walks, with the similar goal of teasing out the economic aspects of what was initially formulated as a statistical discovery (Cootner 1964). In both cases, the assumption grounding their analysis, however, was that markets could be theorized as efficient, but that in practice they were not: for all his enthusiasm about random walks, Cootner (1962: 25, in Jovanovic 2008) thus concluded that “the stock market is not a random walk […] I have enough evidence to demonstrate the nature of the imperfections”.

With Eugene Fama, however, efficiency and random walks become inextricably tied. His main claim that stock price movements could
not be predicted was based on the idea that prices already reflect all available information. But if only new information affects stock prices, to the extent that such information emerges randomly, changes in stock prices are random too. Statistical independence can therefore be “endowed with implicit economic content” (Mehrling 2005: 63).

Fama’s success in turning EMH into an intellectual innovation coupling random walk and efficiency was not just the result of his technical prowess, and his ability to draw from new datasets: his timing and connections were perhaps as important. Fama’s thesis on stock price behavior was published in its entirety, something Fama himself did not expect—but the intellectual environment was ripe, which made Fama’s work the subject of great interest on the part of the financial community (Bernstein 1992). Second, Fama followed up by publishing an extensive (but partial) review of the literature, proposing a classification between different degrees of efficiency that forever moved the debate away from dichotomy (efficient/non efficient) to a matter of degree (how efficient) (Mehrling 2005: 91).

Third, the ideological fit between the Chicago School and Fama’s approach is important too. Fama was a product of a newly professionalized business school, which benefited from close proximity to the Chicago economics department, and thus was the context for important intellectual exchange as well as resource sharing (Overtveldt 2009; Samuels, Biddle and Emmett 2008). To the Chicago School, efficiency was a foundational assumption not to be disputed (Fourcade 2009). However, ideology alone is not sufficient in securing the success of a theory, as the case of CAPM illustrates.

CAPM: from model to empirical approach

To this day, many consider CAPM one of the most revolutionary ideas in finance, but not in the same sense as efficiency. CAPM is at the roots of foundational concepts in investment theory, such as the distinction between “unsystematic” and “systematic” risk, the former a risk that is specific to the asset and thus possible to control through diversification, the latter describing how the asset price varies as the market fluctuates, as captured by measures such as “beta” and the volatility of stocks (Dempsey 2013; Smith and Walsh 2013). Moreover, from the viewpoint of CAPM, the price of risk always reflects market equilibrium: prices indicate the propensity of certain investors
to take on more risk than others, thereby commanding a higher premium than would be the case with safer portfolios. Risk, in this view, is a core aspect of financial behavior, not an aberration to be corrected (Mehrling 2005).

Unlike efficiency, however, CAPM does not occupy a core place in the financial canon. Or, put more precisely, it does not constitute the basis for a research program in the same way that EMH does. Partly, this is because Fama invented a research tradition in which to couch EMH, making it appear as the uncontroversial result of a long stream of analysis (Jovanovic 2008). By contrast, CAPM originated with several independent efforts addressing different kinds of problems, and it took quite some time before analysts even understood that they were working on a similar problem. Jack Treynor is credited with developing the essential insight behind CAPM in 1962, in the context of an attempt to value a risky stream of future earnings, where he understood risk to derive from some underlying economic variables (Mehrling 2005: 57). But that the result of this investigation was a version of CAPM only became clear to economists after Harry Markowitz, William Sharpe and Merton Miller (all central figures in financial economics) shared a Nobel prize in the 1990s, prompting the field to reflect back upon the origins of the model (French 2003). It was initially Sharpe and John Lintner who were credited with the discovery of CAPM, and neither of their models had fully worked out the relationship between risk and equilibrium (Fama 1973).

With so many scholars working on the same model to begin with, it is perhaps not surprising that few followed Fischer Black’s lead. Yet Black would become the most tireless advocate of CAPM, and remain so even when other theorists had moved on (Lehmann 2004: 9). The main contribution of Fischer Black, a colleague of Jack Treynor at the consulting firm A.D. Little in Cambridge, was to develop the original intuition behind CAPM into a full-fledged worldview, in which financial markets play the central role of pricing risk, and thereby faithfully mirror how much risk a society is willing to tolerate. Black, then, saw CAPM as a model of how financial markets in equilibrium shape the economic world. In his eyes, CAPM naturally implied that only unfettered financial markets can generate a proper balance between risk and safety, for financial markets are the source of interest rates that match demand for, and supply of, risky assets. Unlike Sharpe and Lintner, Treynor and Black thus considered equilibrium between risk and returns as a matter of philosophical importance, not a matter of mere technicality (Derman 2004; Lehmann 2004).
It is important to note that equilibrium in the relationship between risk and return is consistent with the idea of efficient markets, and the two are often discussed together (Dempsey 2013). But CAPM, because it is a theory of risk, can also accommodate financial processes in inefficient markets. EMH, because it is a theory of information, is by like token not committed to any specific model of stock price evaluation: it can be consistent with CAPM, positing price to result from a balance between risk and return; but it is consistent with other models too, introducing factors beyond the risk-return equilibrium to explain stock prices—something Fama recognized as early as 1973 (see in particular Fama and MacBeth 1973).

Whether financial markets are defined by an equilibrium between risk and return, in fact, cannot be empirically tested (Roll 1977). But lack of empirical realism alone did not discredit EMH. In both cases, empirical disconfirmation could always be attributed (and indeed, was invariably attributed) to flawed or incomplete data, or to the improper operationalization of key constructs. Further, empirical tests of EMH were often jointly testing CAPM: there was no a priori reason why the tests should disconfirm the former rather than the latter (MacKenzie 2006: 23). Nevertheless, it was CAPM, and its assumption of equilibrium between risk and return, that was ultimately abandoned. The model itself came to be seen less as a worldview on which to reground financial economics, than a useful and rich approach that more empirically accurate models were bound to supersede. In fact, the formula with which Fischer Black’s name is most iconically associated, the Black-Scholes option pricing formula, relied on no asset pricing theory at all. Instead, it relied on a no-arbitrage assumption coming from a different theoretical lineage, provided by Robert C. Merton, who would later share a Nobel prize with Myron Scholes on this account (Lehmann 2004). Awareness of CAPM’s empirical inadequacies hindered the acceptance of work that depended upon it; it was therefore preferable to build option theory on a different foundation.

The fate of CAPM was sealed by empirical attacks carried out by Fama himself. In joint papers, he and French (1992, 1993) published a famous alternative that, instead of restricting its focus to risk and return as the two essential parameters of stock market valuation, introduced additional factors. Once these factors were taken into account, Fama argued that the augmented model provided a better fit to the data and thus “proved” market efficiency. In spite of Black’s and Treynor’s protestations, “beta” was declared dead (Black 1998).
Fischer Black stuck to his intuition, but from then on he was virtually alone in applying a CAPM logic to such disparate phenomena as money and banking, currency exchange rates, and of course options and warrants (Mehrling 2005), while others went on to develop more sophisticated models of efficient portfolios (and specifically, arbitrage portfolio theory).

If lack of empirical fit alone cannot explain why CAPM’s life cycle turned out to be shorter, and why its implications were not worked out in subsequent models in the radical way advocated by Black and Treynor, whereas EMH thrived, what does? It may be suggested that EMH’s longevity derives from its successful application in financial markets: by finding just in what ways markets were efficient, and in what ways they were not, EMH research often opened up profit opportunities for practitioners to exploit (MacKenzie 2006: 94-97). Further, Fama’s work became the basis for, among other things, fair value and rational pricing. But Fischer Black, in his early work with Myron Scholes, similarly suggested how CAPM pointed to specific profit opportunities (what he called the “alpha effect”). Nevertheless, financial market agents refused to implement his investment strategy. Academics similarly questioned whether the effect he described was plausible (Mehrling 2005: 108). I submit that understanding what made Fama’s financial strategy seem worth pursuing, while making Black’s appear less valuable, requires an analysis of both the social networks in which their research was embedded, and the cultural frames on which their respective models were built.

**EMH and CAPM: a network analysis**


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1 I used Neal Caren’s publicly available Python code to construct the dataset, and the open-source software GEPHI to produce the network graphs. Citation data is drawn from Web of Science.
layout of the network meaningful I implement a community detection algorithm (the Louvain algorithm) that spatially groups together authors depending on who is citing whom.

The graph reveals important aspects of the financial economics literature, an extended discussion of which would exceed the scope of this paper. For our purposes, I would first like to highlight the two papers that have some of the highest centrality scores (reflected in the label’s larger size) in Figure 1: Fama 1973 and Black and Scholes.
They both sit at the intersection of two research communities, and their high centrality indicates that they are “obligatory points of passage” for subsequent work (Latour 1987; Yonay 1998). Note, then, how the citation networks evolve around these papers over time.

Both Black and Fama continue publishing a great deal, but, in Black’s case, his subsequent work fails to meet the citation threshold that would make it visible in the citation networks. In contrast, Fama succeeds again with his 1993 paper with French that debunks CAPM. Eugene Fama, put differently, shapes the direction of research and the character of the debate in financial economics over a longer period of
time than does Fischer Black; this, moreover, in the wake of Fama’s falsification of CAPM, and thus at Black’s expense. Black’s subsequent work did not pursue his research agenda and, as a consequence, it did not generate a coherent research program, as none of his subsequent papers acquired as much prominence in the network as Black-Scholes. Put differently, Black-Scholes does become canonical, but it is Fama who successfully seizes the attention space of the discipline with his empirical debunking of CAPM. Black-Scholes is recognized as a model of option pricing, not as one piece within a larger theoretical
framework provided by Black’s vision of finance as the analysis of equilibrium between risk and return.

Citation networks do not differentiate between networks where citations are in agreement with the source paper, and citations that use sources as foils for critique. The picture gains more focus if we shift attention to networks of prestige, as recently suggested by Collins and Guillén (2012) as a part of a larger model of “mutual halos”. Mutual halos capture the degree to which prestigious innovators are linked to other prestigious innovators, thereby highlighting how innovative ideas emerge in the context of dense networks endowed with creative
energy, as opposed to being the product of lone innovators (contra Burt 2004). The prestige of an intellectual/innovator can be measured in three steps: first, as the amount of attention he/she is given in the relevant literature, which indicates how successful the innovation is. Second, through the same method one can measure the amount of attention the intellectual/innovator’s immediate masters, disciples, and collaborators enjoy, thus highlighting the importance of their research programs among contemporaries. Third, one can measure the cumulative prestige enjoyed by the network two links away from the focal intellectual/innovator, which provides an indication of how diffuse the prestige of the innovator is in the time-span of roughly a generation. (Beyond two links, it becomes more difficult to distinguish between rival research programs/schools of thought.) These extended measures of prestige reveal the degree to which the successful innovator is embedded in successful networks.

Focusing on the Top 20 theorists, I show in Table 1 that both Fama and Black are part of the financial canon, in that discussion of their work receives a significant amount of exposure in a variety of venues (see the note to the table for details on sources). They both belong to the Top 5 financial theorists by all measures of prestige. Black’s prestige is canonical and self-reinforcing: his work is recognized in the field, and Black is connected to other prestigious innovators, both in his immediate network, and in the larger, 2-link network. Similarly, Fama’s prestige is tied to, and enhanced by, a lineage of subsequent innovators, coupling Fama’s focus on the efficiency of markets with a market-model of portfolio evaluation (Arbitrage Portfolio Theory).

While Black and Fama enjoy high recognition in the field, differences between the two lurk just beneath the surface, and can be appreciated by comparing the mutual halo rankings with the network citation data discussed above, and paying attention to chronology. Fama’s work constitutes the foundation for the work of other prestigious theorists, with an important generational component to this: younger analysts who could have pursued CAPM instead chose to deepen Fama’s hypothesis by providing alternative methods of asset pricing (thus effectively striving to replace Black’s model). By like token, Fama’s direct competitors (theorists like Grossman and Shleifer who are critical of EMH and make that critique central to their work), while they are certainly prestigious, do not rank as high as his disciples and followers. Fama’s lineage, that is, is strong even in the face of important critiques; Black’s network is not. The one,
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<td>Ross</td>
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potentially major source of prestige in Fischer Black’s network, Jack Treynor, is barely mentioned in the financial economics literature (his prestige score ranks in the bottom tenth percentile). Tellingly, Perry Mehrling’s masterful biography of Fischer Black starts with a vignette illustrating how much the field undervalued Jack Treynor’s contribution.

Network data, in short, help us delineate two points: first, that the group of finance theorists who develop Fama’s approach to financial markets is a coherent group: a “small, tightly woven [group] of researchers oriented toward common intellectual goals who work together in opposition to current intellectual trends” (Parker and Hackett 2012: 22)—in this case, popular and professional perceptions of markets as inefficient, and at least initially, mainstream economics’ refusal to recognize financial economics as a legitimate intellectual discipline. The group pushes forward Fama’s agenda while putting it in a collision course with CAPM.

Second, Fama’s work creates controversy but is not displaced by it: Fama’s network occupies a durable position in the field, which reproduces its relevance to intellectual debate. Fischer Black’s work, by contrast, sits at the intersection of several debates, and while it produces a theoretical controversy of its own, as evidenced by Fama and French’s work in the 1990s, scholars who are inspired by Black’s approach are more likely to attempt to transcend it empirically than to use it as a theoretical building block. In fact, Black’s network is notable for its failure to produce such a common orientation towards finance, in spite of Black’s efforts. It is not that other financial economists do not pursue his insights. But they do so in a different, and more theoretically modest way than Black intended.

The cultural and practical effects of statistics

We could attribute Black’s relative failure at impacting his discipline to biographical trajectories, both his and those of the members of his network. Jack Treynor, for instance, formulated his version of CAPM while employed at A.D. Little, a consulting firm; he then went on to work for Goldman Sachs. As a result, he never had the opportunity of producing students. John Lintner, a more distant member of Black’s circle, taught at Harvard Business School, which put him at some distance from Chicago, the main
center of financial innovation in the 1960s and 1970s. And William Sharpe, one of the forefathers of CAPM, conceded that the model could not be shown to be valid, and therefore cannot be considered a propagator of the model (MacKenzie 2006: 93). Finally, Fischer Black himself had an erratic career, moving from Chicago to MIT and then to the private sector, both due to personal reasons, and to his inability to find sufficiently supportive colleagues. By contrast, Fama’s entire career takes place at Chicago, which gives him unequalled access to resources, both material and intellectual, as well as to students.

Yet, an explanation based on Black’s failure to establish a network due to his mobile career cannot account for the fact that Black also taught at Chicago but failed to build a coherent network around him while working there. This is not because of doubts about his intellectual abilities, or because of ideological differences: even though he often gave the impression that he did not hold consistent views on any topic (Mehrling 2005), Chicago colleagues respected him.

Black’s relationship with Chicago scholars, in fact, deepens the puzzle that CAPM’s focus on beta as a risk/premium coefficient was defeated through empirical work, whereas efficiency continues to be defended, not just on empirical but on normative grounds (Brav, Heaton and Rosenberg 2004). In fact, while in terms of theory Fama was as entrenched in the Chicago School approach as Black was, this was not the case from a methodological point of view, where Black and Chicago spoke the same language. Black understood empirical work as an “engine of discovery”, meant to make theory more “plausible”—where plausibility was defined as the property of a theory that does not “imply consistent, easy to exploit profit opportunities” (Black 1982: 32; Lehmann 2004). While there is much debate as to whether the Chicago School (and economics at large) holds a coherent view on methodology (Reay 2012), the idea that theory does not require fit with empirical data is a central tenet of Friedman’s approach. Black, therefore, was very much in agreement with this point.

Fama, by contrast, exemplified an approach to data (“data-dredging”) of which the Chicago School was very critical (see esp. Black 1982). It is not just that the validity of EMH, much like that of CAPM, could not be proven: Fama’s early and seminal statement on EMH conceded as much, and a theory’s lack of realism would not have raised eyebrows at Chicago (MacKenzie 2006: 95-96). Fama’s work, however, relied on econometric techniques in ways that put him at odds with the Chicago approach; Black’s, by contrast, did not.
To explain both puzzles—Black’s lack of students and collaborators at Chicago, and capm’s capitulation to empirical falsification—attention should be drawn to the social consequences of methods, rather than their empirical and ontological implications, or their fit with theory. In general, analysts of quantification have shown that fields in search of legitimacy resort to quantification to establish their credentials. Faced with skeptical audiences, and a low level of trust among colleagues, contested fields rely on quantification as a source of “mechanical objectivity” and impersonal knowledge, hiding ambiguities behind the seemingly objective quality of numbers (Collins 1984; Daston 1992; Espeland and Stevens 1998; Porter 1996).

The discipline of economics has perhaps profited the most from this framing strategy (Breslau 1997; Fourcade 2009; Yonay and Breslau 2006).

Quantification, however, is a capacious term, which contains several, potentially contrasting approaches or “modes”: a particularly important distinction can be made between frequentist and subjectivist methods. The former attempt to establish the context within which knowledge and expertise can be exercised, and demarcate that zone as outside the influence of other chains of events, thus buffering it from the intrusion of other kinds of expertise (Camic and Xie 1994; Collins 1984; Hacking 1984). At the other end of the continuum, subjectivist methods are captured by Friedman’s writings (Hammond 1990). For instance, in an important discussion, Friedman paraphrases Savage and states: “the role of statistics is not to discover truth. The role of statistical analysis is to lead us to reconsider our personal probabil- ities in the hope that our personal probabilities will come closer and closer together” (Hammond 1992: 167; see also McCloskey 1998).

Subjectivist methods, in spite of their quantitative credentials, present the same problems and contradictions commonly found in non-quantitative techniques: they require trust and tacit knowledge in order to become authoritative. Thus, it is not surprising to find appeals to subjectivist methods in the most cohesive approach to economics, the Chicago School, whereas, in less elite parts of the discipline, methods tend to appeal to objectivity in order to bolster up their power to persuade (Reay 2012).

Focusing on the relationship between trust and methods helps us shed light on the EMH-CAPM controversy. Fama’s use of frequentist statistics, in order to produce a test of efficiency that falsified capm, implied that financial phenomena were real and objective, independently
of the analyst’s interpretation. The implications were not only ontological, but practical as well. Within this framework, seemingly intractable problems could be swept to the side. For instance, as noted by Mehrling (2005) and MacKenzie (2006), it was initially unclear whether random walk models could be easily incorporated into the emerging financial canon. Benoit Mandelbrot, who worked on the research frontier in this area, was calling for the complete abandonment of Gaussian models, leading Paul Cootner to compare him to Churchill: “Mandelbrot […] promises us not utopia but blood, sweat, toil, and tears” (Cootner 1964: 337).

But Fama was undeterred by such fatalistic scenarios. He ignored some of the unsettling implications brought by the hypothesis that the distribution of stock prices was non-normal. Thus Merhling (2005: 90) writes: Fama’s “only intellectual investment was in efficient markets and, so far as he could see, the Mandelbrot hypothesis did not require giving up efficient markets. Quite the contrary, efficient markets (in the sense of serial independence) plus the Mandelbrot hypothesis (in the sense of fat tails) together seemed like a pretty good empirical characterization of the data”. How far, indeed, could Fama see? Standard statistics substituted for the low trust among financial scholars, and between financial economists and economists in the larger discipline, allowing Fama to connect his research program to the concerns of a larger community. Coupled with the appeal to wide audiences of his writings, Fama used statistics to build bridges with others.

Black, by contrast, used statistical analysis as an “engine of discovery”, in the manner advocated by Friedman and by the Savage/De Finetti tradition of statistical analysis. In his view, “a plausible theory is one that fits both everyday experience and correlations that come out of statistical analysis. There just isn’t any easy way to test a theory […] all we can do is to keep our theories more plausible, and to keep testing the theories against measured correlations” (Black 1982: 32). Importantl, though, in the absence of shared epistemological orientations, scholars disagree about what constitutes a plausible theory. In line with this interpretation, Black’s methodological affinity with Chicago was insufficient to building enthusiasm and consensus around his intellectual approach. As one financial scholar noted, “It is distressing to discover that […] viewed in isolation, the hypothesis that asset markets are in equilibrium is just so much empty rhetoric. But we do not view markets in isolation; we implicitly and explicitly bring a mixture of intuition, conjecture,
and knowledge to our research” (Lehmann 2004: 30). The problem for Fischer Black is that he did not have a coherent network to rely upon (as Friedman did); using this mode of quantification as a tool of persuasion therefore skirted the larger issue of how to construct expertise in a low-trust environment. Black is therefore remembered as a genial, but idiosyncratic and individualistic innovator.2

In short, the difference between Fama’s and Black’s use of methods lay in Fama’s pragmatic approach, which led him to privilege frequentism and data-dredging. By contrast, rather than focusing on organizing new networks, Black looked to individual innovation, adopting a set of research techniques that exacerbated the incoherence of the group around him. As both symptom and result of this strategy, Derman, a colleague of Black’s at Goldman Sachs notes, “Fischer [...] always preferred applied research to academic”, because in that context his “independent thinking” and “unorthodox but well thought-out ideas” could flourish (Derman 2004: 171). The paradoxical result of this approach was that it further undermined the already weak cohesion of the group rallied around CAPM, opening the door for alternative approaches to circumscribe its influence.

Discussion and conclusions

In this paper, I investigate intellectual processes in financial economics to show how an innovative idea can shape a research agenda. I focus on the extent to which the idea circulates within a coherent network structure, and the extent to which it is framed within an epistemology and a methodology, that strengthen the cohesion of the network, as conditions leading to success. Accordingly, I suggest that the efficient-market hypothesis acquired core theoretical status in the discipline of financial economics, whereas the capital asset pricing model did not, because of differences in the networks

2 For instance, in Stewart Myers’s words: “Fischer Black’s impact on corporate finance is insufficiently noticed. He did not specialize in that subject, and the fame of “Black-Scholes” has drawn attention from his broader contributions” (Lehmann 2004: 32). His conclusions: “What were Fischer’s contributions to corporate finance? We don’t know yet. He left us with too many open questions and unabsorbed ideas. It’s wrong to presume to wrap up Fischer’s research. So I offer no conclusions” (44). Mehrling reaches similar conclusions about Fischer Black’s more general impact on the discipline: in spite of (or better, precisely because of) his steadfast belief in CAPM, “colleagues of Fischer often remark that you never could predict what his position would be on any issue under discussion” (Mehrling 2005: 287).
propagating these models—in particular differences in cohesion and coherence, and differences in the methodologies underlying the models, leading to different practical outcomes. I showed that Fama’s use of probability theory and frequentist statistics increased the coherence of the intellectual circle around him, turning the analysis of efficiency into a collective project oriented towards the discovery of objective properties, which motivated younger scholars to pursue similar work. By contrast, Black’s adoption of more subjectivist methods exacerbated the individualistic tendencies of his approach and his network, making his research group more incoherent as a result, in spite of a methodological affinity between his style of analysis and that of the Chicago school.

In proposing a dual focus on social networks and methods, my analysis both follows and deepens existing approaches to intellectual innovation in the new sociology of intellectual movements. In particular, the two networks underlying CAPM and EMH illustrate an established, but empirically understudied difference between individualist and collectivist intellectual orientations (Frickel and Gross 2005: 219). EMH resonates both within a cohesive network and across generations; CAPM, by contrast, remains the project of an increasingly lone innovator. In this respect, the article contributes to our understanding of methodology with respect to how we characterize the orientation of an intellectual program: different kinds of methods, and different approaches within seemingly homogenous methods, have different social outcomes. How “modes of statistical analyses” vary, then, yields insight into how intellectual trajectories are defined.

Two broad implications flow from this argument. First, histories of the rise of quantification and positivism show that social scientific research in the post-WWII period tends to be statistical in nature (Steinmetz 2005; Turner 1994), and often, more specifically, frequentist (Porter 1996). Methods have politics, and this trend may therefore be explained by the fact that funding organizations tend to channel money into projects whose style they approve of; the more the style can be justified as objective, the more suitable it is for funding. Frequentism fits the bill. But methodology also serves as a tool intellectuals use to forge alliances with one another, and here frequentism affords an important advantage to the analyst, because it creates a seemingly objective foundation on which to build incremental innovation. Why, then, would intellectuals ever resort to methodologies that, as is the case in Black’s network, both make it harder to win the support of other intellectuals and run against institutional pressure for objectivity?
One possibility is that, in an interdisciplinary field like financial economics, which is part data-driven analysis of financial markets, and part abstract theorizing aimed at building intellectual bridges with economics, the politics of methods is different than in fields where funding comes from foundations alone (Mirowski and Plehwe 2009; Turner 1994). Gaining legitimacy in the eyes of economists is one potential way of gaining access to resources; but producing models that make money in financial markets is another. Different methods may therefore entail different political strategies.

According to this interpretation, on the one hand, a reason why Black’s approach to research is individualist and uses “subjectivist” methods is that Black considers the market as the ultimate arbiter of financial knowledge (Mehrling 2005: 293-295). And markets reward innovations that give early adopters an advantage over their rivals, temporarily sheltering them from competitive imitation (Schumpeter 1911; see also White 1981). Indeed, Black once argued that “the basic problem with research in business (and economics) is not that it is too theoretical, too mathematical, or too divorced from the real world (though all of these are indeed serious problems). The basic problem is that we have too much research, and the wrong kinds of research, because governments, firms, foundations, and generous alumni support it” (Black, October 1994: in Mehrling 2005: 301).

On the other hand, Fama’s collectivist intellectual style, characterized by his use of frequentism and “data-dredging” techniques, on one side, and authorship of wildly popular “state-of-the-art” reviews, on the other, betrays an orientation towards academia (and, to be sure, the funding infrastructure that has sustained it since World War II). For in academic fields, unlike in markets, innovative ideas thrive when they have networks within which they can resonate—networks that shelter the ideas from devastating criticism at too early a stage and that subsequently defend them from the attack of opponents (Collins and Guillén 2012; Farrell 2003; Parker and Hackett 2012). Fama was extraordinarily successful in fostering these networks (Jovanovic 2008). It must be noted that this did not come at the expense of success in markets: many of the anomalies uncovered by EMH scholars reaped handsome profits for those who took them into account in their trades (MacKenzie 2006: 98-105). But the success of Fama’s EMH is not reducible to its practical applications.

Other intellectual movements have adopted a strategy similar to Black’s in the past, relying on market success rather than intellectual prestige, at the cost of intellectual failure (McLaughlin 1998).
But as the academic system increases its reliance on private sources of funding, we can speculate that more intellectuals will use the market as a means of accumulating prestige and that they will be more successful at it.

The second set of implications has to do with how we theorize economic culture. Despite increased scholarly attention directed to beliefs, how economic actors arrive at those beliefs remains unclear (Beckert 2013). To be sure, macro-oriented studies of how theories about the economy derive from political culture have made great inroads (Dobbin 1994; Fourcade 2009). But explaining why models differ means paying attention to the types of boundaries producers of economic culture attempt to erect and then to how cultural producers negotiate transactions across these boundaries (Zelizer 2005, Wherry 2012). Analyses of boundaries and contested negotiations help us understand which beliefs are durable and seemingly unassailable and which beliefs are treated as dispensable. The CAPM/EMH controversy offers an empirically rich context for the exploration of these ideas, but more research is needed in order to better characterize the relationship between institutions and methods, and to better understand how this relationship, in turn, affects beliefs.

BIBLIOGRAPHY

THEORIZING EFFICIENT MARKETS


THEORIZING EFFICIENT MARKETS


Résumé

Cet article étudie l’économie financière pour montrer de quelle manière une idée innovante peut donner forme à un agenda de recherche. Il s’intéresse en particulier aux raisons pour lesquelles l’hypothèse de l’efficience des marchés financiers, cristallisée dans les travaux de Eugene Fama, est devenue centrale pour l’économie financière, au contraire du modèle d’évaluation des actifs financiers, pourtant défendu entre autres par Fischer Black. L’auteur attire en particulier l’attention sur les différences entre ces modèles du point de vue de leurs réseaux de diffusion (cohésion, cohérence) mais également du point de vue de leurs référents méthodologiques. L’utilisation par Fama des techniques de data mining et des statistiques fréquentistes accroît la cohérence de son cercle intellectuel, transformant l’analyse de l’efficience en un projet collectif orienté vers la découvertes de propriétés objectives. À l’inverse, le fait que Black adopte des méthodes plus subjectives tend à exacerber les tendances individualistes de son approche et de son réseau, augmentant par là-même le degré d’incohérence de son groupe de recherche.

*Mots-clés* : Economies financières ; Production culturelle ; Réseaux sociaux.

Zusammenfassung

Dieser Beitrag über die Finanzwirtschaft untersucht, inwieweit eine innovative Idee zu einem neuen Forschungskalender führen kann. Er hinterfragt, wie die Effizienzhypothese der Finanzmärkte, in Eugene Famas Forschung exzerpiert, zu einem zentralen Element der Finanzwirtschaft werden konnte, im Gegensatz zum Evaluierungsmödell der Finanzaktiva, das u.a. von Fischer Black verteidigt worden ist. Der Autor interessiert sich insbesondere für die Unterschiede zwischen diesen Modellen, sowohl aus Sicht der Verbreitung (Kohäsion und Kohärenz), als auch aus Sicht ihrer methodologischen Referenten. Famas Rückgriff auf Data Mining Techniken und Statistiken der Frequenz erhöht die Kohärenz seines intellektuellen Kreises, mit Umwandlung der Effizienzanalyse in ein kollektives Projekt zwecks Darstellung objektiver Eigenschaften. Blacks subjektivere Methoden führen zur Verstärkung sowohl der individualistischen Tendenzen seines Ansatzes und Netzes, als auch des Inkohärenzgrades seiner Forschungsgruppe.

*Schlüsselwörter*: Finanzwirtschaft; Kulturproduktion; Soziale Netzwerke.