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Authors

Bandelj, Nina
Elyachar, Julia
Richardson, Gary
et al.

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**Comprehending and Regulating Financial Crises:
An Interdisciplinary Approach***

Nina Bandelj

Sociology, University of California, Irvine

nbandelj@uci.edu

Julia Elyachar

Anthropology, University of California, Irvine

elyachar@uci.edu

Gary Richardson

Economics, University of California, Irvine

garyr@uci.edu

James Owen Weatherall

Logic and Philosophy of Science, University of California, Irvine

weather@uci.edu

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Comprehending and Regulating Financial Crises:

An Interdisciplinary Approach

Abstract

The 2008 financial crisis revealed that key players in finance, regulation, and the academy failed to understand realities outside their own area of expertise. Within the academy, scholars from an increasing number of disciplines study finance, and yet few of them seem to be in conversation. Perhaps understandably, given the complexity of a phenomenon such as “financial crisis,” no single discipline has yet offered an adequate analysis of what happened in 2008, or what could help prevent another such systemic threat to the economy. In this article, we argue that developing more effective capacity to comprehend and regulate financial markets requires an interdisciplinary approach that moves beyond pluralism and tolerance of other approaches. Rather, in-depth critical engagement with the underlying assumptions, methods, and findings across fields of research and practice is needed. To advance this argument, we discuss four specific, connected intra-disciplinary projects in progress, and show how key assumptions underlying approaches in each are revealed and revised through systematic engagement with other fields.

Introduction

Soon after the 2008 financial crisis, Gillian Tett, an anthropologist and the US Managing Editor of the *Financial Times*, suggested that regulators' and practitioners' inability to anticipate and respond to deep problems in the financial industry could be traced back to what she called "silo thinking," wherein experts in one area know nothing about the methods and research of other areas (Tett 2009a, 2009b). As she put it, "the essential challenges for investors today..." -- and, we might add, for regulators and academics -- is "to understand the micro-details of the silos, and see how all the macro-pieces add up" (Tett 2009b).

In years since, many researchers in many fields have sought to identify causes of the financial crisis of 2008 and to prescribe methods for regulating financial institutions in its wake. These contributions have provided important insights for public policy. And yet, academic responses to the financial crisis of 2008 are hamstrung by the very silo thinking that Tett identified as a key factor in the crisis. Six years after 2008, we still lack a broad perspective on financial crisis, both in our understanding of what led to the crisis or in prescriptions for how to avoid such crises in the future.

In this article, we argue that the only way to achieve such a broad, macro-scale understanding of financial crisis is to develop a critical interdisciplinary approach to financial markets and financial regulation. An interdisciplinary approach of the sort we propose must bring together scholars whose research draws on natural and social sciences and the humanities, and who have expertise in the broadest possible base of methods, including formal modeling, experimental

research, statistical analysis, case studies, historical analysis, and ethnographic research. Such an approach to research about financial markets goes against the grain of prejudices in economics, where economists tend not to talk to other social scientists; in anthropology and sociology, where critiques of economics and finance are ample but expertise in economics rare; and even in philosophy of science, where substantive engagement with the details of real-world practice is often avoided. And yet, without this sort of broad, critical engagement, we see little prospect for developing a more robust understanding of financial crisis and regulation.

Of course, calls for interdisciplinarity are not new. Indeed, “interdisciplinary” has become a buzzword on university campuses and among funding agencies. In itself, the term “interdisciplinary” does not denote a particular way of approaching research problems. (Klein [2010] offers a taxonomy of interdisciplinarity with well over a dozen varieties!) Meanwhile, the idea that interdisciplinarity is to be valued in all (or any) of its guises is itself a matter of some controversy. For instance, in the recent *In Defense of Disciplines* (2013), sociologist Jerry Jacobs argues against the claim that “silo thinking” of the sort Tett bemoans in finance is broadly symptomatic of research in most academic fields. Jacobs suggests, in fact, that the current move toward interdisciplinarity is associated with the centralization of decision making in universities and the erosion of faculty governance, so that interdisciplinarity should be resisted as a rule.

For present purposes, however, we do not need to take a stand on the issue of whether interdisciplinarity is valuable in general. Instead, we claim that there

are certain problems -- including the problem of understanding and regulating financial markets -- that are sufficiently complex and multi-faceted that they cannot be adequately approached within a single discipline. The view that such problems exist is uncontroversial. After all, many problems can be fruitfully studied in more than one discipline. Here, though, we propose that for some problems -- or at least, for *this* problem -- mere pluralism about disciplinary approaches -- what might be called “multi-disciplinarity” (Apostel *et al.* 1972; Klein 2010) -- does not go far enough. In particular, we do not see any systematic integration of work done on financial markets in different disciplines of a sort that might influence market regulation or policy-making. In other words, we argue that, whatever the situation may be in general, in this particular case, silo thinking is a problem, and a specific kind of critical interdisciplinary engagement is a possible remedy.

Our strategy for making this argument is unconventional. In effect, we will argue for the limitations of intra-disciplinary reasoning by describing four research projects in progress, each of which reflects how someone from our specific disciplines -- philosophy of science, sociology, economics, and anthropology -- might approach a single question: *with what kinds of models of finance and financial crises do market actors and regulators operate, and with what consequences?* In each case, after describing the project and its methods, we examine how it rests on assumptions, or raises issues, that the discipline within which the project originates is poorly suited to address. We then show how the approach taken in the other disciplines we consider may be understood to respond to those questions in a way that may then feed back into the original work. In this

way we hope to both argue that in this particular context, interdisciplinary work is called for, while also providing a model of how that work, properly conceived, should proceed.

The article will be structured as follows. We begin by describing the background and motivation for our overarching project. This section includes a discussion of how the present article fits into literatures on interdisciplinarity and on the social studies of financial crisis and financial markets. We then turn to the four interrelated, initially intra-disciplinary research projects in progress. In each case, we reflect on how each project approaches a different facet of the overall question we hope to address, how gaps left by each of our disciplines may be filled through inquiry in the others, and how interdisciplinary collaboration aids in interrogating unstated assumptions of our disciplinary approaches.

Background and Existing Research

On Interdisciplinarity

Since the mid-1980s, funding agencies have been promoting interdisciplinarity, broadly defined as “communication and collaboration across academic disciplines” (Jacobs and Frickle 2009: 44) through programs like the NSF’s CREATIV (Creative Research Awards for Transformative Interdisciplinary Ventures) and IBSS (Interdisciplinary Behavioral and Social Sciences). Concomitantly, many universities are building interdisciplinary centers and initiatives. Several arguments are offered in support of this effort. Some scholars point out that interdisciplinary research is important because many problems do not have their origin or solution in a single

discipline (Golde and Gallagher 1999; McNeill 1999; Roper and Brookes 1999). Others argue that disciplinary boundaries foster “silo thinking” and suppress communication between scholars working on related problems who might benefit from one another’s expertise (Gilbert 2008; Tett 2009b; Jacobs 2013: Ch. 2), or even that without sufficient interdisciplinarity, the academy faces “excessive specialization, the lack of societal relevance, and the loss of the sense of the larger purpose” (Frodeman 2010: xxxii).

In light of such arguments, there is a prevailing sentiment among many university administrators that research “must be interdisciplinary to be world-class” (Pray 2002); that “interdisciplinarity has become almost synonymous with creativity and progress” (Weingart and Stehr, 2000: xi); and that interdisciplinary research is the only way to generate innovation, needed as an “internal motivator of sustained epistemic change” (Fuller 2003 p. 4). A body of empirical research on the impact of interdisciplinary research lends much support to such claims. For instance, at least for the social sciences, the more interdisciplinary the research, the more it is cited (Leahey et al. forthcoming; Schilling and Green 2011; Shi et al. 2009; Uzzi et al. 2013). This holds up even when controlling for characteristics of the scholar (Leahey and Moody 2014) and the journal in which the article is published (Schilling and Green 2011).

Even so, there are dangers associated with interdisciplinarity. For instance, interdisciplinary research tends to be seen as much more risky than disciplinary research, and is less likely to be evaluated favorably by reviewers (Langfeldt 2006). Journal rankings may discourage interdisciplinary research, as top-tier journals in a

discipline are less likely to publish interdisciplinary research (Rafols et al. 2012). Interdisciplinary research centers at universities often lack clear, unifying problem definitions, so participants are loosely tied scholars studying the same broad issue rather than working together to answer the same questions (Rhoten 2004).

For these reasons and others, some authors have criticized the rise of interdisciplinarity and questioned its lasting significance. For instance, Abbott (2001) argues that disciplines are anchors of knowledge and relatively impervious to change. Abbott recognizes interdisciplinarity as a “standing wave” produced by the disciplinary system (p. 150), but not a real threat to disciplinary autonomy. Jacobs (2013) presents research to show that disciplines are not nearly as “siloed” as proponents of interdisciplinarity suggest, and that information flow between disciplines is rapid and substantial. Meanwhile, classic work in sociology of knowledge suggests that a naïve call for interdisciplinarity may reflect a naïve picture of disciplines, which themselves may rise or fall for reasons that have little to do with natural divisions between methods or topics of inquiry (Ben-David and Zloczower, 1962).

Some empirical studies on interdisciplinarity also find potential pitfalls. Interdisciplinarity can reduce productivity (Leahey et al. forthcoming), cause coordination problems for multi-campus projects (Cummings 2005), slow down the review process (Mansilla 2006), and is potentially risky in terms of impact (Shi et al. 2009). Nevertheless, these studies generally conclude that even with the challenges and risk, the benefits of interdisciplinary research are potentially great.

Researchers also warn that not all “interdisciplinary” research is interdisciplinary. In many cases, projects that call themselves interdisciplinary are, in fact, multi-disciplinary, in the sense that each scholar continues to rely on disciplinary philosophies, languages, and methods to tackle a research question (McNeill 1999; Roper and Brookes 1999; Klein 2010). True interdisciplinary research begins with an integrative approach to a problem (Klein 1990; Klein 1996; McNeill 1999; Morawska 2003; Roper and Brookes 1999) and requires finding a common language among the disciplines (Morawska 2003). It is an iterative process, applying different disciplinary insights in the research process, adjusting that process, and repeating (Klein 1990; Lamont, Mallard, and Guetzkow 2006).

The projects described below are iterative in precisely this sense – indeed, as we describe them here, the projects have benefited from a year’s worth of interdisciplinary conversations. Moreover, we take this notion of interdisciplinarity as an iterative process one step further by focusing on identifying and rendering explicit the underlying assumptions of each disciplinary approach to our substantive issue at hand—financial crisis and regulation. In this sense, we engage in *critical interdisciplinarity*, whereby the notion of critical has two meanings.¹ The first one derives from the idea of critical engagement, referring to questioning of disciplinary assumptions and conventional methods. The second meaning refers to problems that the proposed interdisciplinary approach is to tackle; critical in a sense of urgency, whereby disciplinary thinking has failed us, resulting in a moment of crisis.

¹ Note the expression “critical interdisciplinarity” as used here differs somewhat from how it is used in Klein (2010).

On Financial Crisis

The financial crisis of 2008 was the most severe financial crisis in the western world since the Great Depression. Economists and political theorists have dealt with many aspects of this crisis (e.g. Reinhart and Rogoff 2009; Shiller 2009; Friedman 2010; Gorton 2010, 2012; Chinn and Frieden 2011; Blinder 2013). While most of these retrospective accounts ask how relevant players made poor decisions, they vary in terms of assigning psychological, epistemological or political explanations for the crisis. Reinhart and Rogoff (2009) account for the crisis as an accumulation of private debt during a time of prosperity that fostered a conceit of invincibility. Shiller (2009) offers a psychological reflection on the development of crisis as the product of feedback loops of emotions, attentions, and confidence in faulty information from trusted sources. Gorton (2010, 2012) takes an historical view of crises, arguing that panics are endemic to the structure of banking but that shadow banking and its invisible vulnerabilities made the most recent crisis particularly devastating. Blinder (2013), attempts to explain the necessity and relative effectiveness of government bail-outs in the aftermath of the crisis, with an account that emphasizes the role played by extensive counterparty contagion.

Anthropologists and sociologists have also attempted to explain the financial crisis of 2008. Anthropologists began to comment on and debate it early on in journals such as *Anthropology Today* (Hart and Ortiz 2008, Elyachar and Maurer 2009) and in public fora (Tett 2009b), bringing to bear a decade of research in the anthropology of finance to think about current events. Some sociologists have focused on the role of financialization (Davis 2009, Krippner 2011) and the micro-

level structures and the trading instruments (MacKenzie 2011), while others placed governments front and central, arguing that governments helped innovate financial products and structured the market (Fligstein and Goldstein 2012). Still others emphasize that, “financial products, organizations, regulators, and infrastructure organizations (e.g. rating agencies) [are] elements of an interconnected system” (Lounsbury and Hirsch 2010a: 10-11).

Another reaction to the 2008 crisis has been to focus on crisis as a topic of study in its own right. Some authors have questioned whether the concept of “crisis” forecloses our range of thought about what is underway and how to respond (Roitman 2013). For instance, historically, crisis has often been seen as a moment that reveals truth or underlying values (Roitman 2013). And yet, in the post 2007-08 period, crisis became a “new normal” (el-Erian 2008). This upsets our usual frame of reference for crisis, which is often seen as lying outside the norm, and forces a reconceptualization, particularly among regulators.

Others have focused on how regulators come to recognize that a crisis is underway, suggesting that regulators act as critical theorists by rendering explicit emergent practices in the market and practicing “retrospective ethnography,” by studying the past to attempt to model the future (Elyachar 2013, Maurer 2012b). We saw this in the 2008 financial crisis: Geithner and Bernanke are themselves students of the history of financial regulation who brought their studies of the Great Depression into their dealings with 2008.

The emergence of financial crisis as an object of study coincided with the maturation and proliferation of new modeling techniques, pioneered by

mathematician Benoit Mandelbrot, based on stably heavy-tailed probability distributions (Mandelbrot 1997; Taleb 2001, 2007; Sornette 2004; Mandelbrot and Hudson 2004). Such models provide a conceptual scheme for understanding extreme events as not only to be expected, but as the dominant determinant of long-term economic behavior, in contrast to traditional views whereby such events are seen as outliers. These new modeling methods may provide an alternative to the easy resort to “crisis” as an explanation of any outlier event (Roitman 2013).

On Financial Markets

Sociological, anthropological, historical, and philosophical interest in finance did not begin with the 2008 financial crisis (for reviews see Knorr Cetina and Preda 2005, 2012; Carruthers and Kim 2011; Maurer 2006, 2012a). Early studies examined the cultures of financial markets (Abolafia 1996), social networks in financial markets (Mizruchi 1982, Palmer 1983, Baker 1984), and the role of status in financial markets (Podolny 1993). In a seminal study, Fligstein (1990) showed how the hiring of CEOs with backgrounds in finance reshaped the corporation in the second half of the 20th century, replacing more operations and marketing-oriented logics (cf. Lounsbury 2007). Another body of work has shown the role of the state in the creation of modern (financial) markets (Fligstein 2001), for instance by regulating markets (Caruthers 1996) and pioneering securitization (Quinn 2010; Fligstein and Goldstein 2012). Anthropologists have studied the culture of financial institutions, from Wall Street banks (Ho 2009) to the Federal Reserve

(Holmes 2013, Elyachar 2014) and elsewhere, such as in Egypt (Elyachar 2005, 2012), Africa (Roitman 2005), and Japan (Riles 2011; Miyazaki 2013).

The status of the formal models used in economics has been a topic of particular interest across the social sciences. One persistent criticism is that these formal models are not realistic (Hirsch, Michaels and Friedman 1987) in that they do not take into account how real market transactions really work (Friedman 1953; Nagel 1963; Samuelson 1963; Bear and Orr 1969; Boland 1979; Mäki 1992; Hausman 1992, 1998, 2008; Alexandrova 2008; Preda 2009b; Morgan 2012). These debates connect to broader issues regarding realism, methodology, and representation in scientific models and theories more generally (e.g. Popper 1972; van Fraassen 1980, 2008; Laudan 1981; Cartwright 1983, 1999; Stanford 2006; Chakravartty 2011; Frigg and Hartmann 2012). Indeed, financial models provide an unusual case study for philosophical questions concerning representation and realism because of their close relation to decision making. Financial models also provide an interesting case study for debates concerning the role of values in science (cf. Kuhn 1977; Laudan 1986; Longino 1990, 2002; Solomon 2001; Kincaid et al. 2007), since there are relatively clearly delineated incentive structures influencing both the direction of research and the nature of the resulting models, and, as we will explore below, recent shifts in modeling methods may correspond to shifts in epistemic values concerning risk among practitioners.

More recent work on models in the social studies of finance has focused on their performativity (Callon 1998; Elyachar 2005; MacKenzie 2006; MacKenzie, Muniesa, and Siu 2007; Preda 2009a; Beunza and Stark 2011; Lepinay 2011) and

on the epistemic cultures in which they are produced and used (Knorr Cetina 1999; Yonay and Breslau 2006; Lepinay 2011). That financial models are performative means that these models, as material tools used by market actors, do not only describe the markets but they transform them, as when the adoption of the Black-Scholes model led actual prices to gradually converge to theoretical prices (MacKenzie 2006, Lepinay 2011). Work on the epistemic cultures of model production and use, meanwhile, has suggested that the authors of formal models tend to distance themselves from their products, allowing practitioners to use formal models outside of their context of production, forgetting the assumptions on which the models are built and generating more confidence in them than warranted (Yonay and Breslau 2006). This phenomenon may be particularly pronounced in cases where models are imported from other fields, such as physics and mathematics (Jovanovic 2012), where models are often understood in different ways (Weatherall 2013).

Four Disciplinary Projects Benefiting from an Interdisciplinary Approach

In this section of the article, we present four projects in progress grounded in our different disciplines —philosophy of science, sociology, economics, and anthropology—that have been shaped by our ongoing discussions about financial models, financial crisis, and financial regulation. Each was initially situated in the conceptual framework of our separate disciplines and made use of methods common to those disciplines. However, as the projects evolved, they have been mutually influenced by our interdisciplinary conversations. These projects concern

a) formal and conceptual analysis of how derivatives pricing models and risk models treat the possibility of crisis; b) content analysis of writing on financial models and financial crisis in the media; c) experimental research on financial behavior; and d) ethnography of financial regulation and financial crisis.

1. Epistemic Values and the Anticipation of Crisis: Beyond Philosophy of Science

One natural starting point for understanding the models that financial actors use, and how they use them, would be to study the conceptual and mathematical foundations of those models themselves, particularly as they relate to risk and crisis. Here we use methods from philosophy of science to study the methodology of financial model construction, to identify the assumptions used in deriving those models, and to evaluate the evidence justifying their use under various conditions.

We focus on models used in derivatives pricing and risk modeling, such as the Black-Scholes-Merton options valuation model and the RiskMetrics Value-at-Risk model. A foundational assumption of traditional models in this class, including the two just mentioned, is that asset return time-series can be modeled as a continuous random walk, leading to normally distributed (Gaussian, or Bell curve) returns (Osborne 1959; Cootner 1964; Samuelson 1965; Black and Scholes 1973; Merton 1973). These distributions, in turn, make implicit predictions about the likelihood of extreme events, such as market crashes.

For many years, the assumption of normally distributed returns was taken to be both mild and reasonably well-supported by historical returns. Models based on the assumption were adopted by most practitioners without question (albeit with

some notable exceptions [see Weatherall 2013]), beginning in the late 1970s. The 1987 Black Monday crash, however, led many mainstream financial economists to revisit the assumption of normally distributed returns, since according to such models, the crash had a probability on the order of 1 in 10^{160} -- in other words, it was essentially impossible, if returns really were normally distributed. Over the following two and a half decades, several events of a magnitude similar to the Black Monday crash have occurred, further undermining belief that normal distributions accurately reflect the probabilities of market returns.

These startlingly frequent crashes (from the perspective of the traditional models) led to renewed interest in an alternative class of models, based on probability distributions that are heavy-tailed. Heavy-tailed distributions differ from normal distributions in that they assign much higher probabilities to extreme events, which many authors argue is more appropriate, given the historical data (Mandelbrot 1997; Taleb 2001, 2007; Sornette 2004; Mandelbrot and Hudson 2004). Although such models are nearly as old as models based on normal distributions (Mandelbrot 1963; Fama 1965), they were widely viewed as unworkable and unnecessary by early researchers (Cootner 1964), and it was only in the wake of the apparent failure of the standard methods in 1987 that these alternative methods were given serious consideration. Even so, the investment community has not entirely adopted these alternative models. Indeed, important models, including RiskMetrics' Value-at-Risk model, which the SEC requires investment firms to use for some reporting purposes, continue to assume that returns are normally distributed.

One perspective on the continued use of normal distributions is that investors and regulators are epistemically irresponsible (Taleb 2001, 2007). But the situation is more complex than this reaction allows. One central issue concerns the distinction between *transiently* heavy-tailed distributions and *stably* heavy-tailed distributions. In the former case, while extreme events may occur more often than predicted by a normal distribution on short time scales, over the long run returns should be normally distributed, suggesting that for many modeling purposes, traditional methods using normal distributions are appropriate after all. In the latter case, meanwhile, heavy tails persist even for long time scales, meaning that the traditional methods will lead to generically misleading results. The difference comes down to whether in the long run, extreme events will be washed out by general reversion to mean behavior, or whether these extreme events will turn out to be the dominant factor in long-term market returns.

There are a number of theoretical and empirical arguments offered in favor of the thesis that returns are merely transiently heavy-tailed. One such argument begins with the observation that market returns reflect the cumulative outcome of many small events, occurring continuously in time (McCulloch 1996; Rachev and Mittnik 2000). This suggests that a mathematical result known as the central limit theorem should apply. The central limit theorem states that in an appropriate limit, the sum of a large number of independent and identically distributed random variables from a broad class of distributions will be normally distributed. Thus, if the ordinary day-to-day events that shape prices are distributed according to a member of this class -- an assumption with some strong intuitive appeal, since the

class includes all distributions with finite variance (i.e., well-defined volatility) -- returns must be normally distributed in the long run. This argument is also apparently supported by statistical analyses of returns (see Cont 2001 and references therein) that investigate a parameter known as the tail index, which is used as an indicator for whether a distribution is stably heavy-tailed. In this case, the tail index appears to be greater than 2, implying that returns are not stably heavy-tailed.

These arguments, though influential, are open to serious criticism. For instance, it has been observed (Weron 2001) that accurate measures of tail-index from finite data sets are very difficult, and perhaps impossible, even in principle. The reason is that the class of events that distinguish stably fat tailed distributions from transiently fat tailed ones are precisely the events that are most infrequent according to both distributions -- namely, the extreme events. Hence, in general, historical returns do not clearly distinguish between the two cases, substantially weakening the empirical argument for transiently heavy tails. This suggests that modelers who attempt to identify predictive distributions based on historical returns cannot rely on the empirical evidence alone. They must also rely on their beliefs concerning what kinds of distributions are “reasonable,” including assumptions regarding the possibility of infinite variance/volatility. One might even see recent discussions of a “new normal” (el-Erian 2008) as indications of a shift in such beliefs, so that assumptions that previously seemed unintuitive or unreasonable now seem natural or necessary.

Background beliefs of the sort just described, i.e., beliefs that guide determinations of “plausibility” or “reasonableness” of a model, are sometimes referred to as “epistemic values”. The idea that values of this sort play a role in theory and model choice is hardly new in the philosophy of science (Kuhn 1977; Longino 1990, 1996, 2002; Intemann 2005; Douglas 2009). And so it is perhaps unsurprising that such values play a role here, particularly in the face of a straightforward kind of underdetermination. On the other hand, this is a case in which there are unusually clear consequences to choosing a model, in the sense that any epistemic risk – the risk that you are wrong – translates directly into financial risk. For this reason, it seems worthwhile to study in concrete detail how practitioners deal with the problem of underdetermination in this particular context.

From this point, a project in philosophy of science would proceed by further analysis of the arguments glossed here, including detailed case studies of particular models and their interpretations and justifications. In addition, one might pursue myriad technical questions related, for instance, to the status of tail fitting procedures or the role of interpretations of probability in the interpretation of the financial models under consideration; one might also explore how these technical issues bear on the broader theoretical issues described above, concerning underdetermination and epistemic values.

Insofar as one is interested in studying the models themselves -- surely a worthy pursuit, and one that could potentially affect how practitioners ultimately use the models -- pursuing the projects just described would be fruitful. But, if one is ultimately interested in the practice, in the sense of understanding how the

models financial actors use shape their actions and influence decision making, there is a sense in which these methods can at best generate hypotheses. In particular, philosophical analyses would usually proceed independently of the application of models in realistic investment and regulatory settings. The methods described here would not allow us to study either how these values are adopted or what sorts of considerations lead practitioners to change their values. Similarly, they would provide no insight into how modeling methodologies vary among communities, and what sorts of considerations affect how, say, regulators understand these models as opposed to how practitioners understand them. An understanding of these broader issues would require us to study the attitudes held by regulators, practitioners, and other market participants, through detailed content analysis of the financial media and ethnographies of regulatory bodies and investors.

A related issue concerns how to conceptualize “crisis” in the first place. In the models discussed here, it is natural to associate crisis with periods of large negative returns, relative to the central behavior predicted by the distributions. On this way of conceptualizing “crisis”, heavy tailed distributions assign a higher probability to crises than do normal distributions—and indeed, they arguably account for the possibility of major market drawdowns. One idea that has come up in our collaborative discussions, inspired in particular by considerations from anthropology and sociology, is that “crisis” may be better understood as a period in which our standard methods and means of understanding markets break down. (See project 4 below.) Thus, if heavy-tailed distributions were to become the norm

—as they have begun to, at least in some areas—one might argue that market crashes can no longer be understood as crises, and that “crisis” should be reserved for situations in which even these alternative models break down. Thus engagement with sociology and anthropology forces a re-evaluation of what it would mean to generate models of crisis in the first place.

Similarly, the discipline-bound analyses described here would not address another question raised by the sociological and anthropological literatures, concerning how changes in epistemic values may in turn affect the markets the models attempt to describe, perhaps necessitating still further changes in epistemic values. Once again, in order to address this broader question, we need to draw on historical, sociological, and anthropological methods in order to see, for instance, whether the sorts of philosophical analyses proposed here might provide insight into how historical shifts in epistemic values may have contributed to changes in market structure—say through the adoption of derivatives pricing models. In this way, the philosophical analysis proposed here could be extended so as to contribute to the now considerable literature on performativity of economics discussed above, shedding light on how epistemic values that were once thought to be neutral, or which remain in the background as maxims or tacit knowledge, in fact shape financial markets.

2. Content Analysis of Media Writing on Financial Models and Crises: Beyond Sociology

A different way of inquiring into financial models and financial crisis is to examine conceptions and meanings circulated in media. Much research points to the important role media plays in influencing financial markets by acting as a market information intermediary (Shiller 2000, Dyck and Zingales 2002, Pollock and Rindova 2003, Tetlock 2007, Barber and Odean 2008, Pollock et al. 2008, Tetlock et al. 2008, Hirshleifer and Teoh 2009). According to Morris and Shin (2002: 1521) public signals – such as those available through news or social media – serve as a “coordination device” and particular interpretations of models are often proliferated in media (Beunza and Garud 2007). Abolafia and Kilduff (1988) showed that national media organizations have influence as shapers of market actors’ reality and can convey beliefs and interpretations that become self-fulfilling. This is especially important in conditions of true uncertainty (Knight 1921), where actors rely on various social forces – including available cultural conceptions and imitation of peers – to be able to make decisions (Festinger 1954; DiMaggio and Powell 1983; Beckert 1996; Haunschild and Miner 1997; Henisz and Delios 2001; Rao, Greve and Davis 2001; Bandelj 2008). Given this, a sociological study could inquire into the kind of conceptions/notions about financial models and crisis that are proliferated in the media.

Furthermore, sociologists could be interested in the role of confidence in financial models, a topic largely overlooked in the social studies of finance (Swedberg 2012), and in modern theoretical economics (Walters 1992: 423). While John Maynard Keynes discussed confidence in his *General Theory* (1936: 149), he nevertheless conceded that there is “not much to be said about the state of

confidence a priori. Our conclusions must mainly depend upon the actual observation of markets and business psychology.” Behavioral economics takes confidence, or rather overconfidence, into account by acknowledging that “people are more confident in their judgments than is warranted by the facts” (Griffin and Tversky 1992: 411). Recently, confidence features prominently in Akerlof and Shiller’s (2009) theory for macroeconomic analysis that conceives of it as one of the animal spirits, next to temptations, envy, resentment, and illusions. Confidence is often conflated with trust (Swedberg 2012), and treated as an efficiency enhancing mechanism (Arrow 1974).

From this vantage point, sociologists could contribute to our understanding of financial models and financial crisis by performing content analysis of a population of articles from the main news outlets, such as the *New York Times* and the *Washington Post*, and specialized news outlets including *The Wall Street Journal* and *Financial Times*. In addition, given the prevalence of social media, we could analyze content on Twitter. Keywords to search for would include “financial crisis,” “financial models,” and “new normal” over the 1980 to 2014 time period, to cover several recent financial/economic fluctuations, and more recently for Twitter. This would map the ecology of discussion on these topics. Prompted in part by considerations raised by philosophy of science, we would select opinion pieces and contributions by experts, analysts, and regulators rather than general news pieces. We would look for descriptive evidence on the following issues: a) What is the interpreters’ notion of risk and uncertainty as it relates to the use of financial models? b) What is the understanding of potential impending financial

crisis? c) What is the perceived value of financial models? c) What is the level of technical knowledge of models, including their assumptions and limitations? e) To what extent are crises considered the “new normal”? and f) What is the sense in which crises are considered predictable?

In the next step, we would explore how interpretations and implicit models vary over time and by social position of the interpreter. We hypothesize an overall paucity of claims about limitations of financial models, and lack of explicit discussion about crisis before 2007. In other words, and drawing on anthropologist Roitman (2013), crisis lies in the background as an assumed explanatory concept but is rarely explicitly discussed. As shown in ethnography of the Latin American debt crisis of 1982 (Elyachar 2013), and in a new genre of crisis memoirs of 2008 (Geithner 2014), regulators can be slow to recognize that a “crisis” is really underway (Elyachar 2013). We also expect that beginning in 2008, we will find that both “crisis” and “financial models” became a topic of discussion in their own right, perhaps reflecting a change in epistemic values concerning whether crises are “normal”, in the sense described in the previous project.

In terms of social position of interpreters, we would focus on education background (physicists/statisticians/mathematicians, compared to economists, compared to broader interdisciplinary background) and occupation (investors, reporters, analysts, academics, and regulators). We expect that physicists and mathematicians display a greater awareness of the limitations of financial models (Weatherall 2013) but more so when they are addressing their own epistemic community (Knorr Cetina 1999) in specialized outlets rather than more lay

audiences in popular outlets. Concretely, and learning directly from the philosophy of science, we expect that physicists and mathematicians will emphasize that the Gaussian model that is central to modeling in financial economics is at best a low-order approximation for much more complex phenomena (Jovanovic 2012) and thus expect preferences for Levy distributions to describe financial data and assumptions about heavy tails, volatility persistence, and volatility clustering.

3. Experimental Research on Investment Behavior: Beyond Economics

Another way of approaching our questions of how risk and crisis are apprehended and modeled is by experimental economic studies of investor behavior in times of widespread financial crisis. For instance, an experimental economist could consider the following experimental setup. Subjects are assigned to investment groups, usually consisting of three other individuals. The structure of the groups resembles that outlined by Diamond and Dybvig (1983). Each group begins in period zero and lasts for 10 periods. Individuals' investments in the group are short term. Individuals have the right to remove their investments at the beginning of each period. The groups' assets are illiquid, and yield a positive return only if held for all 10 periods. In each period, individuals decide whether to invest in the group or to withdraw their investment. We refer to the game that ends after 10 periods as a round. We refer to a set of 40 rounds as a session.

Payoffs depend upon whether the group holds its investment until the end of the round and which individuals withdraw prematurely. If all members of the group invest in all periods, then at the end of period 10, all group members receive

a payout of $\$10 + R + X$, where R is the return on the investment (which can be a fixed value such as 10% or sampled from a probability distribution of the types described above). X is the minimum payment that an individual requires to join the group. We elicit this value through standard incentive-compatible methods. If one (or more) members of a group withdraw in rounds 1 through 9, then the individuals that withdraw receive compensation of $\$10 + X$. Individuals that do not withdraw receive $\$0$.

Such experiments would be conducted in our Experimental Social Science Laboratory, a facility with 40 desktop computers linked to server running Z-tree software. We divide individuals into 10 investment groups, and brief individuals before or after the experiment about their background, experience, and understanding of assumptions. We also note their demographic profiles. Assignments to groups are random and anonymous. So, individuals do not know the identities of the other members of their group in this round, past rounds, or future rounds. Individuals participate in sessions of 40 rounds, which generally last about 2.5 hours.

In each period, we elicit participants' beliefs concerning the likelihood that members of their groups will withdraw in the next period (which we denote Y) and concerning the number of individuals in any other group that will withdraw. We motivate participants to provide accurate predictions by rewarding them for the accuracy of their assessment. In our baseline experiments, real returns (R) across investment groups are set before the round and are not linked in any way. Participants are clearly instructed that real returns across groups are uncorrelated.

We do, however, provide individuals with a table describing the investment decisions of all members of all groups in all periods within the round. With this table, participants can quickly and easily determine if any individuals withdrew from any groups at any time.

Our preliminary results suggest that these investment groups are, as expected, extremely stable. Most investors who join these groups stay until the end. In cases, however, where some investors pull out, participants often respond to the news by increasing Y , their expectation that someone will withdraw from their own group in future periods. On some (but not all) occasions, this change in expectations coincides with changes in equilibria – or in other words, sometimes receiving reports about withdrawals from other investment groups triggers withdrawals from your investment group. This pattern resembles the contagion of fear that observers often perceive during financial panics. After witnessing one of these rare contagions, the value that most individuals place on playing these games (X), falls. This fall occurs even for individuals whose groups remained in operation and/or suffered no losses.

Informed by interdisciplinary conversations with philosophers of science and sociologists, our experimental design differs from that commonly used in the experimental literature in that we seek to elicit information about X – the marginal value that individuals place on participating in investment groups – and Y – beliefs about individuals' propensity to panic. We show that rare financial events change investors' beliefs about the behavior of other investors, which leads to changes in the value that individuals place upon investing, and in some cases, changes in

investment behavior. These sorts of changes in behavior may correspond to changes in epistemic values concerning “reasonable” probabilities to assign to withdrawals, in the sense discussed in the previous two projects.

Further considerations of the sort raised by philosophers of science also suggest other variations on this experimental design, to attempt to study investors’ sensitivity to the distribution of returns, and also to study whether different information about what distributions of returns are possible or likely affects how investors react to varying returns. In this way, we might gain experimental control over how an epistemic context may affect investors’ perceptions of the game. For instance, by allowing the return on investment from round to round to be governed by a distribution, rather than fixed, we might study whether investor behavior is sensitive to the statistical properties of returns. Similarly, by stipulating an expected average return, and then providing returns far from that average, we might study whether investors change their opinions on how risky it is to stay in the pool. One might also ask whether unexpectedly large positive returns have the same effects on investor behavior as unexpectedly small returns, or even negative returns.

Further, concerns of economic sociology and anthropology urge us also to question assumptions that are usually left unexamined in economic experiments. Therefore, we will also examine the impact of expressed confidence and knowledge of the market on investment behavior. In addition, will take into account how demographic characteristics, pre-assessed confidence in game rules,

and changing the institutional parameters of the experiment revealed to subjects, may influence their decision-making.

4. Ethnography of Financial Crisis and Regulation: Beyond Anthropology

The final project draws on methods from anthropology. Anthropology may not seem like an obvious discipline for studies of financial crisis and regulation, since it emerged as a field specializing in study of “primitive society” without discrete economic or financial systems. But in fact, since the 1990s, contemporary finance has become a topic of study in anthropology (for a review see Maurer 2006). Much of this work has relied on ethnography, the signature methodology of anthropology, which rests on knowledge generated through intersubjective experience in the present. Early ethnography of finance focused on revealing social and cultural aspects of finance (Hertz 1998, Ho 2009, Zaloom 2006); more recently, there has been a growing body of work on the ethnography of financial regulation (Elyachar 2013; Holmes 2009, 2013; Riles 2010; Maurer 2012a; Miyazaki 2012).

Notably, however, there has been considerably less work done on the ethnography of financial crisis. One reason for this is that crises are, by and large, difficult to anticipate. How can an ethnographer choose a site in which to study financial crisis if actors do not even know that it is about to unfold? Another reason is that major systemic crises are rare: for instance, as we have noted, the 2007-08 financial crisis, was the most severe financial crisis in the western world since the Great Depression, which in turn occurred long before anthropology of finance existed. For this reason, ethnography of crisis, insofar as it has been done, has

happened by accident. In one case, an ethnographer worked in the Federal Reserve Bank before becoming an anthropologist (Elyachar 2013). In other cases, anthropologists of finance during the 1990s happened to follow their informants and topics through to 2008 (Miyazaki 2013, Riles 2011). Writing ethnographies on the basis of such accidents often calls for unorthodox methodologies.

Another new area of research in anthropology is ethnography of regulatory agencies. Such agencies offer a location in which to study the interaction between formal models of finance and underlying assumptions or tacit models of finance. We propose that daily practices in regulatory agencies are at least as important as overarching regulatory decisions in the resolution of financial crisis. Based on previous fieldwork with bankers and bank regulators, we expect that regulators depend heavily on their own ‘tacit knowledge’ of crisis in their responses to unfolding crisis. In this framing, ethnographic research gives us a method through which to investigate regulators’ tacit understandings of financial crisis, and their explicit use of financial models, in their work.

As noted above, there has already been some ethnographic research on regulatory agencies, focused on central bankers at higher levels of the NY Fed or the Open Market Committee (Holmes 2009, 2013). By way of contrast, our research would focus on research assistants, lowly economists, mid-level officials, and also mainframe computers with glitches and bugs. This approach draws on two bodies of thought in anthropology. One is a growing body of research in the anthropology of policy (Wedel 2005; Shore, Wright, and Pero 2011). This work shows the importance of studying policy interventions not as they are planned, but also as they work out on

the ground. Policy--and the same can be said of regulation--is not completely performative (Callon 1998). Our work also draws on research from the field of science and technology studies via the sociology and anthropology of finance. From this perspective, we need to study the agency of computers, old mainframes, and different styles of working and generating knowledge, as much as we need to study continuities and differences in approaches to crisis proclaimed by heads of the Federal Reserve Bank.

Our ethnography of regulatory agencies would have two parts. One would be a follow-up ethnography in the Federal Reserve Bank of New York, where the anthropologist on our team worked as a research assistant while a young woman, during the eruption and immediate policy resolution of the Latin American Banking crisis of 1982, with the invention of the Brady Plan. This part of our research will draw on two methods in anthropology that are unorthodox but not unknown: “retrospective ethnography” (Maurer 2012b, Elyachar 2013) and “auto-ethnography” (Ellis et al. 2011).

We expect this work to give a different kind of insight into the role of models, epistemic values, and tacit knowledge in comprehending and responding to financial crises different from that offered by the research methodologies described in previous sections. For example, as has been established by past work, during the 1980s an implicit assumption prevailed in the regulatory community that sovereign borrowers were risk free (Elyachar 2013). This view, which was more an article of faith than an explicit theory, shaped the way that the crisis evolved. From a regulatory point of view, the Latin American debt crisis of 1982 was an impossible event, albeit in a

different sense than the 1987 crisis discussed in the first project described above. To what extent did regulators assume that private bank lending to sovereign borrowers in Latin America was risk free? Our research to date suggests that data sets from a particular period of finance and financial regulation reflect the assumptions of a previous era. If further research confirms this finding, then what are the implications for methods of financial regulation? How should we understand the fact that datasets to discern the degree of exposure of US money center banks to Latin American countries threatening default did not even exist in the early 1980s?

It is worth emphasizing how the framing of these questions already reflects our ongoing interdisciplinary conversation. In particular, while these questions were of interest to our anthropologist of finance, it was only through these interdisciplinary exchanges that we came to realize that assumptions such as those just described, concerning the risk level of sovereign debt, may be analogous to, or examples of, a kind of background assumption that pervades scientific (including economic) reasoning, and which have been studied extensively by philosophers of science interested in the role that such assumptions play in developing scientific knowledge. Ethnography is well suited to identifying the sorts of unstated background assumptions that are in fact made in practice, and to studying how those assumptions come to be challenged, replaced, or reified. Methods from philosophy of science, meanwhile, tend to focus on the epistemic role that such assumptions play in building models of financial markets; and content analysis of text can observe what assumptions are unstated vs. stated. Alone, neither of these disciplines' methods can give a complete picture of the role of modeling and of epistemic values in regulatory

practice, or in crisis; in an ongoing loop of research and interdisciplinary conversation and provocation, however, these methods complement one another well.

While there are publications on the Latin American debt crisis, and its relationship to the 2008 financial crisis (Frieden 1991, Chin and Frieden 2011), we have little knowledge of thinking at the time, as the early 1982 Debt Crisis was unfolding. Books about 1982 proliferated after 2008, but none of them convey a feeling of the time, and how people thought then about the emergent crisis. Beginning with notes from that period written by the anthropologist on our team, we will draw together reports, memos, notes, artifacts and memories of what it was like to work at the NY Fed during the 1982 Debt Crisis. We would then conduct initial stages of an ethnographic method of repeat visits to the field. Studies of how research at the NY Fed was reorganized in the intervening periods would allow us to shed light on, and differently consider, our colleagues' work on shifts in models in response to crisis.

In further stages of this research, we will use a snowball sampling technique, interviewing our anthropologist's former colleagues and employers at the New York Fed from 1982. This will help us trace out the evolution of models of finance and financial crisis from one period to the next. This will involve interviews with two classes of employees at the NY Fed during our anthropologist's tenure: those who became career bureaucrats at the Fed, and those who moved from a short period in financial regulation into the private sector and/or academia. This will help better understand how models are derived from "retrospective ethnography" already: how much did experience at the Fed during financial crisis impact on academic model making about finance and regulation in the scholar's subsequent career outside of the

Fed? To what extent does experience during one financial crisis and its resolution impact on the work of career regulators?

To ensure that our tentative research conclusions will not be too dependent on research carried out in one financial institution, we will also carry out research in researchers' home area of Orange County, which is a superb location from which to study financial crisis from a number of perspectives. Orange County is located smack in the center of the foreclosure crisis in the United States. Right in our home institution of UC Irvine, our colleague Prof. Katherine Porter was independent overseer of a mortgage settlement that provided up to \$18 billion in California borrower and homeowner benefits. Similarly, Irvine was home to a temporary office of the FDIC following the worst of the mortgage crisis, and numerous mid and low-level employees of the FDIC who were involved in bank closures subsequent to the mortgage-lending crisis still live in our region. Orange County is home to some leading financial firms who have had to adjust their strategies in the wake of the mortgage lending crisis and the subsequent financial crisis of 2008. And finally, Orange County went bankrupt in the wake of a derivatives crisis in 1994 (Jorion 1995). Thus, when the mortgage lending and financial crisis struck in Orange County after 2008, regulators and financial actors alike – at the institutional and individual level – possessed the memory of a previous crisis.

We will conduct interviews with employees of these local investment and regulatory groups, to better understand the process through which an abstract model of “financial crisis” was changed by encounters on the ground with closing branch offices, foreclosing on homes, and trying to revive profitability in the wake of a loss

of trust in the financial services industry. In line with our research at the NY Fed, we will ask: how much does experience of one crisis impact on reactions to, and models deployed in, the crisis to come? To gather informants, we will draw on an initial set of contacts in three financial services firms based in Orange County, and on our own colleagues involved in financial regulation starting with Prof. Porter. This part of our research will help us understand the mechanisms for transmission of knowledge and models among academia, the policy world, the financial services industry, and consumers in one geographic region. On the whole, experimental modalities of this research are in conversation with recent calls to strengthen and diversify our methods for studying financial regulation (Holmes 2009, 2013; Riles 2010; Maurer 2012a).

Concluding Remarks

Finance has the potential to be a powerful tool for advancing the common good and national interest. Yet the costs of a financial calamity on the order of the one we experienced in 2008 are in the trillions of dollars, by some estimates. We believe that to avoid future crises of this magnitude, investors, regulators, and academics need to move beyond the silo-thinking that characterized work on markets before the crisis -- and, we fear, has dominated attempts to understand and respond to the crisis post-2008. To this end, we have called for a critical interdisciplinary approach to understanding financial markets, of a sort that involves in-depth and detailed engagement with the assumptions, methods, and practices often taken for granted by researchers across the social sciences, natural sciences, and humanities.

Our argument that such an approach is called for, at least in this case, is that the theoretical and methodological tools of any of the individual disciplines that study markets are suited to studying only limited aspects of financial markets, crises, and regulation. To support this claim, we have described four ongoing projects that arose within intra-disciplinary attempts to address a specific question: with what kinds of models of finance and financial crises do market actors and regulators operate, and with what consequences? In describing each project, we identified concrete ways in which each discipline could address only parts of the question, and how the considerations from the other fields might be used to identify hidden assumptions and fill gaps left by intra-disciplinary approaches. In this way, we also showed how interdisciplinary research might provide a kind of broader and deeper insight than any individual field would allow, in particular for problems when disciplinary thinking has failed us.

Finally, and echoing back to the quote from Gillian Tett with which we began this essay, our hope is that the insights to be gleaned from this sort of interdisciplinary work can help put together the macro-pieces that investors and regulators need to understand to anticipate, comprehend, and respond to future crises. Indeed, insofar as the kind of interdisciplinary engagement we describe here is itself a response to our own frustrations as researchers struggling to see how our work fits into a broader understanding of markets, we believe that the scholarly challenges that make this sort of interdisciplinary work necessary, and the difficulties that arise in trying to perform the work, are strongly analogous to considerations facing investors, regulators, and other market actors who are faced with synthesizing a wide array of approaches to

and practices within markets. It is our hope that facing these challenges within the academy may provide fruitful insight into how future policy-makers should go about developing the large-scale understanding necessary for effective market regulation.

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