

The Social Value of Finance

Mathijs van Dijk



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The Social Value of Finance

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Prof.dr. Mathijs A. van Dijk

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Rotterdam School of Management
Erasmus University
P.O. Box 1738
3000 DR Rotterdam
E-mail: madijk@rsm.nl

Samenvatting

In deze oratie benader ik ‘The Social Value of Finance’ vanuit twee invalshoeken. Ten eerste beschouw ik de maatschappelijke waarde van ‘finance’ als wetenschappelijk vakgebied. De reputatie van het vakgebied is onder druk komen te staan als gevolg van de recente financiële crisis, met als belangrijkste kritiekpunt het onvermogen van financieel economen om de crisis te zien aankomen. Ik ben van mening dat deze kritiek in belangrijke mate ongegrond is, omdat het financiële stelsel zo complex is geworden dat het vrijwel onmogelijk is om te voorspellen wanneer een zware financiële crisis zal optreden, laat staan wat de gevolgen zullen zijn. Dat wil echter niet zeggen dat er geen reden is tot zorg over de stand van finance als wetenschappelijk vakgebied. De maatschappelijke waarde van ons vakgebied zou groter kunnen zijn als we ons meer zouden richten op brede vraagstukken, als we meer oog zouden hebben voor andere vakgebieden en andere methodes, en als we bescheidener zouden zijn over de omvang van onze kennis.

Ten tweede presenteer ik, in navolging op deze bespiegelingen, een nieuwe onderzoeksagenda over de maatschappelijke waarde van ‘finance’ in de zin van een financieel stelsel (dat wil zeggen banken en financiële markten). Hoewel het idee dat het financiële stelsel een belangrijke bijdrage levert aan de samenleving wijdverspreid is, bestaat er verrassend weinig onderzoek naar de mate waarin financiële stelsels in verschillende landen hun maatschappelijke functies vervullen. Het eerste project in deze onderzoeksagenda laat zien dat de kosten voor de maatschappij als het mis loopt in het financiële stelsel immens kunnen zijn. Op basis van gegevens over 187 bankencrisis in 126 landen vind ik een significante afname in de gemiddelde levensverwachting, in geboortecijfers en het aantal schoolgaande kinderen, en een significante toename in armoede en het aantal tienerzwangerschappen. Deze effecten treden voornamelijk op in minder ontwikkelde landen. In het tweede project beoog ik een internationaal vergelijkend empirische onderzoek naar de mate waarin financiële stelsels twee van hun belangrijkste maatschappelijk functies vervullen: de allocatie van kapitaal en het helpen van huishoudens bij het opvangen van inkomensschokken. In het derde project zal ik de ontwikkeling en maatschappelijke bijdrage analyseren van een aanzienlijk aantal nieuw opgerichte aandelenmarkten in de afgelopen 25 jaar.

Abstract

In this address, I consider “The Social Value of Finance” from two perspectives. First, I reflect on the value for society of *finance as an academic field*. The recent global financial crisis has undermined the reputation of the field of finance, with the inability to foresee the crisis as one of the most prominent criticisms voiced by the media and the general public. I argue that this criticism is by and large unwarranted, primarily because the financial system has grown to be so complex that it is virtually impossible to predict the occurrence (let alone the timing and extent) of severe financial crises. However, I do believe that there are reasons for concern about the academic field of finance. The social value of finance as an academic field could be greater if we were more focused on broader questions, more open-minded about other fields and methodologies, and more modest about the extent of our knowledge.

Second, to follow up on these aspirations, I present a new research agenda on the value for society of *finance in the sense of a financial system* (that is, the social value of banks and financial markets). Although the belief that financial systems fulfill an important role in society is widespread, there is surprisingly little evidence on whether and how financial systems in different countries actually fulfill their social functions. The first project in this research agenda shows that significant social costs can be incurred when finance goes wrong. Using data on 187 banking crises in 126 countries, I find a significant decline in average life expectancy, birth rates, and primary school enrollment, and a significant increase in poverty and adolescent fertility in the six years after the start of a banking crisis. These effects mainly stem from less-developed countries. In the second project, I propose a cross-country empirical analysis on the extent to which financial systems fulfill two of their key social functions: capital allocation and consumption smoothing. In the third project, I will examine the development and contribution to society of a considerable number of newly established stock markets (or “nascent markets”) over the past 25 years.

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1. Introduction

*Mijnheer de Rector Magnificus,
Geacht College van Decanen,
Mijnheer de Decaan van de Rotterdam School of Management,
Distinguished colleagues,
Dear friends and family,*

The field of “finance” or “financial economics” has come under increased scrutiny in recent years. Financial economists have been widely criticized for failing to anticipate the recent global financial crisis. Economics is viewed by some as a largely academic profession with limited relevance for real-world problems. Ronald Coase (2012), a Nobel laureate in economics, recently argued that “the degree to which economics is isolated from the ordinary business of life is extraordinary and unfortunate.”

The period surrounding my appointment as a professor of financial markets at Rotterdam School of Management, Erasmus University (endowed by the Erasmus Trustfonds) has been a good time for me to reflect on these issues and on my own position as a financial economist. This inaugural address aims to present my views on recent developments in the profession, outline my research plans and ambitions going forward, and highlight some initial empirical findings of my new research agenda.

The title of this address – “The Social Value of Finance” – can be interpreted in two different ways. In the first part of my talk, I will reflect on the value for society of *finance as an academic field*. In the second part, I will argue that we need more research on the value for society of *finance in the sense of a financial system* (that is, the social value of banks and financial markets).

2. Finance and the crisis

“Why did nobody notice it?” was the question the British Queen Elizabeth asked during a visit to the renowned London School of Economics in November 2008, just after the collapse of Lehman Brothers in September 2008 that was widely viewed as the starting point of the global financial crisis. Having witnessed a decline of her personal wealth by 25 million pounds during the credit crunch (The Telegraph, 2008), this was more than just a matter of professional interest. In the letter from the British Academy (2009) that she received a few weeks later in response to her question, the inability of the economics profession to foresee the crisis was attributed to “a failure of the collective imagination of many bright people (...) to understand the risks to the system as a whole.” This failure has inspired a process of introspection by many economists, including some of the most prominent. Nobel laureates such as Paul Krugman (2009) and Joseph Stiglitz (2010) interpreted the inability to predict the crisis as an indication of fundamental flaws in the academic field of economics.¹

Frankly speaking, I do not take the criticism that economists should have foreseen the crisis too personally. For a start, severe financial crises are, and always will be, to a large extent unpredictable. The increasing complexity of the international financial system makes it even harder to foresee when certain shocks to the financial system will occur and how they will play out.

The recent crisis originated from problems on the U.S. market for subprime mortgages and the bursting of the housing bubble. Although the bursting of the bubble was predicted almost a year before the collapse of Lehman Brothers by Robert Shiller (2007), one of the three recipients of the most recent Nobel prize in economics, he did so in full awareness that predicting the timing and extent of such turning points in the economy is a tricky exercise. In particular, he wrote that the initial house price decline at the time looked “(...) like the beginning of the end of the boom, though, of course, one can never be sure.”

¹ These statements, in turn, have prompted a lively discussion among academic economists. In a direct response to Krugman (2009), Cochrane (2011) writes that “the problem is that we don’t have enough math.” Colander (2011) reviews the history of economic thought and argues that the “future of the science of economics is mathematics” – but also that we need “a critical approach (...) and a commitment to reporting the results with the modesty that our lack of understanding demands” as well as “more economists trained in the subtlety of policy issues and institutional realities.” De Long (2011) calls for “more monetary historians and historians of economic thought and fewer model-builders (...) more Eichengreens, Shillers, Akerlofs, Reinharts, and Rogoffs – not to mention a Kindleberger, Minsky, or Bagehot.” My views accord more with the latter statements.

Similarly, in the years before the crisis, various reports published by policy institutions such as the BIS, the OECD, and the IMF included warnings about excessively loose monetary policy, a boom in house prices, global imbalances, and the hazards of risk management models such as Value-at-Risk and of financial innovations such as collateralized debt obligations (Shigehara and Atkinson, 2011). In 2005, Raghuram Rajan was among a minority of academic economists who realized early on that deregulation, technological advancements, and institutional changes had increased the risk of a meltdown of the financial system. Inevitably, these warnings came without precise predictions of the timing of any realization of such risks, and with a variety of qualifications and reservations.

What has not helped in lending credibility to these and other warnings is that the global economy and financial system looked remarkably stable in the five years preceding the crisis. Macroeconomic fluctuations seemed to have undergone a “great moderation” (Stock and Watson, 2002), financial institutions seemed healthy and stable, global markets were flush with liquidity (IMF, 2005), and market volatility had been low for an extended period across different countries and asset classes (BIS, 2006). Like volcanoes, economies and financial systems can be calm for many years before succumbing to underlying pressures.

Even economists who did see the tell-tale signs of an upcoming crash in the housing market and a subsequent financial crisis could not possibly have foreseen the unprecedented scale of the crisis. The bursting of the housing bubble initially resulted in several hundred billion dollars of losses (Brunnermeier, 2009). Although such losses are by no means small potatoes, they turned out to be just a sliver of the overall economic value destroyed as a result of the crisis, estimates of which run as high as 19 trillion dollars for U.S. households alone (U.S. Treasury, 2012).

In the current global financial system, a relatively small shock can have tremendous consequences through a variety of feedback loops, network effects, and liquidity spirals (Brunnermeier, 2009). Put simply, banks and financial markets around the world have become so complex and interlinked that it has become difficult to anticipate how the incentives of and interactions between various economic agents (such as households, companies, financial institutions, and regulators) can cause a problem that started out in one corner of the financial system to spill over globally.²

² The central role of finance in the economy has led several macroeconomists (e.g., Wyplosz, 2009) to call for more attention to the financial sector in macroeconomic research. My address can be viewed as a call for more attention to macro questions in finance research.

Furthermore, it is not obvious to me that predicting the next financial crisis is the task of academic economists. Is predicting the next tribal war the task of academic cultural anthropologists? I have always considered our job to enhance our understanding of the behavior of economic agents and the functioning of the economy. I do believe that academic research on economics should have policy implications for these economic agents; and research inspiration often does and should come from current developments and events. Economists also have a role to play in signaling imbalances in the economy and financial system, though economists at policy institutions such as central banks are not only in a better position to, but also formally have the duty to do so.

Be that as it may, I do not believe that economists' key strength lies in forecasting, and especially not in forecasting economic turning points. Some observers have suggested that "economic forecasters are there to make weather forecasters look good." Our understanding of meteorology may be as limited as our understanding of economics, but forecasting economic events faces the additional difficulty that decisions based on the forecasts can alter the outcomes (Stevens, 2011). Our strength lies much more in analyzing the incentives and preferences that drive economic decision making, and thus in understanding the potential (adverse) consequences of policy changes. And even this is a formidable challenge.³

Finally, I have not experienced a great deal of encouragement in my professional environment to pay attention to current economic events, or to develop views on the future of the financial system. Especially junior researchers are primarily rewarded for publishing papers, which not only generally takes so long (five years from initial idea to acceptance by a journal is no exception), but also requires so much effort and focus that studying current affairs seems to make little sense. As a result, in the words of Rajan (2011), many academic economists have disengaged from current events and simply stopped paying attention. I will revisit the current incentives for academic researchers later on.

³ My favorite illustration of the power of economic reasoning stems from *The Armchair Economist* by Steven Landsburg (1995). Chapter 1 analyzes the impact of the introduction of mandatory seat belts in U.S. automobiles in the 1960s on the number of accidents and road fatalities. Anyone's gut feeling might be that seat belts promote safety. And indeed, the probability of getting killed in a road accident decreased. However, the reduced threat of being killed in an accident also induces people to drive more recklessly, and the number of accidents actually went up. The net result on road fatalities is an empirical question. Academic research (Peltzman, 1975) suggests that the net effect was zero: the total number of driver deaths remained essentially unchanged. However, pedestrian deaths increased, since pedestrians were exposed to more reckless driving without an increase in protection.

3. Finance in crisis?

Nonetheless, the debates about finance in the wake of the financial crisis have triggered me to reflect on finance as an academic field. Since the start of my PhD, I have now been involved in academic finance research for almost 18 years, and I hope to remain active for a period that is at least as long. For me, this is the right time to figure out what my views on the field really are and what I would like to contribute.

Let me emphasize upfront that I believe that the achievements of the field have been considerable. Finance has developed systematic and often sophisticated approaches to understanding decision making by households, companies, financial institutions, and regulators. The field has also made significant headway in understanding how the interactions of all those decisions play out in the market place. The relevance of the field for practice is beyond dispute. For example, academic research has fundamentally changed the investment management industry, has spurred financial innovation, and has provided key input to policy debates about bank regulation.

Occasionally, I run into people (often representatives of the "hard sciences" – even at meetings of the Netherlands Organisation for Scientific Research or NWO) who voice the opinion that economics is not a science. I do not get upset easily, but on these occasions I am fuming! Although my sensitive response could be interpreted as an indication that there must be some truth to this claim, I truly believe there is not. The Oxford Dictionary defines science as "the intellectual and practical activity encompassing the systematic study of the structure and behaviour of the physical and natural world through observation and experiment." There is no doubt in my mind that this applies to the field of finance (and similarly to other "soft sciences").

That said, there are reasons to be concerned about the state of finance as a scientific discipline.

Let me take my own specialization as an example. Most of my work is on "asset pricing" – which is aimed at understanding how the prices of financial assets (such as stocks, bonds, and derivatives) are determined. This surely is a relevant topic. A better understanding of asset prices can aid decision-making by investors (such as pension funds), risk managers, and regulators. What I am less sure about is whether the amount of time and effort the profession as a whole invests in this topic is justified from the perspective of the field's value for

society. Utpal Bhattacharya and coauthors (2013) review published papers in the leading academic finance journals over 2008-2012 and conclude that “asset pricing remains hot”; no less than one third of the articles in the Holy Trinity of finance journals are dedicated to asset pricing related topics. This is all the more remarkable when one realizes that the vast majority of these articles focus exclusively on the U.S. stock market. Andrew Karolyi (2012) documents that only 23% of all empirical studies published in the top finance journals study non-U.S. markets. It goes without saying that the U.S. is an important market, but it is also just one of many financial markets around the world. Moreover, it is humbling to realize that after all those years of asset pricing research, our knowledge of what determines stock prices is still very incomplete.

A related concern is the field’s almost sacred respect for theory. And by theory, I mean theory in a particular sense, namely mathematical modeling. The most salient example is the Capital Asset Pricing Model, or CAPM. This model was developed in the 1960s by William Sharpe and others, with the 1990 Nobel prize as the result. It describes how stock prices are determined based on assumptions about the behavior of investors. It is intuitive, mathematically elegant, and delivers a clean prediction that is relevant for many practical applications, including the estimation a firm’s cost of equity capital. In many respects, it seems a perfect example of the power of finance as a scientific discipline. Except, we have known for at least 25 years that the CAPM is a very poor description of reality. Yet, in the face of overwhelming empirical evidence that it is simply not a good model, we still routinely teach the CAPM in our undergraduate and MBA programs. It remains our key prescription for estimating the cost of equity capital. The unfortunate “success” of this prescription is indicated by survey studies that show that the CAPM is now the dominant method that Chief Financial Officers (CFOs) of companies around the world use to estimate their cost of equity capital (Graham and Harvey, 2001; Brounen, de Jong, and Koedijk, 2004).⁴

Taken together, the overemphasis on certain research questions and the excessive attachment to theory leave the impression of a field that is aloof, self-perpetuating, and too focused on purely academic questions. A likely explanation is the “physics envy” that seems widespread among academic

4 Ironically, despite the intrinsic respect most financial economists have for theory, publishing theoretical papers is widely regarded as more difficult than publishing empirical papers (as highlighted by the editorial panel discussion on “Publishing in finance” during the 2013 meetings of the European Finance Association). Bhattacharya et al. (2013) find that roughly one quarter of the articles in the top three finance journals are theoretical.

economists.⁵ Perhaps driven by a similar sensitivity as I show when the status of economics as a science is disputed, economists have looked for refuge in mathematics and statistics since at least the 1950s (Lo and Mueller, 2010), in an apparent attempt to become more like natural scientists.

The resulting “mathematization of economics” has been beneficial in many ways. Mathematics forces economists to be specific about their ideas and arguments, ensures a certain degree of analytical rigor, provides a common and precise language, and can yield unexpected insights.⁶

But economics is not a natural science. There are no such things as the “fundamental laws of economics.” The behavior of people is notoriously complex and elusive, and can be influenced by the theories we develop. Hence, making precise statements about financial markets or the economy – systems of millions of interacting people – is an illusion. This is what makes the study of economics so difficult, and at the same time, so interesting!

Perhaps precisely because of the inherent ambiguity of social sciences, the precision of mathematical models is comforting. As a result, a rather arcane strand of economics has developed that was termed “blackboard economics” by Ronald Coase (1991) and “a boy’s game in the sandbox” by Deirdre McCloskey (1997). It involves building mathematical models that serve little more practical use than soliciting appreciation from peer academic economists.⁷

5 My own degree of physics envy is revealed by the title of my NWO Vidi project: “Liquidity black holes” (borrowed from Morris and Shin, 2004).

6 Berk and Green (2004) is one of my favorite examples of a theory that provides an unexpected explanation for an age-old puzzle: professional asset managers do not consistently outperform the market, while many investors seem to be convinced that at least some asset managers have skill. Berk and Green model the institutional setting of the asset management industry and show that when investor flows chase performance, larger funds have a harder time outperforming, and managers have incentives to grow their funds, lack of performance persistence can be fully consistent with some managers having significant skills

7 One of the things that strikes me as remarkable is the ease with which economists talk about maximizing social “welfare” (usually based on some specific utility function) without having anywhere near the necessary (philosophical) background to comprehend what social welfare really means.

Truth be told, the reasoning underlying mathematical models in economics can be very appealing, and I have experienced how easy it is to be seduced. Before you know it, you truly believe their magic.⁸ In his book *The Myth of the Rational Market*, journalist Justin Fox (2009) provides a compelling historical account of how a large part of the academic finance profession grew so attached to one particular idea (that investors are rational and stock markets are always right), that it took decades and lots of evidence to the contrary to convince many of us that markets can also be wrong.

Other examples include arcane asset pricing models like the Consumption CAPM, whose underpinning reasoning is so compelling to financial economists that many seem to feel the model simply *has* to be true. However, the type of economic behavior these models rely on is far from realistic and they have very little empirical support (Nagel and Singleton, 2011). Personally, I do not even see the point of testing the literal predictions of such models. There is no way they can fully explain how asset prices are determined, given the myriad of types of human behavior, agency problems, frictions, and institutional factors in real life. I see much more merit in empirical – ad hoc if you will – approaches to the study of asset prices that may or may not be inspired by one or more formal theories.

David Colander (2007) seeks the roots of these developments in the graduate education of economists. In a survey among economics PhD students at seven top U.S. universities, he asked what attributes they thought are needed for a successful career as an economist. A “thorough knowledge of the economy” was deemed very important by 9% of the students, and unimportant by 51%. In contrast, “excellence in mathematics” was deemed very important by 30%, and unimportant by 14%. Strikingly, Colander concludes that the situation has improved since his previous survey with Arjo Klamer in the 1980s!

Economists are also remarkably arrogant with respect to the position of their discipline within the social sciences (see also McCloskey, 2002). Seventy-seven percent of the PhD students agreed with the statement “Economics is the most scientific of the social sciences.” Frankly speaking, such sentiments are not alien to me, but their origin is a puzzle. I know relatively little about anthropology, psychology, and sociology, and these fields are not even covered in passing in most economics PhD programs.

8 Caballero (2010) argues that macroeconomics has become “mesmerized with its own internal logic” and that its workhorse dynamic stochastic general equilibrium is “an irresistible snake-charmer.” He warns that the field is currently on a track in which “academic macroeconomics [is] playing its internal games and leaving the real world problems mostly to informal commentators.”

This arrogance might also explain the disdain many economists have for “soft” methodologies such as surveys, case studies, interviews, and field research. The preferred toolkit of an economist consists of mathematics and econometrics. In fact, what drew me to the undergraduate program in econometrics at this university as an 18-year old was the exact nature of the training. I slowly and painfully discovered that although econometrics is a powerful tool to tackle many research questions, it rarely provides the complete picture, and it can be particularly blunt when it comes to some of the biggest and most interesting questions. I long for a top finance journal publication on an anthropological field study at the risk management department of a major bank, which I believe could add a great deal to our current understanding of how risk management works in practice.⁹

In terms of methodology, the academic finance field has witnessed a near revolution in the adaption of (quasi) natural experiments and econometric techniques to mitigate so-called endogeneity or causality concerns in the past decade, in particular in the area of corporate finance (Bowen, Frésard, and Taillard, 2014). Identifying causal relations is the Holy Grail in economics, and these developments have undoubtedly benefited the profession (Angrist and Pischke, 2010). However, my concern is that too much emphasis on identification will drive out research on macro questions, for which, in the words of renowned econometrician Edward Leamer (2010), “Our understanding of causal effects (...) is virtually nil, and will remain so.” In my view, Leamer’s provocative statement does not undermine the relevance of research on macro questions. In fact, imprecise answers to broad questions (however unsatisfactory to natural scientist hopefuls) may be at least as valuable as precise answers to narrow questions.

What is the role of incentives in all of this? Economists should not be surprised that my answer is: quite large. Following the tradition of Anglo-Saxon universities, our school adopted the “tenure track system” around seven years ago, and at the same time started hiring on the international job market. The result has been a tremendous increase in productivity, and I feel fortunate to be surrounded by so many talented and dedicated researchers. But the sometimes overwhelming pressure to produce publications, in combination with the peculiarities of the reviewing process at journals (Armstrong, 1997; Spiegel, 2012; Welch, 2013), have induced us to focus on rather narrow papers that stay close to the literature in terms of topic and method, instead of reflecting more broadly

9 I am grateful to Harry Barkema for an inspiring conversation on this topic. An exception to the rule that economists are hesitant to consider alternative methodologies is Brown (2001), who carries out an in-depth case study of the risk management process at a U.S. manufacturing firm.

on questions that we deem to be most relevant.¹⁰ As Harry van Dalen (2010) notes in his inaugural address, it is peculiar that in our relentless commitment to measuring and rewarding publication output, we seem to ignore the large literature in economics that suggests that such incentives can be counterproductive in professions like ours. I admit that there is no easy solution to these issues. The tenure track system is here to stay and publications remain an informative signal about research quality.

4. The social value of finance

So what is the value for society of finance as an academic field? In my view, our field has a lot to offer, but our contribution to society could be greater if, as a field, we attained a better balance in the subjects we study (in particular, a greater focus on broader questions), a greater dose of healthy skepticism about the virtues of mathematics and econometrics, a more open mind regarding other disciplines and other methodologies, and more modesty in recognizing the limitations of our knowledge.¹¹

My recommendations are three-fold:

- Junior researchers should be encouraged (either informally or formally through doctoral education) to seek more exposure to the history of economic thought and the philosophy of science, to other disciplines and methodologies, and to the real-world economy.
- Senior researchers should be encouraged to develop their own, broad research agenda and views (independent of the publication process) and to engage in dialogue with scholars in other fields and in broader debates about their field, also with people outside academia.
- Deans, department chairs, and journal editors should be bolder and more confident in following their own independent opinion about the contribution, innovation, and relevance of academic research.¹²

It is easy to offer recommendations, but it is much harder to follow them yourself. As an empirical asset pricing researcher who primarily analyzes large databases with econometric methods, and who is remarkably oblivious of the workings of finance in the real world, of other academic fields, and of alternative methodologies, it is clear to me that there is work to be done.

¹⁰ In a survey of studies on the peer review process of academic journals, Armstrong (1997) concludes that this process tends to favor quality over importance, and tends to disadvantage innovative research, especially when it conflicts with existing belief systems.

¹¹ These aspirations for the field should not distract from various encouraging developments, such as the advent of behavioral and household finance, the acknowledgement of the importance of frictions and institutional factors, the greater attention for the “economic significance” of empirical results (Karolyi, 2011), the increased awareness of identification issues, and the at least somewhat greater interest in experiments and surveys (such as the work of Graham and Harvey). Also, in terms of topics, the recent crisis has induced a significant shift in the attention of the academic finance community to broader and more relevant questions.

¹² As an example of such boldness, a well-known finance professor at the University of Chicago’s Booth School of Business once told me that the criterion for awarding a PhD at Chicago is that the main dissertation paper should be publishable in one of the top finance journals. However, he added, if a particular paper has been published in one of the top finance journals, this does not imply that he concludes this criterion is satisfied!

I will continue to be active in empirical asset pricing and market microstructure, topics that are relevant and simply fun to work on. But more than twenty years after my start as an undergraduate in econometrics here in Rotterdam, I have also rediscovered my original motivation to study economics: addressing macro questions that are relevant for society. In particular, in this inaugural address I would like to announce a new research agenda that deals with the social value of financial systems.

The inspiration for this research agenda came in the fall of 2012, when I was teaching the master course “Investments” for the seventh subsequent year. As always, I started my first lecture by presenting an overview of the financial system and the role of financial markets and banks in channeling money from savers (that is, households) to companies. Then, following the textbook of Bodie, Kane, and Marcus (2011), I routinely presented a list of the various functions financial systems fulfill in society, such as capital allocation, liquidity provision, consumption smoothing, risk sharing, and information aggregation. But as I was going through the list in the classroom, it suddenly struck me that we know remarkably little about whether and how financial systems in economies around the world actually fulfill these social functions.

Certainly, a huge literature has established a positive relation between the financial development of a country and its economic growth (Levine, 2005). But the recent crisis has exposed the large potential adverse consequences of (too) highly developed financial systems. And recent academic research questions the evidence of a positive link between finance and growth (Dabós and Gantmann, 2010; Rousseau and Wachtel, 2011; Arcand, Berks, and Panizza, 2012). Furthermore, the finance and growth literature neglects other social purposes of financial systems besides stimulating growth. For example, consumption smoothing does little to help growth, but is of great value to households.

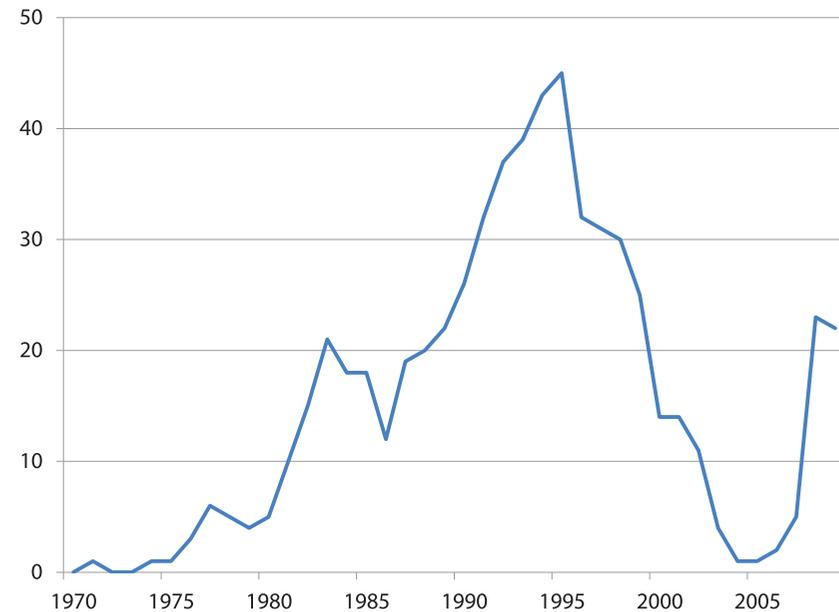
In the remainder of this talk, I will outline three research projects on the social value of finance, in different stages of development. Ironically, I will start with a study of the costs to society when finance goes wrong. I will then directly examine two of the key social functions of a financial system: capital allocation and consumption smoothing. I will conclude with a research project that analyzes the development and contribution to society of a set of newly established stock markets, which I call “nascent markets.” The ultimate goal of this research agenda is to provide insights that can help governments to organize and regulate the financial system in such a way that its value to society is optimized.

5. The social costs of financial crises

The global financial crisis has spurred tremendous interest in the consequences of financial crises. Recent research shows that financial crises are commonplace throughout history and across rich and poor countries, and that their economic consequences can be huge (Dell’Ariccia, Detragiache, and Rajan, 2008; Claessens, Kose, and Terrones, 2009; Reinhart and Rogoff, 2009, 2010; Laeven and Valencia, 2012). The economics profession traditionally measures the well-being of a society by its GDP, or by other economic indicators such as unemployment and inflation. However, focusing on these indicators fails to do justice to the wealth of different aspects of a society’s well-being (Stiglitz, Sen, and Fitoussi, 2010).

We know little about the impact of financial crises on society at large. Media reports suggest potentially severe effects on health, education, and poverty. For example, it was reported that in the wake of the recent global crisis, in Greece, suicides went up by 45% (The New York Times, 2013) and HIV infections by 60% (NPR, 2012); in Spain, fertility fell almost 8% (BBC News, 2013); in Swaziland, schools were forced to shut (BBC News, 2011); and in the U.K., an additional 4% of 18-25 year olds went into poverty (The Independent, 2013).

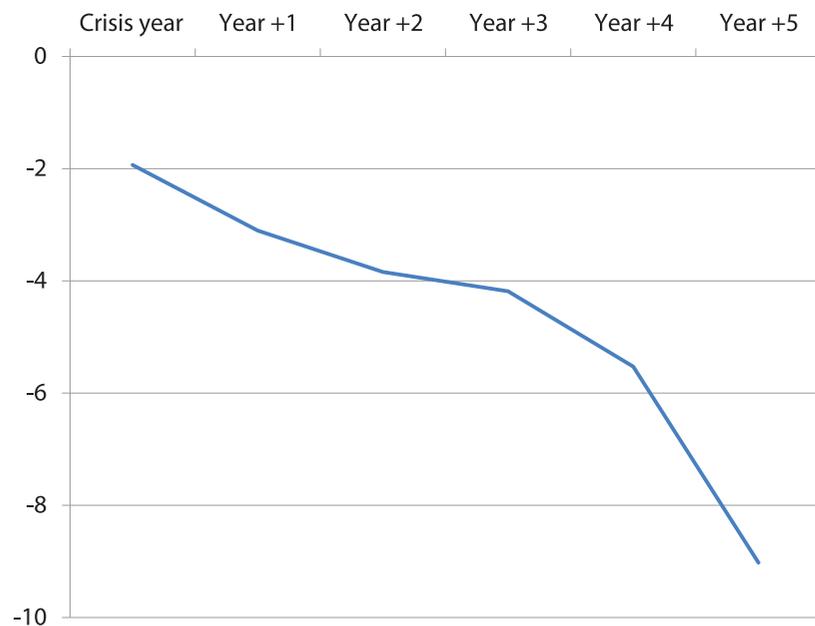
Figure 1: Number of countries in a banking crisis (out of 126, 1970-2009)



In a new study (van Dijk, 2013), I provide a systematic analysis of the social costs of financial crises. I identified 187 banking crises in 126 countries over the period 1970-2009 based on several public databases. Figure 1 shows the number of countries experiencing a banking crisis for each year in the sample period 1970-2009. The figure indicates that financial crises are remarkably common, with 30 to 40 of the 126 countries experiencing simultaneous banking crisis in the early to mid-1990s. There is a clear spike in the final two years of the sample period that corresponds to the recent global financial crisis, although perhaps not as large as expected. All but two of the 126 countries experienced at least one banking crisis over this period. On average, these countries faced 1.5 crisis episodes and were in a crisis for five years out of 40.

Perhaps the most striking result is the strong overall effect of financial crises on the health of the population in a country. Figure 2 shows the cumulative impact of a financial crisis on the average life expectancy in the country, measured over the five years following the crisis. The figure suggests that average life expectancy decreases by a few months soon after a crisis hits, and shows an even greater drop in subsequent years. The average life expectancy in the sample period is around 66 years. My results suggest that in the five years following the crisis, people's life expectancy decreases by nine months, which is a huge effect.

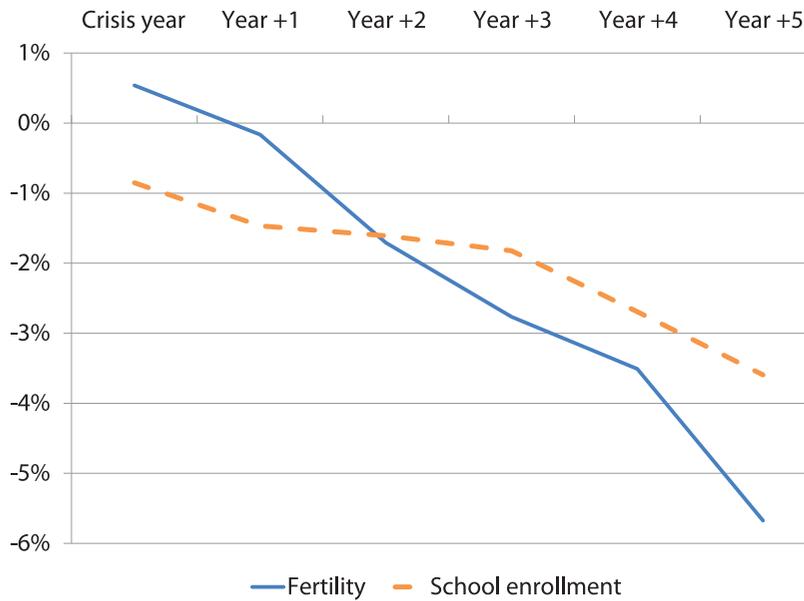
Figure 2: Impact of financial crises on average life expectancy (in months)



My analyses so far do not reveal the underlying causes of the impact of financial crises on health. We know from prior research that economic crises are associated with increased mortality rates due to greater occurrence of cardiovascular diseases, suicides, and homicides (Stuckler, Meissner, and King, 2008; Stuckler et al., 2009a). I also find an increase in HIV prevalence of around 20-25% in the five years after a crisis, especially in less developed countries. Longer-term health effects might be driven by reduced quality of or access to healthcare, increased rates of addiction to alcohol and drugs (Kentikelenis et al., 2011), and/or greater poverty. Furthermore, in developed countries, economic crises have been associated with worsening diets; McDonald's and Kentucky Fried Chicken have been among the few companies that increased recruitment during the most recent crisis (Stuckler et al., 2009b). Consistent with some of these potential channels through which financial crises can affect health, I find a significant decrease in public and private expenditure on healthcare in the years following the crisis. I also detect a 10% increase in poverty immediately following the crisis.

The consequences of financial crises extend beyond health and healthcare. Figure 3 shows the impact of a banking crisis on the fertility or birth rate and on the fraction of children attending primary school. The figure indicates that in the five years following the average crisis, birth rates decline by over 5%, possibly because parents are more pessimistic about the prospects of children. I also find that 3% fewer children enter primary school in the aftermath of a financial crisis. This suggests that schools are forced to shut in crisis times, and/or that parents keep their children out of school to save money or to let them help in making a living. Again, both of these effects are concentrated in less developed countries.

Figure 3: Impact of financial crises on fertility and primary school enrollment (% relative to mean)



Overall, my study shows that financial crises tend to come at a great cost to society. Not only is a country's economy hit by a crisis, so are the health, education, and poverty of its people. More research is needed to identify the channels through which these various consequences for society arise, and to analyze which government policies could help to reduce the social costs of financial crises.

It is not obvious to draw conclusions from these results regarding the social value of finance. On the one hand, these findings underline the damage that problems in the financial system can cause; on the other hand, they could be interpreted as evidence that well-functioning financial systems are crucial for economic and social well-being. In the next project, I propose a direct analysis of the social functions of financial systems.

6. The social functions of finance

The latest book by Robert Shiller (2012), *Finance and the Good Society*, argues that financial systems are of great value to society. However, to my surprise, research on the extent to which financial systems in various countries fulfill their key social functions is limited. My aim is to carry out a cross-country empirical analysis of at least two of these functions: capital allocation and consumption smoothing.

6.1 Capital allocation

Capital allocation is traditionally viewed as the most important social purpose of the financial system. Banks and financial markets are supposed to identify and finance the entrepreneurs or companies with the greatest potential for adding economic value. Efficiently allocating scarce resources to their greatest value use fosters creative destruction (Schumpeter, 1912) and enhances economic prosperity (Bagehot, 1873; Hicks, 1969).

Jeffrey Wurgler (2000) is among the few economists who have actually tried to measure which types of financial systems are better at allocating capital. He proposes a new measure for the efficiency of capital allocation in a country and finds that it is higher for developed countries such as Germany and Hong Kong than for less developed countries like Bolivia and Swaziland. In particular, Wurgler shows that the efficiency of capital allocation is greater in countries with more developed financial sectors. He concludes that “financial markets appear to improve the allocation of capital.”

However, Wurgler's study is silent on how financial markets help to achieve efficient capital allocation. Together with Rogier Hanselaar, I intend to carry out a specific analysis of the role of stock markets in allocating capital. Our starting point is the idea that stock markets influence capital allocation through the pricing mechanism: the stock price determines how much money a company is able to raise by issuing public equity. We will develop a new measure of the allocative efficiency of stock markets that evaluates whether companies with the best prospects are indeed able to raise the greatest amount of capital on the stock market. We will distinguish between the time when companies issue public equity for the first time (the initial public offering or IPO) and when already listed companies raise additional equity (the seasoned equity offering or SEO). Subsequently, we will examine whether cross-country variation in this new measure of capital allocation efficiency can be attributed to different

characteristics of these countries' stock markets such as their size, their regulation, transparency, and price efficiency. Unfortunately, the timing of this address is too early to present some initial results.

6.2 Consumption smoothing

In a second study that is in a slightly more advanced stage, I plan to study cross-country variation in the degree of consumption smoothing. Consumption smoothing refers to the idea that people tend to strive for a stable consumption pattern. To give an example, as a farmer you would not want your ability to sustain your family to depend on the current year's crop only. You want to be able to save in the good years and/or borrow in the bad years so that your consumption does not vary wildly across the years. This idea has been widely accepted since Friedman's (1956) permanent income theory and Modigliani and Brumberg's (1954) life-cycle model.

The financial system can help people to smooth their consumption by saving money and safeguarding their pensions, by insuring against negative income shocks, and by borrowing against future income (e.g., through a mortgage to finance their homes). However, we know next to nothing about the extent to which people in different countries are actually able to smooth their consumption, and about whether financial systems indeed fulfill their social function of making this possible.

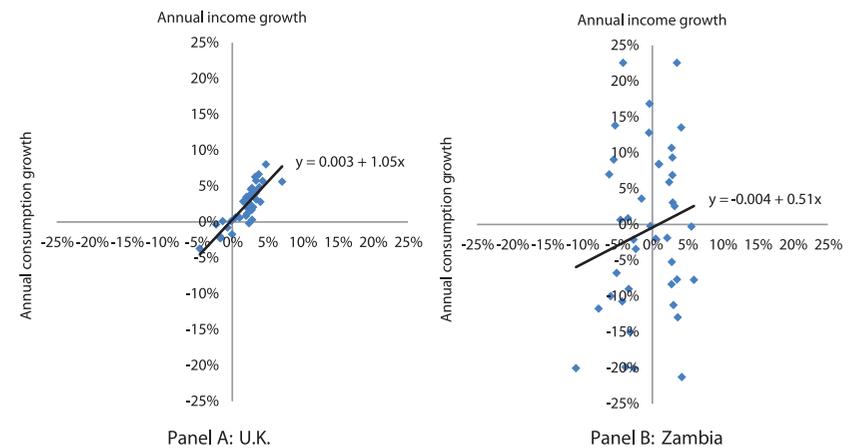
Although the ability to smooth consumption might be taken for granted in developed countries, the picture is altogether different in less developed parts of the world. What I personally experience as a striking reminder of what happens in the absence of a well-functioning financial system is the large number of partially finished houses encountered on visits to Africa. Without a properly functioning banking system, people cannot borrow against their expected future income and have to wait for the next good crop before they can continue building their homes.

The traditional measure of the degree of consumption smoothing is the elasticity of household consumption to income (Campbell and Mankiw, 1989, 1990). The idea is that if there is a one-to-one relation between income shocks and changes in consumption, the degree of consumption smoothing is weak. However, my initial results suggest that the logic of this measure does not translate to less developed countries. Figure 4 shows scatter plots of the annual consumption growth rates of the U.K. and Zambia on the vertical axis relative to

their annual income growth rates on the horizontal axis, measured over the period 1971-2011. The slopes of the regression lines represent the traditional measure of consumption smoothing.

As is clear from the graphs, consumption smoothing is far less effective in Zambia than in the U.K. Both countries experience variations in income growth that lie in the same range, mostly between 5% and +5% per year. But while in the U.K. percentage changes in annual consumption also tend to lie in this range, people in Zambia face much greater volatility in their consumption: consumption growth rates range from 25% to +25%. However, the estimated elasticities of 1.05 for the U.K. and 0.51 for Zambia exhibit the opposite pattern. They suggest that in the U.K., consumption is twice as sensitive to income shocks as in Zambia. I draw two lessons from this figure. First, the extent of consumption smoothing does vary quite dramatically across countries. Second, the traditional elasticity-based measure for consumption smoothing makes little sense in cross-country comparisons.

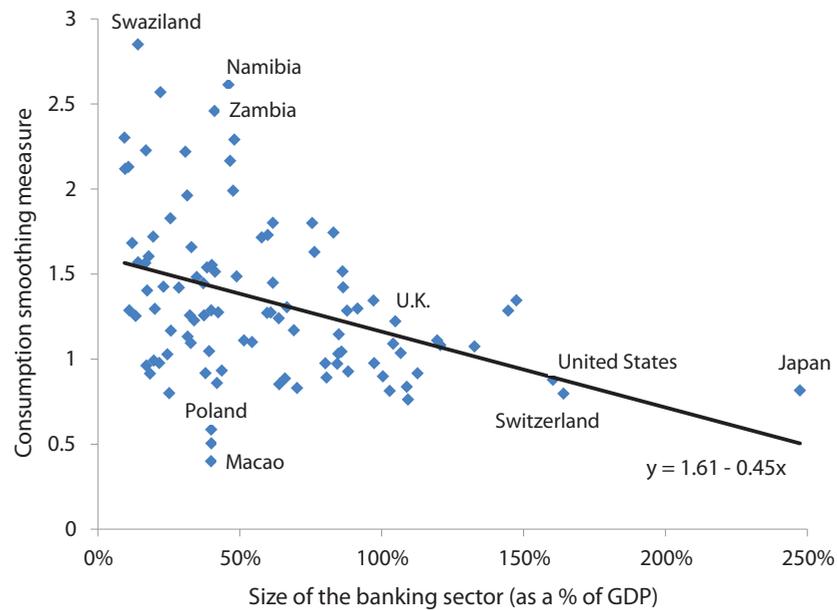
Figure 4: Elasticity of consumption growth to income growth for the U.K. and Zambia (1971-2011)



As an alternative inverse measure for the degree of consumption smoothing in a country, I propose the ratio between the time-series standard deviation of consumption growth and the standard deviation of income growth. This simple measure is 1.22 for the U.K. and 2.82 for Zambia, and thus seems to be a better reflection of the ability of households in these countries to smooth their consumption over time, controlling for variation in their income.

In the next step, I will attempt to explain cross-country variation in this measure of consumption smoothing to attributes of a country's financial system. To this end, I have estimated the degree of consumption smoothing in this way for a sample of 99 countries over 1971-2011. This process is still in an early stage, but Figure 5 gives a preview of a potentially interesting finding. The new consumption smoothing measure on the vertical axis is plotted against a common measure of the development of the banking sector, the amount of domestic credit provided by banks, expressed as a percentage of GDP. The figure suggests that in countries with a highly developed banking sector (such as Japan, Switzerland, and the United States), consumption variability tends to be low relative to income variability, whereas in countries with a small banking sector (such as Namibia, Swaziland, and Zambia), consumption is much more volatile relative to income. However, the relation between financial development and the degree of consumption smoothing may be complex, as indicated by countries like Macao and Poland, which have a small banking sector but a high degree of consumption smoothing. More research is needed to pan out this relation, and to examine whether it survives controlling for other characteristics of the country and its financial system.

Figure 5: Consumption smoothing vs. banking sector development (99 countries, 1971-2011)



7. Nascent markets

Prior studies on the role of financial systems in the economy have uncovered a wide variety of empirical relations between financial development, legal and political institutions, and the economy. For example, the “law and finance” literature suggests that a country's legal system affects how developed its financial market is (La Porta et al., 1997). Many of these studies are based on cross-country correlations in a sample of around 50 relatively developed countries with stable financial structures. An important litmus test is whether support for these predicted relations is found in cases of significant changes in a country's financial system. Together with José Albuquerque de Sousa and Peter van Bergeijk, I have initiated a new research project on what happens when a country newly establishes a stock market and thereby fundamentally alters its financial system.

The number of stock markets around the world has almost doubled over the past 25 years (Figure 6). Different newly established stock markets (or “nascent markets”) show very different degrees of success, as illustrated by Figure 7. This figure shows the development of the number of listed companies on three new stock markets (Czech Republic, established 1993; Tanzania, 1998; Vietnam, 2003) over the first decade after their establishment.

Figure 6: Number of stock markets worldwide (1900-2010)

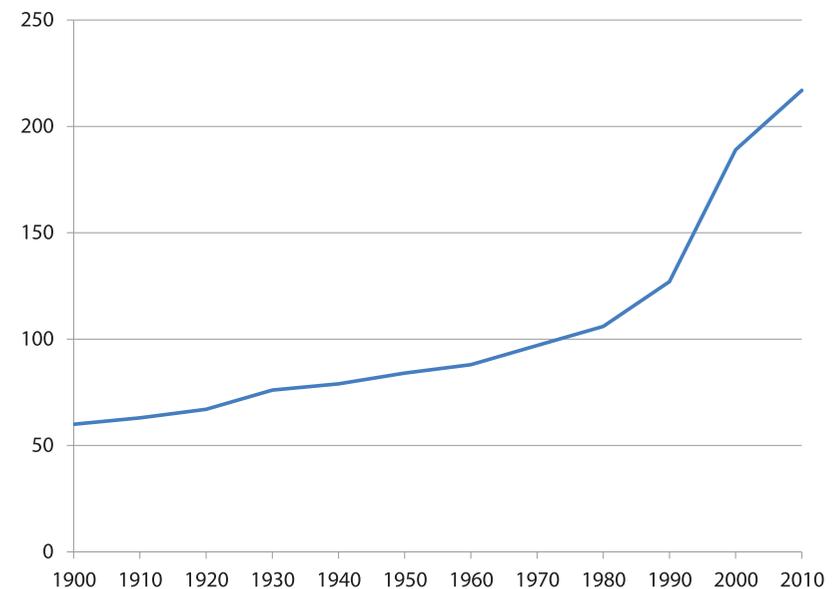
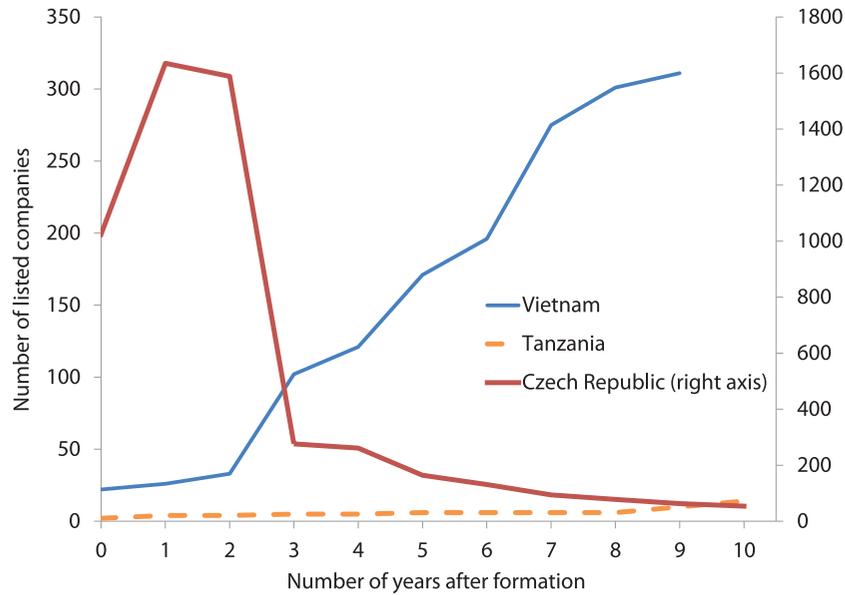


Figure 7: Development of number of listed companies on three nascent markets (first decade)



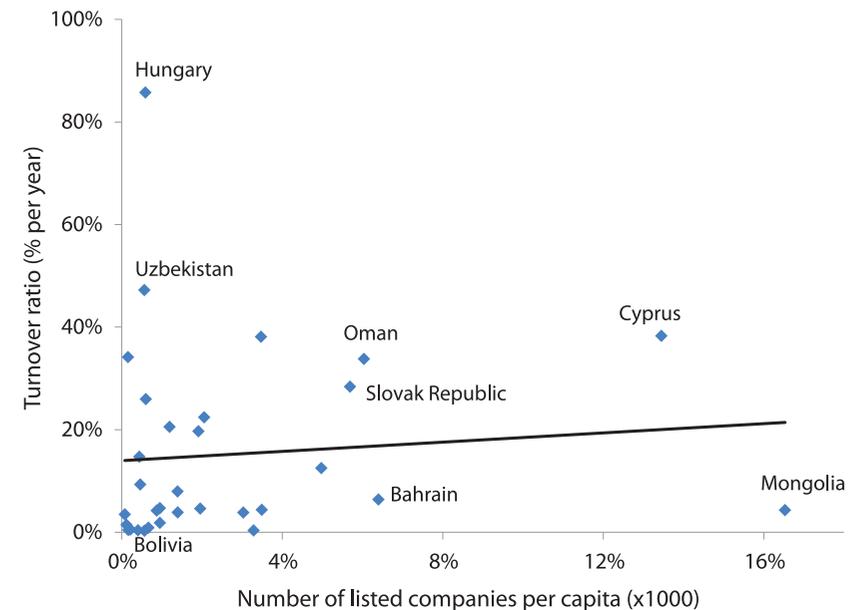
Although this is only one of many potential ways of measuring stock market success, a clear question emerges: why do some new stock markets succeed, whereas others are unable to thrive? This question is important, since prior studies suggest that financial markets play a central role in the economy. However, these studies by and large bypass the question of how financial systems evolve, and whether the introduction of a stock market enhances the fulfillment of the social functions of finance. We started out by collecting data on the years of establishment of all stock markets world-wide. Since 1988, 63 countries have opened their first stock exchange, five of which have opened two or more new exchanges. Twenty-three countries set up a regional exchange, and 14 (mostly formerly communist) countries reopened their stock exchanges after they had been closed for a prolonged period of time. There are still 49 countries without a stock exchange, although there are rumors that 14 of these might be opening one in the near future. It seems that the issue of whether and how to establish a stock market is still relevant.

Our plan is to analyze the determinants of success and failure of nascent markets along two dimensions. First, we will examine whether the stock markets thrive themselves, and whether financial, economic, institutional, and

political factors can help us understand why some markets flourish more than others. Second, we want to examine whether financial markets indeed contribute to the economy and society in a broader sense by fostering capital accumulation and more efficient capital allocation.

So far, we have collected data on the development of 32 nascent markets over the first decade of their existence in terms of number of listings, market capitalization, and turnover ratio. Our first observation is that when measuring success along these dimensions, there is a wide variety of observed degrees of success, both across countries and across success measures. Figure 8 shows the relation between two common measures of the development of stock markets, measured ten years after the establishment of these markets. The horizontal axis shows the number of listed companies, as before, but now scaled by the size of the population. The vertical axis shows the turnover ratio, which is an indication of the trading activity on the stock market. A turnover ratio of 100% means that, on average, each stock is traded once a year.

Figure 8: Turnover ratio vs. number of listed companies (32 nascent markets, after 10 years)



It is clear from the figure that some nascent markets have become quite mature after ten years. For example, the Slovak stock market has attracted 306 listings after ten years, which is a large number given the size of the population.

Its annual turnover of 30% approaches that of developed markets like the Netherlands, which had an annual turnover of 70% in 2012. But the stock market in Bolivia, with 18 listings and a turnover ratio below 1% after ten years, has hardly taken off. Some other markets have a large number of listings, but little trading (such as Mongolia), or a lot of trading in relatively few listed stocks (Hungary). The regression line shows that the correlation between these two indicators of stock market success is weak.

In the near future, we will extend the set of nascent markets in our sample and carry out a systematic analysis of the factors that influence the success of nascent markets, both in terms of listing and trading activity on the market itself and in terms of their contribution to the accumulation and allocation of capital. Furthermore, we intend to go beyond my comfort zone of econometric analysis of large databases and initiate a detailed, qualitative field study of one of the nascent markets in our sample.

8. Conclusion

In this address, I have argued that the potential of the academic field of finance to contribute to society is large, but to fully realize this potential as a field, we would benefit from being more focused on big questions, more open to alternative approaches, and more realistic about what the traditional tools of an economist can attain.

In an attempt to personally follow-up on these views, I have proposed a new research agenda on the social value of financial systems. I have shown the substantial social costs of financial crises, and I have introduced new research on the social functions of finance and the success of nascent stock markets.

In half a life of exposure to the world of academia, one of the key lessons that I have taken away is that science thrives on the exchange of ideas, on the willingness to challenge the viewpoints of yourself and others, and on a healthy dose of doubt about your own truth. I hope this address will spur the debate on the social value of finance, and I look forward to hearing your views!

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Mathijs van Dijk is Professor of Financial Markets, endowed by the Erasmus Trust Fund, at the Rotterdam School of Management, Erasmus University (RSM). This chair was created to promote fundamental research into the organization and functioning of today's financial markets and systems. Van Dijk obtained his BSc / MSc in Econometrics (*cum laude*) from Erasmus University in 1996 and his PhD in Finance from Maastricht University in 2002. He also studied at Warwick Business School and Princeton University and was a visiting scholar at Ohio State University, Duke University, and UCLA. His research on the liquidity of and the price-formation process on financial markets and on the cost of capital has been published in leading academic journals such as the *Journal of Financial Economics*, the *Journal of Accounting and Economics*, the *Review of Finance*, and in practitioner-oriented journals such as the *Financial Analysts Journal*. In 2008, Van Dijk received a "Vidi" grant from the Netherlands Organisation for Scientific Research (NWO) to conduct a five-year research project into liquidity crises on international financial markets. He teaches on financial markets at RSM and the Duisenberg School of Finance.

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Erasmus Research Institute of Management - ERiM
Rotterdam School of Management (RSM)
Erasmus School of Economics (ESE)
Erasmus University Rotterdam (EUR)
P.O. Box 1738, 3000 DR Rotterdam,
The Netherlands

Tel. +31 10 408 11 82
Fax +31 10 408 96 40
E-mail info@erim.eur.nl
Internet www.erim.eur.nl