Essays in Banking and Corporate Finance

by

Jason D. Kotter

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Business Administration)
in the University of Michigan
2014

Doctoral Committee:

Associate Professor Amy K. Dittmar, Co-Chair
Professor E. Han Kim, Co-Chair
Associate Professor Uday Rajan
Associate Professor Melvin Stephens, Jr.
Assistant Professor Hal D. White

To my best friend, Kay Lynn. This journey would not have been complete without you by my side.

ACKNOWLEDGMENTS

I am particularly grateful for the constant support and encouragement of my dissertation co-chairs, Amy Dittmar and Han Kim. They pushed me to think deeply about big, meaningful questions; their influence has made me a better scholar. I also thank Uday Rajan, Mel Stephens, and Hal White for generously sharing their time and insight with me as members of my dissertation committee.

I gratefully acknowledge financial support from the Mitsui Life Financial Research Center at the University of Michigan. This support made it possible to purchase the earnings conference call transcripts used in Chapter 2.

Chapter 3 of this dissertation is co-authored with my former colleagues at the Federal Reserve Board of Governors: Carlos Arteta, Mark Carey, and Ricardo Correa. I have benefited tremendously from their expertise.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

LIST OF APPENDICES

LIST OF ABBREVIATIONS

ABCP asset-backed commercial paper

ABS asset-backed securities

BFSR bank financial strength rating

CCF credit conversion factor

CDO collateralized debt obligation

CEO chief executive officer

CGQ corporate governance quotient

credit-arb credit arbitrage

CRSP Center for Research in Security Prices

DOT dictionary of occupational titles

EDF expected default probability

FIN Financial Interpretation Number

GDP gross domestic product

LIBOR London interbank offered rate

LTCM Long Term Capital Management

MBS mortgage backed securities

MD&A management discussion and analysis

MTN medium term note

Q&A question and answer

ROA return on assets

SAV securities arbitrage vehicle

SIV structured investment vehicle

2SLS two-stage least squares

ABSTRACT

Essays in Banking and Corporate Finance

by

Jason D. Kotter

Co-Chairs: Associate Professor Amy Dittmar and Professor E. Han Kim

This dissertation consists of three essays in banking and corporate finance. The first essay examines how changes in the composition of the human capital of the workforce impact the CEO. Over the last fifty years, technological change has caused the tasks workers perform to shift from routine to nonroutine work. As a result, the role of the CEO has evolved to become more focused on developing the human capital within the firm. I estimate that these changes in the role of the CEO caused CEO pay to double over the last thirty years, explaining roughly one-third of the aggregate increase in CEO pay. The second essay, co-authored with Carlos Arteta, Mark Carey, and Ricardo Correa, empirically examines financial institutions' motivations to take systematic bad-tail risk in the form of sponsorship of credit-arbitrage asset-backed commercial paper vehicles. A run on debt issued by such vehicles played a key role in the crisis that began in the summer of 2007. We find evidence consistent with important roles for both owner-manager agency problems and government-induced distortions, especially government control or ownership of banks. The final essay explores the impact of capital regulation on bank risk taking. I utilize a triple difference specification to identify the effect of FIN 46—which increased regulatory capital requirements for U.S. banks sponsoring off-balance sheet vehicles—on systematic risk exposure. I find that after the regulation, banks' exposure to vehicles with high systematic risk increases relative to vehicles with low risk and relative to non-U.S. banks which are not affected by the regulation. These results suggest that capital regulation has the perverse effect of concentrating systematic risk, potentially increasing the systemic risk of the financial system.

CHAPTER 1

Introduction

This dissertation is comprised of three empirical essays in two distinct areas: first, the intersection of labor and corporate finance; and second, financial institutions. These areas are linked only by my own idiosyncratic interests; as a result, the following chapters should be read as independent, stand-alone research. Below, I elaborate on the research questions that I pursue in this dissertation.

1.1 Labor and Corporate Finance

How does the composition of the workforce affect firm financing decisions, risk-taking, innovation, and productivity? While there is a large, developed literature that examines how firm characteristics such as size, industry, and risk affect employees (for example, consider the labor literature on compensating wage-differentials), we know comparatively little about the extent to which individuals impact firm outcomes.

Chapter 2, titled "Technological Change, Job Tasks, and CEO Pay," broadly seeks to address this by examining the role individuals play in corporate financial decisions. Specifically, I show that changes in the human capital of the workforce impact both the role and pay of the CEO. Over the last fifty years, technological change has caused the tasks workers perform to shift from routine to nonroutine work. I show that this shift changes the role of the CEO: nonroutine workers specialize in the task-specific information related to the production process, while CEOs focus on developing the firm's human capital.

IBM's CEO Sam Palmisano puts it this way, "You just can't impose command-and-control mechanisms on a large, highly professional workforce. I'm not only talking about our scientists, engineers, and consultants. More than 200,000 of our employees have college degrees. The CEO can't say to them, 'Get in line and follow me.' Or 'I've decided what your values are.' They're too smart for that. And as you know, smarter people tend to be, well, a little more challenging; you might even say cynical."

I provide evidence that these changes in the workforce caused CEO pay to double over the last thirty years, explaining roughly one-third of the aggregate increase in CEO pay. This increase in pay is consistent with synergies between CEOs and nonroutine workers. Together, these results suggest that a substantial portion of the increase in CEO pay over the past three decades represents an optimal response to technological change.

1.2 Financial Institutions

Chapter 3 and Chapter 4 seek to understand why institutions take risk, how individual institutional risk taking contributes to systemic risk, and how regulatory restrictions interact with this risk taking. This work is largely motivated by my experience as a research assistant at the Federal Reserve Board of Governors during the 2007–2009 financial crisis. This front row seat to the behind-the-scenes turmoil of the crisis impressed upon me the importance, as well as the limitations, of bank regulation. These chapters provide insights into what types of regulation might help strengthen the stability of the financial system.

Chapter 3, "Revenge of the Steamroller: ABCP as a Window on Risk Choices," co-authored with Carlos Arteta, Mark Carey, and Ricardo Correa (all at the Federal Reserve Board), empirically examines financial institutions' motivations to take systematic bad-tail risk in the form of sponsorship of credit-arbitrage asset-backed commercial paper vehicles. A run on debt issued by such vehicles played a key role in the crisis that began in the summer of 2007. We find evidence consistent with important roles for both owner-manager agency problems and government-induced distortions, especially government control or ownership of banks.

Much of the focus concerning the financial crisis of 2007–09 has been on preventing banks from utilizing regulatory arbitrage to leverage systematic risk exposure. I explore the extent to which this occurs in Chapter 4, "Do Bank Capital Regulations Concentrate Systematic Risk?" As a result of the Enron scandal, new regulations were enacted that increased the capital charge for holding assets in off-balance sheet vehicles. I utilize a triple difference specification to identify the effect of this exogenous regulatory shock on bank systematic risk exposure. After the regulation, banks' exposure to off-balance sheet assets at vehicles with high systematic risk increases relative to vehicles with low systematic risk and relative to non U.S. banks which are not affected by the regulation. These results suggest that capital regulation might have the perverse effect of increasing the systemic risk of the U.S. financial system.

CHAPTER 2

Technological Change, Job Tasks, and CEO Pay

"Allocating human resources in a strategic manner is a key aspect of the CEO's role . . . Little if anything else that I do as CEO will have as enduring an impact . . . "
– A.G. Lafley, P&G CEO

2.1 Abstract

This paper examines how changes in the composition of the human capital of the workforce impact the CEO. Over the last fifty years, technological change has caused the tasks workers perform to shift from routine to nonroutine work. I estimate that these changes in the workforce caused CEO pay to double over the last thirty years, explaining roughly one-third of the aggregate increase in CEO pay. Consistent with this effect being caused by synergies between CEOs and nonroutine workers, I use text analysis of 10-K statements to show that managers of nonroutine workforces focus relatively more on employees and that this focus leads to large increases in firm value and profitability. Together, these results suggest that a substantial portion of the increase in CEO pay over the past three decades represents an optimal response to technological change.

2.2 Introduction

Technology is changing the nature of work: routine, repetitive tasks are being replaced with non-routine tasks requiring complex problem solving, flexibility, and creativity. A vast theoretical and empirical literature examines the effects of this shift on the individual worker, and a similarly large body of macroeconomic work suggests that additional human capital raises aggregate productivity. Surprisingly, though, there is little research exploring how employee characteristics affect firms. Understanding this is important because both the individual returns to investment in human capital and macroeconomic growth depend on human capital increasing firm-level productivity.

Realizing productivity gains from shifts in human capital presents a unique challenge for firms because human capital differs from physical capital in several important ways. For instance, workers can voluntarily leave the firm (Zingales, 2000); firm culture can impact worker effectiveness (Bolton, Brunnermeier, and Veldkamp, 2013; Guiso, Sapienza, and Zingales, 2013); and hierarchal structure and compensation incentives influence worker effort (Garicano and Hubbard, 2007; Black and Lynch, 2001). Thus, the role of the manager is vital to the firm's ability to effectively utilize human capital.

In this paper, I explore how human capital affects the role of the manager by examining how a specific type of human capital—nonroutine job tasks—impacts the chief executive officer (CEO). I show that the increase in nonroutine workers over the past thirty years caused average CEO pay to double, explaining roughly one-third of the aggregate growth in CEO pay over this period. Differences in employees across firms also explain a substantial amount of the cross-sectional variation in pay: an increase from the 25th to the 75th percentile of nonroutine task workers raises annual pay by about $2.6 million dollars.

To the extent that pay reflects the marginal product of CEO labor, these differences suggest the existence of synergies between managers and nonroutine workers. These synergies may result from either nonroutine workers producing valuable information and improving the ability of the CEO to make investment decisions, or from the CEO creating a corporate culture which encourages engagement, teamwork, and knowledge sharing and thus making it easier to successfully complete nonroutine tasks. Within the context of either the contracting model of Edmans, Goldstein, and Zhu (2011) or a CEO matching model similar to Gabaix and Landier (2008), these synergies imply that optimal pay increases with nonroutine task workers.

In order to interpret the increase in CEO pay as evidence of synergies, it is necessary to distinguish between treatment effects (caused by increased human capital) and selection effects (associated with the type of CEO that manages nonroutine workforces). The relationship between nonroutine workers and CEOs suffers from two possible types of selection bias. Highly-skilled CEOs might choose to hire nonroutine workers simply due to preferences to work with people of a similar background. Since CEO skill is positively related to pay, this type of selection leads to an over-estimate of the effect of nonroutine workers on CEO pay. Alternatively, the marginal impact of the CEO might be larger in firms with few nonroutine workers because in these firms the CEO has to make decisions without input from skilled employees. In this case, the estimated effect is biased downward.

To overcome these problems, the ideal experiment would either randomly assign CEOs to workforces of different human capital levels or randomly assign workers of different skill levels to CEOs. While neither of these experiments exist, there is a quasi-natural experiment that approximates the second situation. In the early 1970s, the invention of the microcomputer ushered in what is sometimes called the third industrial revolution (Gordon, 2012). This new technology allowed firms to computerize routine tasks, but has not (yet) allowed firms to computerize nonroutine tasks that involve creativity, critical thinking, and complex communication. As a result, firms that were highly exposed to routine job tasks before the shock experienced large increases in the human capital of their workforce as they replaced routine workers with computers and hired additional nonroutine workers. Alternatively, the composition of the workforce at firms with low exposure to routine job tasks stayed relatively constant.

The key advantage of this technology shock is that firms do not choose ex-ante dependence on routine or nonroutine tasks; it is a feature of the products the firm produces. I exploit this exogeneity and use a difference-in-difference strategy similar to Stevenson (2010) and Kroft and Pope (2013) to estimate the impact of changes in nonroutine task workers on executive compensation. Because my data on CEO pay begins in the mid-1980s, I compare the growth of CEO pay at firms that did and did not rely on routine workers during the IT revolution (which begins in 1995 with the commercialization of the internet). I predict that the pay of CEOs in firms that relied more heavily on routine tasks increases more than those that did not, since these firms will experience a greater change to their workforce composition. To ensure that my approach is not contaminated with endogenous firm responses to earlier computer innovations, I measure reliance on routine workers in 1973—the year the microcomputer was invented. Specifically, I use the pre-technology shock variation in the proportion of routine task workers as an instrument to capture the exogenous variation in changes of the level of nonroutine workers.

To interpret this effect as causal, I need to assume that if the IT revolution had not occured, growth in CEO pay would have been similar across firms that did and did not rely on routine workers. Though it is not possible to test this assumption directly, I present several pieces of supportive evidence. The pre-shock trends of both nonroutine task workers and CEO pay appear very similar across the treatment and control group. Of course, it is possible that pay would have evolved differently between these two groups even if the IT revolution did not occur. To account for this possibility, I control for de-unionization, deregulation, and globalization; the results are unchanged.

As an additional check, I estimate instrumental variables (IV) regressions utilizing an alternative instrument, unanticipated changes in computer prices. Low computer prices lead firms to adopt technology and increase the number of nonroutine task employees. Under the assumption that CEO pay is not directly influenced by unexpectedly low technology prices, this provides a valid instrument for the level of nonroutine task workers. The results using computer prices as an instrument corroborate the difference-in-difference results utilizing pre-technology shock variation in worker composition, suggesting a causal relationship between nonroutine workers and higher CEO pay.

One remaining concern that is not fully addressed by the above estimation strategies is that the estimated effect might reflect manager power. If it is increasingly difficult for shareholders to monitor a firm with more nonroutine workers, the relation between higher CEO pay and nonroutine workers might be due to rent extraction. The evidence suggests that this is not the case. My results are robust to controlling for various proxies for corporate governance and agency problems such as the percentage of independent directors, institutional and CEO ownership, and Khanna, Kim, and Lu's (2013) index of CEO power. In addition, the increase in nonroutine workers leads to higher wealth performance sensitivity, higher share ownership, shorter tenure, and a higher probability of forced turnover. These high-powered incentives are difficult to reconcile with rent extraction, but are consistent with optimal incentive contracts.

To provide additional support for the existence of synergies between the CEO and nonroutine workers, and to shed light on the changing role of the CEO, I examine manager focus (i.e. the attention that CEOs pay to various aspects of the firm). Using a dictionary-based textual analysis of the management discussion and analysis (MD&A) section of 10-K reports similar to Loughran and McDonald (2011), I show that management focus on employees increased from 1994 to 2012. Managers of nonroutine workforces are particularly likely to focus on employees; this focus leads to increases in both value and profitability.

I also use textual analysis to show that managers of nonroutine workers are more likely to delegate to subordinates on earnings conference calls. This is consistent with the CEO relying on nonroutine workers to provide firm specific information, suggesting that nonroutine workers shift the role of the CEO from specialist to generalist and that synergies exist between the CEO and these workers. As further evidence, I use the general ability measure of Custódio, Ferreira, and Matos (2010) to show that managers of nonroutine workers are more likely to be generalists. Finally, CEO fixed effect regressions reveal that managers at nonroutine worker firms matter more for firm profitability and stock returns than managers at routine worker firms. Taken together, the evidence suggests that managing human capital increases the importance of the CEO through synergies between managers and nonroutine workers.

One of the central contributions of this paper is to link the literature on human capital and the literature on CEO pay by providing evidence that a substantial portion of the increase in executive compensation is due to the rise of nonroutine workers. This link sheds light on the longstanding debate on the optimality of CEO pay. Critics of current CEO pay practices argue that CEOs have captured the board of directors and are consequently paying themselves too much at shareholders' expense (Bertrand and Mullainathan, 2001; Bebchuk, Fried, and Walker, 2002; Bebchuk and Fried, 2003; Bebchuk and Grinstein, 2005; Bebchuk, Grinstein, and Peyer, 2010). However, other research suggests that the rise in pay is a result of optimal contracting in the market for CEO talent (Gabaix and Landier, 2008; Murphy and Zabojnik, 2004; Frydman, 2005; Kaplan and Rauh, 2010; Cremers and Grinstein, 2011; Falato, Li, and Milbourn, 2011). Neither approach is fully consistent with the long-run time trend—pay remained relatively stagnant from the 1940s to the early 1970s, when it began to grow rapidly (Frydman and Saks, 2010). My results help reconcile this evidence with the Gabaix and Landier (2008) model by showing that it is not just the overall growth of the firm, but also the growth in skilled labor that leads pay to increase. Further, my paper provides empirical evidence that is largely consistent with the dynamic model of Lustig, Syverson, and Van Nieuwerburgh (2011) showing that optimal manager compensation increases in response to technological shocks.

This paper also contributes to the vast literature that explores the effects of human capital. Ciccone and Papaioannou (2009), Hanushek and Kimko (2000), Gennaioli, La Porta, Lopez-de Silanes, and Shleifer (2013), and Glaeser, La Porta, Lopez-de Silanes, and Shleifer (2004) are a few among many examples of papers that examine the macroeconomic effect of human capital. In particular, Ciccone and Papaioannou (2009) suggest that growth occurs most quickly in industries that rely on educated workforces. My results suggest that growth is especially likely to occur in educated workforces that are led by a manager focusing on human capital. This is consistent with the evidence in Bloom and Van Reenen (2007) and Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013) which argues that management practices that increase employee retention and motivation improve firm productivity.

Finally, this paper adds to the growing literature that explores the connections between labor and corporate finance. Agrawal and Matsa (2012) explore how the risk aversion of the workforce affects firm leverage decisions; Pratt (2011) uses a structural model and Kim (2011) uses Census establishment level data to explore how firm specific human capital affects leverage; Acharya, Baghai, and Subramanian (2012) shows that protecting employees from wrongful discharge spurs innovation; and Brown and Matsa (2012) provides evidence that employees are aware of the financial condition of firms and avoid seeking employment at firms with questionable financial health. My paper broadly fits into this literature by showing that employees impact firm outcomes. To maximize firm value, shareholders must correctly motivate managers to focus on the human capital of the firm.

The rest of the paper is structured as followed. Section 2.3 provides a brief theoretical motivation, Section 2.4 describes stylized facts concerning job task complexity and CEO pay, Section 2.5 describes my methodology and data, Section 2.6 discusses the results, and Section 2.7 concludes. Additional results are found in Appendix A.

2.3 Theoretical Motivation

In this section, I briefly discuss the theoretical motivation behind the positive relationship between CEO pay and nonroutine workers. A more complete model that combines the job task framework of Autor, Levy, and Murnane (2003) with a CEO matching model based on Gabaix and Landier (2008) is developed in Appendix A.1. The intuition of this model is straightforward. Workers perform two types of tasks: routine tasks are repetitive and rule-based, while nonroutine tasks require flexibility, personal interaction, and creative problem solving. New technology allows firms to computerize many routine tasks; as a result, firms replace routine workers with nonroutine workers. Because managers and nonroutine workers exhibit synergies, the marginal product of CEO labor is higher at firms with more nonroutine employees; these firms have incentive to bid up the price of CEO labor. The efficient equilibrium results in positive assortative matching: the most talented CEO works for the largest nonroutine firm and earns the highest salary.

This equilibrium rests on two key assumptions: synergies between CEOs and nonroutine task workers and an upward sloping supply curve in the market for CEO talent. It is important to consider the plausibility of these assumptions. Why do nonroutine workers and CEOs exhibit synergies? Unlike routine workers, nonroutine workers produce information that improves the ability of the CEO to make investment decisions (Bolton et al., 2013). Additionally, many nonroutine tasks, such as innovation and teamwork, are highly dependent on the working environment created by the CEO (Finkelstein, Hambrick, and Cannella, 2008).[1] Consequently, the productivity of both the CEO and the nonroutine task employee are highest when they work together.

However, this does not necessarily imply that overall net productivity is maximized by matching talented CEOs with nonroutine workers. For instance, a routine task firm might be particularly dependent on the CEO's ability because he has to make makes decisions without additional input from nonroutine workers. There is limited empirical evidence that this is not likely to be the case, for instance, Lazear, Shaw, and Stanton (2012) suggest that productivity is maximized by pairing highly skilled workers with talented managers. Ultimately, my data is most consistent with the existence of synergies between nonroutine workers and CEOs.

The second crucial assumption, an upward sloping supply curve for CEO talent, suggests that there are a limited number of individuals with sufficient talent to manage large corporations.[2] To keep the framework simple, I assume that talent is unidimensional—high ability CEOs are equally capable of managing nonroutine and routine workers. In reality, there are multiple dimensions to CEO talent. As long as the value of the synergy is large and the supply of CEO talent with regard

[1] Anecdotally, managers appear to understand this. For instance, in 2005 Jeff Immelt, GE's CEO, wrote in his annual letter to shareholders, "Developing and motivating people is the most important part of my job."

[2] Alternatively, it implies some friction that increases the bargaining power of existing CEOs. In this framework, that friction might naturally arise if the synergy between workers and managers develops slowly.

to managing nonroutine workers is not perfectly elastic, the basic results of the model continue to hold.

An alternative framework that is also consistent with the positive relationship between nonroutine workers and CEO pay is Edmans et al.'s (2011) contracting model. In this model, synergies between workers and managers are defined as a positive externality of CEO effort. The critical assumption is that rather than increasing employees' productivity, CEO effort reduces their cost of effort. In the context of this paper, Edmans et al.'s (2011) framework would require that CEO effort especially reduces the cost of working for nonroutine workers relative to routine workers.[3] To obtain the optimal effort level, shareholders then subsidize CEO effort by paying managers more.

2.4 Job Task Complexity: Stylized Facts

Figure 2.1 shows that job task complexity has increased dramatically over the last several decades. This figure graphs the mean level of routine and nonroutine task intensity from 1973–2009, scaled by the empirical distribution of tasks in 1973.[4] I choose 1973 as the base year primarily because it is the earliest date that the underlying data is available. However, since the first microcomputer was invented in 1972, the distribution of tasks in 1973 should basically reflect the composition of jobs before modern computerization occurred.[5] Routine task intensity is flat throughout the 1970s, but steadily decreases beginning in the 1980s. In contrast, nonroutine task intensity steadily increases; by 2009, the median nonroutine task intensity is at the seventieth percentile of the 1973 distribution. In sum, the nature of work has shifted to become much more focused on nonroutine tasks.

While data limitations prevent me from examining the earlier evolution of task complexity, Philippon and Reshef (2012) examine task complexity within the U.S. financial sector from 1910 until the present. They show that nonroutine task intensity decreased from 1910 until 1950 and then began to increase rapidly after 1970. They also document that routine task intensity increased from 1910 until 1970 when it began to decrease. These results suggest that the shifting composition of job tasks shown in Figure 2.1 does not simply reflect long-run pre-existing trends, but that task complexity was altered by the introduction of microcomputer technology.

The secular trend in employee job tasks fits well with the trend in CEO pay. While CEO pay was roughly flat in the several decades prior to 1970, pay has increased dramatically since the 1970s, whether measured in absolute or relative terms (Frydman and Saks, 2010). Figure 2.2 shows

[3] One way to justify this is to recognize that nonroutine tasks are, by definition, nebulous. As a result, nonroutine workers disproportionately benefit from a firm culture that helps define the scope of their work.

[4] The nonroutine and routine task measures are ordinal, not cardinal, indexes. To interpret the magnitude of the changes in these indexes, I scale by the empirical distribution in 1973. My empirical results use the raw measures, but are robust to using the scaled versions.

[5] The first complete microcomputer, the Sac State 8008, was built by a team at Sacramento State University. Widespread use of the microcomputer did not occur, though, until the release of the Altair 8800 in 1975.

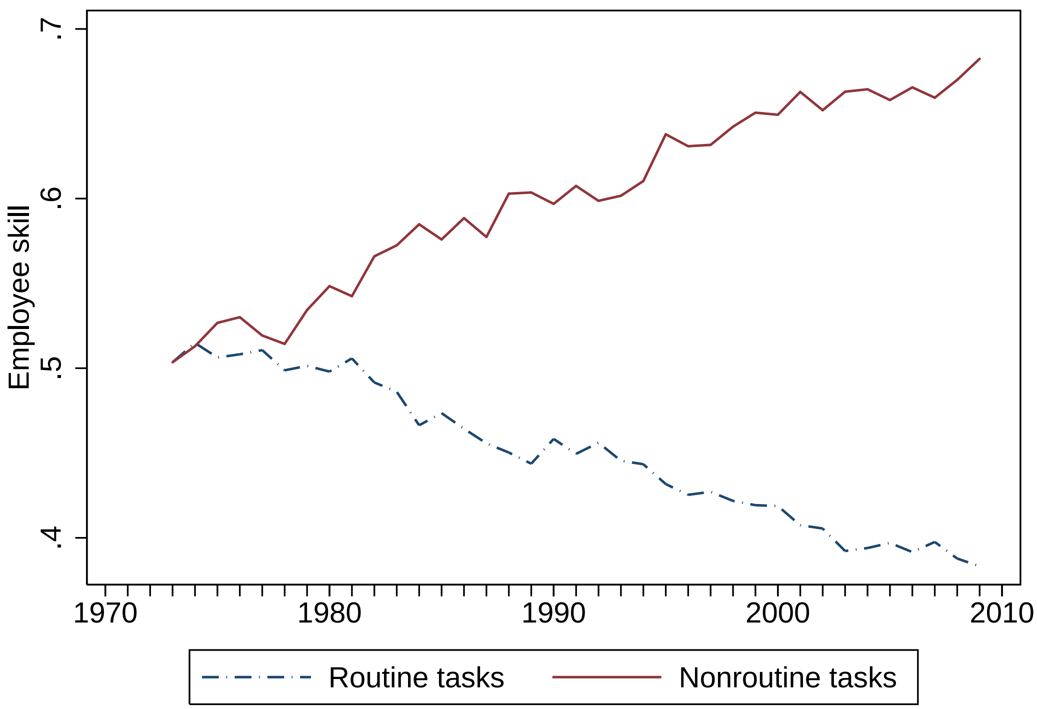

Figure 2.1. Average task skill profile, 1973–2009. This figure displays the evolution of mean industry-level employee skill, separated by the type of job task. The skill measures are scaled as a percentile of the 1973 distribution of industry-level employee skill, and are constructed using Current Population Survey data, following Autor et al. (2003).

that mean real CEO compensation increased from about 1.3 million dollars in 1984 to about 5 million dollars in 2010. Figure 2.2 also reveals that average pay began to grow much faster post-1995, a time period which corresponds to the emergence of the internet. The evolution of nonroutine task intensity follows a very similar trend. Given that reverse causality is not likely, this figure suggests that either the change in the composition of the workforce explains part of the increase in CEO compensation or some other driving force determines both measures. The most plausible forces that could affect both pay and employee skill levels are de-unionization, deregulation, and globalization; my results are robust to controlling for these variables in my analysis.

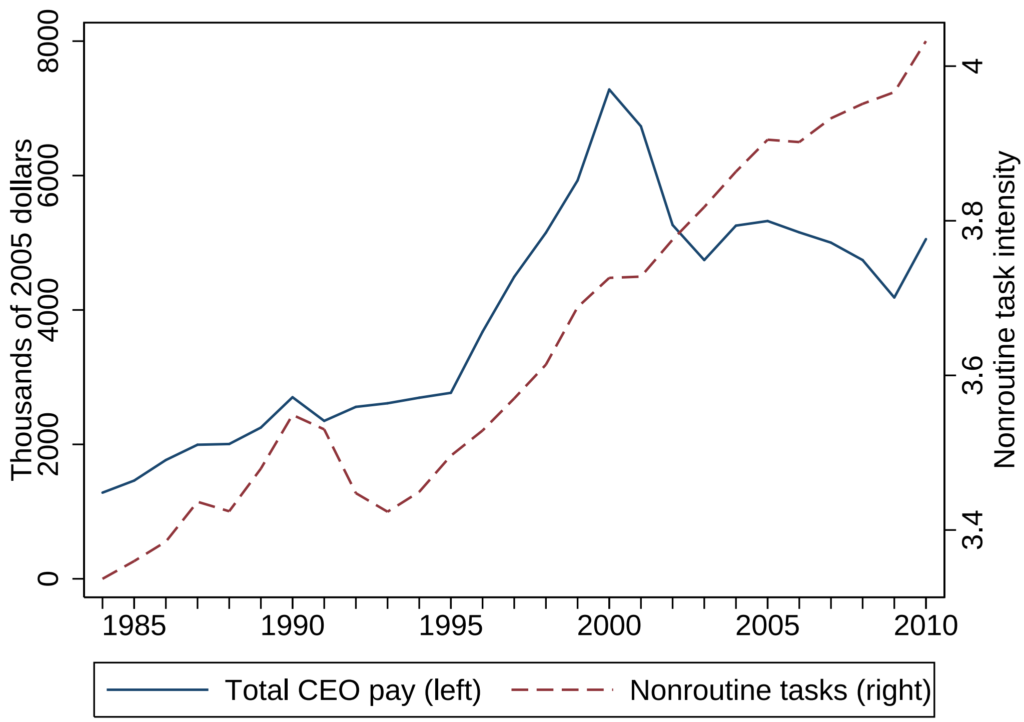

Figure 2.2. Executive compensation and employee skill, 1984-2010. This figure displays the evolution of average total executive pay from 1984 to 2010. Compensation data from 1984 to 1991 is from Yermack (1995), and compensation data from 1992 to 2010 is from Execucomp. CEO pay is defined as the sum of salary, bonus, restricted stock granted, long-term incentive payouts, and the Black-Scholes value of stock-options granted. Nonroutine tasks is a measure of the average industry-level employee skill, constructed from Current Population Survey data following Autor et al. (2003).

To create measures of job task complexity, I follow Autor et al. (2003) and use public Census data to aggregate person-level information to the industry-level. Here I briefly review the most salient features of this measure; additional details of its construction are available in Appendix A.2 and in the Data Appendix of Autor et al. (2003) and Acemoglu and Autor (2010).

The task composition measure is based on 330 time consistent occupations.[6] I classify each occupation in the Census along two task dimensions—routine and nonroutine—using the Fourth (1977) Edition of the U.S. Department of Labor's Dictionary of Occupational Titles (DOT). The DOT determines the characteristics of each occupation by sending analysts to observe actual employee behavior in real workplaces. I use the DOT to create ordinal indexes, ranging from 0 to 10, of the routine and nonroutine task intensity of each occupation.

Routine occupations include both manual as well as cognitive work. For example, both production line workers and bank tellers are routine occupations. While these jobs utilize different skills, the tasks performed in each job are repetitive. Nonroutine occupations include analytical jobs, such as an engineer, and interpersonal jobs such as a sales agent.[7]

I match the task intensities by occupation to each person in the Combined Current Population Survey May and Outgoing Rotation Group samples (May/ORG CPS) from 1973 to 2010.[8] The CPS is a monthly survey of about 50,000 households administered by the Census Bureau; it is designed to reflect the composition of the civilian non-institutional U.S. population. To obtain compatibility in industries across time, I use the crosswalk developed by Autor et al. (2003) to aggregate to 142 time-consistent census industry codes. Using each employed worker aged 18 to 64, I measure the average task intensity by industry (k) for each task and year,

$$task_{k,t} = \frac{\sum_{i \in k} task_{i,t} \lambda_{i,t} h_{i,t}}{\sum_{i \in k} \lambda_{i,t} h_{i,t}}. \tag{2.1}$$

The average is weighted by the CPS weight, λ, and the number of hours worked, h, so that the measure represents the average task intensity for a full time worker in industry k.

Although the DOT provides the most complete time series of job task requirements in the U.S., there are several limitations. Most importantly, since the characteristics of occupations are measured once (in 1977), the time variation in my sample results from the changing composition of occupations within industries. Undoubtedly, the change over time in complexity within occupations is an important, omitted source of variation. The task intensity is also measured at the industry, rather than firm, level. Both deficiencies reduce the precision of my analysis leading to likely attenuation bias.

To check if this measure of job tasks seems reasonable, Table 2.1 shows the ten most frequently observed Census industries in the highest and lowest nonroutine task quintiles. Banks, pharmaceuticals, and computers are among the most frequently observed high nonroutine task industries, while restuarants, steel works, and trucking are among the most frequently observed low nonrou-

[6]These are based off of the 1990 definitions of Census occupations.

[7]My definition of nonroutine occupations does not include nonroutine manual work, such as a janitor, because there is little scope for current technology to complement these tasks.

[8]1973 is the first year where there is data for the May/ORG CPS survey. I use the May/ORG CPS because it has larger sample sizes than other CPS waves.

tine task industries. The concentration of nonroutine tasks among industries appears to be higher at the upper end of the distribution, as the top 10 industries make up 85% of the highest quintile observations but only 39% of the lowest quintile observations. Four of the top 10 (roughly 54%) upper quintile industries are from the financial sector, suggesting that some of the large compensation packages observed in this sector might be due to employee composition.

Table 2.1
Industries and task specialization. This table shows the industry composition of my sample, split by employee skill. Panel A reports the top 10 most frequently observed census industries in the top quintile of employee skill and Panel B reports the top 10 most frequently observed industries in the bottom quintile of employee skill. Employee skill measures the intensity of nonroutine tasks and is constructed using aggregated Current Population Survey (CPS) data for each year as in Autor et al. (2003). Each observation is an unique CEO-year pair.

Top quintile nonroutine intensive industries			
Census Industry	Count	Percent	Cumulative
Banking	1348	24.1	24.1
Security, commodity brokerage, and investments	1202	21.5	45.6
Computers and related equipment	465	8.3	53.9
Drugs	436	7.8	61.7
Computer and data processing services	299	5.4	67.0
Savings institutions	266	4.8	71.8
Aircraft and parts	255	4.6	76.4
Office and accounting machines	204	3.7	80.0
Credit agencies	198	3.5	83.5
Advertising	87	1.6	85.1
Bottom quintile nonroutine intensive industries			
Eating and drinking places	543	7.1	7.1
Motor vehicles and motor vehicle equipment	510	6.7	13.8
Pulp, paper, and paperboard mills	405	5.3	19.2
Blast furnaces and steelworks	278	3.7	22.8
Trucking service	239	3.1	26.0
Construction	238	3.1	29.0
Furniture and fixtures	226	3.0	32.0
Miscellaneous food preparations	187	2.5	34.5
Miscellaneous paper and pulp products	187	2.5	37.0
Oil and gas extraction	187	2.5	39.4

As an additional robustness check, I run a regression of nonroutine skill on the average wage for the firm (untabulated). Wage data is only available for about one-third of my sample. There is a positive and statistically significant correlation between average wages and nonroutine skill, corroborating my measure of human capital.

2.5 Methodology and Data

Estimating the effect of nonroutine employees on CEO pay is problematic because of potential selection biases. CEO talent is likely correlated with nonroutine employees, perhaps because high-ability CEOs are better able to attract high-skilled workers. Alternatively, firms with low levels of nonroutine workers might disproportionately hire talented CEOs because these managers have to make decisions without additional input from skilled workers.

My solution for dealing with this potential selection bias is to implement a difference-in-difference estimator that mimics the natural experiment of randomly assigning workforces of different human capital levels to CEOs. Because improvements in technology allow firms to replace routine workers with computers and hire additional nonroutine workers, technology shocks change the composition of firms' workforces. This change in the level of nonroutine task workers is as good as random because it depends on firm characteristics that are fixed ex ante. Specifically, the extent to which technology impacts the labor composition of a firm depends on the level of routine tasks that need to be done. This level is a function of the product or service that the firm produces; given its product market choice, a firm cannot choose its reliance on routine tasks. For example, Ford can choose whether to use production line workers or robots to install windshields on its new vehicles, but cannot choose to avoid the routine task of installing thousands of windshields per day. Thus, exogenous technology shocks mimic the random assignment of nonroutine workers to firms that happen to have relied on routine tasks before the shock.

The specific technology shock that I have in mind is the invention of the microcomputer in 1972. Microcomputer technology ushered in what Gordon (2012) refers to as the third industrial revolution; this revolution continues to the present. This technology shock occurred in two major waves: the first, from the early 1970s to the early 1980s, is defined by the rise of the microprocessor and personal computer. The second, from the mid-1990s to the present, is characterized by the spread of the internet. While ideally I would examine the effect of both waves, data limitations only permit me to identify the effect of the internet shock, which I refer to as the IT revolution. To ensure that any endogenous firm response to the first technology wave does not contaminate my estimates of the effect of the IT revolution, I measure routine task exposure as of 1973. This captures industry reliance on routine tasks before modern computer technology was available.

I use the variation in exposure to routine tasks, measured in 1973, as an instrument to capture the exogenous variation in nonroutine workers. Figure 2.3 and Figure 2.4 document that this shock to nonroutine workers varied across industries. Specifically, Figure 2.3 shows that there is substantial cross-sectional variation in exposure to routine tasks in 1973. Figure 2.4 shows that this variation in routine task exposure predicts changes in nonroutine workers from 1984 to 2009. Combining the internet technology shock with variation in exposure to routine tasks before the shock generates a useful identification strategy for estimating the effects of nonroutine workers on CEO pay. I predict that CEO pay at firms that relied more heavily on routine tasks increases more than those that did not, since these firms will experience a greater change to their workforce composition.

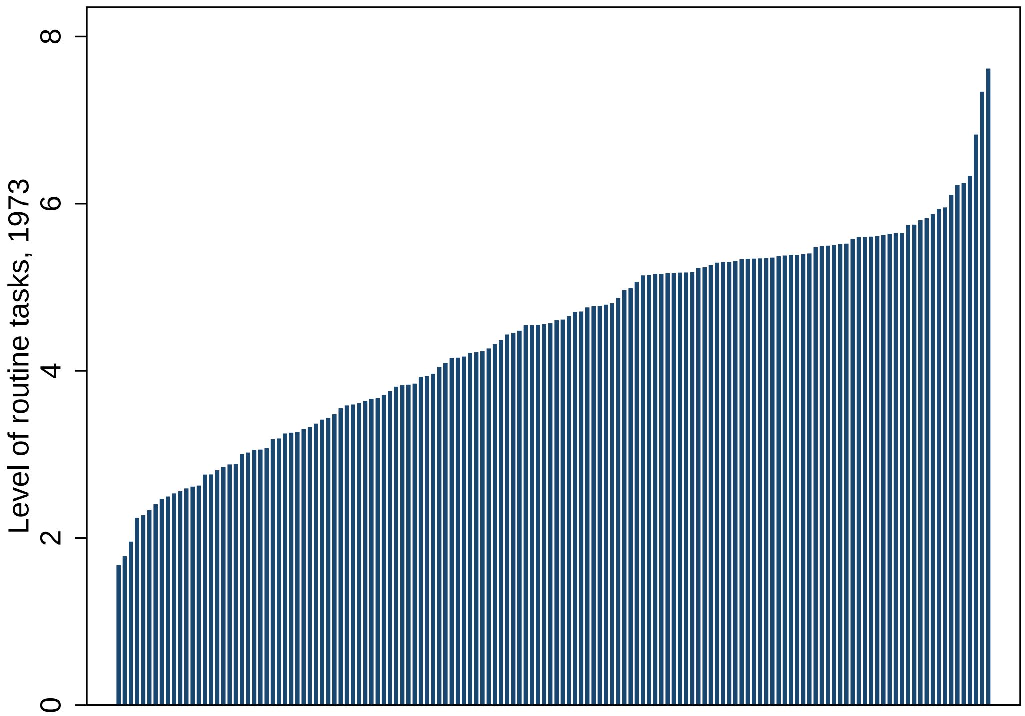

Figure 2.3. Industry Distribution of Routine Task Intensity in 1973. This histogram charts the level of routine task exposure of 140 Census industries in 1973.

Conceptually, my approach compares the growth rate over time in CEO pay at a firm that was highly dependent on routine labor before the IT revolution to the growth rate of CEO pay at a firm that did not depend on routine labor. To implement this strategy, I estimate the following difference-in-difference model:

$$\ln y_{ijt} = \beta_1 t + \beta_2 \text{routine}_{k_{1973}} + \beta_3 * t * \text{routine}_{k_{1973}} + \Gamma X_{ijt-1} + \epsilon_{ijt}, \tag{2.2}$$

for CEO i in firm j and industry k. The difference-in-difference estimate of the effect of changes in nonroutine labor on CEO pay is captured by β_3. By modeling the effect of the technology shock as a differential growth rate over time, rather than as the difference in pay pre- and post-1995, I acknowledge that the effects of the technology shock begin before my sample starts. This specification allows the data to endogenously determine the extent of the technology shock. In

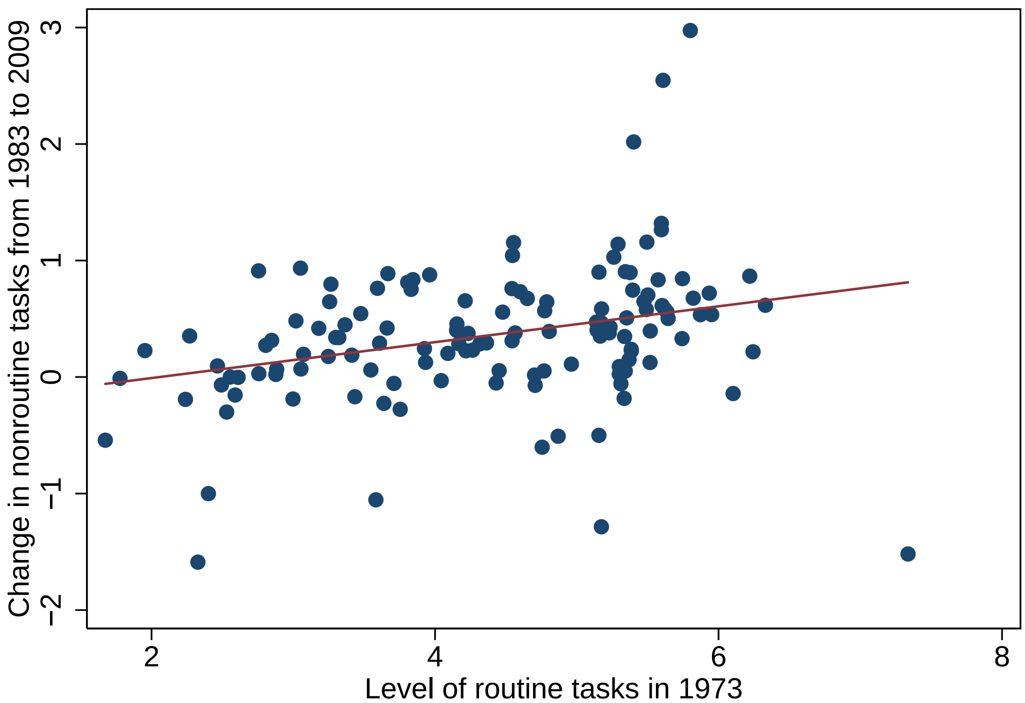

Figure 2.4. Change in Nonroutine Tasks by Routine Task Exposure. This figure plots the average change in nonroutine tasks from 1983 to 2009, measured at the industry level, as a function of the original level of routine tasks (measured in 1973). The trend line represents the linear regression of changes in nonroutine tasks on the original routine task level.

Section 2.6.1, I explicitly examine the case where the technology shock is defined as the advent of the internet in 1995.

Equation 2.2 suggests one immediate problem with this estimation. The independent variable of interest, the interaction between the time trend and the original level of routine tasks ($t * \text{routine}_{k_{1973}}$), is measured at the industry-year level. The dependent variable, CEO pay, is measured at the firm level. This means that measurement errors in routine tasks will be correlated across industry-years. To address this, throughout my analysis I cluster the standard errors by the interaction of industry and year. My main results are also robust to two-way clustering on industry and year and to estimating the standard errors through bootstrapping (untabulated).[9]

I examine both total CEO pay, defined as the sum of salary, bonus, restricted stock granted, long-term incentive payouts, and the Black-Scholes value of stock-options granted; and relative CEO pay which is total CEO pay divided by the median pay of a full-time equivalent worker in the CEO's industry, calculated using CPS data. The relative CEO pay measure helps to control for any general effects of the technology shock that affect both CEOs and employees; it also alleviates concerns that macroeconomic forces are driving changes in pay.

My sample includes 6,090 CEOs at 3,157 firms from 1984 to 2010. The CEO pay data from 1984 to 1992 comes from Yermack (1995); data from 1993 to 2010 is from Execucomp. I merge this sample of CEOs with firm-level accounting data from Compustat and stock return data from CRSP. I match CEO pay in fiscal year t to all other variables measured at year $t-1$ This ensures that the variables are known at the time when the CEO contract is finalized. All nominal quantities are converted to millions of 2005 dollars using the GDP deflator of the Bureau of Economic Analysis. Continuous variables are winsorized at the 1% level. The variables used in this study are summarized in Table 2.2.

I include controls for the known determinants of executive pay. CEO pay is increasing in firm size; I use the natural logarithm of firm revenue *ln(Revenue)* to control for this relationship. I include *Tobin's Q* to capture the effect of growth opportunities on CEO pay. *Income to assets* is measured as earnings before interest and taxes divided by total assets. *Shareholder return* is the previous fiscal-year cumulative return on the stock. I expect CEO pay to be positively related to these measures, since shareholders want to incentivize good performance. *Std. dev. return* is the standard deviation of daily stock returns calculated over the previous fiscal year, and is a proxy for the riskiness of the firm. Since CEOs are risk averse, they require higher compensation for managing risky firms; consequently, pay should be positively related to *Std. dev. return*. *Age* is the

[9] Later, my dependent variable is nonroutine tasks. Unlike the interaction variable in the difference-in-difference regression, nonroutine tasks is not explicitly measured at the industry-year level. However, I still expect it to be correlated with errors at the industry-year level because the task measure is formed using occupational characteristics measured in 1977. This implies that the measurement error due to task changes within occupations changes over time, inducing industry-year correlation.

Table 2.2
Descriptive statistics for regression sample. This table shows descriptive statistics for each of the variables used in the paper. The sample is made up of 6,092 CEOs from 3,152 firms over the years 1984 to 2010. Definitions for each of the variables can be found in Table A.1 in the Appendix. The CEO pay data from 1984 to 1991 comes from a sample of large, publicly traded firms used in Yermack (1995) and the sample from 1992 to 2010 is extracted from Execucomp.

	N	Mean	Median	Std. Dev.	Min	Max
ln(CEO pay)	31,781	7.77	7.71	1.03	5.46	10.42
CEO Pay	31,781	4.13	2.22	5.37	0.00	33.13
Cash salary	31,685	6.89	6.84	0.69	5.12	8.89
Options	21,027	6.95	6.96	1.36	3.42	10.21
ln(Revenue)	31,781	7.33	7.31	1.54	3.28	11.00
Tobin's Q	31,781	2.28	1.17	4.35	−3.30	29.84
Income to assets	31,781	0.09	0.09	0.09	−0.30	0.36
Shareholder return	31,781	0.17	0.11	0.50	−0.77	2.40
Std. dev. return	31,781	0.03	0.02	0.01	0.01	0.08
Beta	31,781	1.01	0.94	0.54	0.03	2.70
CEO Tenure	31,781	7.04	4.84	7.25	0.00	34.86
Age	31,781	55.67	56.00	7.13	39.00	76.00
Original Routine Tasks	31,781	4.89	5.23	0.86	2.27	6.82
Nonroutine tasks	31,730	0.79	0.88	0.20	0.07	1.00
Analytical skill	31,730	0.72	0.82	0.26	0.03	1.00
Interpersonal skill	31,730	0.70	0.75	0.19	0.02	0.99
RAM price shock	31,781	−0.23	−0.30	0.59	−1.21	0.96
Ownership	30,796	0.03	0.00	0.06	0.00	0.40
Board Size	20,539	10.59	10.00	3.72	2.00	39.00
Pct. Indep. Directors	20,511	0.66	0.69	0.18	0.16	0.92
CEO is Chair	31,781	0.49	0.00	0.50	0.00	1.00
Institutional Ownership	31,781	0.60	0.61	0.24	0.08	1.00

executive's age, and *Tenure* is the length of time that the executive has worked for her current firm. Standard models predict that pay is positively related to these variables because CEOs become more effective with experience.

2.5.1 Identification Assumption

To interpret my difference-in-difference results as causal, the parallel trends assumption must hold. That is, absent any technology shock, CEO pay would have evolved similarly at firms that relied and did not rely on routine workers. This assumption can not be directly tested, but I present some suggestive evidence.

Figure 2.5 shows the evolution of nonroutine tasks by routine task exposure. I define my treatment group as firms in the highest quartile of routine task exposure in 1973, while my control group is made up of firms in the lowest quartile. The graph confirms that nonroutine task intensity followed similar trends at both groups of firms throughout the 1980s and early 1990s. For firms in the bottom quartile of routine tasks, nonroutine task levels are roughly constant across all years. This supports my use of this group of firms as a control group. Nonroutine task levels increase over time for my treatment group, and the trends between the treatment and control group show a noticeable break around the mid-1990s. This is consistent with the timing of the internet technology shock.

As further evidence that the parallel trend assumption is plausible, Figure 2.6 graphs the evolution of CEO pay by treatment and control group. The top panel of Figure 2.6 shows the natural log of total CEO pay. The bottom panel scales total CEO pay by the median pay of an employee in the CEO's industry, calculated using CPS data. The trends are similar for both measures. In the years before the internet technology shock, CEO pay was significantly lower for firms that relied on routine workers; however, pay evolved similarly in both the treatment and control group. Beginning in 1992 and continuing throughout the 1990s, pay grew considerably faster at firms that relied on routine workers. By the end of the sample, pay levels between the two groups had converged.

Finally, Figure 2.7 presents suggestive evidence that this differential growth rate was not due to differences in firm characteristics. I plot the empirical cumulative distribution functions (CDFs) for several firm characteristics split by treatment and control group. The firms appear broadly similar across most of these dimensions. Two notable exceptions are income to assets and firm beta. Firms that rely on routine workers are less profitable and have a lower beta; the differences in these distributions are roughly the same magnitude as the difference in the distribution of nonroutine tasks. Less profitable, lower risk firms should pay their CEOs less, so these characteristics likely explain some of the difference in CEO pay between these two groups before the technology shock. Increases in profitability or risk that are not related to changes in the workforce could lead to

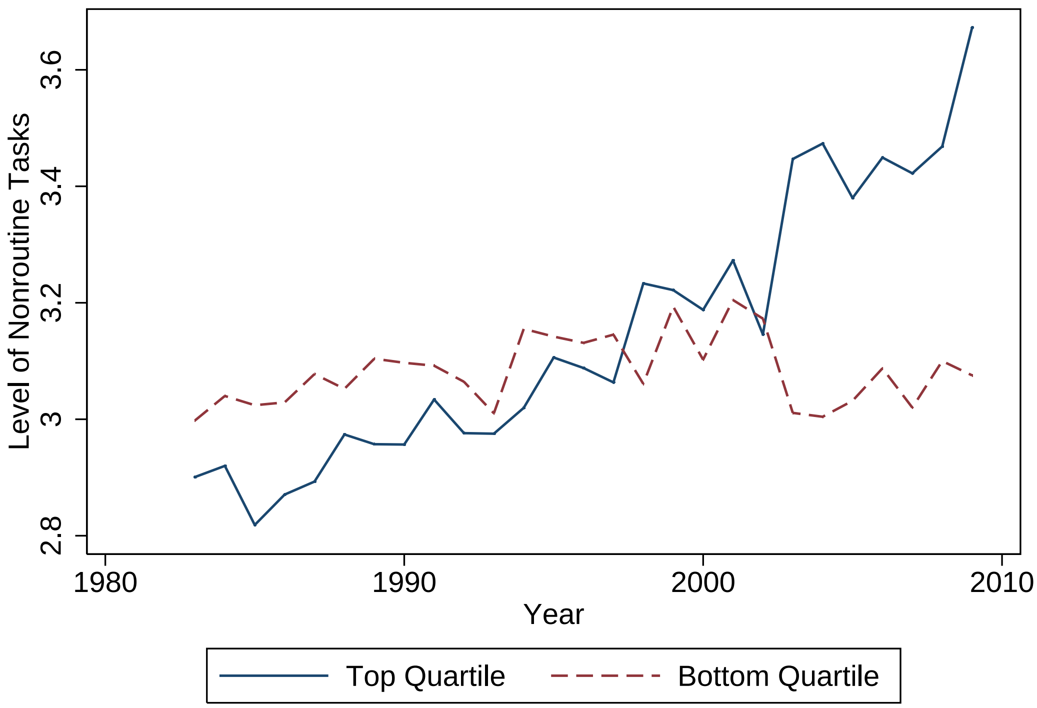

Figure 2.5. Evolution of Nonroutine Tasks by Routine Task Exposure. This figure shows the evolution of nonroutine task workers split by by exposure to original routine tasks (measured in 1973). The bottom quartile represents firms in the 0–25% range of the distribution of original routine tasks, while the top quartile represents firms in the 75–100% range of the distribution.

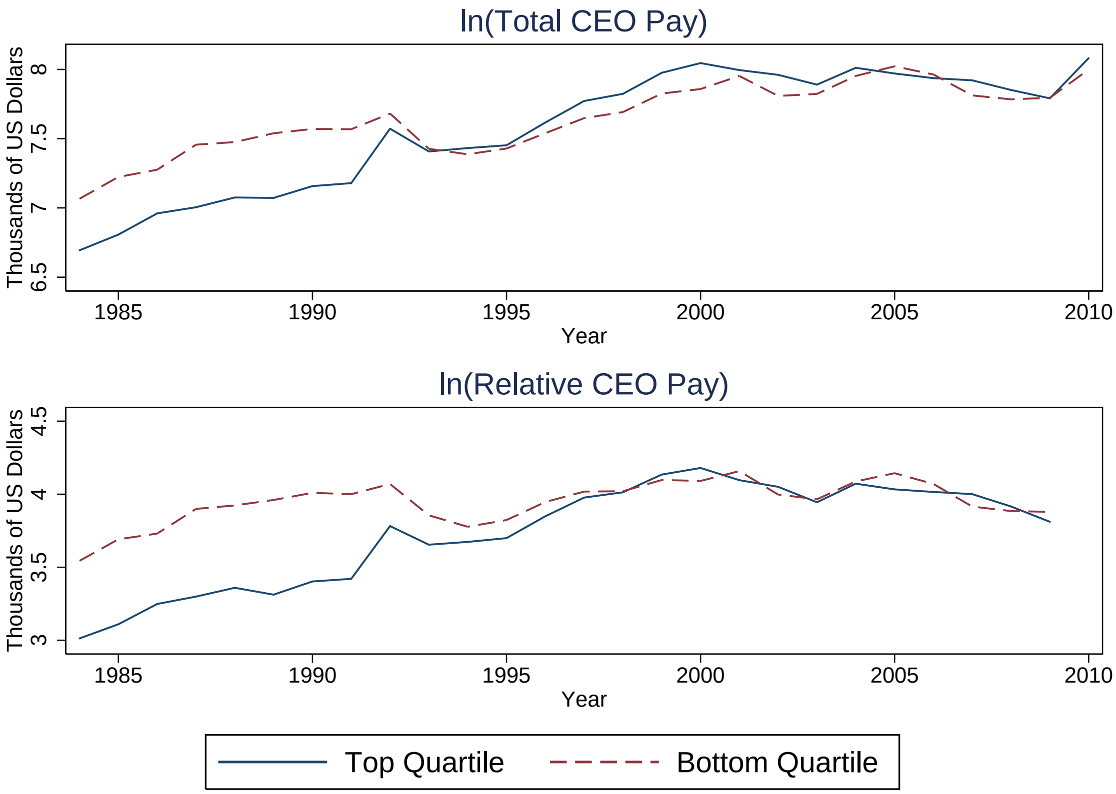

Figure 2.6. Evolution of CEO Pay by Routine Task Exposure. This figure displays the change in CEO pay split by exposure to original routine tasks (measured in 1973). The bottom quartile represents firms in the 0–25% range of the distribution of original routine tasks, while the top quartile represents firms in the 75–100% range of the distribution. CEO pay is defined as the sum of salary, bonus, restricted stock granted, long-term incentive payouts, and either the Black-Scholes value of stock-options granted (expected compensation) or the net value of stock-options exercised (realized compensation). Relative pay is the ratio of total CEO pay to the median pay of an employee in the CEO's industry.

overestimates of the effect of nonroutine workers on CEO pay, so I include these variables as controls in my regression estimations in Section 2.6.

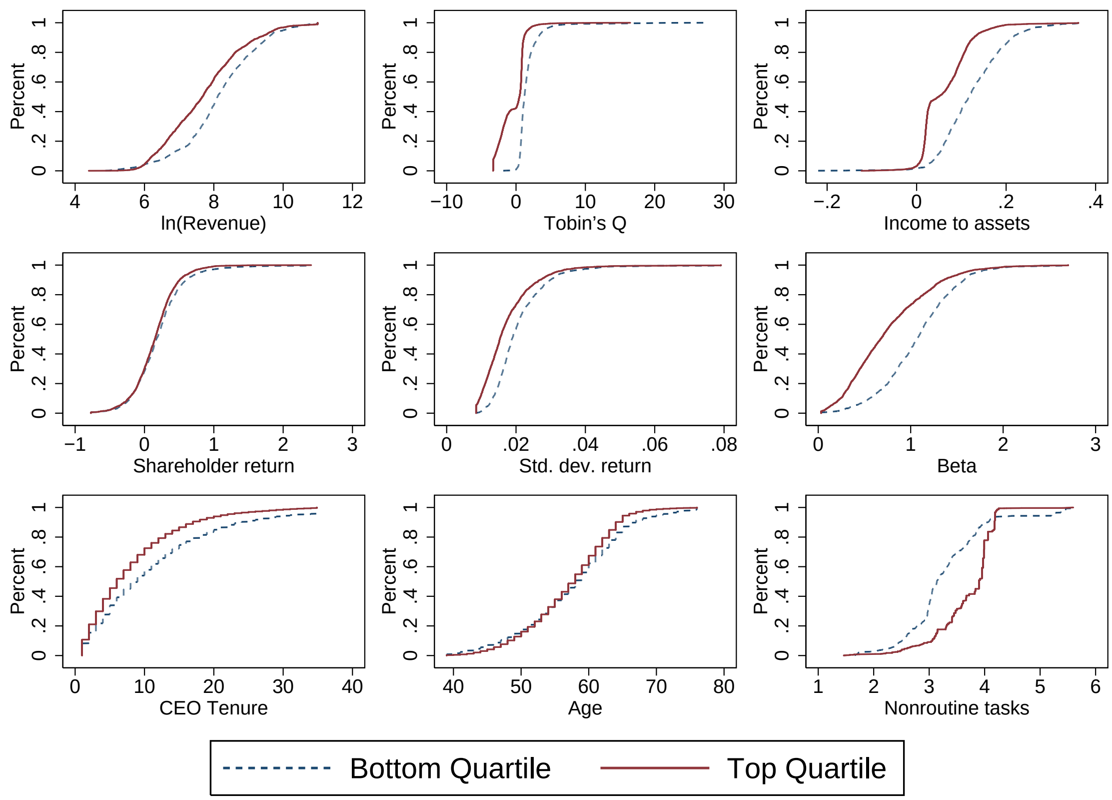

Figure 2.7. Empirical Cumulative Distribution Functions for Selected Firm Characteristics by Routine Task Exposure. This figure displays empirical cumulative distribution functions (CDFs) for several firm characteristics split by exposure to original routine tasks (measured in 1973). The bottom quartile represents firms in the 0–25% range of the distribution of original routine tasks, while the top quartile represents firms in the 75–100% range of the distribution.

As final evidence that the parallel trends assumption holds, I use unanticipated computer price shocks as an instrument for the level of nonroutine tasks. As the price of technology falls, it becomes easier for firms to computerize routine tasks. To the extent that these price shocks do not directly influence executive pay, they identify exogenous variation in the level of nonroutine task workers. This instrument does not require the common trends assumption to hold, but it leads to similar or stronger results, suggesting that the difference-in-difference approach described above is valid. I discuss the strengths and limitations of this instrumental variable approach in Section 2.6.3.

2.6 Results

2.6.1 Non-Parametric Difference-in-Difference Estimates

Before directly implementing the multivariate estimation approach described in Section 2.5, I first calculate the simple difference-in-difference estimate of the effect of nonroutine labor on CEO pay. I separate firms into treatment and control groups based on routine task intensity as of 1973. Firms in the top quartile of routine tasks form my treatment group, while firms in the bottom quartile of routine tasks make up my control group. I expect CEO pay to grow faster during the IT revolution at firms in the treatment group, since the composition of their workforce changes dramatically as routine tasks are computerized. Consistent with the graphical evidence presented in Figure 2.5 and the fact that the commercialization of the internet occurred in 1995, I define the IT revolution as post-1995.[10]

Panel A of Table 2.3 shows that before 1995, on average high routine task firms paid their CEOs roughly $650,000 less than low routine task firms. Pay grew much faster at high routine task firms, though, and post-1995 these CEOs actually made on average $200,000 more than CEOs at low routine task firms. The difference-in-difference estimate implies that increases in the human capital of the workforce increased CEO pay by $829,000; this difference is statistically significant at the 5% level.

The obvious problem with this estimate is that it does not adjust for differences in firm characteristics, such as size or profitability. I adjust for this using a semi non-parametric kernel matching difference-in-difference estimator (Heckman, Ichimura, and Todd, 1998). I first estimate the propensity score of being in the treatment group based on log revenue, Tobin's Q, income to assets, shareholder return, standard deviation of stock returns, beta, CEO tenure and age. For each firm in the treatment group, I then choose a match from the control group based on the common support of the propensity score.[11] Panel B of Table 2.3 reports the results of this procedure. Using the matched-sample control group increases the estimate of the difference-in-difference effect to about $1.1 million. Mean CEO pay increased by about $3.7 million dollars from 1984 to 2010, so changes in workforce composition explain about 30% of the aggregate increase in executive pay.

While this evidence is suggestive, it does not fully control for other differences between high and low skill firms that might influence executive pay, nor does it control for global factors that might simultaneously affect pay and employee skill. To control for these differences, I proceed to the multivariate analysis outlined in Section 2.5.

[10]The results are robust to choosing reasonable alternative definitions for the treatment period such as the first five years versus the last five years of the sample or the 1980s versus the 2000s.

[11]I weight using the epanechnikov kernel with a bandwidth of 0.1. The results are not sensitive to the choice of bandwidth.

Table 2.3

Nonparametric difference-in-difference Estimates. This table presents nonparametric estimates of the effect of employee skill on CEO pay. Panel A shows univariable difference-in-difference estimates. The treated firms are labeled *High Routine* and are defined as firms in the top quartile of routine tasks as of May 1973. The control firms are labeled *Low Routine* and are firms in the bottom quartile of routine tasks as of May 1973. The treatment period is post 1995, chosen to represent both the approximate midpoint of the sample and the beginning of the commercial internet. The columns report the mean level of CEO pay measured in thousands of US dollars; the last column shows the difference-in-difference estimate. Panel B shows the results of a semi-parametric kernel matching difference-in-difference estimate. For each firm in the treatment group, I choose a match from the control group based on log revenue, Tobin's Q, income to assets, shareholder return, standard deviation of stock returns, beta, CEO tenure and age using the epanechnikov kernel. I only match firms on the common support of the estimated propensity score.

Panel A: Univariable			
	High Routine	Low Routine	Difference
Pre 1995	1,896	2,552	-656^{**}
			(-2.40)
Post 1995	4,714	4,540	173
			(0.45)
Difference	2,818	1,989	829^{**}
			(2.17)

Panel B: Kernel Matching			
	High Routine	Low Routine	Difference
Pre 1995	1,951	2,547	-596^{**}
			(-2.02)
Post 1995	5,588	5,113	475
			(0.62)
Difference	3,638	2,566	$1{,}071^{*}$
			(1.68)

2.6.2 Multivariate Difference-in-Difference Estimates

Throughout this section, I estimate all models using both total CEO pay and relative CEO pay. The results are similar for both measures, so I focus my discussion on total CEO pay.

Table 2.4 presents estimates of Equation 2.2. This is the multivariable, continuous version of the simple difference-in-difference estimates presented in the previous section. One benefit of this approach as compared to the nonparametric estimate is that I do not need to take a stand on the cutoff between treatment and control groups. Instead, I use the continuous measure of routine tasks in 1973 (*original routine tasks*) as a proxy for the intensity of treatment. Higher values of this variable represent more exposure to routine task workers in 1973, which implies that the technology shock will have a larger effect on the workers of the firm. Thus, the intensity of the treatment varies positively with this variable.

Another benefit of the multivariable setting is that I do not need to explicitly specify a pre- and post-treatment period. Instead, I include a time trend and measure the differences in growth rates of CEO pay between firms with various levels of exposure to *original routine tasks*.[12] The estimated coefficient on this interaction in Column 1 is 0.47 and is statistically significant at the 1% level, implying that for a firm at the average level of routine intensity in 1973, the change in the composition of the workforce caused CEO pay to increase by 84%.[13] Column 4 reveals that the effect is similar in both magnitude and significance for relative CEO pay.

Columns 2 and 5 of Table 2.4 estimate a more stringent version of Equation 2.2 that includes CEO fixed effects. Since *original routine tasks* does not vary within a firm, the identification in this specification comes from comparing the pay changes of CEOs that switch firms. By controlling for unobserved CEO characteristics, the CEO fixed effect specification strengthens the causal interpretation of the estimate. The results should be interpreted with caution, though, since the number of individuals that work as CEO for two separate firms in my sample is quite small (234). With that caveat in place, including CEO fixed effects increases the point estimate to 0.7 (p-value < 0.01), implying that the increase in nonroutine workers led a 140% increase in CEO pay. This explains roughly one-half of the 300% increase in average CEO pay over this time period, suggesting that a significant portion of the rise in CEO pay is the result of optimal contracting.

One concern with the CEO fixed effects estimates is that CEOs that switch firms are likely to be talented. Since talented CEOs should be paid more, this might bias upward the estimated effect of nonroutine workers on executive pay. To eliminate concerns that the results are driven by this potential selection, Columns 3 and 6 repeat the analysis with CEO-firm fixed effects. Identification

[12] While not tabulated, the multivariable results are robust to defining the treatment as pre- and post-1995, as in the previous section.

[13] The average level of routine intensity in 1973 is 4.87. The economic magnitude of the estimate over the 27 years from 2010 to 1984 is then calculated as $exp(0.46 * 27 * .0486) = 1.84$. Note that routine intensity is scaled by 100 so that the coefficient is easier to read.

Table 2.4

Multivariable difference-in-difference Estimates. This table shows the results from estimating the following multivariable difference-in-difference model:

$$\ln y_{ijkt} = \beta_1 t + \beta_2 \phi + \beta_3 t\phi + \Gamma X_{ijkt-1} + \epsilon,$$

where y_{ijkt} is total pay for CEO i at firm j in industry k at time t, t is a linear *Time Trend*, ϕ is *original routine tasks* of industry k in 1973, and β_3 is the difference-in-difference estimate. All control variables are measured as of the previous fiscal year end. Definitions for each of the variables can be found in Table A.1 in the Appendix. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	ln(CEO Pay)			ln(Relative CEO Pay)		
	(1)	(2)	(3)	(4)	(5)	(6)
Interaction	0.47***	0.70***	0.53***	0.54***	0.57***	0.42**
	(3.18)	(3.67)	(2.79)	(2.81)	(2.74)	(2.00)
Time Trend	0.02***	0.02**	0.03***	0.01	0.02	0.03**
	(3.25)	(2.04)	(3.13)	(0.88)	(1.64)	(2.52)
Original Routine Tasks	−0.06**	−0.23***		−0.16***	−0.26***	
	(−2.16)	(−3.36)		(−4.97)	(−3.70)	
ln(Revenue)	0.39***	0.23***	0.22***	0.39***	0.22***	0.21***
	(68.28)	(16.38)	(14.26)	(63.59)	(14.68)	(13.11)
Tobin's Q	0.03***	0.02***	0.02***	0.02***	0.01***	0.02***
	(11.49)	(6.84)	(7.07)	(7.44)	(5.73)	(6.21)
Income to assets	−0.04	0.69***	0.75***	0.40***	0.83***	0.86***
	(−0.43)	(7.27)	(7.74)	(3.86)	(8.39)	(8.62)
Shareholder return	0.21***	0.16***	0.16***	0.24***	0.17***	0.16***
	(13.18)	(15.36)	(15.31)	(13.76)	(14.32)	(14.10)
Std. dev. return	0.70	0.81	1.00	3.26***	1.04	1.13
	(0.71)	(0.98)	(1.22)	(3.12)	(1.10)	(1.19)
Beta	0.12***	−0.01	−0.01	0.11***	−0.01	−0.01
	(6.57)	(−0.98)	(−0.81)	(5.73)	(−0.56)	(−0.36)
CEO Tenure	−0.00***	−0.01***	−0.02***	−0.00***	−0.01***	−0.02***
	(−4.30)	(−4.47)	(−6.28)	(−3.46)	(−4.78)	(−6.27)
Age	−0.00**	0.01**	0.01***	−0.00**	0.01**	0.01***
	(−2.33)	(2.52)	(4.24)	(−1.97)	(2.37)	(3.82)
Constant	4.35***	5.79***	4.46***	1.15***	2.39***	0.89***
	(30.17)	(14.94)	(26.04)	(6.90)	(5.83)	(5.02)
CEO FE	No	Yes	No	No	Yes	No
CEO-Firm FE	No	No	Yes	No	No	Yes
Number of CEOs	6,090	6,090	6,090	5,965	5,965	5,965
Observations	31,730	31,730	31,730	30,282	30,282	30,282
R^2	0.40	0.77	0.78	0.36	0.77	0.77

in this setting comes from changes in the workforce composition that occur during an executive's tenure. Note that because the industry does not vary at the CEO-firm level, this specification absorbs *original routine tasks*. While the magnitude of the effect drops slightly, these estimates are highly statistically significant and still imply that changes in the composition of the workforce from 1984 to 2010 lead CEO pay to double.

An alternative difference-in-difference specification is to use year dummy variables rather than a time trend, that is to estimate

$$\ln y_{ijt} = \sum_{i=1985}^{2010} \sum_{j=1985}^{2010} d_{ij}\alpha + \beta_2 \text{routine}_{k_{1973}} + \sum_{i=1985}^{2010} \sum_{j=1985}^{2010} d_{ij} * \text{routine}_{k_{1973}} * \gamma + \Gamma X_{ijt-1} + \epsilon_{ijt}, \quad (2.3)$$

where $d_{ij} = 1$ if $i = j$ and 0 otherwise. This places less structure on the way that the technology shock propagates through time, but comes at the cost of making it more difficult to interpret the economic magnitudes of the effect. The difference-in-difference estimates for each year are contained in the γ vector. Figure 2.8 plots γ along with the 95% confidence interval for each year from 1985 to 2010. The estimated effect of nonroutine workers on CEO pay is around zero (or slightly negative) until 1994. After 1994, the effect grows until around 2000 and then it stays relatively constant for the remainder of the sample. This approach allows the data to endogenously determine the timing of the technology shock; the results are consistent with my interpretation of the shock as the spread of the internet.

2.6.2.1 Rescaling the Difference-in-Difference Estimate

The above estimates can be interpreted as the reduced form regression of the following 2SLS model:

$$\text{nonroutine}_{kt} = \beta_1 t + \beta_2 \text{routine}_{k_{1973}} + \beta_3 * t * \text{routine}_{k_{1973}} + \Gamma X_{ijt-1} + \epsilon_{kt} \quad (2.4)$$

$$\ln y_{ijt} = \gamma_1 t + \gamma_2 \text{routine}_{k_{1973}} + \gamma_3 \widehat{\text{nonroutine}}_{kt-1} + \Gamma X_{ijt-1} + \epsilon_{ijt}. \quad (2.5)$$

In Equation 2.4, I use the interaction term from the difference-in-difference regression ($t * \text{routine}_{k_{1973}}$) as an instrument for nonroutine tasks. Equation 2.5 is the second stage estimate of CEO pay as a function of the instrumented level of nonroutine tasks. This 2SLS approach is essentially a rescaling of the effect reported in Table 2.4. The advantage of this rescaling is that it allows for a direct interpretation of the effect of nonroutine workers on CEO pay.[14]

Panel A of Table 2.5 shows the first stage estimates of nonroutine tasks. As anticipated, the

[14] In Table 2.4, I interpret the effect of changes in nonroutine workers on CEO pay using the values for the 1973 level of routine workers and the number of years in my sample. The rescaling presented here allows me to directly calculate the effect of changes in nonroutine workers.

Figure 2.8. Year Dummy Variable difference-in-difference Estimates. I estimate the natural log of total CEO pay as a function of year fixed effects, the level of routine tasks in 1973, the interaction of these two variables, and other covariates. This graph shows the estimated coefficient on the interaction for each year. The solid blue line represents the estimated coefficient, while the dotted red lines show the 95% confidence level.

interaction term is positively correlated with the level of nonroutine tasks and the first stage F-statistics are all well over 100; weak instruments are not a problem. Identification in this setting requires the same assumptions as the reduced form estimates, so the exogeneity condition is satisfied only if the parallel trends assumption holds. I discuss the plausibility of this assumption in Section 2.5.1; the evidence suggests that the parallel trends assumption is likely valid for this sample.

Panel B of Table 2.5 presents the second stage estimates of CEO pay as a function of nonroutine tasks. Columns 1 and 3 estimate the model using CEO fixed effects, while Columns 2 and 4 use CEO-firm fixed effects. In all cases, the coefficient on nonroutine tasks is positive and statistically significant at the 1% level. From 1984 to 2010, the average level of nonroutine tasks increases by about 0.7. Together with the estimates in Table 2.5, this implies that the change in nonroutine workers caused total CEO pay to increase between 42% and 63% and relative CEO pay to increase between 34% and 52%. These estimates are somewhat smaller than those implied by the difference-in-difference model, but still explain about 20% of the aggregate increase in CEO pay from 1984 to 2010.

This specification also allows me to examine the cross-sectional effects of workforce composition. A one standard deviation increase in nonroutine tasks raises CEO pay between 50–80%. Evaluated at the mean level of CEO pay, this implies an increase of between $2 to $3.2 million dollars. Thus, differences in workforce composition help explain the variation in executive manager salaries.

2.6.2.2 Robustness Tests for the Effect of Nonroutine Tasks on CEO Pay

Although Section 2.5 suggests that the evolution of both CEO pay and nonroutine workers followed similar trends before the IT revolution, my identification can still fail if there are time-varying omitted variables that drive both changes in nonroutine workers and CEO pay. My experimental setting helps alleviate this concern, because any time varying omitted variable would have to affect nonroutine workers and executive pay at firms that relied on routine workers before the IT revolution (such as manufacturing firms and commercial banks), but not affect firms that did not rely on these workers (such as pharmaceuticals and services). This cross-sectional heterogeneity forces any omitted variable story to be reasonably complex.

One possible explanation is that a combination of de-unionization and deregulation drive the results. The decline of unions might plausibly be correlated with both the rise of nonroutine workers and the increase in CEO pay. Since unionized firms are primarily in my treatment group, this might bias my estimates. Additionally, Philippon and Reshef (2012) show that the deregulation of the financial industry led to a sharp increase in the use of nonroutine workers. Banks relied on many routine tasks before the IT revolution, so this also has the possibility to affect my estimates.

Table 2.5
Rescaled Difference-in-Difference Estimates. The difference-in-difference regression in Table 2.4 can be interpreted as the reduced form regressions of a 2SLS estimate of the effect of *Nonroutine tasks* on CEO pay. This table reports that 2SLS estimate using the interaction of a linear time trend and *original routine tasks* from Table 2.4 as the instrument for *Nonroutine tasks*. Though not reported to save space, the regression includes all other covariates shown in Table 2.4. The first stage regression is shown in Panel A. Panel B shows the second stage regression. Definitions for each of the variables can be found in Table A.1 in the Appendix. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Panel A: First Stage Regrsesions				
	Nonroutine tasks			
	(1)	(2)	(3)	(4)
Time Trend X Original Routine	1.02***	1.06***	0.96***	1.00***
	(13.22)	(13.69)	(11.91)	(12.25)
First-stage F statistic	174.86	187.30	141.80	150.18
R^2	0.14	0.14	0.13	0.13

Panel B: Second Stage Regressions				
	ln(Total pay)		ln(Relative Pay)	
	(1)	(2)	(3)	(4)
Nonroutine Tasks	0.69***	0.50***	0.59***	0.42***
	(4.80)	(3.69)	(3.59)	(2.68)
Time Trend	0.04***	0.04***	0.03***	0.04***
	(8.63)	(10.50)	(6.57)	(8.10)
Original Routine Tasks	0.05		−0.03	
	(0.82)		(−0.39)	
CEO FE	Yes	No	Yes	No
CEO-Firm FE	No	Yes	No	Yes
Number of CEOs	6,090	6,090	5,965	5,965
Observations	31,730	31,730	30,282	30,282
R^2	0.76	0.77	0.75	0.77

I confirm that my results are not driven by either de-unionization or deregulation; to conserve space the results are untabulated. Using data from Hirsch and Macpherson (2002) to control for firm unionization does not affect my difference-in-difference estimates. Similarly, my results are robust to controlling for financial deregulation using the index in Philippon and Reshef (2012). As further evidence, I exclude firms that were likely to experience unionization or deregulation changes during the IT revolution and find similar results.

It is well known that CEO pay varies with firm size, and there is some evidence that suggests that high skill firms are larger than low skill firms. I explicitly control for firm size in the multivariate results, but to ensure that my results are not driven by firm size or the superstar effect (Rosen, 1981; Gabaix and Landier, 2008), I employ the following procedure. In each year from 1984 to 2009, I first sort firms into quintiles based upon revenue. Within each size quintile, I then sort firms into quintiles based upon nonroutine task intensity. This double sort helps account for the fact that larger firms pay their managers more. I then compare the median CEO pay across each bin. The results of this procedure are shown in Figure 2.9. Across each size quintile, executive compensation is increasing in nonroutine task intensity. Moving from the lowest skill quintile to the highest skill quintile raises median pay by about $400,000 (about 33%) for the smallest firms, but raises median pay by about $6 million (about 48%) for the largest firms. This positive interaction between nonroutine labor, firm size, and CEO pay is consistent with theoretical predictions of production-based complementarities between CEOs and nonroutine workers.

Additionally, I repeat my difference-in-difference regression separately for firms in each size quintile. While the magnitude of the effect is largest for the top three size quintiles, it is positive and significant across all quintiles (untabulated).[15]

To further support my claim that my results are driven by changes in the composition of the workforce, Table 2.6 performs a placebo test. Recall that there is nothing mechanical in the estimation strategy employed in Table 2.4 that ensures that firms actually experience a change in workforce composition. Rather, I am identifying off of the potential for workforce change. If the effects reported above are truly identified through the channel I have proposed, I should only see effects for firms that actually experienced an increase in human capital, i.e. firms that increased their nonroutine worker intensity. Table 2.6 tests this by re-estimating column 2 of Table 2.4 for various subsets of firms. Column 1 limits my sample to firms that actually increased their nonroutine task intensity from the beginning of the sample to the end of the sample.[16] As expected, the difference-in-difference for this group of firms is positive and similar in magnitude to the estimates in Table 2.4. Column 2 repeats this exercise for firms that started and ended the sample in the top

[15] The effect for the smallest quintile is only marginally significant at the 10% level.

[16] I define this group as firms that both experienced a positive change in nonroutine workers from 1984 to 2010 and that did not begin and end the sample in the same quartile of nonroutine tasks, so that the change represents a meaningful increase in task complexity.

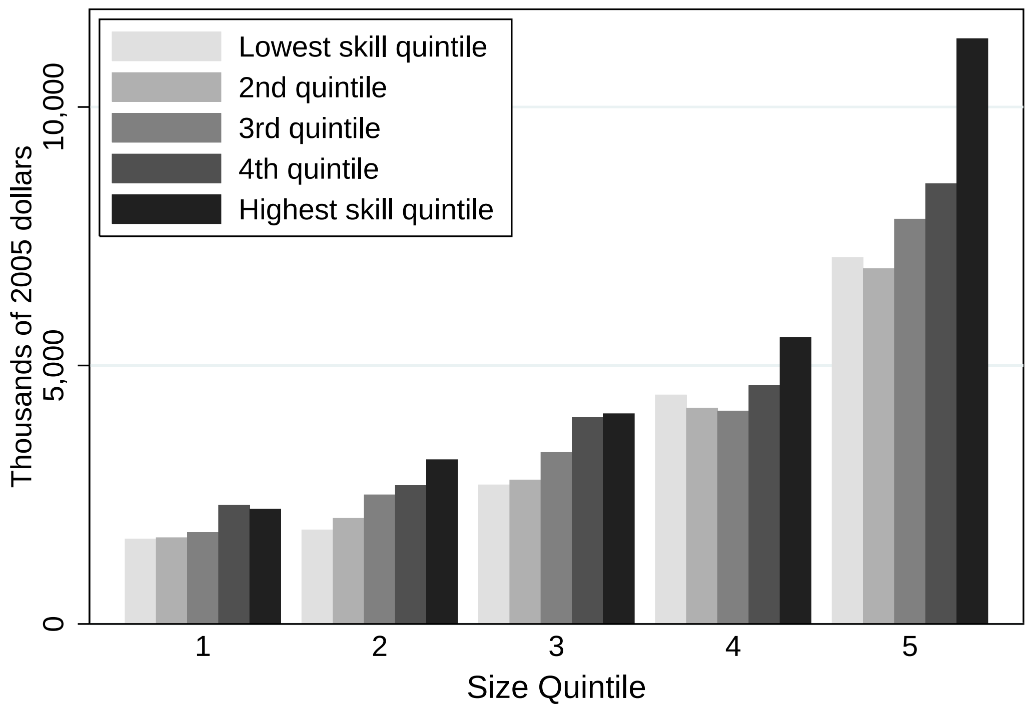

Figure 2.9. Median executive compensation by skill and size quintiles. This figure shows the median CEO expected pay by skill and size quintiles. To control for size effects, in each year firms are first sorted into size quintiles based on revenue and then sorted within each size quintile into quintiles of nonroutine tasks. Compensation data from 1984 to 1991 is from Yermack (1995), and compensation data from 1992 to 2010 is from Execucomp and is defined as the sum of salary, bonus, restricted stock granted, long-term incentive payouts, and the Black-Scholes value of stock-options granted. Employee skill is the level of nonroutine job tasks measured at the industry level using Current Population Survey data following Autor et al. (2003).

quartile of nonroutine task use. These firms did not experience much change in the composition of their workforce, so if the results are driven by nonroutine workers the estimated effect should disappear. If, however, the results are driven by the direct effects of technology (e.g., if technology increases the scale of the CEOs' effort, leading to higher pay) the results should similar within this group of firms. Consistent with changes in nonroutine workers driving changes in CEO pay, the difference-in-difference estimate in Column 2 is small and statistically indistinguishable from zero.

Columns 3 and 4 of Table 2.6 perform a similar exercise for high-tech firms. The technology shock the underlies my identification is plausibly exogenous to most firms, but might be endogenous to high-tech firms. Excluding these firms strengthens my estimates; Column 4 implies that the increase in nonroutine workers over the last three decades caused CEO pay to increase by 285%. If I estimate the difference-in-difference model with only high-tech firms, the estimated effect of nonroutine workers switches signs and is again indistinguishable from zero. This is consistent with the nonroutine worker channel, since high-tech firms experienced little change in the composition of their workforce.

Taken together, these results suggest that the estimates in Table 2.4 represent the causal effect of changes in workforce nonroutine task intensity on executive pay.

2.6.3 2SLS Estimates of the Effect of Nonroutine Workers on CEO Pay

The difference-in-difference methodology used in this paper provides a useful setting to identify causal effects; however, it suffers from the fact that the effect is identified primarily from industry-level cross-sectional heterogeneity. Thus, it is possible that industry differences account for some of the effect. To directly take advantage of the heterogeneity in workforce skill across time, I implement a two-stage least squares model. Autor et al. (2003) provide evidence that the fall in the price of computer technology caused firms to replace routine workers with computers and hire additional nonroutine workers. Motivated by that result, I use unanticipated computer price shocks as an instrument for the level of nonroutine task intensity.

Specifically, I use the average retail cost of one mebibyte (1,048,576 bytes) of computer RAM for my measure of computer prices.[17] I choose this for my computer price series both because it is available for the my entire sample and because Intel released the first DRAM chip in 1970, which roughly corresponds to the start of the computer technology shock. Of course, there is a strong downward time trend in the price of RAM. To alleviate the effect of the time trend driving any results, I hindcast computer prices and take the difference between the actual price in year t+1 and my estimate as the computer price shock. For each year t, I use data on RAM prices from 1970 to

[17]This data is from John C. McCallum, and is available at http://www.jcmit.com/memoryprice.htm.

Table 2.6
Channel driving difference-in-difference results. This table explores the channel driving the results in Table 2.4. My hypothesis is that changes in the composition of the firm's workforce led to increased CEO pay. If the difference-in-difference estimates are operating through this channel, the estimated effect should be limited to firms that actually experienced changes in workforce skill. In this table, I estimate the difference-in-difference model shown in Column 2 of Table 2.4 for various subsamples of firms. While not shown, these regressions include all of the covariates displayed in Table 2.4. Column 1 limits the sample to firms that increased their use of nonroutine task employees and that were not always in the top or bottom quartile of nonroutine task use. Column 2 limits the sample to firms that begin and end the sample in the top quartile of nonroutine task use. Column 3 limits the sample to high tech firms and Column 4 omits high tech firms. Definitions for each of the variables can be found in Table A.1 in the Appendix. Standard errors are clustered at the CEO level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Panel A:

	ln(Total Pay)			
	(1) Low to High	(2) Always High	(3) High Tech	(4) No High Tech
Interaction	0.63**	0.22	−0.93	1.06***
	(2.28)	(0.34)	(−1.19)	(5.64)
Time Trend	0.03*	0.02	0.10**	0.01
	(1.91)	(0.71)	(2.45)	(0.65)
Original Routine Tasks	0.02	−0.19	−0.27	−0.27***
	(0.13)	(−0.98)	(−1.05)	(−3.46)
CEO FE	Yes	Yes	Yes	Yes
Number of CEOs	1,661	365	387	2,432
Observations	14,936	6,981	9,473	22,257
R^2	0.77	0.75	0.74	0.80

Panel B:

	ln(Relative Pay)			
	(1) Low to High	(2) Always High	(3) High Tech	(4) No High Tech
Interaction	0.55*	−0.16	−1.29	0.98***
	(1.84)	(−0.23)	(−1.51)	(4.82)
Time Trend	0.02	0.04	0.11**	0.00
	(1.31)	(0.94)	(2.42)	(0.21)
Original Routine Tasks	−0.07	−0.04	−0.08	−0.35***
	(−0.53)	(−0.18)	(−0.31)	(−4.27)
CEO FE	Yes	Yes	Yes	Yes
Number of CEOs	1,599	343	372	2,333
Observations	14,352	6,459	8,995	21,287
R^2	0.77	0.74	0.73	0.79

t-1 to estimate Moore's Law. I then use this estimation to predict the price in *t*+1. I define *computer price shock* as the cumulative three-year sum of the residuals. The time series of *computer price shock* has no clear visual time trend and a regression of the price shock on a linear time trend results in a small positive, but statistically insignificant, coefficient.

In addition to *computer price shock*, I use *original routine tasks* as a second instrument. For these instruments to be valid, they must be correlated with the level of nonroutine task intensity. Panel A of Table 2.7 shows the first stage estimates. *Computer price shock* is negatively correlated with nonroutine labor and is highly significant. An unexpected fall in computer prices (a negative shock) leads to an increase in nonroutine workers, consistent with my prediction. The level of *original routine tasks* is negatively correlated with nonroutine tasks, intuitively because firms with high levels of routine tasks begin the sample with few nonroutine workers. The F-statistic on the instruments ranges between 11 and 17, suggesting that weak instruments are unlikely to be a problem.

In addition being relevant, these instruments must also pass the exclusion criteria—that is, they should be uncorrelated with CEO pay except through changes in employee skill. *Original routine tasks* is not likely to be a problem. The negative and significant coefficient on *original routine tasks* in the reduced form equation estimated in Table 2.4, combined with the zero coefficient in the 2SLS equation in Table 2.5, suggests that any estimated changes in CEO pay attributed to *original routine tasks* likely occurs due to changes in the level of nonroutine employees. *Computer price shock* is potentially more problematic. While this variable is plausibly exogenous to CEO pay, it is possible that changes in technology directly increase the productivity of the CEO, causing higher pay irrespective of changes in the labor force. Thus, these instrumental variable estimates are likely biased upward and should be interpreted as an upper bound on the effect of nonroutine labor on CEO pay. This effect is likely to be less severe for relative CEO pay, since general technology effects likely also increase worker pay.

Table 2.7 reports the coefficients from the 2SLS estimate of

$$\text{nonroutine}_{kt} = \beta_1 t + \beta_2 \text{computer price shock}_{t-1} + \beta_3 \text{routine}_{k_{1973}} + \Gamma X_{ijt-1} + \epsilon_{kt} \qquad (2.6)$$

$$\ln y_{ijt} = \gamma_1 t + \gamma_2 \widehat{\text{nonroutine}}_{kt-1} + \Gamma X_{ijt-1} + \epsilon_{ijt}, \qquad (2.7)$$

where y_{ijt} is total pay for CEO i at firm j at time t. I instrument for *Nonroutine tasks* using *original routine tasks* and *computer price shock*. Panel B of Table 2.7 shows the second stage regressions; all estimates include CEO fixed effects. Note that because *computer price shock* does not vary across firms within a given year, it is not possible to include year fixed effects. Instead, I include a linear time trend. The 2SLS estimate of the effect of nonroutine workers on CEO pay in Column 1 is significant at the 1% level and implies an incredible 1600% increase in CEO pay; the estimate in Column 3 suggests a 250% increase in relative CEO pay. While these effects are huge, it is important to remember that they are upper bounds. Adding *original routine tasks* as an additional instrument reduces the magnitude of the effect considerably, to a roughly 75% increase in both total and relative CEO pay. Although identified in a completely different way, the magnitude and statistical significance of these estimates is strikingly similar to the difference-in-difference estimates presented earlier.

2.6.4 Efficient Pay or Rent Extraction?

One potential concern with the preceding estimates is that the relationship between nonroutine workers and CEO pay might be driven by rent extraction. Nonroutine work is, by definition, difficult to monitor. As a result, shareholders of nonroutine firms might have a more difficult time preventing CEOs from extracting private benefits.

To control for this possibility, I repeat the main estimations reported above controlling for the fraction of institutional ownership, the size of the board, the percentage of independent directors, a dummy for whether or not the CEO is chairman of the board, and Khanna et al.'s (2013) index of CEO power. The effect of nonroutine workers on CEO pay is unaffected; the results are reported in Appendix A.4.

As additional evidence against rent extraction, I use the difference-in-difference framework from Table 2.4 to estimate the effect of changes in the composition of the workforce on CEO pay incentives. First, I follow Edmans, Gabaix, and Landier (2009) and create a measure of CEO wealth performance sensitivity. Column 1 of Table 2.8 reports the results. The coefficient on the interaction term is positive and statistically significant at the 1% level, implying a large increase in performance incentives. Column 2 shows that managing nonroutine workers leads to a lower percentage of compensation paid in cash, though the effect is not statistically significant. Column 3 reveals that changes in the composition of the workforce cause an increase in CEO share ownership. CEOs that manage nonroutine workers appears to receive higher powered incentive pay.

While compensation incentives are important, the threat of being fired is another important incentive mechanism. Column 4 demonstrates that CEOs of nonroutine firms have shorter tenure than CEOs of routine firms. Column 5 examines forced turnover, which I define as a CEO that leaves the firm before age 60 and does not get rehired as a CEO in my sample.[18] I use a linear probability model to estimate the effect of managing nonroutine workers on the probability of forced turnover. The estimate in Column 5 shows that an increase from the 25th percentile to

[18] I do not count any CEO that leaves in the last two years of my sample as forced, since I want to have sufficient time to observe a potential new position.

Table 2.7
2SLS Estimates of the effect of employee skill on CEO pay. This table shows 2SLS estimates of the following model:

$$\ln y_{ijkt} = \alpha_i + \beta\rho_{kt-1} + \Gamma X_{ijkt-1} + \epsilon,$$

where y_{ijkt} is total pay for CEO i at firm j in industry k at time t, $\rho_{kt_{-1}}$ is *Nonroutine tasks* of industry k in the prior fiscal year, and other covariates are measured as of the previous fiscal year. I instrument for *Nonroutine tasks* using *original routine tasks* and *RAM price shock*. The first stage regression is shown in Panel A. Panel B shows the second stage regression. Though not shown to conserve space, the regressions include all other covariates shown in Table 2.4. Definitions for each of the variables can be found in Table A.1 in the Appendix. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Panel A: First Stage Regrsesions				
	Nonroutine tasks			
	(1)	(2)	(3)	(4)
Computer Price Shock	-0.44^{***}	-0.46^{***}	-0.43^{***}	-0.45^{***}
	(-3.55)	(-3.72)	(-3.38)	(-3.56)
Original Routine Tasks		-0.24^{***}		-0.24^{***}
		(-4.39)		(-4.23)
First-stage F statistic	12.59	17.10	11.39	15.66
Hansen J statistic	0.00	20.37	0.00	1.84
p-value for J-statistic		0.00		0.18
R^2	0.12	0.12	0.11	0.12

Panel B: Second Stage Regressions				
	ln(Total pay)		ln(Relative Pay)	
	(1)	(2)	(3)	(4)
Nonroutine Tasks	4.03^{***}	0.79^{***}	1.80^{**}	0.81^{***}
	(3.07)	(3.27)	(2.02)	(2.98)
CEO FE	Yes	Yes	Yes	Yes
Number of CEOs	6,095	6,090	5,969	5,965
Observations	31,764	31,730	30,311	30,282
R^2	0.26	0.75	0.66	0.74

the 75th percentile in the level of nonroutine tasks increases the probability of forced turnover by nearly 30%; this effect is significant at the 5% level.

The theme that emerges from Table 2.8 is that increases in nonroutine workers are associated with stronger CEO incentives. This evidence is difficult to reconcile with models of CEO rent extraction, but consistent with optimal pay contracts.

2.6.5 Textual Analysis of the Role of the CEO

The increase in CEO pay documented in this paper is consistent with the existence of synergies between managers and skilled employees. As additional evidence of these synergies, and to provide insight into the role of the CEO, I create a measure of management focus, or attention. This measure is based on textual analysis of the management discussion and analysis (MD&A) section of the firm's 10-K.

To create this measure, I extract the MD&A for each year that a firm files a 10-K with Edgar. I randomly select 100 MD&A sections and carefully read them to classify content based on four areas: people, operations, strategic innovation, and external stakeholders. Management literature suggests that the role of the CEO can be summarized in these four areas (Hart and Quinn, 1993). I further divide external stakeholders into customers, competitors, and shareholders. Through reading these randomly selected MD&As, I create lists of words that correspond to each of these categories. For example, people related words include employee, staff, and labor. I further check my list of words against lists used in other financial text analysis (Li, 2010; Loughran and McDonald, 2011). For each category, I count the number of words from that category that are used in the MD&A and scale each count by the total number of words in the MD&A.

Figure 2.10 shows the average of these measures over time. Note that although the absolute level of operations words far exceeds people words, there is a clear increase in the focus on people and a decrease in the focus on operations over time. From 1994 to 2012, the percentage of people words increased by about 10 basis points. The average length of the MD&A section is about 6,700 words, so this implies an increase of about 7 people words per MD&A section. While small, this represents a 20% increase relative to the sample mean of 34 people words. Over the same time period, the focus on operations declined roughly 15% relative to the mean.

One concern is that these trends might simply reflect changes in the structure of the economy (i.e., a switch from a manufacturing to a service economy). Figure 2.11 shows the evolution of people focus across 9 Fama-French Industries.[19] While the pattern is stronger in some industries than others, the broad shift in attention to the firm's employees is apparent across industries.

If there are synergies between managers and nonroutine workers, I expect CEOs of nonroutine

[19] I exclude the 10th industry, "other".

Table 2.8
Employee tasks and CEO incentives. This table shows the results from estimating the difference-in-difference regression shown in Column 3 of Table 2.4 for various measures of CEO incentives. In Column 1, the dependent variable is the natural log of the sum of CEO salary and bonus. The dependent variable in Column 2 is the natural log of the Black-Scholes value of options granted. Column 3 estimates the effect of employee tasks on the percent of equity owned by the CEO, and Column 4 estimates the effect on CEO tenure. The difference-in-difference estimate is given by the coefficient on *interaction*. Definitions for each of the variables can be found in Table A.1 in the Appendix. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	Measures of Incentive Pay				
	(1) Performance Sensitivity	(2) Percent Cash	(3) Ownership	(4) CEO Tenure	(5) Forced Turnover
Time Trend	−2.76***	−0.02***	−0.00***	0.23***	
	(−3.95)	(−4.93)	(−3.42)	(4.77)	
Original Routine Tasks	−6.05***	0.05**	−0.01***	1.23**	
	(−2.80)	(1.97)	(−3.86)	(2.23)	
Interaction	27.16***	−0.09	0.04***	−2.71***	
	(3.26)	(−1.49)	(3.65)	(−3.55)	
Nonroutine Tasks					0.29**
					(2.28)
ln(Revenue)	−8.14***	−0.03***	−0.01***	−0.08	0.00**
	(−8.02)	(−5.74)	(−12.58)	(−1.27)	(2.44)
Tobin's Q	−0.01	−0.00***	0.00***	−0.02***	0.00**
	(−0.09)	(−4.30)	(2.97)	(−5.03)	(2.28)
Income to assets	24.86***	−0.23***	0.03***	0.98***	−0.04***
	(5.31)	(−6.97)	(6.02)	(3.35)	(−2.66)
Shareholder return	0.55	−0.01***	0.00***	−0.07**	−0.01***
	(0.77)	(−4.03)	(3.93)	(−2.42)	(−4.34)
Std. dev. return	24.78	0.08	0.05*	−4.22**	0.69***
	(1.26)	(0.28)	(1.78)	(−2.14)	(4.97)
Beta	0.39	−0.01	−0.00**	0.14***	−0.00
	(0.67)	(−1.58)	(−2.08)	(3.42)	(−0.38)
CEO Tenure	0.28	0.00	0.00***		0.00
	(1.64)	(1.43)	(3.12)		(0.72)
Age	1.56***	0.00***	−0.00	0.74***	−0.00***
	(2.93)	(2.87)	(−0.86)	(24.55)	(−11.42)
Constant	44.90**	0.66***	0.16***	−41.22***	0.08***
	(2.11)	(4.45)	(9.20)	(−13.69)	(6.81)
CEO FE	Yes	Yes	Yes	Yes	No
Year FE	No	No	No	No	Yes
Number of CEOs	4,136	6,090	6,032	6,114	6,119
Observations	15,937	31,730	30,951	31,950	31,984
R^2	0.83	0.62	0.88	0.96	0.01

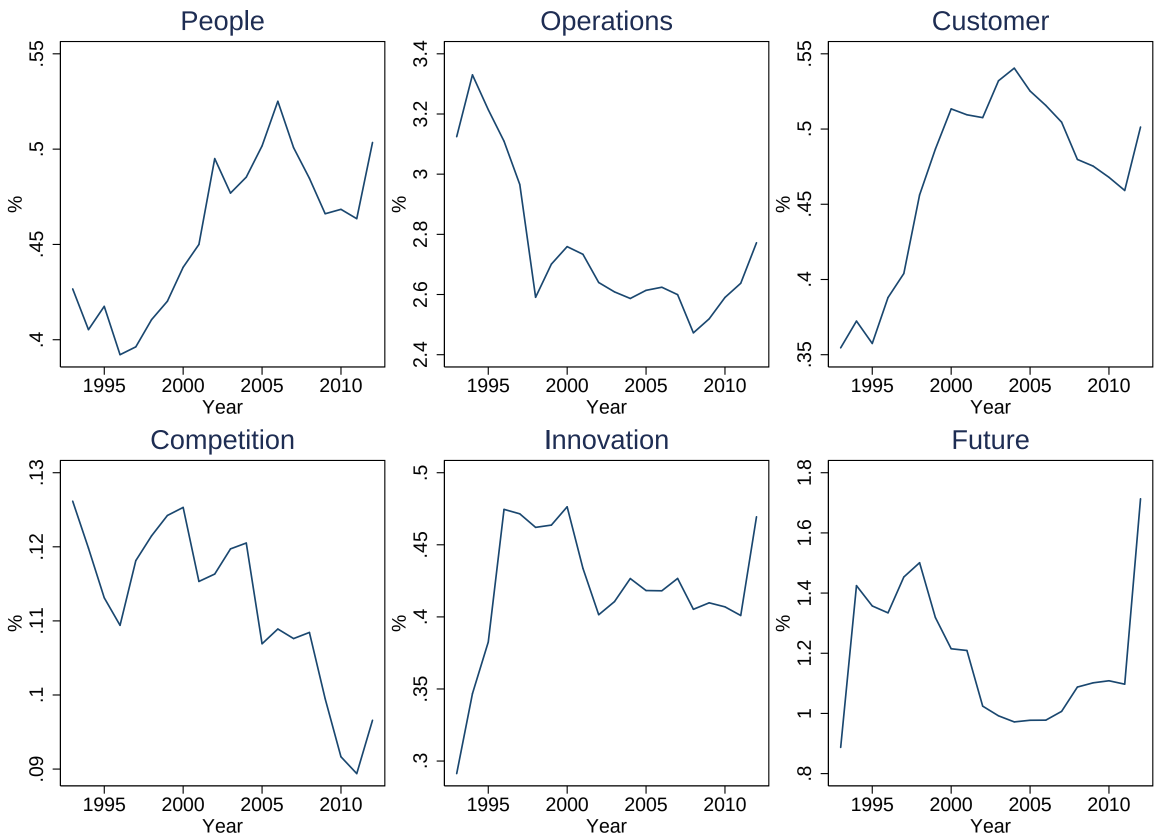

Figure 2.10. Management Focus, 1994-2010. This figure displays the average percentage of total words in the management discussion and analysis (MD&A) section of the 10-K report that are related to various areas of management focus. For each year, I plot the average across all firms that file 10-Ks with Edgar.

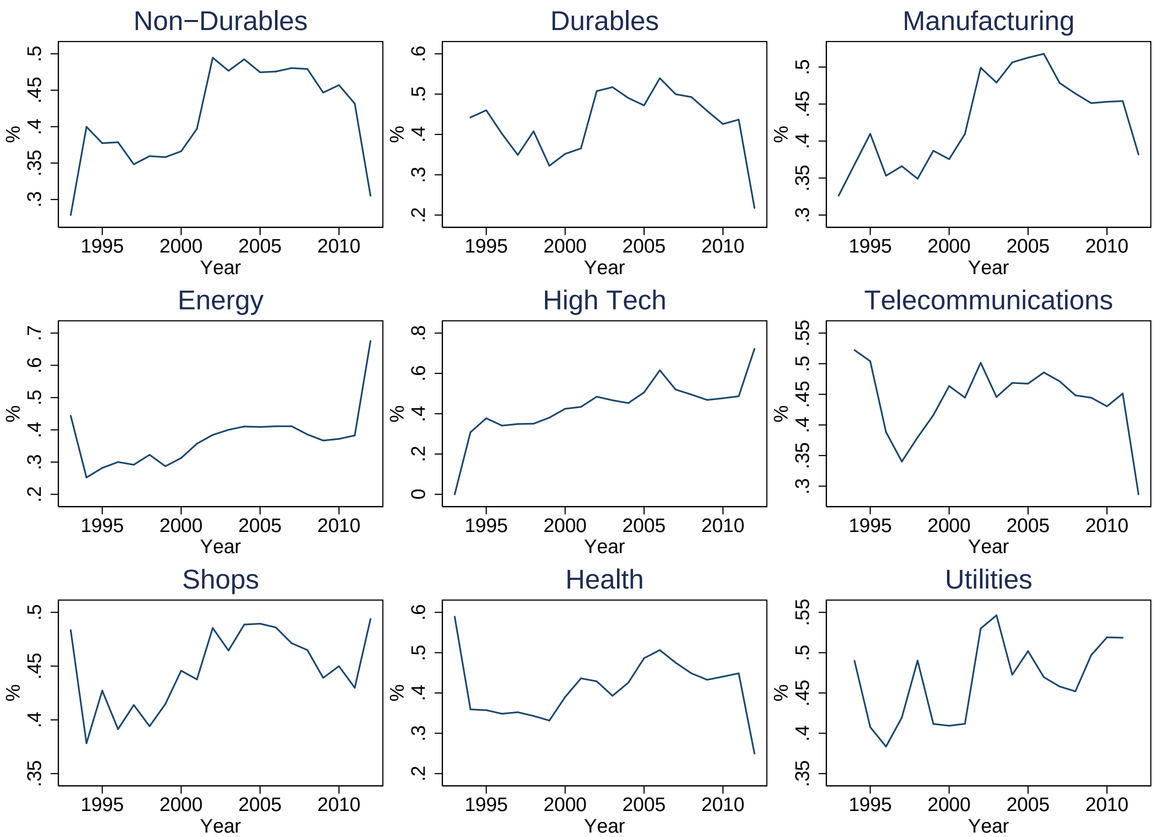

Figure 2.11. Management Focus on People: Industry Detail, 1994-2010. This figure displays the average percentage of total words in the MD&A section of the 10-K report that are related to employees of the firm. For each year, I plot the average across all firms that file 10-K statements with Edgar split by Fama-French 10 Industries (excluding other).

Table 2.9
Employee tasks and CEO focus. This table estimates how the nonroutine task intensity of the workforce changes CEO focus and how this change in focus affects firm profitability and returns. For columns 1 and 2, I estimate a linear regression of management people focus on the prior year level of nonroutine tasks. I include year fixed effects, industry fixed effects, and the firm level control variables used in Table 2.4. Some specifications include CEO fixed effects. For columns 3 and 4, I estimate a linear regression of firm ROA and stock returns on management people focus, nonroutine task intensity, and the interaction. Management people focus measures the percent of words in the MD&A section of the firm's 10-K that are related to employees of the firm. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	People Focus		ROA	Firm Value
	(1)	(2)	(3)	(4)
Nonroutine tasks	0.12**	0.10*	−0.02**	−0.29***
	(2.95)	(1.88)	(−8.42)	(−4.69)
People Focus			−0.056***	−0.06***
			(−4.02)	(−2.62)
Interaction			0.016***	0.015***
			(4.36)	(2.71)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
CEO FE	No	Yes	Yes	Yes
Observations	11,568	11,568	11,818	12,143
R^2	0.09	0.54	0.06	0.96

workforces to focus more on employees. I provide evidence that this is the case in Table 2.9. Columns 1 and 2 show the results of a linear regression of management people focus on the prior year level of nonroutine tasks. Both specifications include year fixed effects, industry fixed effects, and the firm-level control variables used in Table 2.4. In addition, Column 2 includes CEO fixed effects. The magnitude of both estimates is similar, though the CEO fixed effect estimate is less precise. These estimates imply that a one standard deviation in nonroutine tasks leads to a 9–11% increase in people focus. Caution is warranted in interpreting these as causal estimates, since the CEO has at least some impact on both the focus of the firm and the composition of the workforce. CEO fixed effects help alleviate this problem, since identification comes from changes in management focus that occur when CEOs switch firms. Even still, CEOs choose whether or not to take the new job, and that choice might be influenced by the workforce of the new firm, so these results are best interpreted as strong correlations.

Columns 3 and 4 of Table 2.9 provide additional support for the existence of synergies by showing that management focus on employees increases firm profitability and value, but only in firms that have nonroutine workforces. I estimate a linear regression of total firm value and return on assets on people focus, nonroutine tasks, and the interaction between people focus and nonroutine tasks. In both specifications I include CEO fixed effects. The interaction term reflects the value of management focus on people in nonroutine firms. Column 3 shows that the unconditional effect of people focus on profitability is negative. However, the interaction term is positive and highly statistically significant, suggesting people focus improves profitability in nonroutine firms. The effect is also economically meaningful; evaluated at the mean levels of people focus and nonroutine tasks, the coefficient implies a 1% increase in ROA which is large relative to the unconditional mean of 8.5%. Column 4 shows a similar relationship between people focus, nonroutine tasks, and total firm value. The coefficient on the interaction term is positive, statistically significant at the 1% level, and economically meaningful. Again evaluated at the mean of nonroutine tasks, a one standard deviation increase in people focus implies a 6% increase in firm value. These estimates suggest both that there are important synergies between managers and nonroutine workers and that shareholders have strong incentives to ensure managers focus on these synergies.

If CEO focus on nonroutine labor truly has the potential to increase shareholder value as much as the above estimates imply, models of the market for CEO talent suggest that high human capital firms should hire the most talented CEOs (Gabaix and Landier, 2008). To test this prediction, I estimate a CEO fixed effects regression of CEO pay on industry fixed effects (at the Fama-French 17 industry level), year fixed effects, and the natural logarithm of firm sales. Row 1 of Table 2.10 reports the F test for the significance of the overall CEO fixed effects.[20] Not surprisingly, CEO fixed effects matter for CEO pay (the F statistic is 6.45 and significant at 1% level). In this context, the CEO fixed effect can be interpreted as the talent or ability of the CEO. With that in mind, Column 2 and 3 report the mean CEO fixed effect for firms in the bottom and top quartile of nonroutine task intensity. Column 4 reports the difference in means and performs a two-sided t test to determine if this difference is statistically different from zero. Interestingly, high nonroutine task intensity firms hire high fixed effect CEOs, which is consistent with high ability CEOs matching with high human capital firms. The difference in fixed effects implies that CEO ability accounts for 20% of the difference between CEO pay at high and low human capital firms; this difference is both economically large and statistically significant.

In this context, I can statistically estimate the fixed effect even if the CEO does not change firms; the coefficient is identified off of variation in CEO pay within the CEO's tenure at a firm. To be conservative, though, I limit the sample to the relatively small number of individuals in my sample that work as CEO in multiple firms across time. For these 234 CEOs, fixed effects are primarily identified by the CEO moving to a new firm. Using this sample, the estimated effect actually increases—the CEO fixed effect of the average high nonroutine task firm implies a compensation

[20] Fee, Hadlock, and Pierce (2013) suggests that F tests are not appropriate for identifying manager style. I am not interested in the significance of the fixed effect, but in the difference in fixed effects between two groups. Still, Fee et al.'s (2013) critique likely applies so these results should be viewed as suggestive.

Table 2.10

Employee tasks and CEO fixed effects. This table estimates the importance of the CEO for determining total executive pay, firm profitability (income to assets), and firm stock returns. For each row, I estimate a CEO fixed effects regression of the characteristic on industry fixed effects (at the Fama-French 17 industry level) and year fixed effects. For the total pay regression, I also include the natural log of sales. The profitability regression includes the natural log of sales and Tobin's Q, and the stock returns regression includes the return on the market (value weighted CRSP return). Column 1 reports the F-test for the significance of the overall CEO fixed effects. Columns 2 and 3 report the mean of the CEO fixed effects for low and high skill firms, respectively. Column 4 reports the difference in means and performs a two-sided t test if the difference is significantly different from zero. Low skill firms are defined as firms that are in the bottom quartile of nonroutine employee tasks at the beginning and end of the sample, while high skill firms are defined as firms that are in the top quartile of nonroutine employee tasks at the beginning and end of the sample. The rows labeled "Movers Only" repeat the analysis only for individuals that work as CEO for multiple firms during the sample period. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	CEO Fixed Effects			
	(1) F-test	(2) Low Skill Firms	(3) High Skill Firms	(4) Difference
ln(Total Pay)	6.45***	5.02	5.22	−0.20***
- Movers Only		4.79	5.66	−0.87***
Profitability	11.11***	−0.12	−0.12	−0.00
- Movers Only		−0.22	−0.16	−0.06***
Stock Returns	1.10***	0.33	0.44	−0.11***
- Movers Only		0.23	0.37	−0.14***
Number of CEOs	6,748			
Number of Movers	234			
Number of Observations	36,056			

level more than double that of a nonroutine task firm.

Rows 2 and 3 of Table 2.10 estimate similar fixed effect regressions for firm profitability and stock returns. In addition to the controls included for the CEO pay regression, the profitability regression includes Tobin's Q. The stock returns regression includes industry and year fixed effects and the return on the market (value weighted CRSP return) so that the fixed effect represents industry adjusted excess returns. The F-statistic indicates that CEO fixed effects matter for both profitability and returns. In both cases, high human capital firms employ CEOs with higher fixed effects and the difference is statistically significant from zero. The differences are large: using the movers only sample, the difference in fixed effects explains a 6% increase in ROA and a 14% increase in stock returns.

The evidence presented so far in this section is consistent with manager effort having a synergistic effect on nonroutine employees. Theoretically, I also expect nonroutine employees to have an effect on the CEO. For instance, unlike routine employees, nonroutine employees produce information. As long as this information is valuable to the firm, CEOs should consider nonroutine workers input when making decisions.

One place to potentially observe this effect is on earnings conference calls. To the extent that nonroutine workers produce information, I expect that CEOs will delegate to other employees on conference calls more frequently so that this information can be shared with investors. To test for this effect, I use a textual analysis following Li, Minnis, Nagar, and Rajan (2012). I count the percentage of company text (i.e., ignoring questions from analysts) in earnings conference calls spoken by the CEO. Table 2.11 investigates how managing nonroutine workers affects CEO delegation on earnings conference calls. The dependent variable in Columns 1 and 2 is the percentage of company text in the call spoken by the CEO. The dependent variable in Columns 3 and 4 is the percentage of text in the Q&A section of the call spoken by the CEO. I regress these variables on the level of nonroutine workers, year fixed effects, and the other independent variables included in earlier tables.

Table 2.11 shows that managers of nonroutine workers delegate more, that is they spend less time talking on earnings conference calls. This effect is robust to the inclusion of CEO fixed effects (in Columns 2 and 4), suggesting that this is not merely an institutional feature of the firm or a difference in CEO preferences.

Delegation of firm-specific functions to subordinates suggests that nonroutine workers shift the role of the CEO from specialist to generalist. Table 2.12 provides evidence that this is the case. I use the general ability measure of Custódio et al. (2010) and classify generalists as CEOs in the top quartile of general ability. I then use a linear probability model to estimate the effect of nonroutine workers on the probability of being a generalist CEO. Column 1 shows that the effect of nonroutine workers is positive and statistically significant at the 1% level. A one standard deviation increase in nonroutine workers increases the probability of begin a generalist CEO by 2%. Columns 2 and 3 show that this effect is robust to including industry fixed effects (at the Fama-French 12 industry level) and firm fixed effects.

As a whole the evidence in this section implies that there is a significant synergy between CEOs and nonroutine labor. Talented CEOs that focus on this synergy can provide large returns to shareholders. These potential synergy gains rationalize increased CEO pay, and shift the role of the CEO toward managing human capital.

2.7 Conclusion

At least since Schultz (1961) and Becker (1962), academic economists have struggled to quantify the effect of human capital. Much of the existing research focuses on the relationship between schooling and human capital accumulation or the role of human capital in explaining macroeconomic growth. More recently, the finance literature has started to explore the value of employees

Table 2.11

CEO Delegation on Earnings Conference Calls. This table estimates how frequently CEOs delegate to others on earnings conference calls. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	CEO % Time on Call		CEO % Time on Q&A	
	(1)	(2)	(3)	(4)
Nonroutine Tasks	−2.24***	−3.05**	−2.35***	−3.10**
	(−3.70)	(−2.48)	(−3.71)	(−2.15)
ln(Revenue)	−4.31***	−3.06	−4.53***	−5.46**
	(−13.63)	(−0.98)	(−13.40)	(−2.04)
Tobin's Q	−0.82***	−0.12	−0.76***	−0.29*
	(−5.81)	(−0.78)	(−5.23)	(−1.85)
Income to assets	26.22***	−3.92	27.02***	3.07
	(4.27)	(−0.44)	(4.22)	(0.31)
Shareholder return	1.04	−0.50	1.41	−0.25
	(1.00)	(−0.66)	(1.29)	(−0.27)
Std. dev. return	24.62	−216.30***	17.21	−302.27***
	(0.35)	(−2.90)	(0.24)	(−3.37)
Beta	1.59	2.67**	1.63	3.43***
	(1.42)	(2.55)	(1.39)	(2.80)
CEO Tenure	−0.08	−2.17	−0.06	−1.70
	(−1.08)	(−1.52)	(−0.89)	(−1.31)
Age	−0.47***	0.29	−0.49***	−0.04
	(−7.91)	(0.44)	(−8.24)	(−0.06)
Constant	108.04***	77.86*	111.40***	112.99***
	(18.90)	(1.84)	(18.16)	(2.71)
CEO FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Number of CEOs	1,405	1,405	1,405	1,405
Observations	3,597	3,597	3,597	3,597
R^2	0.11	0.85	0.11	0.82

Table 2.12
General Ability and Nonroutine Workers. This table estimates the effect of nonroutine task workers on the general ability of the CEO. General ability is measured as in Custódio et al. (2010). Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	Top Quartile General Ability		
	(1)	(2)	(3)
Nonroutine tasks	0.02***	0.04***	0.02*
	(3.09)	(6.51)	(1.67)
ln(Revenue)	0.08***	0.09***	0.07***
	(30.15)	(35.45)	(10.48)
Tobin's Q	0.01***	0.00***	0.00
	(8.80)	(4.67)	(0.30)
Income to assets	−0.43***	−0.40***	−0.33***
	(−11.41)	(−11.75)	(−7.67)
Shareholder return	−0.01	0.00	0.00
	(−1.46)	(0.33)	(0.83)
Std. dev. return	1.60***	1.48***	0.46
	(3.80)	(3.97)	(1.27)
Beta	−0.03***	−0.02***	−0.02***
	(−3.44)	(−2.61)	(−3.15)
Constant	−0.39***	−0.50***	−0.26***
	(−12.67)	(−15.69)	(−3.01)
Year FE	Yes	Yes	Yes
Industry FE	No	Yes	Yes
Firm FE	No	No	Yes
Number of Firms	2,536	2,536	2,536
Observations	20,793	20,793	20,793
R^2	0.07	0.10	0.64

as key assets of the firm (Edmans, 2011; Rajan and Zingales, 1998; Berk, Stanton, and Zechner, 2010). This paper examines how managers enhance the value of the human capital of their employees; consequently, this work provides insight on one particular channel through which employees add value to the firm.

I show that the increase in nonroutine task workers induced by the IT revolution causes CEO pay to double between 1984 and 2010; this explains about one-third of the aggregate increase in executive compensation over this period. The overall increase in nonroutine task workers has been accompanied by a shift in the focus of executive mangers away from the operations of the firm and towards the people of the firm. This shift in focus is particularly strong for managers of high human capital workforces, and the shift for these types of managers results in a significant increase in firm value.

This evidence is consistent with large synergies between CEOs and nonroutine workers. Consistent with Edmans et al. (2011), shareholders increase the pay of CEOs to induce managers to focus on these synergies. These synergies appear to be large; talented CEOs at nonroutine worker firms raise total firm value from between 6 to 14%. As a result, a substantial portion of the growth in CEO pay can be justified by the increased value that comes from managing human capital.

CHAPTER 3

Revenge of the Steamroller: ABCP as a Window on Risk Choices

3.1 Abstract

We empirically examine financial institutions' motivations to take systematic bad-tail risk in the form of sponsorship of credit-arbitrage asset-backed commercial paper vehicles. A run on debt issued by such vehicles played a key role in the crisis that began in the summer of 2007. We find evidence consistent with important roles for both owner-manager agency problems and government-induced distortions, especially government control or ownership of banks.

3.2 Introduction

We use credit-arbitrage asset-backed commercial paper (credit-arb ABCP) vehicles to offer evidence on the reasons major banks exposed themselves to systematic bad-tail risk in the period leading up to the financial crisis that began in 2007. By systematic bad-tail risk we mean exposure to large losses in low-probability states of the world, especially states in which such losses are unusually costly, such as when risk premiums are high. In July of 2007, just before a run on their liabilities began, credit-arb ABCP vehicles had about $700 billion in assets. Most were sponsored by banks that provided their vehicles with committed backup lines of credit and other support, so sponsors bore the vehicles' risks. When vehicles experiencing ABCP runoffs turned to their sponsors for funding, the sponsors sought large amounts of new funding in interbank and other money markets. Most sponsors were European banks but most vehicle assets and liabilities were denominated in U.S. dollars; thus, sponsoring banks were forced to raise funds outside their home money markets and their national central banks were not immediately able to provide dollar liquidity support. This increased the cost to sponsors and also helped transmit the ABCP shock throughout the global financial system.[1]

Understanding the reasons why financial institutions take systematic bad-tail risk is important to policy design as well as to governance of the institutions. Many studies have empirically examined bank risk taking (e.g., Keeley, 1990; Laeven and Levine, 2009; De Jonghe, 2010; DeYoung and Torna, 2013), but such studies have largely focused on measures of the average risk from all projects undertaken by the financial institution. The nature of credit-arb vehicle risks allows us to empirically analyze bad-tail risk decisions by financial institutions, which to the best of our knowledge has not been done previously.

Systematic bad-tail risk was the main risk that credit-arb ABCP vehicles posed to sponsors. The vehicles combined maturity transformation and tail credit risk: Their investment strategy was to go long the high-grade credit spread with mismatched funding and very high leverage.[2] They borrowed at the short end of the credit spread term structure by issuing ABCP and lent at intermediate and long maturities, primarily by investing in AAA and AA rated asset-backed securities.[3] The vehicles were almost immune to all but two events: significant declines in the credit quality of high-grade asset-backed securities, or a prolonged loss of access to funding in the ABCP market. Such events were predictably more likely to occur in tandem with high risk premiums or broad disruptions in financial markets, making costs to sponsors of vehicle distress especially large. The credit-arb vehicle strategies were different than those associated with catastrophe bonds (Coval, Jurek, and Stafford, 2009) because trouble was likely to be associated with trouble in ABCP markets. The strategies had some similarity to those of Long Term Capital Management (LTCM), which have been likened to "picking up nickels in front of a steamroller" because they produce low per-unit returns with modest volatility in most states of the world but large negative returns in some states of the world (Duarte, Longstaff, and Yu, 2007).

Credit-arb ABCP vehicles are also useful for studying tail-risk decisions because the risk-taking decision is observable and is not closely linked to the sponsoring bank's other businesses. Most credit-arb vehicles bought securities in the capital markets. Such vehicles typically did not buy loans issued by the sponsoring bank and thus sponsorship was not an integral element of the sponsor's lending businesses. Moreover, some large banks sponsored vehicles and others did not. We use cross-sectional variation in the characteristics of actual and potential sponsors to evaluate the empirical relevance of hypotheses about determinants of sponsorship.

Our evidence implies that both agency problems and government-induced distortions were ma-

[1]Money markets play a key role not only in traditional intermediation of savings and investment, but also in arbitrage and financial engineering activity, so a shock to money markets has broad consequences.

[2]The vehicles' liabilities included no equity or subordinated debt, or small amounts of subordinated debt.

[3]These characteristics also make the credit-arb vehicles different from typical bank activities. Although banks are commonly thought to borrow short and lend long, large banks engage in a diversified array of activities, their assets span the credit quality spectrum, and their exposure to common macroeconomic risks is mitigated by cyclical patterns in liability flows (Gatev, Schuermann, and Strahan, 2009).

terial motivations. Banks with better compensation practices were less likely to sponsor risky ABCP vehicles. In contrast, banks without a large shareholder were more prone to sponsor these vehicles. Proxies for bank opaqueness are economically significant predictors of sponsorship. In particular, the scale of a bank's underwriting of asset-backed securities, as well as bank size, are important predictors of sponsorship. Relative to traditional lending, the risks associated with underwriting, trading, and related activities are more difficult for outsider shareholders to understand and govern.

We find evidence that government ownership or control of banks makes systematic bad-tail risk-taking more likely: Some vehicles were sponsored by German Landesbanks, which are government controlled. Moreover, Landesbank-sponsored vehicles were more likely to be a relatively large fraction of the sponsor's assets, so the chance of severe distress at the sponsoring Landesbank as a result of vehicle distress was larger. However, it is not clear that governments chose to take the risks associated with ABCP vehicles their banks sponsored. It may be that governance of such banks was weak, in which case one might interpret behavior of government-controlled banks as an instance of owner-manager agency problems (Hau and Thum, 2009).

We find mixed evidence that government safety nets for privately owned banks encouraged sponsorship. Safety nets are more valuable for more levered banks, and leverage is sometimes a significant predictor of sponsorship, with more-levered banks more likely to sponsor, but statistical significance is not robust. A measure of the likely degree of government support of distressed banks is also a significant predictor of sponsorship, but again not robustly so. Large banks were more likely to sponsor vehicles than medium-sized banks, and large banks are thought to be more likely to be treated as too-big-to-fail.[4] However, size might also proxy for the severity of agency problems or for economies of scale in managing ABCP vehicles.[5] Overall, the evidence offers support for safety nets as one among several motivations for systematic bad-tail risk-taking but does not strongly confirm such a motivation.

Using differences in regulatory capital treatment of credit-arb ABCP vehicles in Europe and the United States, we find no evidence that regulation per se affected sponsorship decisions. Acharya, Schnabl, and Suarez (2013) offer evidence that exemptions from regulatory capital requirements encouraged banks to sponsor ABCP vehicles. Our findings are compatible with theirs because their empirics include all types of ABCP vehicles, most of which posed risks different from credit-arb vehicles. The most common pre-crisis ABCP vehicle was a "multiseller" vehicle that invested in short-term debt from diverse issuers. Multiseller vehicles were often a material element of the sponsoring bank's lending businesses. Maturity mismatches and liquidity risk associated with such

[4] Carbo-Valverde, Kane, and Rodriguez-Fernandez (2013) show that, before the financial crisis began, large European and U.S. banks enjoyed higher ex-ante safety-net benefits.

[5] If vehicles must be larger than some minimum size to be profitable, and only large banks can provide the credit and liquidity backstops for large vehicles, then only large banks should be observed to sponsor.

vehicles were smaller than for credit-arb ABCP vehicles, as was systematic credit risk.[6] Taken together, our evidence and that of Acharya et al. (2013) implies that banks moved some of their lending into ABCP vehicles to escape capital requirements but that regulatory treatment was not a primary driver of the decision to take systematic bad-tail risk in the form of credit arbitrage vehicles.

As to why most sponsors of vehicles were European entities, our evidence implies that the majority of sponsors were global universal banks. Most such banks were headquartered in Europe, not the United States. In this sense, the popular perception that the financial crisis was caused entirely by U.S. banks and the U.S. financial system is misplaced. Although subprime mortgage loans were largely a U.S. phenomenon, the liquidity-shock vulnerability that made the crisis global and systemic was to some extent a product of European banks' risk-taking decisions.

Our work is also related to recent work on rare disasters in the presence of heterogeneity, such as Chen, Joslin, and Tran (2012), though we analyze risk decisions, not asset-pricing consequences; on causes and dynamics of the crisis (e.g. Brunnermeier, 2009; Gorton, 2010; Covitz et al., 2013); on hedge fund risks, since credit-arb vehicles are similar to hedge funds in some respects (e.g. Fung and Hsieh, 2001; Chan, Getmansky, Haas, and Lo, 2007), and on shadow banks (Pozsar, Adrian, Ashcraft, and Boesky, 2010; Acharya et al., 2013; Gorton and Metrick, 2010).

The remainder of the paper is structured as follows. The first section describes hypotheses and identification strategies, while the second describes ABCP vehicles and the risks they pose to sponsors, and also describes how credit-arb vehicles differ from others. The third section describes the data and the fourth presents results, while the fifth provides concluding remarks.

3.3 Hypotheses and Identification

Prior literature has offered several potential motivations for systematic bad-tail risk-taking, including government-induced distortions, agency problems and corporate governance failures, mistakes in the form of underestimation of risk (Calomiris, 2009; Coval et al., 2009), and ex ante first-best decisions that simply turned out poorly ex post.[7] We test formally for government-induced distortions and governance failures. Though data are not available to formally test the risk-underestimation and first-best explanations, we shed light on them.

[6] As noted in Covitz, Liang, and Suarez (2013), some multiseller ABCP vehicles experienced funding problems in the late summer of 2007. However, such problems were relatively short-lived as market participants became fully aware of the differences in investment strategy across types of vehicles. Many multiseller vehicles still exist today, but almost all credit-arb ABCP vehicles have disappeared.

[7] With regard to the latter, banks may simply have struck an ex ante reasonable risk-return balance. Even if profits were low, risks may have been commensurately low. Problems at ABCP vehicles may simply have been bad luck, and systemic consequences were an externality that did not affect bank decisions.

Three hypotheses focus on government-related distortions: First, banks' risk-reward tradeoff may be altered by regulations (Acharya et al., 2013) or, second, by deposit insurance or other implicit or explicit government guarantees (the "safety net") (Merton, 1977; Keeley, 1990; Gropp, Hakenes, and Schnabel, 2011). Third, government ownership or control of banks might also distort their decision-making (Dinç, 2005).

Another hypothesis is that decisions were distorted by agency problems. Gorton and Rosen (1995) suggest that low-skill bank managers may take negative-NPV bad-tail risks in order to boost earnings and avoid being fired (such managers accept a higher probability of being fired eventually in order to avoid being fired soon when low earnings reveal their type). Compensation-related problems may also be material.[8] For example, a combination of opaque bank risk postures and incentive compensation may cause bank CEOs to boost their compensation by taking tail risk that shareholders cannot observe.

Data are not available to support formal testing of the first-best and mis-estimation hypotheses, but informal evidence is not very supportive. As discussed below, profitability and volume trends seem incompatible with first-best choices to sponsor vehicles, and events of the 1990s and 2000s demonstrated the risks.[9]

Our identification strategy resembles a natural experiment in that credit-arb vehicles allow us to isolate one form of systematic bad-tail risk taking. However, the potential motivations for sponsorship are not randomly distributed across banks. We use proxy variables to represent motivations in probit regressions of sponsorship.

Focusing on potential endogeneity of the variables of most interest, our proxies for government ownership or control and for regulatory capital treatment of vehicles are exogenous and predetermined. We believe one of the proxies for governance and agency problems (the executive compensation index) also can be safely viewed as exogenous, as can our primary tercile-based measure of bank size and our measure of government support. Exogeneity is less clear for our measure of whether a bank is widely held (it is conceivable that large shareholders might choose to sell after a bank sponsors a credit-arb ABCP vehicle), volume of underwriting (perhaps a bank might underwrite more ABS in the expectation that it could place some in its vehicle), and capitalization (a bank might choose both sponsorship and to be more leveraged if it expects government support). For these variables we check robustness of the main results to use of instruments.

[8] Bannier, Feess, and Packham (2013); Fahlenbrach and Stulz (2011); and Diamond and Rajan (2009) are three among many relevant papers.

[9] There was recent historical experience of the relevant kinds of meltdowns. A fixed-income hedge fund blowup (LTCM) occurred less than ten years before sponsorship decisions were made, an asset-backed securities meltdown occurred in 1994 (collateralized mortgage obligations), and a commercial paper market meltdown occurred in 2001.

3.4 ABCP vehicles and Risks to Investors and Sponsors

This section presents information about credit-arb vehicle design and operation. It suggests the risks of credit-arb ABCP vehicles were primarily systematic bad-tail risk and that such risks were borne largely by sponsors. Other types of ABCP vehicles are briefly described.

3.4.1 Vehicles were designed to transfer risk to their sponsor

ABCP vehicles are designed to transfer risk to sponsors and to limit risks borne by ABCP investors in order to obtain low-cost short-term funding.[10] Vehicles have no employees and no offices (see Gorton and Souleles (2007) for more about such firms). All vehicle operations are conducted by service providers, such as law firms and investment managers, which are required to employ risk mitigants built into governing documents of a vehicle. Examples include hedging requirements, investment strategy restrictions, and requirements for third party guarantees, often from the vehicle's sponsor.[11]

The sponsor usually is the investment manager and receives most of the net revenue ("excess spread") from a vehicle after funding costs and operating expenses. The sponsor is the usual provider of committed backup liquidity lines that fund repayment of maturing ABCP if it cannot be rolled over. The sponsor provides credit enhancements for some types of vehicles.

If the sponsor fails, vehicle investors have the sole claim on vehicle assets and are isolated from the sponsor's bankruptcy proceeding. Even though the vehicle is bankruptcy remote, the value of risk mitigation provided to ABCP investors by a sponsor's commitments is reduced if the sponsor's default risk rises, so distress at the sponsor is usually associated with increased spreads on vehicle liabilities or a refusal of ABCP investors to roll over maturing debt, which we will refer to as a run. If the sponsor does not fail but vehicle assets suffer losses, such losses are very likely to be borne by the sponsor, either via the credit enhancements or the backup lines of credit they provide or due to implicit commitments. Acharya et al. (2013) find that during the crisis sponsor banks realized over 97 percent of the total losses on assets of ABCP vehicles of all types.

3.4.2 Types of vehicles

Different types of ABCP vehicles have different investment strategies, different relationships with the sponsor's other businesses, and pose different risks to the sponsor. The right panel of Table 3.1 gives amounts of ABCP outstanding as of mid-2007, with the top panel showing amounts for the

[10] Low costs are important to vehicle profitability. Obtaining A1/P1 ratings is a practical necessity.

[11] For example, the interest payment streams from fixed-coupon assets are swapped into floating-rate equivalents in order to limit net asset value fluctuations associated with changes in risk-free interest rates. Foreign exchange risks are also hedged.

Table 3.1
ABCP outstanding by vehicle type. This table shows the total amount of asset backed commercial paper (ABCP) outstanding by vehicle type at the end of the second quarter of 2007, measured in millions of USD. The top panel shows ABCP outstanding for the types of vehicles that we study in this paper, while the bottom panel covers other types of vehicles. The first column examines vehicles sponsored by U.S. and European banks (the focus of this paper), while the second column shows total ABCP outstanding.

	Sponsored by major U.S. and European Banks	Global Total
Hybrid	4,851	17,956
Sec. Arbitrage	274,426	296,347
SIV	66,831	104,094
Subtotal	346,108	418,397
CDO	2,642	68,833
Loan-Backed	3,539	3,539
Multi-Seller	456,125	651,224
Repo/TRS	4,979	83,251
Single-Seller	122,966	227,104
Other	5,806	11,936
Total	942,165	1,464,284

types of vehicles we examine and the bottom panel covering other types. As described below, SIVs also issued about $300 billion of medium term notes (MTNs), bringing the aggregate size of credit-arb vehicles to about $700 billion.

3.4.3 Credit arbitrage vehicles

We focus on credit arbitrage vehicles, including securities arbitrage vehicles (SAVs) and structured investment vehicles (SIVs). Hybrid vehicles have portfolios that mix SAV and multiseller characteristics, but the ones included in our sample closely resemble SAVs. Credit-arb vehicles invest in diversified portfolios of assets, mostly asset-backed securities (ABS), that appeared to pose low credit risk (about 90 percent of assets were rated AAA and AA before the crisis and almost all the remainder were rated A).[12] Such vehicles diversify by 1) investing in a variety of types of ABS and other bonds (see Figure 3.1) and 2) by virtue of the fact that each ABS is backed by a pool of loans that are diversified along some dimensions. Of course, they were still exposed to systematic risk.

[12]For brevity, we use ABS to refer to the full menu of asset backed securities, including MBS, CDO, ABS, etc.

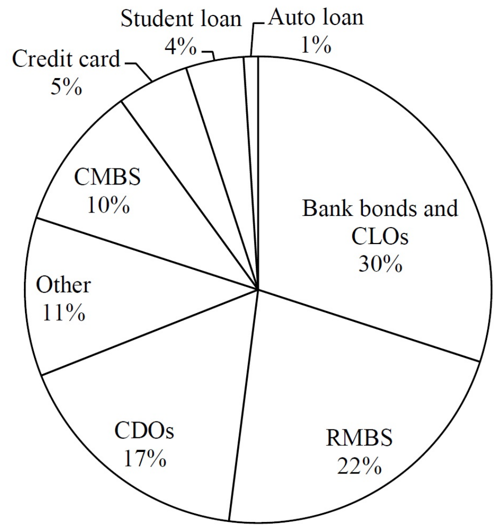

Figure 3.1. Credit arbitrage vehicle asset mix as of March 2007. This figure shows the percentage breakdown of each asset type held by the average credit arbitrage vehicle in March 2007. Source: Moody's Investors Service.

3.4.3.1 Securities arbitrage vehicles

Investors in a securities arbitrage vehicle are effectively protected from default in any state of the world that does not involve a failure to perform by providers of backup commitments. SAVs are buy-and-hold entities that are governed by accrual accounting and that have corporate charters that forbid the sale of assets at less than par.[13] As long as assets do not default and funding costs (including the effect of hedges) remain stable, such vehicles can be expected to produce fairly steady streams of net revenue for sponsors. Of course, ABCP investors may withdraw funding or demand higher spreads, either because of exogenous disruptions in money markets or because they perceive that the risk that they will bear losses has risen. Credit enhancements and committed backup lines of credit are key sources of investor confidence.

Details of credit enhancements varied across vehicles, but in most cases their purpose was to: 1) limit variation in vehicle net asset value associated with changes in ratings of individual assets in the vehicle's portfolio; and 2) to achieve a low risk that individual assets would default.[14] Liquidity backup lines of credit are also usually bought from the sponsor and are for the full amount of vehicle assets. If ABCP funding availability dries up for any reason, the provider of the line will

[13] Vehicle operations are conditional on net income in some cases. For example, if daily net income is negative for an extended period, the investment manager may be forbidden to buy new assets or the vehicle may go into liquidation.

[14] For example, if an originally AAA asset is downgraded a little bit, the provider of the enhancement (typically the sponsoring bank) adds assets to the vehicle to restore net asset value. If the asset is downgraded further, say below single-A, the terms of the enhancement give the provider an incentive to buy the asset out of the vehicle at par. In principle, ABCP investors bear the risk of a jump to default without an intervening downgrade, but such jumps are very rare for assets rated A or better, and liquidity backstops offer additional protection.

pay off ABCP investors and assume the risks posed by portfolio assets.[15]

Thus, as a practical matter, the sponsor of a securities arbitrage vehicle effectively bears all of the credit, market, and liquidity risk associated with the vehicle's portfolio in states of the world in which the sponsor does not fail. The risk borne by the sponsor is systematic bad-tail risk: ABCP investors are likely to withdraw funding in only three circumstances: 1) An exogenous disruption in money markets, in which case the sponsoring bank is likely to be forced to enter interbank markets to fund the vehicle at a time when funding liquidity is impaired; 2) A sharp deterioration in the credit quality of a substantial fraction of vehicle assets, which is likely to occur only when market credit spreads are high and when buying vehicle assets at par will impose substantial mark-to-market losses on the sponsor; and 3) When doubts arise about the ability of the sponsor to meet its obligations to the vehicle. In the latter case, the sponsor's own distress is likely to be worsened by a need to attract interbank deposits to fund the vehicle. In August and September of 2007, both 1) and 2) occurred, and 3) occurred for some sponsors.

3.4.3.2 SIVs

SIVs have assets similar to SAVs but are funded by a tranche of subordinated notes that represents 4 to 8 percent of vehicle liabilities, by ABCP, and by medium-term notes (MTN). Sponsors do not provide contractual credit enhancements. The ABCP and MTN are equal in priority and differ mainly in term to maturity. SIVs are governed by mark-to-market accounting with net asset value triggers that force the vehicle to stop investing and to enter a liquidation mode if the net asset value of the vehicle falls below preset percentages of the par value of liabilities. In some states of the world, a liquidation might permit holders of the ABCP and MTNs to be paid in full if they hold the debt to maturity; however, in other states of the world the subordinated debt would be wiped out and some hold-to-maturity ABCP and MTN investors would bear losses (of course, those that run by failing to roll over maturing debt will avoid losses).

In principle, SIV liquidity risk is managed by matching maturities of assets and liabilities, with ABCP used to fund short-term mismatches and to allow some flexibility to buy and sell assets. Because maturity-matching limits liquidity risk, the vehicles purchase a liquidity backup line of credit covering only a portion of liabilities. Some SIVs may have used a higher proportion of ABCP than maturity-matching would have implied, making them more vulnerable to runs. A refusal of investors to roll over MTN funding would also effectively put a SIV into liquidation, since it would have to use cash from maturing assets to repay maturing MTNs.

SIV sponsors appeared less exposed to vehicle risks than sponsors of SAVs because they did

[15] For SAV investors to suffer a credit loss, a variant of a double-default event must occur: The sponsoring bank must fail to meet its contractual obligations to the vehicle, and the value of vehicle assets must be insufficient to pay off investors as ABCP matures.

not provide full contractual backups and they did not hold vehicles' subordinated notes. However, almost all bank-sponsored SIVs were rescued by their sponsors. In most cases, the sponsors bought all vehicle assets or injected assets to prevent net asset value from falling below liquidation trigger points. Perhaps sponsors would not have done so had they foreseen the severity of the crisis, but in late 2007 or early 2008 they honored implicit commitments to vehicle investors. Thus, in practice, they were exposed to the same systematic bad-tail risk as SAV sponsors.

3.4.4 Types of vehicles omitted from empirical analysis

We omit multi-seller vehicles from our analysis because most of them pool nonfinancial-firm receivables and other short-term assets and consequently have a smaller maturity mismatch between assets and liabilities. Entities other than the sponsor often provide some credit enhancements. In addition, banks often use multi-seller vehicles as an element of their lending businesses (sometimes providing loans to clients via vehicles rather than on-balance-sheet loans). Thus, a bank's decision to sponsor a multiseller vehicle is not necessarily well-separated from its other businesses. Few multi-seller vehicles became deeply distressed, and most are still operating, whereas most SAVs and SIVs have disappeared.

We also omit single-seller vehicles. Though there was some diversity of single-seller strategies, a typical single-seller vehicle was a holding pen for loans awaiting securitization. For example, a mortgage lender might issue securitizations only a few times a year and might "park" loans in its vehicle until securitization occurred. Such vehicles are not the result of a risk-taking decision that is separate from the sponsor's other businesses.

Of the remaining types of vehicles shown in Table 3.1, some might involve systematic bad-tail risk-taking, but all have structures or are related to sponsors' businesses in ways that make them less useful candidates than credit-arb vehicles for our study of systematic bad-tail risk-taking.

3.5 Data

To examine the motivations that led banks to sponsor credit-arb vehicles, we need information on the characteristics of ABCP vehicles and their sponsors. We focus on the period ending in mid-2007. Data on ABCP vehicles are from Moody's Investors Service ("Moody's"), primarily from quarterly "Program Index" spreadsheets, which include information on the characteristics of all ABCP vehicles rated by Moody's.[16] The spreadsheets capture well over 90 percent of global

[16]The spreadsheets include average amount of ABCP outstanding—issued in the U.S. and European commercial paper markets—each quarter. For commercial paper denominated in currencies other than the U.S. dollar, we convert the amount outstanding to nominal U.S. dollars using the historical exchange rates published by the Federal Reserve in Table H.10.

vehicles by assets.[17] Figure 2 shows the amount of ABCP outstanding by vehicle type starting at year-end 1999. Growth was positive until 2007 and accelerated after 2004. ABCP outstanding increased by a factor of five between 2000 and 2007 for the set of credit arbitrage vehicles included in our sample, a much larger increase than at other types of vehicles. After the crisis began in the summer of 2007, issuance and outstandings decreased sharply for all vehicles, not just credit arbitrage vehicles, though the fall in multiseller ABCP began later, in late 2008 as the crisis was reaching its peak and as output and demand for financing from nonfinancial firms and consumers were falling sharply, whereas the fall in credit-arb ABCP began in 2007.

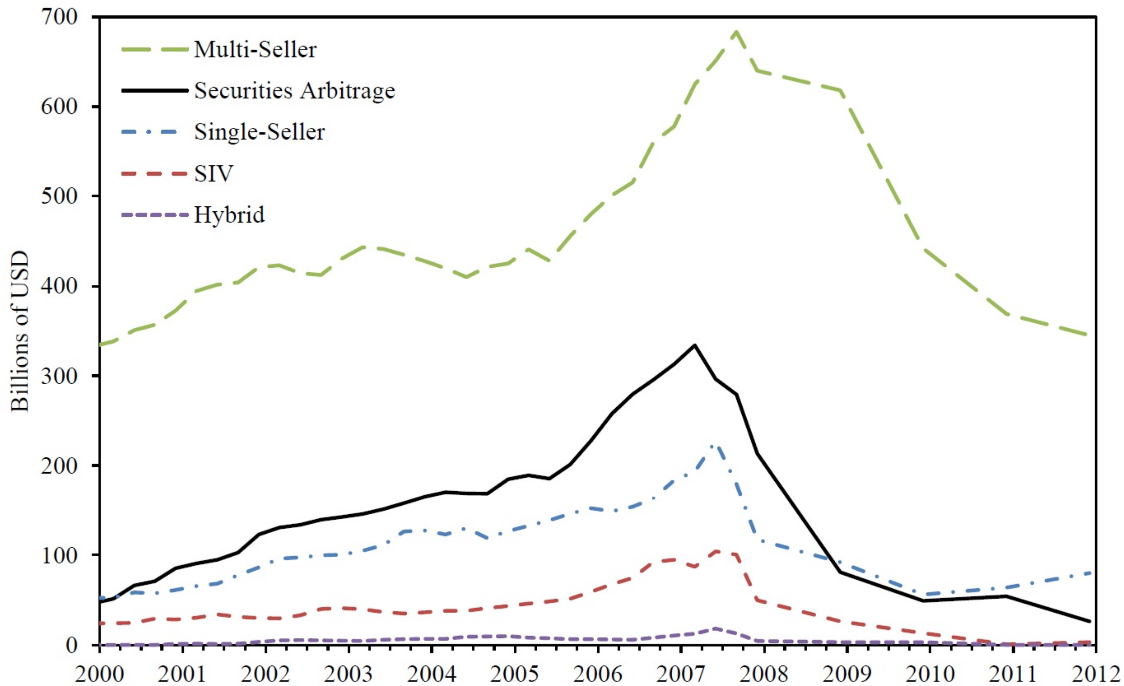

Figure 3.2. ABCP vehicle global CP outstanding by vehicle type. This figure shows the total quarterly amount of ABCP outstanding by vehicle type from December 1999 to December 2011. Source: Moody's Investors Service.

As noted previously, we include in our analysis only credit-arb vehicles sponsored by U.S. and European banks.[18] A comparison of the amounts of ABCP outstanding in the first and second columns of Table 3.1 for the second quarter of 2007 shows that our sample captures almost 90 percent of outstanding ABCP for securities arbitrage vehicles (including hybrid vehicles), but only about 65 percent of outstandings for SIVs (a few large SIVs were sponsored by U.K. nonbanks).[19]

[17] Standard and Poors and Fitch are the other two major ABCP rating agencies in the United States.

[18] We omit a small number of vehicles sponsored by nonbanks or Asian banks because we are unable to determine regulatory treatment of them or we are unable to find data on sponsor characteristics. We also omit a small number of vehicles sponsored by Canadian banks because liquidity backup lines of credit were structured differently in Canada than elsewhere.

[19] "Hybrid" vehicles as classified by Moody's combined characteristics of SAVs and other vehicle types. We include

We obtain balance sheet and income statement data about sponsor banks from Bankscope. We collect annual data on sponsors' underwriting activity for high yield bonds, ABS, and mortgage backed securities (MBS) from Bloomberg and Dealogic's DCM Analytics.

Figure 3.3 shows the amount of bank-sponsored credit arbitrage vehicle ABCP outstanding grouped by the domicile of the vehicles' sponsor. ABCP outstanding from European bank-sponsored vehicles grew rapidly from 2002.[20] Vehicles sponsored by U.S. banks grew moderately and ABCP outstanding was less than half the amount for European-sponsored vehicles by June 2007.

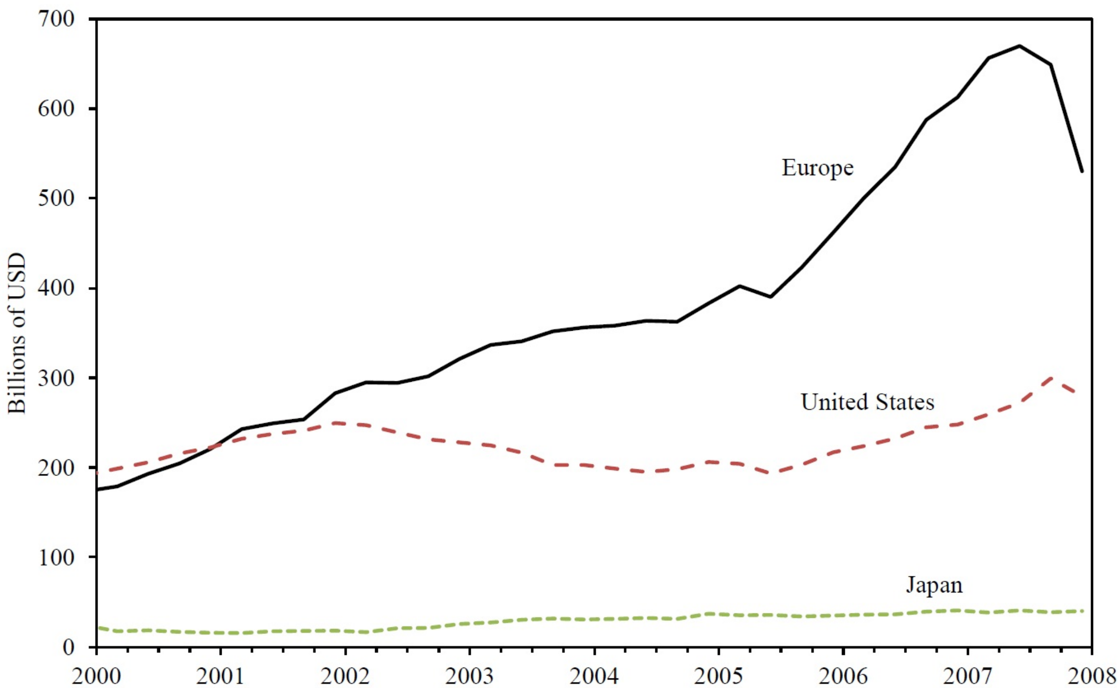

Figure 3.3. Global ABCP outstanding by region. This figure plots the total quarterly amount of ABCP outstanding grouped by the domicile of the vehicles' sponsor from December 1999 to December 2007. Only vehicles sponsored by banks are included. Europe is defined as all members in the EU-15 (the 15 Western European member countries of the European Union before its expansion in 2004) plus Norway and Switzerland. Source: Moody's Investors Service.

To evaluate the propensity of banks to sponsor vehicles, we must include both sponsor and non-sponsor banks in the sample. We include all sponsors as well as non-sponsor banks with $25 billion or more in total assets that were domiciled in the United States or in Europe. We do not include smaller banks because none of them sponsor the types of vehicles we analyze. Table 3.2 presents the number of sponsor and non-sponsor banks in each country in mid-2007. Germany

in analysis only hybrids that were effectively SAVs. We exclude some vehicles classified as SAVs by Moody's where the vehicle invested mainly in securities originated by the sponsor, which for our purposes makes it more like a multi-seller vehicle.

[20]For the purpose of this paper, Europe includes all members in the EU-15 (the 15 Western European member countries of the European Union before its expansion in 2004) plus Norway and Switzerland.

Table 3.2
Number of banks with assets greater than $25 billion by country. This table presents counts of the number of sponsor and non-sponsor banks in each country at the end of the second quarter of 2007. To be included as a sponsor, a bank must sponsor at least one SIV, securities arbitrage, or hybrid ABCP vehicle. Non-sponsors must have at least $25 billion in total assets to be part of our sample.

	Banks by Country		
	Not sponsoring ABCP vehicles	Sponsoring ABCP vehicles	Total
Austria	4	0	4
Belgium	2	2	4
Denmark	1	1	2
Finland	2	0	2
France	12	2	14
Germany	14	11	25
Greece	5	0	5
Ireland	5	0	5
Italy	12	1	13
Luxembourg	1	0	1
Netherlands	4	4	8
Norway	1	0	1
Portugal	3	0	3
Spain	7	0	7
Sweden	4	0	4
Switzerland	4	0	4
United Kingdom	5	6	11
United States	27	8	35
Total	113	35	148

has a relatively large number of sample banks and an especially large number of sponsor banks. Many German sponsors are government-owned or controlled, such as Landesbanken. As described below, we check robustness by excluding this group of banks from some specifications.

Table 3.3 lists the sponsor banks. About one-fifth are from the United States (8 banks). The remainder are from Europe (27 banks, representing about 72 percent of sponsors). In most cases the vehicles are modest in size relative to sponsor bank assets. (The biggest outliers in this regard, IKB Bank and Sachsen Landesbank, both government-controlled, failed early in the crisis; results presented below are robust to their elimination from the sample.) As shown in the last two rows of Table 3.3, only $48 billion of a total of $347 billion of ABCP outstanding at vehicles of the type we analyze was in U.S.-sponsored vehicles.

Table 3.3
List of banks sponsoring credit-arb ABCP vehicles. This table lists the sponsor banks in our sample at the end of the second quarter of 2007. A sponsor bank is required to sponsor at least one SIV, securities arbitrage, or hybrid ABCP vehicle. Banks that we perceive to be widely recognized global banks are shown in boldface.

	Second quarter 2007		
	ABCP vehicles	Total ABCP ($US Millions)	Total ABCP to total assets (%)
European Banks			
ABN Amro	1	9,263	0.7
Abbey National	–	–	–
Barclays	1	3,840	0.2
Bayerische Hypo-und Vereinsbank	–	–	–
Bayerische Landesbank	2	12,687	2.9
BNP Paribas	–	–	–
Commerzbank	1	1,007	0.1
Danske Bank	1	2,500	0.5
Deutsche Bank	2	6,391	0.4
Deutsche Zentral	1	4,033	0.7
Dexia	–	–	–
Dresdner Bank	1	5,292	0.7
Erste Bank	–	–	–
Fortis	1	26,375	2.6
HBOS	1	36,002	3.1
HSBC Holdings	3	32,918	1.8
HSH Nordbank	2	9,174	3.7
IKB Deutsche Industriebank	1	18,577	22.0
ING Groep	2	10,964	0.7
KBC Group	2	4,266	1.0
Landesbank Baden-Wuerttemberg	1	9,113	1.7
Landesbank Berlin Holding	1	2,138	1.1
Lloyds TSB Group	1	22,889	3.4
Nationwide Building Society	1	2,936	1.4
Natixis	1	2,820	0.5
NIBC Holding	1	506	1.2
Rabobank Group	5	15,181	2.1
Sachsen LB	1	17,875	23.1
Socit Gnrale	1	724	0.1
Standard Chartered	2	6,205	2.3
UniCredito Italiano	1	19,289	1.8
WestLB	3	16,096	4.3
Subtotal	41	299,061	
U.S. Banks			
Bank of America	3	2,685	0.2
Bank of New York	1	139	0.2
Citigroup Inc	7	26,021	1.4
JP Morgan Chase & Co.[a]	1	3,352	0.2
Mellon Bank	1	3,790	14.5
State Street Corporation	1	4,188	3.9

Continued on next page

Table 3.3
(Continued)

	Second quarter 2007		
	ABCP vehicles	Total ABCP ($US Millions)	Total ABCP to total assets (%)
Wachovia Corporation	1	3,641	0.5
Zions Bancorporation	1	3,736	8.0
Subtotal	16	47,552	
Total	57	346,613	

For comparison, Table 3.4 lists sample banks that are not sponsors. To limit the size of the table, only those with $150 billion or more in total assets are shown. Those that we perceive to be among the most widely known global banks are highlighted in bold face. Although there are several such banks in both tables, the proportion that we perceive to be widely known global banks is larger in Table 3.3.[21]

When examining the role that government-induced distortions played in vehicle sponsorship, we use a bank-specific measure of government support similar to that in Brandão-Marques, Correa, and Sapriza (2013). We exploit the fact that Moody's assigns two separate ratings to each bank that it covers. The bank financial strength rating (BFSR) is intended to measure the bank's intrinsic safety and is constructed to ignore external support that the bank might receive from any other entity (including the government). The bank deposit rating measures the bank's ability to repay its deposit obligations and incorporates both the BFSR and Moody's opinion of the likelihood and amount of external support. Our government support measure is defined as the difference (in rating notches) between a bank's BFSR and its long-term foreign currency deposit rating.

We use proxies for manager incentives and bank governance to test the importance of agency problems. The main executive compensation measure is from RiskMetrics Group's Corporate Governance Quotient (CGQ). We follow Aggarwal, Erel, Stulz, and Williamson (2010) and use the questions included in the CGQ index related to compensation and ownership. For each bank, we measure the percentage of compensation and ownership attributes that satisfy a threshold defined by the RiskMetrics Group. A higher value of *Compensation Index* means that the bank has established compensation practices that are more in line with some pre-defined standards. We also use the percent of shares held by individual insiders, collected from FactSet/Lionshares, as another measure of incentive alignment.

We follow Erkens, Hung, and Matos (2012) in creating three corporate governance indicators. One is the share of independent directors on a bank's board, calculated using data from BoardEx. A second is the share of institutional ownership at each bank, from FactSet/Lionshares. A third is

[21] We classify 32 percent of the large non-sponsor banks (Table 3.4) as widely known, while 45 percent of the sponsor banks (Table 3.3) are widely known.

Table 3.4
List of banks with total assets above $150 billion not sponsoring credit-arb ABCP vehicles. This table lists major European and U.S. banks that did not sponsor SIV, securities arbitrage, or hybrid ABCP vehicles. To limit the size of the table, only banks with $150 billion or more in total assets are shown. Banks that we perceive to be widely recognized global banks are shown in boldface.

European Banks	U.S. Banks
Allied Irish Banks	SunTrust Banks
Banco Bilbao Vizcaya Argentaria	US Bancorp
Banco Santander	Washington Mutual Inc.
Bank of Ireland	**Wells Fargo & Company**
BNP Paribas	
Caisse Nationale des Caisses d'Epargne	
Cassa Depositi e Prestiti	
Credit Mutuel Centre Est Europe	
Credit Suisse Group	
Credit Agricole Group	
Deutsche Postbank	
Dexia	
DnB Nor	
Groupe Banques Populaires	
Gruppo Monte dei Paschi di Siena-Banca	
Hypo Real Estate Holding	
Intesa Sanpaolo	
KfW Group	
Landesbank Hessen-Thueringen Girozentra	
Norddeutsche Landesbank Girozentrale NO	
Nordea Bank	
Northern Rock	
Royal Bank of Scotland	
Skandinaviska Enskilda Banken	
Svenska Handelsbanken	
UBI Banca-Unione di Banche Italiane	
UBS	

concentration of ownership, denoted *Widely Held* and measured by an indicator variable equal to 1 if a bank does not have a single owner with voting shares larger than 10 percent, constructed from data from FactSet/Lionshares, Bankscope, and banks' annual reports.

Finally, we use as control variables loans-to-assets, deposits-to-assets, and the characteristics of banks' home countries, including measures of the degree of financial market development (stock market capitalization to GDP and private bond market capitalization to GDP) and macroeconomic performance (the annual average percent change of real GDP).

Table 3.5 reports summary statistics for our sample and Table 3.6 examines how bank and country characteristics vary across sponsor and non-sponsor banks.[22] Definitions and sources for each of the variables are in Appendix B.1. Table 3.6 reveals some differences between sponsor and non-sponsor banks: As of June 2007, relative to non-sponsors, sponsor banks were larger, were more complex in their assets and activities, had smaller loan to assets and equity to assets ratios, and underwrote larger volumes of ABS and MBS.

3.6 Results for determinants of vehicle sponsorship

In this section, we test hypotheses that distortions from government activities or from agency problems motivated banks' sponsorship of ABCP vehicles. We begin with informal evidence about the possibility that sponsorship decisions resulted from undistorted risk-return tradeoffs.

3.6.1 Just bad luck

Data are not available to support formal testing of a hypothesis that banks made entirely undistorted risk-reward decisions to sponsor credit-arb vehicles. However, in such a case, we would not expect proxy variables for government or agency distortions to be predictive. Informal evidence also does not support it: Vehicle profitability appears low, and grew lower at the same time as vehicles expanded.

Profits are generally not disclosed, but the example of Mellon Bank gives a sense of vehicle profitability. At year-end 2006, Mellon's vehicle had \$3.2 billion in assets, which if consolidated would have increased Mellon's total assets by 7.7 percent (a large fraction). But the vehicle provided only \$3 million of gross revenue to the bank (10 basis points of vehicle assets). Somewhat unrealistically assuming that Mellon itself had no expenses associated with the vehicle (all revenue

[22] Bankscope variables are measured as of the previous fiscal year-end. Approximately 95 percent of our sample has a December fiscal year end date. For the remaining banks, we associate fiscal years ending in January through March with the prior year and fiscal years ending in April through November with the current year. Consequently, financial variables for a bank with a March fiscal year are measured as of the current March, while financial variables for a bank with a September fiscal year are measured as of the previous September.

Table 3.5
Sample summary statistics. This table presents summary statistics for the variables used in our analysis. The sample consists of all banks domiciled in the United States or in Europe with at least $25 billion dollars in total assets. Sponsor is a dummy variable equal to one if the bank sponsored at least one SIV, securities arbitrage, or hybrid ABCP vehicle as of June 2007. The remaining variables are measured as of December 2006; definitions for each variable are found in Appendix B.

	N	Mean	Median	Std Dev	Min	Max
Sponsor	148	0.24	0.00	0.43	0.00	1.00
Return on Assets	147	0.88	0.80	0.54	0.06	3.07
Total Assets	148	333.40	101.33	484.40	25.09	1956.71
Equity to Assets	148	6.31	5.97	3.36	1.44	19.07
Loans to Assets	148	52.85	58.19	20.47	0.26	93.07
Deposits to Assets	146	54.31	58.13	18.37	0.18	93.58
Non-Interest Operating Income to Assets	147	1.52	1.25	1.44	−0.13	8.17
High Yield Underwriting	148	0.04	0.01	0.16	0.01	1.00
Securitization Underwriting	148	0.11	0.01	0.29	0.01	1.00
Dummy Landesbank	148	0.06	0.00	0.24	0.00	1.00
Dummy US	148	0.24	0.00	0.43	0.00	1.00
Stock Market Cap.	148	123.45	119.95	68.36	54.14	384.66
Private Bond Market Cap.	147	58.37	41.03	39.19	5.25	137.83
Real GDP Growth	148	3.05	2.87	0.87	1.30	6.12
Government Support	91	0.78	0.00	1.71	−1.00	8.25
Deposit Insurance	148	0.66	1.00	0.47	0.00	1.00
Level of Deposit Insurance	148	56.52	50.00	42.19	20.00	340.00
Prefunded	148	0.70	1.00	0.46	0.00	1.00
Monetary Policy Independence	148	0.48	0.27	0.42	0.00	1.00
Compensation Index	89	65.34	62.50	22.66	14.29	100.00
Insider Ownership	85	0.02	0.00	0.05	0.00	0.40
Board Independence	97	0.75	0.80	0.17	0.34	1.00
Institutional Ownership	86	0.36	0.34	0.22	0.01	0.81
Widely Held	148	0.49	0.00	0.50	0.00	1.00

Table 3.6
Bank Characteristics by sponsorship decision. This table reports the mean value as of December 2006 of the independent variables used in our regression analysis for banks that sponsored at least one SIV, securities arbitrage, or hybrid ABCP vehicle as of June 2007 and banks that did not sponsor any of these vehicle types. The sample consists of all banks domiciled in the United States or in Europe with at least $25 billion dollars in total assets. We also report the difference in means between sponsor and non-sponsor banks and perform a two-sample t test to determine if this difference is statistically significant; standard errors are shown in parenthesis. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Definitions for the variables are found in Appendix B.

	Sponsor	Non-Sponsor	Difference
Return on Assets	0.745	0.929	-0.183^{*}
			(0.106)
Tot. Assets	739.627	207.579	532.048^{***}
			(105.888)
Equity to Assets	4.829	6.764	-1.935^{***}
			(0.548)
Loans to Assets	41.701	56.297	-14.596^{***}
			(3.485)
Deposits to Assets	54.263	54.328	−0.065
			(2.852)
Non-Int. Op. Inc to Assets	1.601	1.490	0.111
			(0.279)
High-Yield Underwriting	0.107	0.018	0.089^{*}
			(0.049)
Securitization Underwriting	0.321	0.043	0.278^{***}
			(0.076)
Dummy Landesbank	0.171	0.027	0.145^{**}
			(0.066)
Dummy US	0.229	0.239	−0.010
			(0.083)
Stock Market Cap.	131.692	120.894	10.798
			(12.675)
Private Bond Market Cap.	57.790	58.547	−0.757
			(7.914)
Real GDP Growth	2.904	3.091	-0.187^{*}
			(0.104)
Observations	35	113	148

was net profit), the vehicle contributed only 0.3 percent of Mellon's 2006 net income.[23] Similarly, Acharya et al. (2013) note that Deutsche Bank's 2007 annual report reveals that its vehicles generated €6 million of fees relative to commitments of €6.3 billion, again about 10 basis points before expenses.

In the aggregate, vehicles grew as profitability fell. Figure 3.4 plots a proxy for the gross spread earned on vehicle investments (the yield on ABS less LIBOR for two types of underlying assets; ABCP rates were generally close to LIBOR until the crisis hit). As shown in Figure 3.2, outstanding ABCP at credit-arb vehicles grew from 2004 on, while Figure 3.4 shows gross spreads fell to low levels during the same period.[24] A bank increasing its commitment to a strategy the expected returns of which have fallen appears to be a violation of portfolio theory unless the risk associated with the vehicles fell even more rapidly than did gross spreads, or unless the returns to all other strategies fell more. As we argued previously, risks posed by the vehicles were almost entirely bad-tail risk borne by the sponsors, and such bad-tail risk does not vary much during booms (or perhaps is increasing).[25] Overall, the volume and profitability data do not obviously imply that sponsorship decisions were undistorted.

3.6.2 Regressions

We use multivariate probit regressions to examine the effect of government and agency distortions on ABCP sponsorship. The dependent variable is an indicator equal to 1 for banks that sponsored at least one credit-arb vehicle and 0 otherwise. Specifically, we estimate

$$Pr(Sponsor_i = 1) = F(\alpha + \beta BankTraits_i + \gamma CountryTraits_i) \tag{3.1}$$

for each bank *i* as of June 2007. Independent variables are measured, in most cases, as of the previous December.

Results presented in Table 3.7 through Table 3.12 shed light on variants of hypotheses and are discussed below. All but Table 3.8 report average marginal effects; consequently, coefficient magnitudes can be interpreted as the incremental probability of sponsorship. Standard errors (in parenthesis) are clustered at the country level. Table 3.8 shows the impact on probabilities of sponsoring an ABCP vehicle for large discrete changes in the values of some explanatory variables, using the model shown in column 2 of Table 3.7.

[23] By early 2008, Mellon had used "excess liquidity" to pay off ABCP investors and wind up the vehicle, taking the assets onto the bank's balance sheet.

[24] The pattern raises the possibility that bank managers tried to compensate for reduced per-unit revenue by increasing the size of vehicles, but we cannot verify such a conjecture.

[25] The vehicles are difficult to wind up, so a sponsor that increased vehicle size was committing to bear increased tail risk for many years.

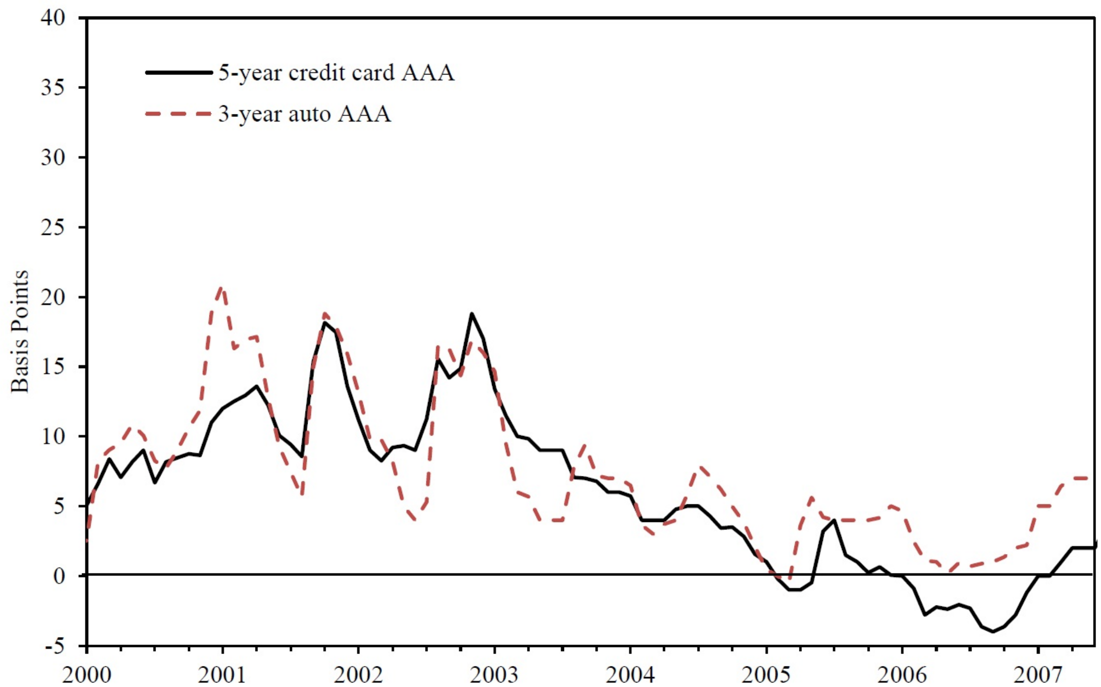

Figure 3.4. ABCP Interest Rate Spreads. This figure plots a proxy for the gross spread earned on ABCP vehicle investments from December 2009 to June 2007. The credit card spread represents the yield on 5-year AAA credit card asset backed securities (ABS) less LIBOR, while the auto spread is the yield on 3-year AAA automobile loan ABS less LIBOR. Source: Salomon Smith Barney and Citigroup Global Markets.

Table 3.7
Full-sample results from probit models of sponsorship. This table shows the results from estimating a probit regression in which the dependent variable is a binary indicator equal to 1 for banks that sponsored at least one SIV, securities arbitrage, or hybrid ABCP vehicle. We examine sponsorship as of June 2007. Most independent variables are measured as of December of the previous year. The values in the table represent average marginal effects. Definitions for the independent variables are found in Appendix B. Standard errors (shown in parenthesis) are clustered at the country level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Dependent Variable	Sponsoring a Credit Arbitrage ABCP Vehicle=1	
	(1)	(2)
Year	2007	2007
Return on Assets	−0.045	−0.023
	(0.085)	(0.075)
Dummy Intermediate Tercile	0.023	0.034
	(0.061)	(0.050)
Dummy Top Tercile	0.235***	0.234***
	(0.082)	(0.084)
Equity to Assets	−0.028**	−0.033***
	(0.011)	(0.011)
Loans to Assets	−0.000	−0.001
	(0.003)	(0.003)
Deposits to Assets	−0.000	0.000
	(0.001)	(0.001)
Non-Interest Operating Income to Assets	0.044*	0.035*
	(0.024)	(0.021)
High Yield Underwriting	−0.001	−0.011
	(0.145)	(0.127)
Securitization Underwriting	0.165***	0.145**
	(0.040)	(0.055)
Dummy Landesbank	0.222***	0.241***
	(0.047)	(0.062)
Dummy US	0.152*	0.047
	(0.079)	(0.178)
Stock Market Cap.		0.000
		(0.001)
Private Bond Market Cap.		0.002
		(0.002)
Real GDP Growth		−0.055
		(0.052)
Observations	145	144
Countries	18	17
Psuedo R^2	0.325	0.338

Table 3.8
Changes in the probability of establishing an ABCP vehicle given discrete changes in selected explanatory variables. This table reports changes in the probability of sponsoring at least one SIV, securities arbitrage, or hybrid ABCP vehicle for discrete changes in selected explanatory variables. All other variables are evaluated at the mean for the full sample. We estimate changes in probability using the model shown in column (2) of Table 3.7. Standard errors are clustered at the country level.

	Probability of Risky ABCP vehicle=1 (Initial value for X variable)	Probability of Risky ABCP vehicle=1 (Final value for X variable)	Change in the Probability of Risky ABCP vehicle=1	Confidence Interval (95%)
Change in Equity to Assets from 25th perc. to 75th perc. in 2007	0.288	0.086	-0.202	[-0.338,-0.066]
Change in Securitization Underwriting from 50th perc. to 90th perc. in 2007	0.138	0.272	0.135	[0.017,0.253]
Change in Non-Interest Operating Income to Assets from 25th perc. to 75th perc. in 2007	0.122	0.168	0.046	[-0.000,0.0921]
Increase in Bank Size from the Bottom Tercile of the Assets Distribution to the Top Tercile	0.071	0.372	0.300	[0.093,0.507]
Change in Securitization Underwriting from 50th perc. to 90th perc. in 2007 for Small Banks (in the Bottom Tercile–Assets)	0.062	0.146	0.084	[-0.028,0.196]
Change in Securitization Underwriting from 50th perc. to 90th perc. in 2007 for Large Banks (in the Top Tercile–Assets)	0.343	0.533	0.190	[0.039,0.340]

We do not analyze in detail the determinants of credit-arb vehicle size. A barrier to empirical analysis is that the proper normalization is not obvious.[26] We ran Tobit regressions of credit arbitrage vehicle assets as a fraction of the sponsor's total assets and of the sponsor's total equity on the predictors shown in Table 3.7, Table 3.9 and Table 3.11 and they produced qualitatively similar results, except that coefficients on the securitization underwriting measure were not statistically significant (see Appendix B.3). Because vehicle size was modest relative to bank assets in most cases, its value as a measure of systematic bad-tail risk is not clear. In contrast, existence of sponsored vehicles was a signal to counterparties early in the crisis that a bank had taken systematic bad-tail risk, in addition to increasing the bank's contingent funding needs and credit losses in bad states of the world.

3.6.3 Government-induced distortions

We consider distortions associated with three kinds of government activity:

1. Capital regulations.
2. Safety nets: The loss-shifting ability of equity holders due to the existence of some combination of deposit insurance, regulatory forbearance, too-big-to-fail government bailouts of distressed banks, and other safety-net features.
3. Direct or indirect government ownership or control of banks. The governance mechanisms the government puts in place may lead to risk decisions that differ from undistorted decisions.

3.6.3.1 Government ownership or control

Consistent with Brandão-Marques et al. (2013), we find strong evidence that direct or indirect government ownership or control was a factor in sponsorship. Almost all of the government-controlled banks in our sample are German Landesbanks, and the coefficient on *Dummy Landesbank* in Table 3.7 is statistically and economically significant. The Landesbanks lost their formal government guarantee of liabilities in 2005, which might have motivated them to move their credit-arbitrage investment activity into ABCP vehicles (most had no vehicles or only small vehicles in 2000). Because large private banks also sponsored vehicles, government control is not the whole story of sponsorship, but it is does appear to be part of the story. We cannot identify whether government-controlled banks sponsored vehicles in order to conceal continued risk-taking from stakeholders

[26] Total assets of a bank's credit-arb vehicles relative to consolidated total assets of the sponsoring bank does not fully reflect both the capacity of the bank to obtain liquidity in the event of a run on the vehicles and its capacity to absorb credit losses in the event that vehicle assets have to be taken onto the bank's balance sheet. The literature has so far not put forward a convincing way to simultaneously measure both credit loss-bearing capacity and funding capacity in a way that is useful for studying credit-arb vehicle size.

(including the government) or due to government influence. In the former case, the activity might be characterized as a governance failure.

3.6.3.2 Regulation per se

We use differences in regulatory capital treatment in Europe and the United States to examine the impact of regulation. European banks were not required to have extra capital for the funding backstops and credit enhancements that most bank sponsors provided for their SAVs, but U.S. banks did face such requirements. That might account for the concentration of sponsors in Europe.

In 2001 and 2004, the United States adopted risk-based capital requirements for ABCP vehicle credit enhancements and for liquidity backstop lines of credit, respectively. Though the precise cost to banks of such requirements is not known, back-of-the-envelope calculations described in Appendix B.2 imply that a capital-constrained bank would bear annual costs on the order of 2 basis points of SAV assets for each requirement, or 4 basis points in total, which is large relative to the profitability described previously and thus represented a significant disincentive to U.S. banks to sponsor SAVs.

Accounting rules were changed in 2003 to require consolidation of vehicle assets, but U.S. bank sponsors could escape U.S. leverage-ratio capital requirements by avoiding such consolidation, which they could do by purchasing "expected loss" protection from a third party. Our back-of-the-envelope calculations imply that such protection cost about 3 basis points of vehicle assets, though such costs are probably already reflected in the 10 basis point profit rate described previously (see Appendix B.2).

Given the cost differential, a hypothesis that regulatory capital treatment incentivized sponsorship implies that European banks should have been more likely to sponsor at the margin, after controlling for other characteristics. Results in column 2 of Table 3.7 do not support this hypothesis: The average marginal effect for a dummy variable that is 1 for U.S. banks and 0 for European banks is positive rather than negative, near zero, and not statistically significantly different from zero. As discussed in Appendix B.2, the geographic differences in regulatory treatment were much more pronounced for SAVs than for SIVs. When we identify as sponsors only banks that sponsor at least one SAV, results for the U.S. dummy are similar (not tabulated). The greater propensity of European banks to sponsor may be due to other characteristics by which European banks differed from U.S. banks, such as size, presence of government-controlled banks, and other characteristics. These are discussed further below.

An additional bit of circumstantial evidence that regulation per se was not a driver of sponsorship is that European banks' vehicles grew rapidly during 2004 to 2007 even though it was clear that the impending implementation of Basel II would soon implement risk-based capital requirements similar to those in the U.S. ABCP vehicles are difficult to wind up quickly, so the growth amounted to a pre-commitment by European banks to continue operations at scale even after Basel II implementation. Such a pre-commitment is inconsistent with regulatory capital incentives being a primary motivation for European banks to sponsor ABCP vehicles relative to U.S. banks.[27]

3.6.3.3 Government safety nets

Bank leverage is a commonly used proxy variable for distortions arising from government safety nets. A smaller equity-to-assets ratio is assumed to imply that a bank is closer to insolvency, so the safety net subsidy and thus the distorting effect on bank decisions is assumed to be larger. Of course, leverage may also be related to the severity of agency problems, so we also use the bank-specific measure of government support described previously. Higher values imply more support.

Results are mixed. In the full-sample specification shown in Table 3.7 and Table 3.8, the equity-to-assets ratio is a statistically and economically very significant predictor of sponsorship. When the equity ratio increases from the 25th percentile to the 75th percentile, the probability of sponsorship decreases by about 0.2, which is large relative to the unconditional probability of 0.24. Table 3.9 reports results for the subsample for which the bank-specific measure of government support is available. *Government Support* is a positive, statistically and economically significant predictor for sponsorship. Using the model estimated in column 2, an increase from the 25th percentile of *Government Support* to the 75th percentile implies a 7 percentage point increase in the probability of sponsorship (not tabulated).

However, most of this effect is driven by German Landesbanks, which usually have high government support. If we exclude Landesbanks from the sample, the effect of *Government Support* drops by two-thirds and becomes statistically insignificant (not tabulated). Consequently, results for *Government Support* can also be interpreted as evidence of distortions driven by government ownership or control rather than by safety nets available to all banks.

Moreover, the equity to assets ratio is not a significant predictor of sponsorship for the subsample reported in Table 3.9. This is a characteristic of the smaller sample, not of the presence of the government support measure. The subsample is smaller mainly because it excludes banks for which financial strength ratings are unavailable. However, the subsample includes most of the largest, globally active banks in the full sample.[28]

[27] Had they been in effect, high incremental capital requirements associated with bank sponsorship of credit-arb vehicles might have greatly reduced the extent of sponsorship. However, existence of low capital requirements alone is not necessarily the marginal motivation for sponsorship. We do not argue that capital requirements were irrelevant in principle, only that the evidence is more consistent with other factors driving decisions at the margin. Relatedly, Iannotta and Pennacchi (2012) argue that ratings-based regulations incentivizes financial firms to choose bonds that have more systematic risk. However, such a mechanism could not have driven systematic risk of credit-arb vehicles because European sponsors' vehicles were not subject to such regulations.

[28] The subsample disproportionately excludes U.S. banks, raising the possibility that equity-to-assets is proxying in part for geographic variations in equity-to-asset ratios, which might be driven by other factors. However, in untabulated

Table 3.9
Risky ABCP vehicle sponsorship and government support. This table shows the results from estimating a probit regression in which the dependent variable is a binary indicator equal to 1 for banks that sponsored at least one SIV, securities arbitrage, or hybrid ABCP vehicle. We examine sponsorship as of June 2007. The independent variables are measured as of December of the previous year. The values in the table represent average marginal effects. Government Support is a ratings-based measure from Brandão-Marques et al. (2013) that captures the bank-specific value of explicit and implicit government support. Definitions for the independent variables are found in Appendix B. Standard errors (shown in parenthesis) are clustered at the country level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Dependent Variable	Sponsoring a Credit Arbitrage ABCP Vehicle=1	
	(1)	(2)
Year	2007	2007
Government Support	0.053***	0.055***
	(0.016)	(0.012)
Return on Assets	−0.100	−0.077
	(0.150)	(0.123)
Dummy Intermediate Tercile	0.065	0.093
	(0.085)	(0.068)
Dummy Top Tercile	0.261***	0.254***
	(0.088)	(0.086)
Equity to Assets	−0.021	−0.026
	(0.020)	(0.019)
Loans to Assets	−0.000	−0.001
	(0.002)	(0.003)
Deposits to Assets	−0.002	−0.001
	(0.003)	(0.003)
Non-Interest Operating Income to Assets	0.054*	0.043
	(0.028)	(0.031)
High Yield Underwriting	−0.116	−0.107
	(0.132)	(0.101)
Securitization Underwriting	0.259**	0.247*
	(0.100)	(0.120)
Dummy US	0.222*	−0.001
	(0.128)	(0.177)
Stock Market Cap.		0.000
		(0.001)
Private Bond Market Cap.		0.003*
		(0.001)
Real GDP Growth		−0.091*
		(0.054)
Observations	91	91
Countries	16	16
Psuedo R^2	0.325	0.361

As reported in Table 3.10, when we exclude Landesbanks from the full sample, the equity to assets ratio continues to be a statistically significant predictor. When we limit the sample to European banks, it is at best weakly statistically significant, and when we drop Landesbanks from the European subsample it is no longer statistically significant. It is also insignificant in a U.S. subsample (untabulated).

We are concerned that the equity to assets ratio may be simultaneously determined with sponsorship decisions. A bank that decides to take more tail risk may simultaneously sponsor a vehicle and reduce its equity-to-assets ratio in order to take more conventional leverage risk, or it may increase its ratio if other assets are moved off the balance sheet. The challenge in instrumenting for leverage is to find variables exogenous to the sponsorship decision but predictive of leverage that results from expectations of government support. Our primary instrument is government involvement in management and/or funding of national deposit insurance schemes; all the nations represented in our sample had deposit insurance, but some systems were purely privately funded and managed. We expect that governments with such systems are more likely to provide safety-net assistance to distressed banks. We also use as instruments the level of deposit insurance coverage, whether the deposit insurance system is pre-funded (the alternative is to assess surviving banks to cover the cost of failures), a variant of our government-support variable in which missing values are set to zero, and a measure of monetary policy independence which has the value of 1 for nations with their own central bank and, for nations in the euro area, the share of a nation's GDP in euro area GDP. Table 3.11 displays results for two-stage least squares estimation of a linear probability model, which has the advantage of supporting evaluation of instrument quality, and an instrumental-variables probit procedure, which is consistent with the probit procedure used to produce other tables. We also estimate the IV probit procedure using the regulatory capital ratio rather than equity to assets.

Though the strength of statistical significance varies, results consistently imply that banks with higher capital relative to assets are less likely to sponsor vehicle. F-statistics for the first stage are relatively weak, but we cannot reject the instruments using the Hansen J statistic. These results are robust to dropping Landesbanks from the sample.

In summary, government-induced incentive distortions play an important role in banks' decision to sponsor ABCP vehicles. We find strong evidence that government ownership or control influenced sponsorship decisions, no evidence that banks sponsored vehicles differentially in Europe and the United States due to regulatory capital requirements, and mixed evidence that on the whole implies that government safety nets encouraged banks to sponsor vehicles.

results, we do not find interactions of the U.S. dummy with equity to assets to be significant or to materially affect other results.

Table 3.10

Robustness checks. This table shows the results from estimating a probit regression in which the dependent variable is a binary indicator equal to 1 for banks that sponsored at least one SIV, securities arbitrage, or hybrid ABCP vehicle. In this table, we examine sponsorship at mid-2007. The independent variables are measured as of December of the previous year; definitions for the independent variables are found in Appendix B. The values in the table represent average marginal effects. Columns 1 and 2 exclude German Landesbanks from the sample, Columns 3 and 4 exclude U.S. banks from the sample, and Columns 5 and 6 exclude both U.S. banks and German Landesbanks. Standard errors (shown in parenthesis) are clustered at the country level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Dependent Variable	Sponsoring a Credit Arbitrage ABCP Vehicle=1					
Sample	All banks Excluding Landesbanks		European Banks		European Banks Excluding Landesbanks	
	(1)	(2)	(3)	(4)	(5)	(6)
Return on Assets	−0.041	−0.033	−0.275**	−0.216**	−0.283**	−0.240*
	(0.086)	(0.083)	(0.094)	(0.095)	(0.108)	(0.118)
Dummy Intermediate Tercile	0.020	0.036	0.049	0.034	0.001	−0.010
	(0.063)	(0.059)	(0.100)	(0.096)	(0.101)	(0.101)
Dummy Top Tercile	0.211**	0.224***	0.322***	0.305***	0.296***	0.286***
	(0.089)	(0.086)	(0.068)	(0.074)	(0.070)	(0.080)
Equity to Assets	−0.020**	−0.030***	−0.022	−0.034*	−0.016	−0.025
	(0.008)	(0.007)	(0.019)	(0.016)	(0.016)	(0.013)
Loans to Assets	−0.000	−0.000	0.001	0.001	0.003**	0.003
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Deposits to Assets	0.001	0.000	0.001	0.002*	−0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Non-Interest Operating Income to Assets	0.058*	0.041	0.012	−0.020	0.069*	0.040
	(0.032)	(0.030)	(0.038)	(0.041)	(0.031)	(0.040)
High Yield Underwriting	0.031	−0.003	−0.097	−0.076	−0.060	−0.050
	(0.144)	(0.122)	(0.171)	(0.189)	(0.144)	(0.156)
Securitization Underwriting	0.183***	0.144**	0.150**	0.126*	0.138***	0.111*
	(0.050)	(0.047)	(0.057)	(0.065)	(0.048)	(0.058)
Stock Market Cap.		0.000		0.000		0.000
		(0.001)		(0.001)		(0.001)
Private Bond Market Cap.		0.002**		0.001		0.001
		(0.001)		(0.002)		(0.002)
Real GDP Growth		−0.052		−0.071		−0.061
		(0.050)		(0.058)		(0.055)
Observations	136	135	112	111	103	102
Countries	18	17	17	16	17	16
Psuedo R^2	0.294	0.322	0.342	0.358	0.361	0.380

Table 3.11

IV Regressions. This table repeats the analysis from Table VII, but uses government guarantees as instruments for the equity to assets ratio. The dependent variable is a binary indicator equal to 1 for banks that sponsored at least one SIV, securities arbitrage, or hybrid ABCP vehicle. In this table, we examine sponsorship at mid-2007. Though not shown in this table, we include the same independent variables as in Column 2 of Table VII, measured as of December 2006. The instruments for the equity to assets include: a dummy variable equal to one if the government is involved in the management of the deposit insurance system (*deposit insurance*), the level of insured deposits in U.S. dollars (*Level of Deposit Insurance*), a dummy variable equal to one if the deposit insurance is prefunded (*Prefunded*), a measure of monetary policy independence that is equal to one if the country has its own central bank; otherwise, it is equal to the GDP of the country divided by the total GDP of the European Central Bank member countries (*Monetary Policy Independence*), and a ratings-based measure from Brandão-Marques et al. (2013) that captures the bank-specific value of explicit and implicit government support (*Government Support*). Columns 1 and 2 present estimates of the first stage OLS regression of equity to assets on the shown instruments and the other independent variables (not shown). Columns 3 and 4 show the estimated 2SLS coefficient on the instrumented value of equity to assets. Columns 5 and 6 show the estimated coefficient using an instrumental variable probit technique where both stages are simultaneously estimated through maximum likelihood; the values in columns 5 and 6 represent average marginal effects. Definitions for the independent variables are found in Appendix B. Standard errors (shown in parenthesis) are clustered at the country level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Dependent Variable	Sponsoring a Credit Arbitrage ABCP Vehicle=1					
Method	2SLS		IV Probit			
	(1)	(2)	(3)	(4)	(5)	(6)
Equity to Assets	−0.199*	−0.139*	−0.151***	−0.162***		
	−0.104	−0.084	−0.021	−0.014		
Capital Ratio					−0.040***	0.032***
					−0.013	−0.013
Instruments from First Stage:						
Deposit Insurance	−0.906*	−0.766	−0.906**	−0.235	−2.428	4.031
	−0.457	−0.797	−0.434	−0.65	−2.592	−4.075
Level of Deposit Insurance		−0.28		0.165		−2.155
		−0.478		−0.319		−2.386
Prefunded		−0.001		0.003		0.006
		−0.008		−0.004		−0.016
Monetary Policy Independence		−0.303		0.055		−3.237
		−1.006		−0.305		−4.26
Government Support		0.012		−0.041		0.432
		−0.154		−0.087		−0.494
Observations	144	144	144	144	144	144
Countries	17	17	17	17	17	17
First Stage F-Statistic	3.93	1.04				
Hansen J-Statistic		3.20				
p-value for J-Statistic		0.53				

3.6.4 Agency problems

Owner-manager agency problems might reduce or increase risk-taking relative to shareholders' desires. On the one hand, senior executive wealth may be very exposed to bank performance due to lack of diversification. If executives are risk averse, they may therefore take too little risk in the absence of corrective actions by shareholders, such as tailored compensation functions (Smith and Stulz, 1985). On the other hand, compensation arrangements might provide incentives to take too much risk by rewarding earnings without regard to risk (e.g., John and John, 1993; Diamond and Rajan, 2009; Bannier et al., 2013), or low-quality executives might increase risk in hopes that associated increases in short-term earnings will help them retain their jobs (Gorton and Rosen, 1995; Saunders, Strock, and Travlos, 1990). Thus, differences in compensation arrangements and other mechanisms by which shareholders influence risk-taking differ across firms may influence managers' incentives and ability to sponsor vehicles.[29] Moreover, other things equal, the more difficult it is for shareholders to monitor and exert control, the more that risk-taking may deviate from shareholders' desired level.

3.6.4.1 Compensation and governance proxies

Table 3.12 reports results using the same specification as in column 2 of Table 3.7, but adding measures of executive compensation and corporate governance practices within banks.[30] The sample is smaller because such measures are not available for all banks (for example, no Landesbanks are included in this subsample).[31] As shown in the first column of Table 3.12, higher values of *Compensation Index*, which imply better practices, are associated with a statistically and economically significant reduction in the probability of sponsorship. A move from the 25th percentile to the 75th percentile in the sample distribution for *Compensation Index* implies a decrease of 22 percentage points in the probability of sponsoring a credit arbitrage vehicle, which is economically large, similar to the impact of being a Landesbank.

We employ five measures of ownership or quality of corporate governance. Column 3 of Table 3.12 shows that the share of independent directors is not significantly related to a bank's decision to sponsor a credit arbitrage vehicle. Columns 2, 4 and 5 show results for insider ownership, institutional ownership, and the degree of dispersion of share ownership, included one at a time,

[29] Berger, Imbierowicz, and Rauch (2012) find that higher CEO ownership reduces the probability of bank default during the financial crisis. In contrast, Beltratti and Stulz (2012) show that banks with more shareholder-friendly boards have significantly worse stock performance during the financial crisis, and Fahlenbrach and Stulz (2011) find that CEOs with compensation packages that better align shareholder-CEO interests perform at best no better than CEOs with worse incentives during the crisis.

[30] With the exception of banks in the United States and United Kingdom, disclosure of detailed managerial compensation and insider ownership is not available for many banks in our sample.

[31] The one exception to this is column 5 of Table 3.12. The results in this column are robust to controlling for or excluding Landesbanks.

Table 3.12
Risky ABCP vehicle sponsorship and agency problems. This table shows the results from estimating a probit regression in which the dependent variable is a binary indicator equal to 1 for banks that sponsored at least one SIV, securities arbitrage, or hybrid vehicle as of mid-2007. The independent variables are measured as of December of the previous year. The values in the table represent average marginal effects. Higher values of *Compensation Index* represent better firm-level compensation practices as defined by RiskMetrics Corporate Governance Quotient. *Insider ownership* is the percentage stake held by individual insiders. *Board Independence* is the share of independent directors on the bank's board of directors, *Institutional Ownership* is the percent of shares owned by institutions, and *Widely Held* is a dummy variable equal to 1 if the bank does not have a single owner with voting shares larger than 10%. Definitions for the remaining independent variables are found in Appendix B. While not shown to conserve space, these estimations include the same bank and country-level control variables used in column 2 of Table 3.7. Note that with the exception of column (5), this estimation sample does not include Landesbanks. Standard errors (shown in parenthesis) are clustered at the country level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Dependent Variable	Sponsoring a Credit Arbitrage ABCP Vehicle=1					
	(1)	(2)	(3)	(4)	(5)	(6)
Compensation Index	-0.005^{***}					-0.004^{***}
	(0.002)					(0.001)
Board Independence		-0.195				-0.049
		(0.126)				(0.179)
Insider Ownership			0.107			0.125
			(0.510)			(0.793)
Institutional Ownership				0.453^{**}		0.263
				(0.182)		(0.199)
Widely Held					0.126^{**}	0.101^{*}
					(0.060)	(0.053)
Observations	89	96	84	85	144	74
Countries	17	17	17	17	17	17
Psuedo R^2	0.424	0.327	0.407	0.425	0.329	0.450

while column 6 shows results when all measures are included in the probit regression. Though the fraction of shares in the hands of institutional owners has a positive and statistically significant relationship with sponsorship when it is the only agency-related variable, it is not statistically significant in column 6.[32] Among the ownership variables, only *Widely Held* is statistically significant, with a more widely held bank having a larger probability of sponsorship (note that weaker significance in column 6 is partly a result of the smaller sample relative to column 5). This is consistent with a greater role for sponsorship if greater dispersion of ownership makes it more costly for shareholders to form coalitions to improve governance of the firm.

3.6.4.2 Opaqueness and complexity

The impact of compensation, governance and ownership on risk-taking may depend upon the opaqueness of the firm, with shareholders in more opaque firms having less success in governing managers (John, Saunders, and Senbet, 2000). We use three proxies for opacity and complexity: Volume of underwriting of complex securities, ratio of noninterest income to total assets, and bank size. We also include ratios of deposits to assets and loans to assets as control variables for portfolio composition. Caveats about these proxies are discussed further below.

With respect to underwriting, risks related to high-yield bonds are usually well known and measured. In contrast, ABS are more complex and their risks are harder for outsiders to evaluate. The greater the securitization underwriting activity, the more opaque the bank, and the more magnified the impact of any existing agency problems. If managers prefer to take too much risk, as is implied by our results for compensation discussed above, the likelihood of sponsoring credit-arb vehicles would be higher for more opaque firms.

As measures of *High Yield Underwriting* and *Securitization Underwriting*, we compute a logistic transformation of the total face value of debt of the given type underwritten by each bank during 2006, such that the resulting value on the (0,1) interval is 0.5 where the amount is $12.5 billion.[33] We chose $12.5 billion based on back-of-the-envelope calculations of revenue per dollar of debt underwritten and of the number of fixed-income staff that could be supported by such revenue. Results are robust to other specifications, such as dummy variables for banks with material underwriting volumes. However, if we use raw underwriting volume in combination with total assets as the measure of bank size, collinearity between bank size and underwriting becomes severe (large underwriters are also among the largest banks).

Returning to Table 3.7, we find that ABS underwriting is an economically and statistically

[32] Using the specification in column 4, an increase in institutional ownership from the 25th percentile to the 75th percentile is associated with a 16 percent increase in the probability of sponsorship.

[33] We apply the following transformation: $Y = 1/(1 + exp^{(-\frac{X-12.5}{2.5})})$ where *X* is the dollar value of ABS plus MBS underwriting measured in billions.

significant predictor of sponsorship. High-yield (junk) bond underwriting does not predict sponsorship. In Table 3.8, the second row reports results of comparing fitted values for the 50th and 90th percentiles of the ABS underwriting variable. As a practical matter, the 50th percentile of underwriting activity has an amount underwritten of zero, so the change is from none to a substantial amount. The change in predicted probability of sponsorship is nearly 14 percentage points, which is economically large.

Banks that engage in substantial volumes of activity other than traditional deposit-taking and lending are usually more difficult to understand than traditional banks. To measure non-traditional activities, we use the ratio of non-interest income to assets. Table 3.7 and Table 3.8 show that the relationship between this measure and sponsorship is positive, but it is only marginally statistically significant and the economic magnitude is small.

Larger organizations are not necessarily more complex, but the correlation between size and complexity is arguably positive. We divide banks into terciles of total assets and include dummies for the middle and largest terciles (*Dummy Intermediate Tercile* and *Dummy Top Tercile*). The coefficient for the top tercile is always positive and economically and statistically significant. In the fourth row of Table 3.8, fitted values are produced for the lowest and highest tercile, with the implied change in probability of moving from the small to the large category estimated to be 30 percentage points, a large effect. This result is robust to a variety of alternative measures of bank size, but we use terciles in the tabulated specifications because it limits problems of collinearity with other variables.

Of course, all of our proxies for opacity may also represent other things. Perhaps most importantly, economies of scale and scope may play a role in a bank's decision to sponsor vehicles. If setup and operation of a vehicle involves fixed costs, and given that net spreads per dollar invested were small, a positive net income constraint may have motivated banks to sponsor only vehicles of substantial size (few sponsors with vehicles totaling less than $2 billion of assets appear in the data). Moreover, the sponsor must itself be large enough to convince the rating agencies that it can fulfill its commitment to support its vehicles. Since the sponsor was generally also the investment manager, economies of scope would exist if the sponsor already had on its payroll staff that was expert in fixed-income investments.[34]

Economies of scale cannot fully explain the strong effect of bank size on sponsorship because a number of medium-sized banks in our sample sponsored vehicles. Similarly, economies of scope

[34] A sizeable debt underwriter will typically house fixed-income dealing and trading units, fixed-income investment bankers that do issuance transactions, and specialized risk managers. Such individuals have the expertise to provide portfolio management services to the ABCP vehicles we study, but they are high-cost labor. Low underwriting activity likely implies a small staff of this type, which may not reliably have enough excess labor capacity to service vehicles. A larger staff may occasionally be fully occupied with underwriting and with trading for the bank's own account, but is more likely than a small staff to regularly have enough labor slack to be able to attend to a vehicle.

are probably not the whole story behind the significance of underwriting measures. The last two rows in Table 3.8 show an exercise were we measure the change in the probability of sponsorship for small and large banks in the sample, when the levels of ABS underwriting increase from the 50th percentile in the sample to the 90th percentile. The change in the probability of sponsorship is more than twice as large for large banks when their level of underwriting increases. Size should not matter to scope economies, whereas banks that are both large and engaged in opaque activities seem particularly likely to be difficult to monitor and thus subject to owner-manager agency problems. Also note that high yield underwriting does not affect the decision to sponsor ABCP, but securitization underwriting strongly predicts sponsorship (see Table 3.7). We expect both types of underwriting to have similar economies of scope with credit-arb ABCP vehicles, but securitization underwriting is less transparent. Taken together, these results are consistent with agency problems as well as with economies of scale and scope.

3.6.5 Other factors

We include in regressions deposit-to-assets and loans-to-assets ratios as controls for the degree to which a bank engages in traditional activities. The marginal impact on the probability of sponsorship is sometimes weakly statistically significant, but the economic size of the relationships are small.

The last three rows of Table 3.7 report results for variables that proxy for differences in national financial structure. Coefficients on such variables are generally statistically insignificant. Private Bond Market Capitalization is significant if the U.S. dummy is omitted, but as the U.S. bond market is by far the largest, this variable could be standing in for any of the differences between the United States and Europe.

3.7 Concluding Remarks

Overall, the combined evidence from probit regressions and from other facts and statistics suggests that a combination of government-controlled banks, agency problems, and perhaps safety nets and economies of scale and scope motivated bank decisions to take systematic bad-tail risk in the form of credit arbitrage ABCP vehicles. The fact that the majority of the western world's major universal banks sponsored vehicles is consistent with a view that relevant incentive distortions were widespread. Such risk-taking is of particular interest to designers of financial reform. We perceive that reforms to date have focused more on the impact of safety nets than on other factors. Our results imply that attention to government controlled banks and agency problems is also warranted.

CHAPTER 4

Do Bank Capital Regulations Concentrate Systematic Risk?

4.1 Abstract

As a result of the Enron scandal, new regulations were enacted that increased the capital charge for holding assets in off-balance sheet vehicles. I utilize a triple difference specification to identify the effect of this exogenous regulatory shock on bank systematic risk exposure. I find that after the regulation, banks' exposure to off-balance sheet assets at vehicles with high systematic risk increases relative to vehicles with low systematic risk and relative to non-U.S. banks which are not affected by the regulation. These results suggest that capital regulation might have the perverse effect of concentrating systematic risk, potentially increasing the systemic risk of the financial system.

4.2 Introduction

The primary goal of bank regulation is to maintain the stability of the financial system. To achieve this goal, regulators frequently employ capital requirements. Regulatory capital requirements are predicated on the idea that better capitalized banks are less risky, both because lower leverage reduces the incentives for risk shifting and because equity buffers reduce the risk of bank insolvency. But do capital regulations actually make banks safer? While a number of theoretical models suggest that they do (Furlong and Keeley, 1989; Keeley and Furlong, 1990; Rochet, 1992), other models suggest that capital requirements lead to increased risk taking (Koehn and Santomero, 1980; Buser, Chen, and Kane, 1981; Kim and Santomero, 1988; Gennotte and Pyle, 1991; Blum, 1999). The empirical evidence is similarly mixed, partly because risk taking is difficult to measure and regulatory capital levels are frequently endogenous. In this paper, I use the relatively clean setting of bank-sponsored asset backed commercial paper (ABCP) conduits to quantify the effect of capital regulation on bank risk.

Three features of ABCP conduits make them a particularly attractive laboratory to study risk taking. First, both European and U.S. banks actively sponsor ABCP conduits, but these conduits invest in the same pool of U.S. assets. Second, a series of regulatory shocks in mid-2002 through 2004 known as Financial Interpretation No. 46 (FIN 46) raised the regulatory capital requirements for U.S. banks that sponsored these conduits, but did not affect regulatory capital costs for European banks. These regulations were motivated by the collapse of Enron, so the change in required capital is not an endogenous response to conditions inside the financial sector. Third, sponsoring an ABCP conduit represents a nearly pure systematic risk to the bank; the potential magnitude of this risk varies with the structure of the vehicle which is determined at its creation. Importantly, FIN 46 has a similar effect on banks regardless of the type of vehicle.

I exploit these features to estimate the effect of FIN 46 on bank risk taking. Using European banks as a control group, I calculate the differential impact of FIN 46 on U.S. banks' ABCP sponsorship and exposure. The trends in ABCP sponsorship were similar in both regions before the regulation, so the difference-in-difference estimate eliminates changes in sponsorship due to market forces. I then split ABCP conduits into two groups based on their expected risk-level and take a third difference. I find that the increased regulatory capital charges instituted by FIN 46 increase bank sponsorship of and exposure to high systematic risk ABCP conduits relative to low systematic risk conduits and relative to European banks. While FIN 46 decreases total bank exposure to ABCP, it concentrates exposure to the riskiest types of ABCP. This risk-shifting clearly weakens the intended effect of the capital regulation, and depending on the asset correlation structure, might actually increase the probability of a systemic financial crisis.

This difference-in-difference-in-difference approach identifies the causal effect of the FIN 46 capital regulation on systematic risk taking. For identification to fail in this setting, there would have to be an omitted variable that does not affect bank ABCP decisions before 2002, but post-2002 causes U.S. banks to decrease their sponsorship and exposure to ABCP conduits—but only at relatively safe conduits, and not at risky conduits. The same omitted variable has no effect on the sponsorship decisions of European banks. Further, my estimates include bank-level fixed effects, so they are identified off of the change in the composition of the ABCP portfolio at the same U.S. bank compared to the change at a similar European bank. Consequently, the results are not driven by a change in the types of banks sponsoring ABCP vehicles or by any other omitted, relatively constant bank characteristic.

After documenting that capital regulation can concentrate systematic risk exposure, I use cross-sectional variation in bank characteristics to examine the determinants of the magnitude of this risk-shifting. Laeven and Levine (2009) show that the effect of regulations on bank risk taking varies by governance and ownership structure. Motivated by this result, I examine how the governance indices used in Aggarwal et al. (2010) affect banks' responses to FIN 46. I also examine the impact of institutional and inside ownership, since Laeven and Levine (2009) show that banks with larger shareholders are more likely to risk-shift in the face of various regulations.

I find evidence that banks with more inside ownership react particularly strongly to FIN 46; these banks exhibit the strongest shift toward the riskiest types of ABCP exposure. I further show that banks with more incentive-compatible compensation schemes take on relatively more risky ABCP exposure as a result of the regulations, though these banks also strongly reduce their overall ABCP exposure. In contrast to recent papers that argue that poor compensation structures (particularly large cash bonuses) of managers and traders led banks to take on additional systematic risk in the quest for "fake alpha" (Rajan, 2006; Diamond and Rajan, 2009; Kashyap, Rajan, and Stein, 2008; Crotty, 2009), these results suggest that increased exposure to risky ABCP was consistent with shareholder incentives. In that vein, the results are consistent with Fahlenbrach and Stulz (2011) who find that banks with CEOs that received a greater proportion of compensation in cash bonuses performed no worse during the crisis and with Erkens et al. (2012) and Beltratti and Stulz (2012) who both find evidence that banks with better measures of corporate governance took more risks in the years preceding the crisis.

The primary contribution of my paper is to provide a relatively clean setting to identify the causal effect of regulatory capital restrictions on systematic risk taking. Many previous papers have looked at this question, at least with regards to general risk taking. For example, Demirguc-Kunt, Detragiache, and Merrouche (2013) argue that capital regulations made banks safer; they support this argument with evidence that better capitalized banks had better stock market performance during the financial crisis of 2007–2009. In contrast, Hovakimian and Kane (2000) show that capital regulations did not prevent risk-shifting by U.S. banks from 1985–1994 and Gonzalez (2005) uses cross-country differences in regulation to show that regulatory restrictions increase bank riskiness.

My paper differs from this existing work in two important dimensions: first, I use an exogenous (to the financial sector) change in regulatory capital requirements, which helps alleviate the simultaneity bias found in much of the existing work; and second, I examine a setting (ABCP conduits) where the nature of the risk is clearly systematic and varies in a predictable way. This approach allows me to clearly show how capital regulation concentrates systematic risk in one small part of the banking sector. The disadvantage of this approach is that my conclusions might not be applicable to more general capital regulations. Given that the incentives are similar in other bank settings, though, I expect that similar effects exist with all types of bank capital regulation.

Following the financial crisis of 2007–2009, there has been a vigorous debate about whether or not to increase capital requirements (see, e.g., Admati, DeMarzo, Hellwig, and Pfleiderer, 2011). I contribute to this debate by demonstrating that equity requirements cause banks to shift the risk composition of their existing asset pools. Accounting for this incentive is important in both academic and policy debates about the costs and benefits of capital regulation.[1]

In addition to contributing to the understanding of how capital regulation affects risk taking, this paper also adds to the growing literature on risk taking in the ABCP market. Prior to the financial crisis, the ABCP market had been an important and rapidly growing source of short-term funding for banks, with total U.S. ABCP outstanding exploding from less than $200 billion in 1997 to become the largest short-term debt instrument in the U.S. with $1.2 trillion outstanding in July 2007. For perspective, the second largest instrument was Treasury Bills with approximately $940 billion outstanding.[2] As the turmoil in the U.S. subprime market became apparent in early August, investors panicked and refused to roll over existing ABCP, causing outstanding ABCP to drop 20 percent by the end of the month. The panic was widespread; Covitz et al. (2013) show that nearly 40 percent of conduits were in a run by the end of 2007. Faced with massive liquidity shortages, conduits turned to their sponsors (mostly U.S. and European banks) for help. In turn, banking institutions were forced to seek funding in interbank markets. In particular, since most of the growth in ABCP had been driven by European banks (see Figure 4.1), many banks were forced to quickly raise funds outside of their home currency. The resulting shock to global money markets led to the worst credit crisis since the great depression.

Numerous papers examine the incentives that led banks to participate in this market. Most relevant to this paper is Acharya et al. (2013) who find evidence that U.S. commercial banks structured their support of ABCP conduits to reduce regulatory capital. My results complement their paper by showing that in addition to structuring vehicles to avoid regulatory capital, banks also shifted the composition of sponsored vehicles toward riskier assets. This is consistent with Shin (2009) and Acharya, Cooley, Richardson, and Walter (2010) who demonstrate that banks utilized ABCP conduits to consolidate, rather than disperse, credit risk. While my results are probably not directly applicable to the crisis, they are consistent with regulatory induced distortions playing a role in driving banks' risk choices. [3]

The remainder of the paper is structured as follows. Section 4.3 briefly reviews the key features of ABCP conduits that I exploit in this study, Section 4.4 summarizes the triple difference methodology that I use, Section 4.5 reviews the data and summary statistics, Section 4.6 presents the results, and Section 4.7 concludes.

[1] In a sense, this point is similar to Koehn and Santomero (1980), Kim and Santomero (1988), and Rochet (1992) who point out that if the risk weights are wrong on risk weighted capital requirements, total bank risk will actually increase. In this case, if regulators do not carefully consider how capital regulation will shift the composition of asset portfolios, total systemic risk might increase.

[2] ABCP has plummeted since the crisis, with levels currently hovering around $200 billion.

[3] I explain a shift toward riskier assets at U.S. banks, but many of the most problematic ABCP vehicles during the crisis were sponsored by European banks.

4.3 Key Features of ABCP Conduits

Banks created the first asset backed commercial paper (ABCP) conduits in the mid 1980s as a way to provide inexpensive trade receivables financing for their clients. These off-balance sheet vehicles, referred to as multi-seller conduits, funded the purchase of trade and credit card receivables by issuing commercial paper backed by these assets. The Basel Capital Accord of 1988 created additional incentives for banks to sponsor ABCP conduits by reducing capital requirements for off-balance sheet assets. That same year, Citigroup created a new type of conduit designed to arbitrage the credit spread term structure—the structured investment vehicle (SIV). As the securitization market continued to develop, other types of vehicles emerged differentiated mainly by the types of assets that they could invest in and the extent and types of credit and liquidity guarantees that bank sponsors provided. Despite the variety of structures, until the mid 1990s ABCP conduits remained a relatively unimportant source of financing. However, the ABCP market exploded in the late 1990s, and by 2001 the amount of ABCP outstanding exceeded the amount of commercial paper.

An in depth review of the details of ABCP conduits is beyond the scope of this paper. Arteta, Carey, Correa, and Kotter (2013) describe the main features of the different types of conduits, with a particular emphasis on securities arbitrage and SIVs. Acharya et al. (2013) provide a thorough discussion of the various credit and liquidity guarantees that banks provide to ABCP conduits. Covitz et al. (2013) and Kacperczyk and Schnabl (2010) review the growth, and subsequent collapse, of the ABCP market and a host of papers explore the role ABCP conduits played in the 2007 financial crisis (see, e.g., Shin, 2009; Acharya et al., 2010; Crotty, 2009). Though clearly important, this paper does not examine the connection between ABCP conduits and the financial crisis. Rather, I use ABCP vehicles as a laboratory to examine the effect of capital regulation on bank risk taking. As such, I only briefly review two salient features of ABCP vehicles and refer the reader the the above cited literature for a more thorough description of bank-sponsored conduits.

4.3.1 Sponsoring ABCP Conduits: Systematic Risk Taking

ABCP conduits are bankruptcy remote special purpose vehicles primarily established by large commercial banks. Similar to banks, conduits provide maturity transformation services by issuing short-term ABCP in order to invest in highly-rated medium and long-term securities. Unlike banks, conduits' liabilities are uninsured and largely unregulated, making conduits particularly vulnerable to runs as in Diamond and Dybvig (1983). In order to mitigate this risk, conduit sponsors provide liquidity and credit guarantees that in effect provide full insurance against bad-tail systematic risk. In return for providing these guarantees, banks receive fee revenue. There is very little data available on vehicle profitability, but the anecdotal evidence suggests ABCP vehicles provide very low per unit returns. For example, Mellon bank sponsored a vehicle that had $3.2 billion in assets at the end of 2006, which provided Mellon with $3 million of gross fee revenue, or a return of about 10 basis points (Arteta et al., 2013).

From 2000 to 2007, there were 6 primary types of conduits: multi-seller, single-seller, securities arbitrage, hybrid, SIV, and CDO. Regardless of the type of conduit, the bank sponsor is primarily exposed to bad-tail systematic risk—that is, the bank agrees to provide credit and liquidity support in states of the world where short-term funding dries up and the credit quality of existing highly-rated securities plummets. While all types of conduits are exposed to this risk, the magnitude of the risk depends on two things: the maturity of the assets and the extent of the guarantee.

Since the vast majority of ABCP vehicle investments are in investment grade, highly liquid securitized assets, the primary difference in risk is the maturity mismatch between the 30-day commercial paper typically used to fund the vehicle and the assets themselves. Credit arbitrage vehicles and SIVs primarily invest in longer maturity assets such as mortgage backed securities, collateralized loans, and collateralized debt obligations (CDOs). The types of assets that these vehicles invest in are set by the vehicle charter; it is difficult to change this once the vehicle is created. Other vehicle types, such as multi-seller conduits, invest primarily in shorter-term trade receivables and credit card securities. The former vehicles have a much larger maturity mismatch than the latter vehicles, implying that they are riskier.

In addition to the maturity mismatch, ABCP conduits differ in their type of guarantee. These guarantees range from very strong credit guarantees that require the bank to pay off maturing ABCP regardless of underlying asset values to weaker liquidity guarantees that only require the bank to pay off maturing ABCP if the assets are not in default. The commercial paper rating of the vehicle is dependent on the type of guarantee offered by the sponsoring bank, which typically does not change over time.

I utilize the difference in risk exposure inherent in the maturity-mismatch and guarantee structure of ABCP vehicles to examine the effect of capital regulation on systematic risk taking.

4.3.2 Regulatory Capital Treatment of ABCP Conduits

The regulatory and accounting treatment of ABCP vehicles changed considerably in the 2000s. Before that time, banks in both Europe and the U.S. were allowed to keep ABCP vehicle assets off-balance sheet; in addition, they were not required to hold any capital against liquidity guarantees offered to conduits.

In the U.S. two major regulatory changes occurred beginning in 2002. First, U.S. bank regulators introduced risk-based capital requirements for “direct credit substitutes” which included credit enhancements commonly provided to ABCP vehicles. The capital charge depended on the rating composition of the vehicle's assets and on the size of the credit enhancement. Arteta et al. (2013) estimate that for a typical vehicle, approximately 16 basis points of Tier 1 capital were required per dollar of vehicle assets.

Second, the Enron bankruptcy scandal of late 2001 led FASB to consider revisions to accounting standards for special purpose vehicles. These revisions culminated in the release of FIN 46 in July of 2002, which effectively required U.S. banks to consolidate ABCP vehicles' assets on balance sheet. This change required banks that sponsored ABCP vehicles to hold Tier 1 capital equal to 5 percent of vehicle assets. This dramatic increase in required capital led banks to seek for additional clarification on FIN 46 rules. As a result, in December 2003 FASB issued FIN 46R. This revised rule, brokered by the Office of the Comptroller of the Currency, the Federal Reserve Board, the Federal Deposit Insurance Corporation, and the Office of Thrift Supervision, allowed banks to exclude consolidated ABCP conduit assets from regulatory capital requirements if they issued "expected loss notes" (ELNs) to third parties (Bens and Monahan, 2008). Issuing such notes was far cheaper than meeting the 5% leverage ratio requirement. In addition to allowing banks to avoid consolidating ABCP assets, FIN 46R also increased the capital requirement for full liquidity guarantees offered to conduits from 0% to 10% relative to on-balance sheet financing.

While these regulatory capital costs are small in absolute terms, they are large relative to vehicle profits. Arteta et al. (2013) show in their appendix that the costs associated with holding this regulatory capital and selling ELNs to avoid consolidation represent approximately 40% of the revenue of bank-sponsored vehicles.

These regulatory costs put U.S. banks at a disadvantage compared to European banks. European banks were required to consolidate conduits with the adoption of International Financial Reporting Standards (IFRS); however, most European countries did not require banks to hold regulatory capital against conduit assets.[4] This changed with the move to Basel II (announced in June 2004) which implemented capital requirements very similar to the U.S. Basel II was phased in slowly over time, though, and by the end of my sample (2007) the majority of banks had not yet adopted Basel II accounting standards.

This implies that after 2002 European banks had a cost advantage at sponsoring ABCP conduits compared to U.S. banks. Importantly, despite regulatory changes, accounting rules in both the U.S. and Europe allowed for potential "regulatory arbitrage" of capital requirements throughout the entire sample period of 1999-2007. However, the extent of benefits from this arbitrage differed across countries and years. Interestingly, the overall potential regulatory advantage was decreasing over time, which contrasts with the rapid growth in ABCP over this period.

[4]Spain and Portugal are two important exceptions.

4.4 Methodology

How does capital regulation affect bank risk taking? In general this is a difficult question to answer. Quantifying systematic risk is hard; additionally, changes in capital regulation frequently are driven by financial or economic events which also likely affect risk taking. The two features described in the previous section provide a potential laboratory to overcome these difficulties. First, sponsoring ABCP vehicles is nearly a pure systematic risk and the extent of this risk varies in a way that is predictable ex ante. Second, FIN 46 is a large regulatory capital shock that affected all types of ABCP vehicles in a similar way. FIN 46 was motivated outside of the financial sector (by Enron's use of special purpose entities), so the regulation is relatively exogenous to banks. I use these features to implement a triple difference estimation.

First, I compare changes in the extensive and intensive margins of ABCP sponsorship (i.e., the probability that a bank sponsors a vehicle and the size of the vehicle) at the same bank before and after the regulation (FIN 46). By comparing changes within the same bank, I alleviate concerns that changes in sponsorship are driven by bank-specific factors.

Next, I examine the difference before and after FIN 46 between U.S. banks and European banks. Both U.S. and European bank-sponsored conduits invested in the same pool of U.S. securitized assets and raised funding primarily from U.S. ABCP investors. Consequently, any general changes in the profitability of sponsoring vehicles should affect European and U.S. banks in the same way. By taking this difference in difference, since the change in regulation does not affect European banks I isolate the effect of FIN 46. As with any difference-in-difference estimate, to interpret these results in a causal sense I have to assume that the parallel trends assumption holds. In this context, that means that ABCP sponsorship needs to have evolved similarly between U.S. and European sponsors before FIN 46; further, I assume that sponsorship would have continued to evolve similarly in the absence of any regulatory changes.

While this assumption is not testable, Figure 4.1 illustrates its plausibility. Figure 4.1 shows that the total value of ABCP assets at bank-sponsored conduits was nearly identical before July 2002 when FIN 46 was first proposed. After FIN 46, ABCP continues to grow rapidly at European bank-sponsored vehicles, but is roughly stagnant at U.S. bank-sponsored vehicles, consistent with the regulation increasing costs of sponsoring ABCP for U.S. banks relative to European banks.

Further support for the parallel trends assumption is seen in Figure 4.2. This figure graphs the evolution of ABCP exposure, defined as ABCP assets to total bank assets, for European and U.S. bank-sponsored vehicles. While the U.S. banks have a much higher level of exposure, the trend from 2000 to early 2002 is similar for both regions. Following the introduction of FIN 46 in mid-2002, the trend in U.S. ABCP exposure turns negative. In contrast, exposure at European banks continues to grow slowly before exploding in mid-2005. Together, Figure 4.1 and Figure 4.2

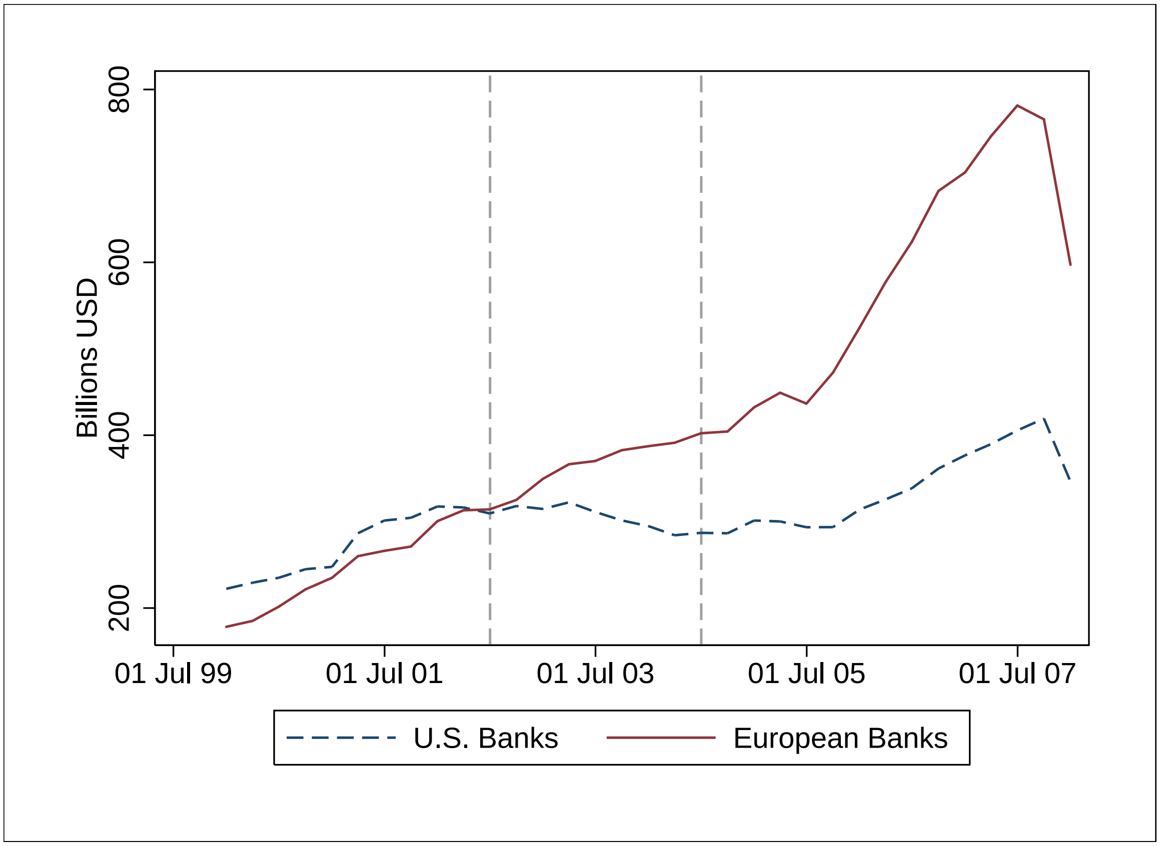

Figure 4.1. Total ABCP Assets Outstanding. This figure graphs quarterly total ABCP assets outstanding from December 1999 to December 2007. The ABCP data is from Moody's. The dashed vertical lines represent the passage FIN 46 and FIN 46R, which are capital regulations that increased the cost to U.S. banks of sponsoring ABCP vehicles.

suggest that the identifying assumption for the difference-in-difference estimate holds.

Finally, to examine the effect of FIN 46 on systematic risk taking I take the difference in the previous results between high and low systematic risk vehicles. The parallel trends assumption required to identify this portion of the estimate is that exposure to safe and risky ABCP followed a similar trend before FIN 46. The plausibility of this assumption can be judged in Figure 4.3.

Figure 4.3 shows the growth of exposure to risky and safe ABCP conduits from 2000–2007. Both types of exposure grew prior to FIN 46, though risky exposure clearly grew faster. Importantly, the trends clearly diverge after FIN 46: exposure to safe conduits plummets, while exposure to risky conduits continues rising.

To the extent that capital regulation concentrates systematic risk, I expect to find that ABCP sponsorship and exposure increases as a result of FIN 46 in high systematic risk vehicles as compared to low systematic vehicles and as compared to similar vehicles in Europe.

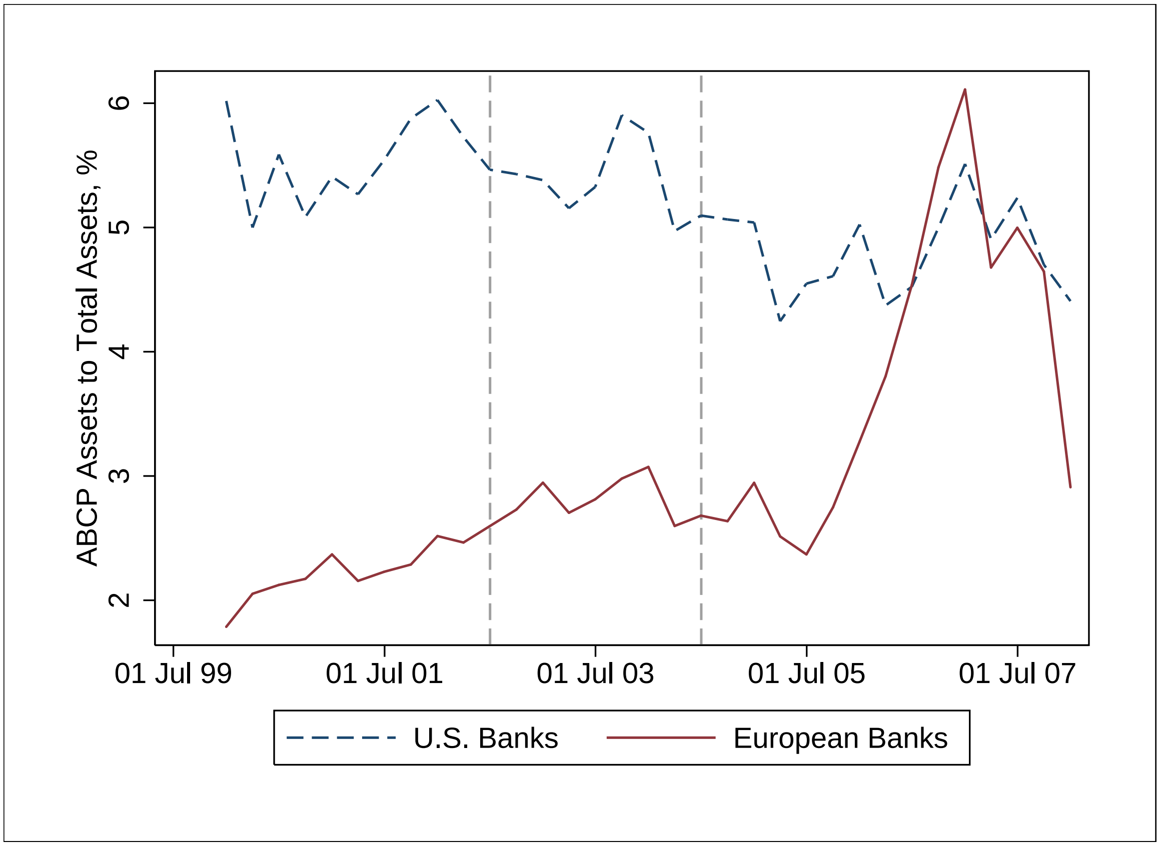

Figure 4.2. ABCP Exposure by Region. This figure graphs quarterly mean ABCP exposure across sponsor banks, measured as the ABCP conduit assets to bank assets, across U.S. and European banks from December 1999 to December 2007. The ABCP data is from Moody's, and the bank asset data is from Bankscope. The dashed vertical lines represent the passage of FIN 46 and FIN 46R, which are capital regulations that increased the cost to U.S. banks of sponsoring ABCP vehicles.

4.5 Data and Descriptive Statistics

I use data from several sources. Data on ABCP conduits are hand-collected from Moody's quarterly "Program Index" spreadsheets. These data cover characteristics of all vehicles rated by Moody's from December 1999 to December 2007, including the average amount of outstanding ABCP issued in the U.S. and European commercial paper markets each quarter.[5] This process results in a database of 589 conduits.

Moody's provides quarterly information on the primary assets of each vehicle it rates. Using this information, I divide vehicles into two types based on the risk and origin of the vehicles' assets. I classify ABCP programs that invest primarily in asset-backed securities, including residential and commercial mortgage-backed securities, as *risky*. This group includes the majority of security arbitrage and hybrid vehicles and all SIVs and CDOs.[6] I also include single-seller vehicles that

[5]Moody's rates over 90 percent of global conduits by assets.

[6]In contrast to other recent studies, my sample includes some CDO conduits, but my results are robust to excluding

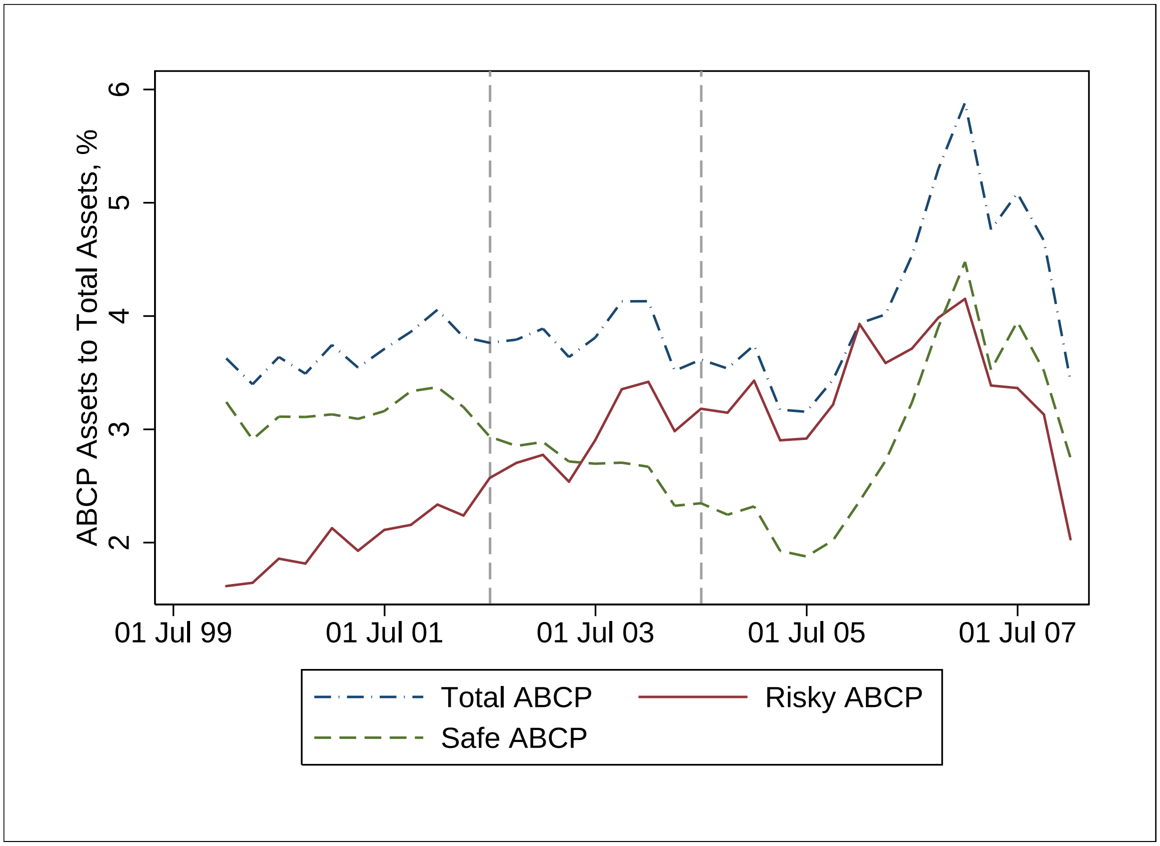

Figure 4.3. ABCP Exposure by Type of Asset. This figure graphs quarterly mean ABCP exposure across sponsor banks by type of underlying assets from December 1999 to December 2007. Risky ABCP assets are securities and mortgage backed products, while safe ABCP assets are mostly trade receivables. The ABCP data is from Moody's, and the bank asset data is from Bankscope. The dashed vertical lines represent the passage of two ABCP regulations that increased the cost to U.S. banks of sponsoring ABCP vehicles.

specialize in mortgages in the risky category, since these programs are generally used by mortgage banks as warehouses for loans until they can be sold as mortgage-backed securities. My results throughout this paper are robust to excluding all single-seller vehicles from the risky group. I classify all other programs as *safe*.[7] These conduits are mostly invested in trade receivables, and their primary purpose is generally to provide cheap funding to their customers. This group includes the majority of multi-seller vehicles and the non-mortgage single seller vehicles. From an *ex ante* perspective, these conduits are safe compared to risky programs because they have a smaller maturity mismatch between assets and liabilities. The *ex post* evidence is consistent with this relative risk ranking; Covitz et al. (2013) show that ABCP issued by SIVs and mortgage single-sellers plummeted more than 80 percent from August to December 2007, while ABCP issued by multi-seller conduits only fell about 11 percent. Given the vastly different investment strategies, I

---

these observations.

[7]My results are also robust to using the classification of risky programs found in Arteta et al. (2013), where all security arbitrage, hybrid, and SIV vehicles are risky and other program types are safe.

expect the motives underlying exposure to risky and safe ABCP to differ.

After classifying each vehicle, I use data from Moody's to determine the sponsor. I exclude conduits that are not sponsored by a commercial bank or similar financial institution.[8] I then match each conduit to data on its sponsor from Bankscope, if available. This database includes financial statement information for financial institutions across the world, and covers approximately 90 percent of bank assets in each individual country.

To form my control sample, I also include banks that do not sponsor any ABCP vehicles. For each year, a bank is in my sample if it has data available in Bankscope, more than $3 billion in total assets, and is domiciled in the United States or in Europe.[9] I choose $3 billion dollars as my lower asset limit because the smallest bank that sponsors an ABCP program in my sample, the U.S.-based First Republic Bank, sponsors a security arbitrage program and has $3.6 billion dollars in total assets. However, my results are robust to higher asset limits of $5, $10, and $25 billion dollars. I omit Canadian banks because the regulatory environment of the Canadian commercial paper market differs significantly from that in the U.S. and Europe. I exclude Australian, Japanese, New Zealand, and South African banks because although these countries do have banks that sponsor ABCP vehicles, they represent a very small percentage of the global ABCP market.

Panel A of Table 4.1 summarizes the number of banks in my sample by country. The process described above results in a sample of 236 banks across 15 countries. The 79 sponsor banks in my sample sponsor a total of 284 conduits. U.S. banks represent about forty percent of the sponsoring banks in my sample, or about one-third of the total ABCP assets outstanding as of June 2007 (see Figure 4.1). Panel B of Table 4.1 describes how the sample varies across time; although not a balanced panel, my sample is split roughly evenly across years. On average, my sample includes 54 sponsor banks and 154 non-sponsor banks each year. I begin my sample in the fourth quarter of 1999 due to data availability, and I end my sample in the second quarter of 2007 to avoid the run in the ABCP market that began in August 2007.[10] My sample includes 31 quarters of data for a total of 6,539 bank-quarter observations.

My main variable of interest is exposure to ABCP. ABCP sponsors are ultimately responsible for the assets underlying the ABCP conduit. Generally, a vehicle's assets are approximately equal to its outstanding ABCP. However, SIV programs are an exception. SIV assets are typically about four times outstanding ABCP, with the difference funded by issuing medium term notes and a small equity tranche. Technically, ABCP sponsors only guarantee the assets funded with ABCP. However, the model presented in Gorton and Souleles (2007) implies that in a repeated game context, a conduit is unable to obtain funding without the implicit support by its sponsor of its

[8]I exclude former U.S. investment banks as they were not deposit taking institutions during my sample period.

[9]For this paper, Europe is defined as the EU-15 plus Norway and Switzerland.

[10]However, my main results hold when I extend the sample to December 2007.

Table 4.1
Number of Banks by Country and Year. This table summarizes the number of banks in our sample by country and by year. In Panel A, banks not sponsoring ABCP vehicles did not sponsor an ABCP vehicle during our sample period of December 1999 to June 2007, while banks sponsoring ABCP vehicles sponsored one or more vehicles in at least one quarter during that time period. In Panel B, banks not sponsoring ABCP vehicles did not sponsor any ABCP vehicle during that year, while banks sponsoring ABCP vehicles sponsored one or more vehicles in at least one quarter during that year.

Panel A:

	Not Sponsoring ABCP Vehicles	Sponsoring ABCP Vehicles	Total
Austria	10	1	11
Denmark	3	1	4
Finland	2	0	2
France	17	8	25
Germany	18	16	34
Greece	5	0	5
Ireland	4	1	5
Italy	25	2	27
Netherlands	9	4	13
Portugal	5	0	5
Spain	8	2	10
Sweden	4	2	6
Switzerland	18	1	19
United Kingdom	9	9	18
United States	20	32	52
Total	157	79	236

Panel B:

	Not Sponsoring ABCP Vehicles	Sponsoring ABCP Vehicles	Total
1999	135	42	177
2000	142	49	191
2001	153	52	205
2002	154	57	211
2003	162	57	219
2004	162	58	220
2005	164	57	221
2006	161	56	217
2007	155	58	213
Mean	154	54	208

entire assets. Anecdotal evidence during the crisis supports this model, since many banks chose to support the entire assets of the SIVs they sponsored.[11] Consequently, I view banks as exposed to the total value of SIV assets. For each sponsor bank and each quarter, I measure total ABCP assets as four times the sum of outstanding ABCP at all of the bank's SIVs plus the sum of outstanding ABCP at all of the bank's other conduits.[12] I then define total ABCP exposure as total ABCP assets to total on-balance sheet bank assets. Risky ABCP and safe ABCP exposure are defined similarly, using the risk classifications described above.

Figure 4.2 graphs the average ABCP exposure over time for sponsor banks in my sample. For the majority of my sample, U.S. sponsor banks were more exposed to ABCP than European banks. However, beginning in July 2005, European sponsor banks sharply increased their ABCP exposure from around 2.5% of assets to 6% of assets. Just prior to the onset of the financial crisis, U.S. and European banks had a similar level of exposure of about 5% of assets.

In addition to variation over time, exposure to ABCP varies significantly across countries as well. Table 4.2 shows the average amount of ABCP exposure by country for all sponsor banks. Note that three countries—Finland, Greece, and Portugal—do not sponsor any ABCP vehicles over my sample period. There is considerable heterogeneity in exposure both across and within countries; for example, the standard deviation of total exposure within the United States (4.6%) is nearly as large as the standard deviation of total exposure across the entire sample (5.3.%). Germany has the highest average risky ABCP exposure of any country at 2.7% of assets. This is largely due to the fact that a number of state-sponsored Landesbanks sponsor risky ABCP vehicles on relatively large scales. I control for Landesbank sponsors throughout all of my estimations, and my results are also robust to dropping Landesbanks from my sample.

Table 4.2 reveals that the average safe ABCP exposure within a country is usually quite different than the average risky ABCP exposure. Figure 4.3 explores that difference over time. The growth trend of average exposure is quite different between safe and risky ABCP, and the difference is particularly pronounced after ABCP regulation stage 1 (the area between the dotted vertical lines). This graph helps confirm my suspicions that the motives underlying risky and safe ABCP exposure are potentially quite different.

To control for country-level macroeconomic conditions and financial market structure, I gather annual data on stock market capitalization, bond market capitalization, GDP per capita, and various other macroeconomic indicators from the Bank for International Settlements, OECD, and World Bank. To control for each country's regulatory environment, I take the regulatory indices used in Caprio, Laeven, and Levine (2007) and described in Barth, Caprio Jr, and Levine (2001) and Barth,

[11] Perhaps the most famous example is Citigroup, which offered full support to all seven of the SIVs that it sponsored in December 2007.

[12] My results are robust to ignoring this correction and measuring total ABCP assets as the sum of ABCP outstanding at each of the bank's conduits.

Table 4.2

Average ABCP Exposure, by Country. This table describes the average ABCP exposure across banks that sponsored ABCP programs by country over the sample period of December 1999 to June 2007. Exposure is defined as total ABCP assets to bank assets. Risky ABCP exposure includes ABCP programs that primarily invest in securities and mortgage products, while safe ABCP exposure includes programs that primarily invest in trade receivables. Note that Finland, Greece, and Portugal do not sponsor any ABCP vehicles during our sample period. The table reports average exposure in percentage terms, standard deviation is reported in parentheses.

	(1) Total ABCP Exposure	(2) Risky ABCP Exposure	(3) Safe ABCP Exposure
Austria	0.71	0.71	0.00
	(0.26)	(0.26)	(0.00)
Denmark	0.15	0.15	0.00
	(0.05)	(0.05)	(0.00)
France	1.03	0.24	0.78
	(1.68)	(0.43)	(1.28)
Germany	1.69	1.11	0.59
	(2.22)	(2.03)	(0.83)
Ireland	33.80	0.00	33.80
	(38.07)	(0.00)	(38.07)
Italy	0.17	0.09	0.08
	(0.22)	(0.18)	(0.05)
Netherlands	1.27	0.47	0.80
	(0.90)	(0.45)	(0.54)
Spain	0.14	0.06	0.08
	(0.09)	(0.07)	(0.10)
Sweden	0.15	0.14	0.01
	(0.16)	(0.17)	(0.01)
Switzerland	0.32	0.10	0.22
	(0.21)	(0.10)	(0.11)
United Kingdom	0.62	0.42	0.21
	(0.47)	(0.51)	(0.28)
United States	0.65	0.25	0.40
	(0.61)	(0.44)	(0.52)
Total	1.07	0.43	0.63
	(4.03)	(1.03)	(3.90)

Caprio, et al. (2004). These indices are measured as of 2004. I obtain the dollar-level of deposit insurance, measured in 2003, from Asli, Kane, and Laeven (2008). Although measured in 2003, this limit was the prevailing limit in the countries I study at the beginning of the crisis in 2007. The regulatory variables do not vary through time. However, I expect that the relative rankings of regulatory environments in the countries I study do not change much over my sample period, so I view this as only a minor defect.

To control for firm-level corporate governance, I utilize several sources. I obtain annual levels of institutional and insider ownership from FactSet Lionshares. Lionshares' definition of insider ownership includes some corporations and government entities, so I limit insider ownership to the individual shareholders that also meet Lionshares' insider definition. This definition of inside ownership is more broad than traditional definitions, so for robustness I use the ownership percentage of directors and executives hand-collected from company annual reports and regulatory filings as of the most recent fiscal year ending before June 2007. The results are similar, so I report my preferred measure from Lionshares since this measure varies over time. I obtain corporate governance indicators from RiskMetrics CGQ index for December 2006, and I follow Aggarwal et al. (2010) to create four sub-indexes from the CGQ data covering board, audit, anti-takeover, and compensation and ownership. Higher values of these indexes represent governance policies that are more friendly to shareholders. To supplement this data, I follow Erkens et al. (2012) and gather information on the composition of the board of directors as of December 2006 from BoardEx. I supplement missing board of director information with data from annual reports and regulatory filings. With the exception of ownership, the governance variables used in this study are not time-varying. To ensure that this does not bias my results, I repeat the governance analysis limiting my sample to 2007 data and find that my results continue to hold.

Finally, I gather additional bank level information to use in my analysis. Stock price and return information is obtained from Datastream, and debt ratings and expected default probabilities (EDF) are taken from Moody's KMV CreditMonitor. The EDF measure implements Merton (1974) structural model and represents the probability that a firm will default within one year, on a scale of 0.01% to 35%. For each quarter, I match each bank with its balance sheet information, ownership levels, and country controls as of the previous year-end. Table 4.3 shows descriptive statistics for each of the variables used in my study.

Table 4.3

Summary Statistics. This table presents descriptive statistics for those variables used in our tobit regression models. Total ABCP, Risky ABCP, and Safe ABCP are measured as the total dollar value of ABCP, in millions of USD. ABCP exposure is defined as ABCP assets divided by equity. Risky ABCP is limited to programs that primarily invest in securities and mortgage products, while safe ABCP is limited to programs that primarily invest in trade receivables. This data is from Moody's. The bank-level controls are from Bankscope. Log Assets is the natural logarithm of bank assets in millions of USD. ROA, Equity to Assets, Deposits to Assets, and Loans to Assets are the corresponding financial statement ratios. Securitization underwriting is a logarithmic transformation of the dollar amount of asset backed securities and mortgage backed securities underwritten by the bank, found in Arteta et al. (2013). ABCP experience is the number of years that the bank has sponsored ABCP vehicles. The Country-level controls are from the Bank for International Settlements, OECD, and World Bank. Log GDP per Capita is the natural logarithm of real GDP per capita, GDP per Capita Growth Rate is the year to year growth rate in real GDP per capita, Stock Market Cap. to GDP is stock market capitalization to GDP, and Private Bond Market to GDP is private bond market capitalization to GDP. The regulatory variables are from Caprio et al. (2007). Official is an index of the regulatory authority's power. Independence is an index of the level of independence of the regulatory authority from the government. Capital is an index that measures regulatory capital stringency and Restrict is an index that measures the restrictions on bank activities. Log Deposit Insurance is the natural logarithm of the 2003 dollar value of deposit insurance found in Asli et al. (2008). Compensation & Ownership, Audit, Anti-takeover, and Board are indexes of firm-level corporate governance created from RiskMetrics CGQ index as in Aggarwal et al. (2010); higher values represent better governance practices. Institutional Ownership is the percentage of shares held by institutions and inside ownership is the percentage of shares held by individual insiders. This data is from FactSet Lionshares.

	Count	Mean	Std. Dev.	Min	Max
Depedent Variables					
Total ABCP	6,362	3,116.97	11,139.63	0.00	178,165.59
Risky ABCP	6,362	1,274.36	5,665.12	0.00	104,085.96
Safe ABCP	6,362	1,842.62	6,644.69	0.00	74,079.63
ABCP Exposure	6,362	0.27	2.10	0.00	107.93
Risky Exposure	6,362	0.11	0.56	0.00	14.28
Safe Exposure	6,362	0.16	2.00	0.00	107.93
Bank-level Controls:					
Log Assets	6,362	10.12	2.58	1.55	14.49
Equity to Assets	6,362	6.74	4.31	−1.86	48.49
ROA	6,362	0.79	0.77	−3.86	9.24
Deposits to Assets	6,362	59.59	18.58	0.00	122.77
Loans to Assets	6,362	55.26	21.16	−8.34	223.23
Securitization Underwriting	6,362	0.06	0.22	0.01	1.00
ABCP experience	6,362	1.63	3.58	0.00	24.10
Country-level Controls:					
Log GDP per Capita	6,362	10.14	0.29	9.27	10.54
GDP per Capita Growth Rate	6,362	1.72	1.44	−1.50	9.48
Stock Market Cap. to GDP	6,362	146.47	110.57	20.79	536.24
Private Bond Market to GDP	6,362	53.88	33.54	0.15	137.83
Landesbank Dummy	6,362	0.08	0.27	0.00	1.00
Regulatory Controls:					
Official	6,362	10.07	2.64	6.00	13.00
Independence	6,362	3.24	0.98	1.00	4.00
Capital	6,362	3.02	1.16	1.00	5.00
Restrict	6,362	7.80	2.79	5.00	12.00
Log Deposit Insurance	6,362	10.85	0.67	10.10	11.78

Continued on next page

Table 4.3
(Continued)

	Count	Mean	Std. Dev.	Min	Max
Governance Controls:					
Compensation & Ownership	2,552	66.39	21.95	14.29	100.00
Audit	2,552	81.41	23.49	33.33	100.00
Anti-takeover	2,552	61.02	13.28	16.67	83.33
Board	2,552	57.54	13.33	27.27	84.00
Institutional Ownership	2,366	36.36	22.03	1.24	81.46
Inside Ownership	2,336	2.52	5.46	0.00	40.18

4.6 Results

4.6.1 Difference-in-Difference: Effect of FIN 46 on bank sponsors

To implement the difference-in-difference methodology described in Section 4.4, I estimate the following regression model

$$Exposure_{it} = \alpha_i + \delta_t + \beta_1 US \times Stage1_t + \beta_2 US \times Stage2_t + \sum \gamma X_{it} + \epsilon_{it}, \qquad (4.1)$$

for each bank i at quarter t for each quarter between January 2000 and July 2007. US is a dummy variable equal to 1 if the bank-sponsor is located in the United States. $Exposure_{it}$ is either an indicator variable for banks which sponsor at least one ABCP vehicle at quarter t (the extensive margin), or it is the ratio of total ABCP assets to equity for bank i at quarter t (the intensive margin). $Stage1_t$ is an indicator variable for all quarters after July 2002—the date of the first announcement of FIN 46—and before July 2004. $Stage2_t$ is a dummy variable for quarters after July 2004, when the final version of FIN 46R is in effect. The key coefficients of interest are β_1 and β_2; these coefficients represent the difference-in-difference estimate of the effect of FIN 46 on ABCP exposure. Because FIN 46 increased the cost to U.S. banks of sponsoring ABCP vehicles, I expect that β_1 and β_2 are negative.[13]

I also include bank and quarter fixed effects (α_i and δ_t) and 7 bank-level control variables (X_{it}) measured as of the previous year end. To receive the highest ABCP ratings necessary to make conduits financially viable, sponsoring banks need to have sufficient assets available to absorb

[13] I allow for the revision of FIN 46 to differentially affect ABCP exposure becuase FIN 46R dramatically lowered the cost of sponsoring ABCP conduits relative to FIN 46. Consequently, it is possible that $\beta_2 > 0$. However, before FIN 46R, there is uncertainty over the extent to which FIN 46 will be enforced. FIN 46R resolves this uncertainty in a way that makes sponsorship more costly than it was before FIN 46. To the extent that banks waited to make large adjustments in their exposure until the regulation was made permanent, the effect of $beta_2$ should still be negative.

ABCP assets. Consequently, I expect a positive relationship with variables that proxy for size and financial strength. I include the natural logarithm of bank assets (*Log Assets*), return on assets (*ROA*) , and *Deposits to Assets* to measure financial strength and available short-term funding. Highly levered banks have incentives to risk shift; consequently, I expect a negative coefficient on *Equity to Assets*.

Because ABCP vehicles are a form of securitization, I expect that banks with more experience managing sophisticated financial products will seek higher levels of ABCP exposure. *Loans to Assets* reflects the composition of the bank's portfolio; banks with larger loan portfolios are likely more traditional and less experienced with sophisticated financial products and thus less likely to have ABCP exposure. *Securitization Underwriting* is a logarithmic transformation of the dollar amount of asset backed securities and mortgage backed securities underwritten by the bank, found in Arteta et al. (2013). Banks with underwriting experience are likely to have the experience necessary to sponsor ABCP programs. Finally, when examining the intensive margin of sponsorship I include the number of years since the bank first sponsored an ABCP conduit (*ABCP Experience*) as a direct control for experience.

The extensive margin of $Exposure_{it}$ is an indicator variable while the intensive margin is a ratio bounded below by zero. It would thus be natural to estimate Equation 4.1 using probit and tobit regressions, respectively. However, interaction terms in nonlinear models are difficult to interpret (Ai and Norton, 2003). Additionally, including fixed effects in probit and tobit models leads to biased coefficients due to the incidental parameters problem (Greene, 2004). Consequently, I estimate Equation 4.1 using OLS for both the intensive and extensive margins. The main conclusions are robust, however, to probit and tobit estimations without firm fixed effects.[14] I adjust my errors by clustering at the bank level (Bertrand, Duflo, and Mullainathan, 2004), but the results are also robust to two-way clustering by bank and year (Petersen, 2009).

Table 4.4 shows the results of estimating Equation 4.1. Columns 1 and 2 examine the probability that a bank sponsors at least one ABCP vehicle during a given quarter, while Columns 3 and 4 look at the bank's level of exposure to ABCP vehicles. The results are similar whether I include country fixed effects or bank fixed effects (note that bank fixed effects subsume the U.S. dummy necessary for this difference-in-difference specification). The main coefficients of interest are the interaction between Stage 1 and Stage 2 and the U.S. dummy. These interactions represent the difference-in-difference estimate of the effect of the regulation on ABCP exposure.

Column 2 shows that Stage 1 decreased the probability of sponsoring an ABCP vehicle by 7%; this estimate is statistically significant at the 5% level. A 7% drop in the probability of sponsorship represents about a 28% decrease in the unconditional probability of sponsoring an ABCP vehicle,

[14] These results are available from the author.

Table 4.4

Effect of FIN 46 on Bank ABCP Decisions. This table presents OLS regressions of the impact of various regulatory measures on bank decisions to sponsor ABCP at each quarter from Q1 2000 to Q2 2007. In columns 1 and 2, I use a linear probability model to estimate the probability that a bank sponsors at least one ABCP vehicle. In columns 3 and 4, I estimate the total ABCP exposure of the bank, where exposure is defined as total ABCP assets to bank equity. *Stage 1* is a dummy variable that is equal to one for quarters after FIN 46 was first proposed in July 2002. *Stage 2* is a dummy variable that is equal to one for quarters after July 2004 when the final revision of FIN 46R was enforced. Both FIN 46 and FIN 46R required U.S. banks to hold additional regulatory capital against ABCP vehicle assets; the final revision considerably loosened these restrictions, but still imposed higher regulatory costs than existed both before the regulation and as compared to most European countries. All bank-level variables are measured as of the year-end prior to the quarter of ABCP exposure. Standard errors are adjusted for bank-level clustering; t-statistics are in parentheses. Regressions include a constant term which is suppressed to conserve space. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	Extensive Margin		Intensive Margin	
	(1) Has ABCP	(2) Has ABCP	(3) ABCP Exposure	(4) ABCP Exposure
Log Assets	0.10***	0.06	−0.05	0.17
	(9.46)	(1.23)	(−1.22)	(0.49)
Equity to Assets	−0.00	−0.01	−0.02	−0.13
	(−1.23)	(−0.96)	(−1.29)	(−1.05)
ROA	0.00	0.03	−0.08	0.05
	(0.15)	(1.35)	(−1.14)	(0.55)
Deposits to Assets	0.00*	−0.00	−0.00	0.01
	(1.71)	(−0.42)	(−0.43)	(0.63)
Loans to Assets	−0.00*	0.00	−0.01	−0.04
	(−1.88)	(0.13)	(−1.42)	(−1.02)
Securitization Underwriting	0.02	0.30*	−0.34	−0.71
	(0.14)	(1.93)	(−1.25)	(−1.49)
ABCP experience			0.09***	0.07***
			(4.98)	(2.90)
US Dummy	0.54***		0.16	
	(4.53)		(0.97)	
ABCP Regulations:				
US Dummy × Stage 1	−0.08**	−0.07**	−0.19***	−0.29*
	(−2.29)	(−2.12)	(−2.82)	(−1.91)
US Dummy × Stage 2	−0.08	−0.07	−0.41*	−0.41*
	(−1.45)	(−1.40)	(−1.88)	(−1.75)
Quarter Fixed Effects	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	No	Yes	No
Firm Fixed Effects	No	Yes	No	Yes
Observations	6,362	6,362	6,362	6,362
Number of Banks	236	236	236	236
Adjusted R^2	0.47	0.83	0.06	0.23

so the regulation has an economically meaningful effect on sponsorship.[15] The estimated effect of Stage 2 is also negative, but not statistically significant.

Column 4 reveals similar effects of regulation on the intensive margin of sponsorship. Stage 1 reduced the ratio of ABCP assets to equity by 0.29 (t-stat 1.91); this represents about a 25% decline in exposure relative to the sample average of 1.07 for banks that have nonzero exposure. The estimated coefficient on the Stage 2 interaction is negative and weakly significant. Taken together, Table 4.4 suggests that FIN 46 had a large effect on both the extensive and intensive margins of ABCP sponsorship and that this effect was concentrated during the first stage of the regulation.

Somewhat surprisingly, few of the bank characteristics seem to matter for sponsorship decisions. The signs of the effects are mostly in the predicted direction, but most of the coefficients are statistically insignificant. The one robust exception is ABCP experience. As anticipated, banks with more experience sponsoring ABCP vehicles choose higher levels of exposure.

4.6.2 Triple Difference: Effect of FIN 46 on Systematic Risk Exposure

The results in the previous section confirm that FIN 46 decreased ABCP sponsorship at U.S. banks as compared to European banks. At face value, this seems to imply that the regulatory capital restrictions reduced bank risk. However, it is possible that the regulation also changed the composition of risk taking. To explore this possibility, I estimate the triple difference model

$$\begin{aligned} Exposure_{cit} = &\sum_{i=1}^{2}(\beta_i US \times Stage_i + a_i R \times Stage_i + \phi_i US \times Stage_i \times R) \\ &+ \alpha_i + \delta_t + b_1 R + b_2 R \times US + \sum \gamma X_{it} + \epsilon_{cit}, \end{aligned} \tag{4.2}$$

where R is an indicator variable equal to one if the conduit type is risky (defined in Section 4.5). The coefficients $\phi_{1,2}$ on the triple interaction of US, $Stage_i$, and R represent the difference-in-difference-in-difference estimate of the effect of the regulation on ABCP sponsorship. Unlike Equation 4.1, exposure here is measured at the conduit-type level (risky and safe). Thus there are two observations for each bank in my sample at each quarter.

Table 4.5 presents the results of the OLS estimate of Equation 4.2. To preserve space, I omit the bank-level characteristics from the table, but the regressions include all covariates specified in Table 4.4. Column 1 shows the results for the extensive margin of ABCP sponsorship. Similar to the previous results, the difference-in-difference estimate indicates that the first stage of FIN 46 reduced the probability of sponsoring ABCP vehicles by 8% (significant at the 1% level). The effect of the second stage of regulation is also negative (-13%), and in contrast to the previous

[15] The unconditional probability of ABCP sponsorship in this sample is about 25%.

results, significant at the 1% level. Of most interest, however, are the triple interaction terms. Both of these terms are positive and highly statistically significant, indicating that the capital regulation actually led banks to sponsor more of the riskiest type of ABCP vehicles relative to safer vehicles. The magnitudes are quite large; the probability of sponsoring risky ABCP vehicles increases by 11% (21%) during the first (second) stage of regulation.

Column 2 presents the results for the intensive margin of ABCP exposure. The difference-in-difference estimate of ABCP exposure is once again negative. The first stage of FIN 46 reduces the ABCP to equity ratio by 0.19 (or about 18%); this effect is significant at the 5% level. The estimated effect of the second stage of regulation on ABCP exposure is also negative, though only weakly significant. Similar to Column 1, the triple difference estimates are positive, though only the first stage interaction is statistically significant (at the 5% level). This estimate suggests that FIN 46 increased the exposure of risky ABCP to equity by 0.10, or about 9% relative to the sample mean.

Together, Columns 1 and 2 provide evidence that capital regulation concentrates systematic risk. Because these estimates include bank fixed effects, they imply that FIN 46 led banks that sponsored both types of conduits to shift the composition of their portfolio toward riskier vehicles. To examine this more directly, Columns 3 and 4 of Table 4.5 estimates the difference-in-difference model of Equation 4.1 using the ratio of risky ABCP assets to total ABCP assets as the dependent variable. Because this variable is only defined for banks that sponsor ABCP conduits, the sample size is much smaller. The estimated effect of the two stages of FIN 46 is positive, consistent with regulation leading banks to shift the composition of their ABCP portfolio toward higher systematic risks, but the estimate is statistically insignificant after including bank fixed effects. However, the effect is much larger and statistically significant when omitting bank-level fixed effects, but including country-level fixed effects. Column 4 shows that in this specification, the first stage of FIN 46 increases the proportion of risky ABCP at sponsoring banks by 17%, while the second stage increases it by 24%. These are large changes relative to the sample mean of about 40%.

The assets in safe and risky vehicles are treated similarly by the regulator, so all else equal a change in the required regulatory capital should have a similar effect on both types of vehicles. Table 4.5 shows that it does not; in fact, FIN 46 increases the relative exposure to systematic risk. Including bank fixed effects in this triple difference setting strengthens the causal interpretation of the results, because it rules out the possibility of the results being driven by bank specific factors such as governance or ownership structures.

However, many banks in my sample do not sponsor both types of conduits.[16] Does capital regulation also drive these banks to concentrate their risk exposure? The conduit-level bank fixed

[16]There are 40 banks that only sponsor a safe ABCP vehicle in at least one quarter and 27 banks that only sponsor a risky vehicle in at least one quarter.

Table 4.5

Effect of FIN 46 on Conduit Type. This table presents OLS regressions of the impact of various regulatory measures on bank decisions to sponsor ABCP at each quarter from Q1 2000 to Q2 2007. In columns 1, I use a linear probability model to estimate the probability that a bank sponsors at least one ABCP vehicle. In columns 2–4, I estimate the total ABCP exposure of the bank, where exposure is defined as total ABCP assets to bank equity or as the percent of total ABCP held through risky vehicles. *Stage 1* is a dummy variable that is equal to one for quarters after FIN 46 was first proposed in July 2002. *Stage 2* is a dummy variable that is equal to one for quarters after July 2004 when the final revision of FIN 46R was enforced. Both FIN 46 and FIN 46R required U.S. banks to hold additional regulatory capital against ABCP vehicle assets; the final revision considerably loosened these restrictions, but still imposed higher regulatory costs than existed both before the regulation and as compared to most European countries. Risky is an indicator variable for risky ABCP conduits. These regressions include the bank-level variables shown in Table 4.4, but these coefficients are suppressed to save space. Standard errors are adjusted for bank-level clustering; t-statistics are in parentheses. Regressions include a constant term which is suppressed to conserve space. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	Extensive Margin	Intensive Margin		
	(1) Has ABCP	(2) ABCP Exposure	(3) % Risky	(4) % Risky
ABCP Regulations:				
US Dummy × Stage 1	−0.08***	−0.19**	3.86	16.57***
	(−3.00)	(−2.48)	(1.03)	(2.88)
US Dummy × Stage 2	−0.13***	−0.32*	6.65	24.02***
	(−2.78)	(−1.71)	(1.14)	(2.81)
Risky	0.00	−0.02		
	(0.10)	(−0.88)		
US Dummy × Risky	−0.22**	−0.14*		
	(−2.54)	(−1.83)		
Stage 1 × Risky	−0.01	0.01		
	(−0.50)	(0.59)		
Stage 2 × Risky	−0.02	−0.07		
	(−0.94)	(−0.40)		
US Dummy × Stage 1 × Risky	0.11***	0.10**		
	(3.07)	(2.07)		
US Dummy × Stage 2 × Risky	0.21***	0.23		
	(3.22)	(1.19)		
Quarter Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	No
Country Fixed Effects	No	No	No	Yes
Observations	12,724	12,724	1,640	1,640
Number of Banks	236	236	77	77
Adjusted R^2	0.61	0.12	0.90	0.24

effect framework of Table 4.5 doesn't tell us anything about these banks. To gain insight into this broader sample, I re-estimate Equation 4.1 at the bank-level but separately for risky and safe ABCP sponsorship. Table 4.6 presents the results.

Column 1 of Table 4.6 reveals that FIN 46 had no effect on the probability of sponsoring a risky ABCP vehicle. The coefficients on the interaction terms are positive, but insignificant. In contrast, Column 2 shows that the regulation reduced the probability of sponsoring a safe ABCP vehicle by 8% during the first stage and by an additional 3% during the second stage; both estimates are significant at the 5% or better level. The difference in these estimates (i.e., the triple difference) suggests that relative to safe vehicles, the regulation increased the probability of risky sponsorship by 10% during the first stage and 17% during the second stage. This triple difference is statistically significant at the 5% level, economically meaningful, and similar in magnitude to the estimates in Table 4.5.

The results are qualitatively similar for ABCP exposure (shown in Columns 3 and 4). The regulation appears to have no effect on risky exposure, but a large negative effect on safe exposure. This effect is not precisely estimated, however, and the difference between risky and safe exposure is not statistically significant. While the evidence is weaker than that presented in Table 4.5, it suggests that capital regulation decreased bank involvement with safe ABCP conduits, but had little effect on risky vehicles.

4.6.2.1 Robustness

Finally, as a robustness test I examine the effect of the regulatory environment on bank ABCP sponsor decisions. To measure the regulatory environment, I use the regulatory indices used in Caprio et al. (2007) and described in Barth et al. (2001) and Barth et al. (2004). I then estimate probit and tobit regressions of the impact of these regulatory differences on the extensive and intensive margin of ABCP sponsorship. The variation in these regressions comes from cross-country heterogeneity. Because the indices do not vary over time, I cannot include either bank or country fixed effects. Consequently, to control for the macroeconomic environment, I include two variables. *Log GDP per Capita* is the natural logarithm of real GDP per capita and *GDP per Capita Growth Rate* is the year to year growth rate in real GDP per capita. To control for the financial market structure, I also include two variables. *Stock Market Cap. to GDP* is stock market capitalization to GDP, and *Private Bond Market to GDP* is private bond market capitalization to GDP. The results are shown in Table 4.7; to conserve space I omit the country-level and bank-level variables.

Looking across Columns 1 and 3 and Columns 2 and 4, the effect of the regulatory environment is consistent across both the extensive and intensive margins of sponsorship. More powerful regulatory authorities (*Official*) appear to reduce ABCP involvement, though the effect is not sig-

Table 4.6
Effect of FIN 46 split by Conduit Type. This table presents OLS regressions of the impact of various regulatory measures on bank decisions to sponsor ABCP at each quarter from Q1 2000 to Q2 2007. Risky is defined as SIV, Securities Arbitrage, or Hybrid vehicles. Safe is defined as multiseller or single-seller vehicles with trade receivables as the primary asset. In columns 1 and 2, I use a linear probability model to estimate the probability that a bank sponsors at least one ABCP vehicle of the designated type. In columns 3 and 4, I estimate the risky and safe ABCP exposure of the bank, where exposure is defined as total ABCP assets of the designated type of vehicle to bank equity. *Stage 1* is a dummy variable that is equal to one for quarters after FIN 46 was first proposed in July 2002. *Stage 2* is a dummy variable that is equal to one for quarters after July 2004 when the final revision of FIN 46R was enforced. Both FIN 46 and FIN 46R required U.S. banks to hold additional regulatory capital against ABCP vehicle assets; the final revision considerably loosened these restrictions, but still imposed higher regulatory costs than existed both before the regulation and as compared to most European countries. All bank-level variables are measured as of the year-end prior to the quarter of ABCP exposure. Standard errors are adjusted for bank-level clustering; t-statistics are in parentheses. Regressions include a constant term which is suppressed to conserve space. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	Extensive Margin		Intensive Margin	
	(1) Risky	(2) Safe	(3) Risky Exposure	(4) Safe Exposure
Log Assets	0.00	0.10^{**}	−0.15	0.32
	(0.09)	(2.49)	(−1.28)	(0.97)
Equity to Assets	−0.00	−0.00	−0.01	−0.12
	(−0.89)	(−0.16)	(−1.21)	(−0.99)
ROA	−0.00	0.02	−0.01	0.05
	(−0.48)	(0.93)	(−0.37)	(0.64)
Deposits to Assets	−0.00	0.00	−0.00	0.01
	(−0.06)	(0.04)	(−0.71)	(0.79)
Loans to Assets	0.00	0.00	−0.00	−0.04
	(1.00)	(0.09)	(−1.00)	(−0.97)
Securitization Underwriting	0.23^{*}	0.17	−0.14	−0.58
	(1.78)	(1.52)	(−0.87)	(−1.35)
ABCP experience			0.04^{**}	0.03
			(2.55)	(1.62)
ABCP Regulations:				
US Dummy × Stage 1	0.02	-0.08^{***}	−0.05	-0.23^{*}
	(0.96)	(−2.83)	(−1.25)	(−1.66)
US Dummy × Stage 2	0.06	-0.11^{**}	−0.12	−0.29
	(1.48)	(−2.44)	(−1.22)	(−1.41)
Quarter Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	6,362	6,362	6,362	6,362
Number of Banks	236	236	236	236
Adjusted R^2	0.78	0.86	0.48	0.19

Table 4.7
Effect of Regulatory Environment on ABCP Sponsorship. This table presents Probit and Tobit regressions of the impact of various regulatory measures on bank decisions to sponsor ABCP at each quarter from Q1 2000 to Q2 2007. In columns 1 and 2, I use a probit model to estimate the probability that a bank sponsors at least one ABCP vehicle. Risky is a dummy variable equal to one if the bank sponsors at least one SIV, Securities Arbitrage, or Hybrid vehicle. Safe is a dummy variable equal to one if the bank sponsors at least one multiseller or single-seller vehicle with trade receivables as the primary asset. I report average marginal effects. In columns 3 and 4, I use a tobit model to estimate the total ABCP exposure of the bank, where exposure is defined as ABCP to total assets and risky versus safe exposure is defined as in columns 1 and 2. I report the average partial effects on the censored value of exposure. Official is an index of the regulatory authority's power. Independence is an index of the level of independence of the regulatory authority from the government. Capital is an index that measures regulatory capital stringency and Restrict is an index that measures the restrictions on bank activities. These indexes are found in Caprio et al. (2007). Log Deposit Insurance is the natural logarithm of the 2003 dollar value of deposit insurance found in Asli et al. (2008). All time-varying variables are measured as of the year-end prior to ABCP exposure. Standard errors are adjusted for bank-year level clustering; t-statistics are in parentheses. Specifications in all columns control for year effects, as well as all bank-level and country-level variables, whose coefficient estimates are suppressed. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	Extensive Margin		Intensive Margin	
	(1) Risky	(2) Safe	(3) Risky Exposure	(4) Safe Exposure
General Regulatory Environment:				
Official	−0.01	−0.02*	−0.01	−0.06*
	(−0.98)	(−1.96)	(−0.75)	(−1.73)
Independence	0.07**	0.14***	0.07**	0.36*
	(2.47)	(3.27)	(1.98)	(1.79)
Capital	0.06**	0.05	0.05*	0.02
	(2.17)	(1.51)	(1.88)	(0.27)
Restrict	−0.03**	0.00	−0.03**	0.08
	(−2.38)	(0.19)	(−2.25)	(1.04)
Log Deposit Insurance	0.04	−0.03	0.04	−0.09
	(1.01)	(−0.71)	(1.03)	(−0.82)
Quarter Fixed Effects	Yes	Yes	Yes	Yes
Observations	6,362	6,362	6,362	6,362
Number of Banks	236	236	236	236
Psuedo R^2	0.35	0.52	0.23	0.22

nificant. More independent regulatory bodies (*Independence*) seem to increase the probability of sponsorship from between 7–14%; they also are associated with higher exposure. These effects exist across both safe and risky ABCP and are statistically significant. More restrictive (*Restrict*) financial systems are effective in the sense that they reduce both sponsorship and exposure to risky ABCP, but do not affect safe ABCP. Deposit insurance levels have no effect on ABCP sponsorship, at least in this sample.

The variable of most interest in Table 4.7 is *Capital* which measures the regulatory capital stringency. A one standard deviation increase in capital stringency increases the probability of sponsoring a risky ABCP vehicle by about 6%; this effect is significant at the 5% level. The effect of capital standards on sponsoring safe ABCP vehicles is indistinguishable from zero. Similarly, capital stringency increases risky ABCP exposure but does not affect safe ABCP exposure. These results are consistent with the evidence presented earlier. Faced with higher regulatory capital charges, banks concentrate their exposure on riskier assets.

4.6.3 Governance, Capital Regulation, and Risk Taking

Two types of shareholder-manager agency problems may exist in the context of systematic risk. First, managers may have incentives to take more risks than shareholders desire. Gorton and Rosen (1995) argue that low-skill managers may take systematic risk in an attempt to boost earnings and avoid revealing their type. Managers at complex banks will be more successful at this strategy, since it is harder for shareholders to distinguish the extent returns are due to systematic risk when banks are complex. Additionally, the compensation structure at financial firms may have encouraged managers to take tail risks. Along these lines, Dechow, Myers, and Shakespeare (2010) suggest that CEOs use securitization as an earnings management tool to increase their personal compensation. A second type of agency problem occurs when managers have incentives to take fewer risks than shareholders desire (Jensen and Meckling, 1976).

In this section, I explore the possibility that agency problems interact with regulation to influence bank risk taking. Because agency problems can theoretically lead to either more or less risk taking, better governed banks might be either more or less likely to concentrate systematic risk when required regulatory capital increases. I examine this relationship using a similar framework to Equation 4.2, but replacing the dummy variable for risky conduits with various measures of governance and ownership structures. Though each regression includes the full factorial interactions of *US Dummy*, *Stage 1(2)*, and the governance measure, as well as bank-level control variables, I report only the triple interaction term. The coefficient on this term represents the differential effect of FIN 46 on bank risk taking for better governed banks. Table 4.8 reports the results.

To capture the governance environment of the bank, I follow Aggarwal et al. (2010) to create four indexes from the CGQ index provided by RiskMetrics: *Compensation & Ownership*, *Audit*, *Anti-takeover*, and *Board*. Each index represents the proportion of pre-defined standards that the bank meets, and higher values of these indexes represent governance policies in that area that are more beneficial to shareholders. A negative coefficient on the interaction term of these indices suggests that better governed banks reduce their ABCP conduit risk more in response to capital regulation than worse governed banks.

In addition to these governance indices, I also examine the effect of the ownership structure on risk taking as a result of capital regulation. *Institutional Ownership* is the percent of shares owned by institutions and *Inside Ownership* is the percent of shares owned by insider individuals, as defined by Lionshares. If there are significant shareholder-manager agency conflicts, I expect the signs of *institutional ownership* and *inside ownership* to differ.

Table 4.8 reports the results. The sample size drops considerably, as ownership information and governance indices are only available for around 80 banks in my sample. Additionally, because the governance indices are not time-varying, I do not include firm fixed effects. Columns 1 and 2 reveal that governance and ownership structures have no effect on changes in the extensive margin of sponsorship due to FIN 46. Columns 3 and 4 examine the intensive margin. In Column 3, *Compensation & Ownership* has a negative, statistically significant effect on ABCP exposure throughout both stages of FIN 46. The coefficient of -0.01 implies that for a one standard deviation increase in the *Compensation & Ownership* index, the introduction of FIN 46 reduces ABCP exposure by an additional 0.22—approximately 20%. This implies that banks that pay their managers in more incentive compatible ways are particularly likely to reduce risk in response to capital regulation. None of the other governance indices has a systematic effect on the interaction of regulation and risk taking.

Institutional ownership does not appear to influence the effect of regulation on risk taking, but Column 4 reveals a positive effect on the interaction with inside ownership. The effect persists across both stages of FIN 46 and is statistically significant at the 1% level. The economic magnitude is also significant; a one standard deviation increase in inside ownership implies an increase of approximately 0.22 in the ratio of ABCP exposure, or approximately a 20% increase relative to the sample mean. Together, the results in Column 3 and Column 4 are a bit perplexing, since paying managers in a more incentive compatible way should have the same effect as giving insiders more equity in the bank. Part of this puzzle is resolved by looking more closely at the type of risk taking. I estimate the triple difference shown in Table 4.5 and interact that triple difference with the governance indices. These interactions are positive and statistically significant for *Compensation & Ownership* and *Anti-takeover* during Stage 1 and significant for *Board* during stage 2 (untabulated). The magnitudes are also sizeable, suggesting increases in ABCP exposure ranging from 0.11–0.37, relative to the conditional sample mean of 1.07. These estimates suggest that

Table 4.8
Effect of Governance on the Interaction of Regulation and ABCP Decisions. This table presents Tobit regressions of the impact of probability of default and bank ratings on bank decisions to sponsor ABCP at each quarter from Q1 2000 to Q2 2007. We use a tobit model to estimate the total ABCP exposure of the bank, where exposure is defined as ABCP to total assets. Risky ABCP represents SIV, Securities Arbitrage, or Hybrid vehicle ABCP, while safe ABCP is multiseller or single-seller vehicles with trade receivables as the primary asset. We report the average partial effects on the censored value of exposure. All time-varying variables are measured as of the year-end prior to ABCP exposure. Standard errors are adjusted for bank-year level clustering; t-statistics are in parentheses.. Specifications in all columns control for year effects, as well as all bank-level and country-level variables, whose coefficient estimates are suppressed. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	Extensive Margin		Intensive Margin	
	(1) Has ABCP	(2) Has ABCP	(3) Exposure	(4) Exposure
US × Stage 1 × Compensation	−0.00		−0.01**	
	(−0.02)		(−2.08)	
US × Stage 1 × Audit	−0.00		0.00	
	(−0.18)		(0.55)	
US × Stage 1 × Anti-takeover	−0.00		−0.00	
	(−0.05)		(−0.07)	
US × Stage 1 × Board	−0.01		−0.01	
	(−0.68)		(−1.38)	
US × Stage 2 × Compensation	−0.00		−0.01**	
	(−1.21)		(−1.99)	
US × Stage 2 × Audit	0.00		0.00	
	(0.20)		(0.42)	
US × Stage 2 × Anti-takeover	−0.00		−0.01	
	(−0.29)		(−0.76)	
US × Stage 2 × Board	−0.01		−0.01	
	(−0.51)		(−0.96)	
US × Stage 1 × Institutional Ownership		−0.00		−0.00
		(−0.83)		(−0.04)
US × Stage 1 × Inside Ownership		0.00		0.04***
		(0.51)		(2.67)
US × Stage 2 × Institutional Ownership		−0.00		0.00
		(−0.15)		(0.24)
US × Stage 2 × Inside Ownership		−0.01		0.04*
		(−0.70)		(1.73)
Quarter Fixed Effects	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes
Observations	2,552	2,336	2,552	2,336
Number of Banks	88	80	88	80
Adjusted R^2	0.59	0.57	0.44	0.35

while better governed banks reduce their overall ABCP exposure as a result of capital regulation to a greater extent than worse governed banks, they simultaneously shift the composition of their ABCP assets more heavily toward higher risk vehicles. Taken at face value, this means that well governed banks are particularly likely to concentrate systematic risk as result of complying with capital regulations.

4.6.4 Additional Tests

Riskier banks are plausibly more likely to attempt to concentrate systematic risk. I explore this using using similar difference-in-difference and triple difference settings interacted with the probability of default or with Moody's long-term credit rating. These bank risk measures have no effect on the tendency of banks to concentrate ABCP systematic risk, so the results are not tabulated.

4.7 Conclusion

Much of the focus following the financial crisis of 2007–09 has been on preventing banks from taking excessive risks by increasing required equity ratios. While appropriate, this approach fails to acknowledge that increased regulatory capital requirements might lead banks to change the composition of their asset portfolios. In the context of off balance sheet ABCP vehicles, I show that increased capital requirements imposed by FIN 46 and FIN 46R did indeed reduce banks' sponsorship of and exposure to ABCP vehicles. However, these regulatory changes also caused banks to increase their relative exposure to the riskiest types of vehicles. Since sponsoring ABCP vehicles represents a pure systematic risk to banks, increased regulatory capital requirements had the perverse effect of concentrating systematic risk within the financial system.

At the very least, this relative shift toward riskier ABCP vehicles means that total risk went down less than it first appears. Moreover, depending on the correlation structure of vehicle assets, this concentration of systematic risk likely increased the possibility of a systemic financial event. More broadly, this paper emphasizes that capital regulation is a dual-edged sword: while it has the possibility to make banks safer, it provides clear incentives for banks to increase the risk of their existing assets.

APPENDIX A

Technological Change, Job Tasks, and CEO Pay

A.1 Theoretical Model

In this paper, I define skill-biased technological change as the technological shock that began in the 1970s with the invention of microcomputer technology. I take the price of computer capital as exogenous to the firm. To understand the connection between falling computer prices and executive compensation, I use the task framework developed by Autor et al. (2003) combined with the CEO pay model laid out in Gabaix and Landier (2008). I assume that production consists of a combination of four types of tasks: routine cognitive, routine manual, nonroutine cognitive, and nonroutine manual. Tasks are defined as routine if they can be accomplished by an exhaustive set of programmable rules. Assembly line work is an example of a routine manual task, while balancing a firm's ledger is an example of a routine cognitive task. Nonroutine tasks, in contrast, do not have an exhaustive set of well-defined rules. An example of a nonroutine manual task is delivering packages, while an example of a nonroutine cognitive task is designing a new vaccine. The nature of routine tasks makes them particularly suited to be performed by a computer, while nonroutine tasks are not easily completed by current levels of computer technology.

This basic framework suggests that computers substitute for workers that perform routine cognitive and manual tasks, but complement workers that perform nonroutine cognitive tasks. For my purposes, I assume that nonroutine manual tasks and computers are neither strong substitutes nor compliments.[1] I combine this framework with the exogenous decline in the price of computers to create quasi-exogenous changes in executive compensation.

To understand the effect of falling computer prices on CEO pay, I introduce a simplified model.

[1] Current technology is not yet advanced enough to substitute for most nonroutine manual tasks, though for at least some tasks it is moving in that direction (e.g., computer-driven cars). Further, the extent to which computers can complement manual tasks is naturally limited by the physical limitations of human employees (e.g., even with a G.P.S. system, a single UPS driver can only deliver so many packages in a day.

I assume an aggregate production function of the form,

$$
\begin{aligned}
Q &= F(n, r, c)(1 + A \times T), \\
F(n, r, c) &= (r + c)^{1-\beta} n^{\beta}, \beta \in (0, 1), \\
A &= F(n, \Phi),
\end{aligned}
\tag{A.1}
$$

where T is the talent of the manager, A is the firm's organizational capital, r and n are routine and nonroutine labor inputs, and c is computer capital. All inputs are measured in efficiency units. $F(n, r, c)$ is a Cobb-Douglas production function as in Autor et al. (2003).[2]

The firm's organizational capital, A, quantifies the effect of CEO talent on production. Organizational capital is a function of nonroutine labor n, i.e. the skill level of the firm's workforce, and CEO specific traits, Φ, such as age, experience, and education. I assume that A is increasing in Φ; that is, I assume that some of the skills that make an effective CEO can be learned. I also assume that

$$
\frac{\partial A}{\partial n} > 0. \tag{A.2}
$$

Eq. A.2 is the key assumption of the model. In words, there are positive synergies between managers and nonroutine task employees. This can be viewed as a reduced form way of modeling the effect of CEO effort on employees; the assumption implies that CEO effort increases productivity (or reduces the cost of effort) of nonroutine employees more than routine employees. Since the validity of my empirical results rests on Eq. A.2, it is important to carefully consider the plausibility of this assumption. Why should CEO effort matter more to skilled employees? I argue that the role of a manager is fundamentally different when managing routine tasks versus nonroutine tasks. As a manager of routine tasks, the CEO is essentially the "colonel" of the firm giving orders and ensuring that these orders are followed through. Managing nonroutine tasks this way, though, is inefficient. Instead, a manager of nonroutine tasks acts as a "coach", leading his employees but allowing them freedom to find innovative solutions to the task at hand.

To illustrate the switch from "colonel" to "coach", consider an academic professor. When the professor hires an undergraduate research assistant, it tends to be to perform routine tasks such as data collection. The relationship between the professor and the undergraduate is generally command and control; rarely is there two-way feedback. When the professor works with a PhD student on research, though, the relationship is often much different. Acting as a coach or mentor,

[2] Eq. A.1 can be modified as in Gabaix and Landier (2008) to allow decreasing returns to scale to manager productivity, i.e. $Q = F(n, r, c) + F(n, r, c)^{\gamma} \times A \times T$. γ is what Lustig et al. (2011) refer to as the "span of control" parameter of the manager; if CEOs have less effect on big firms than small firms, then $\gamma < 1$. To simplify the exposition, I take $\gamma = 1$ which is consistent with the evidence in Gabaix and Landier (2008).

the professor and student collaborate together on a project. In the process, new and innovative ideas often emerge. While a professor might get a single research project out of the work of an undergraduate research assistant, a skilled mentor will likely become a co-author on many projects with a PhD student. In this sense, nonroutine labor leverages the ability of the manager.

Given Eq. A.2, the rest of the model is straightforward. Intuitively, a fall in the price of computer capital causes firms to substitute computers for routine task employees. Since computers compliment skilled workers, the firm demands and hires more skilled employees. This increase in employee skill makes it more beneficial to the firm to hire a more talented manager, and the firm is willing to pay more money to the CEO to convince her to take (or keep) the position.

Formally, as in Gabaix and Landier (2008), there are a continuum of firms and potential CEOs. CEO $m \in [0, N]$ has talent $T(m)$, where low m denotes a more talented manager: $T'(m) < 0$. Assuming perfect competition and normalizing the price of output to 1, the firm's optimization problem can be written as

$$\max_{m,n,r,c} F(n, r, c)(1 + A \times T(m)) - w_r r - \rho c - w_n n - w(m), \tag{A.3}$$

where ρ is the price of computer capital. It is clear from Eq. A.1 that that computer capital and routine labor are perfect substitutes.[3] Consequently, the wage for routine labor equals the price of computer capital. First order conditions for productive efficiency require that

$$\begin{aligned} w_r = \rho = \frac{\partial Q}{\partial r} &= (1-\beta)\left(\frac{c+r}{n}\right)^{-\beta}(1 + A \times T(m)), \\ w_n = \frac{\partial Q}{\partial n} &= \beta\left(\frac{c+r}{n}\right)^{1-\beta}(1 + A \times T(m)) + (c+r)^{1-\beta} n^{\beta} (\frac{\partial A}{\partial n} \times T), \\ w'(m) = \frac{\partial Q}{\partial m} &= AF(n, r, c)T'(m). \end{aligned} \tag{A.4}$$

Let $n \in [0, N]$ index the organizational capital scaled output of the firm, $S(n) = A \times F(n, r, c)$, with small n equal to the largest firms. Any efficient equilibrium involves positive assortative matching. In particular, if there are two firms with $S_1 > S_2$ and two CEOs with $T_1 > T_2$, then $S_1 \times T_1 + S_2 \times T_2 > S_1 \times T_2 + S_2 \times T_1$. As a result, the highest net surplus is achieved by assigning the most talented managers to the largest effective firms. Since there is associative matching,

$$w'(m) = S(m)T'(m). \tag{A.5}$$

In effect, a modified version of the result from Gabaix and Landier (2008) holds. The most tal-

[3] While this assumption simplifies the model, it is only necessary that computer capital is more substitutable for routine than for nonroutine tasks.

ented CEOs are hired by the largest effective firms, and consequently CEO pay is increasing in organizational capital scaled output of the firm.[4]

We are now in a position to examine the effect of an exogenous change in the price of computing power, ρ. Note that since computer capital and routine labor are perfect substitutes, $\rho = w_r$ and $\partial(\ln w_r)/\partial(\ln \rho) = 1$, and consequently

$$\frac{\partial \ln \left(\frac{c+r}{n}\right)}{\partial \ln \rho} = \frac{\partial \ln \frac{w_n}{w_r}}{\partial \ln \rho} = -\frac{1}{\beta} < 0. \tag{A.6}$$

The basic results of Autor et al. (2003) hold: a decline in the price of computing power ρ increases demand for routine input. Given that routine workers and computers are perfect substitutes, the firm will choose to meet this demand with an investment in computers.[5] Since routine tasks and nonroutine tasks are q-complements, the decrease in ρ also causes the demand for nonroutine tasks to rise. Consequently, marginal workers switch from supplying routine to nonroutine labor. This increases the nonroutine task level of the firm, n. The increase in skilled employees increases the effective size of the firm, since both output and manager effectiveness increase (i.e. $\partial S/\partial n > 0$). Now, Eq. A.5 shows that an increase in effective size, $S(m)$ induces the firm to hire a more talented CEO at a higher wage. Consequently, a decline in the price of computers leads firms to optimally increase CEO pay. However, the extent to which a given firm increases executive compensation depends on how much the skill-level of the firm's workforce rises.

This model provides a mechanism through which an exogenous fall in the price of computers leads firms to optimally increase executive compensation. Importantly, it also provides a reasonable explanation for why CEO pay began to increase in during the 1970s. It is plausible that the organizational capital function, A, is structured such that the effective size, $S(m)$, is constant or even decreasing from the mid 1930s until the 1970s. This would reconcile the single largest discrepancy of Gabaix and Landier (2008)—that firm size steadily rose throughout the 1900s but that CEO did not rise until the 1970s. I do not have data available to test this relationship prior to the 1980s, but I provide empirical evidence consistent with this model from 1984 to 2010.

[4] I borrow the term "effective size" from Gabaix and Landier (2008), who also note that in cases where the CEO impact varies across firms pay is increasing in a scaled version of size (see Proposition 25 in their paper). Gabaix and Landier are able to use extreme value theory to obtain a functional form for manager compensation by assuming that A and $F(n, r, c)$ are independent. In my model, A and $F(n, r, c)$ are clearly not independent; additionally, the functional form of A is unknown. As a result, I do not attempt to model the exact functional form for CEO pay.

[5] See Autor et al. (2003) for a formal proof of this statement.

A.2 Data

A.2.1 Task Complexity Measures

To proceed with the firm-level analysis, I develop a concordance to match the four-digit Compustat historical SIC code to the Census industry code. I then match the employee skill measures to each firm based on the firm's Census industry. The quality of this match rests both on the accuracy of the concordance and the extent to which the four-digit SIC code accurately reflects the business activities of the firm. If a firm does not update its SIC code over time, the historical SIC code might not accurately describe the current firm. Additionally, I use the primary SIC code of the firm, which typically represents the industry of the segment of the firm with highest sales. For large, diversified firms this might not be a very accurate representation of firm activity. Consequently, this matching process adds noise to the employee skill variables. However, there is no particular *ex ante* reason to believe that this noise biases the results in a particular direction.

I define routine tasks as the average of finger dexterity (routine manual) and set limits, tolerances, and standards (routine cognitive). Nonroutine tasks is the average of math aptitude (nonroutine analytical) and direction, control, and planning (nonroutine interpersonal).

To illustrate the tasks that these characteristics capture, consider the following examples. Textile production line workers have high finger dexterity; clerks have high set limits, tolerances, and standards; computer programmers have high math aptitude; and managers have high direction, control, and planning.

A.3 Variable Definitions

Table A.1

Variable Definitions. This appendix defines the variables used throughout the paper.

Variable	Definition
ln(CEO pay)	Natural log of the dollar value of salary, bonus, restricted stock granted, long-term incentive payouts, and the Black-Scholes value of stock-options granted. *Source*: Execucomp and Yermack (1995).
CEO Pay	Sum of salary, bonus, restricted stock granted, long-term incentive payouts, and the Black-Scholes value of stock-options granted (TDC1). *Source*: Execucomp and Yermack (1995).
Cash salary	Sum of the CEO's salary and bonus. *Source*: Execucomp and Yermack (1995).
Options	The Black-Scholes value of stock options granted during the fiscal year. *Source*: Execucomp and Yermack (1995).
ln(Revenue)	The natural logarithm of firm revenue (SALE). *Source*: Compustat.

Continued on next page

Table A.1 – *Continued*

Variable	Definition
Tobin's Q	The market value of equity (CSHO*PRCC_F) plus the book value of debt (DLTT+DLC+PSTKRV) minus the value of financial assets (CHE+RECT+ACO) divided by the total value of assets (AT) less financial assets. *Source*: Compustat.
Income to assets	Income (INCOME) divided by total assets (AT). *Source*: Compustat.
Shareholder return	Fiscal-year cumulative return on the stock. *Source*: CRSP.
Std. dev. return	The standard deviation of daily stock returns calculated over the fiscal year. *Source*: CRSP.
Beta	The firm's CAPM beta calculated using the previous year of stock returns. *Source*: CRSP.
CEO Tenure	The length of time, measured in years, that the executive has worked as CEO for her current firm. *Source*: Execucomp and Yermack (1995).
Age	The executive's age, measured in years. *Source*: Execucomp and Yermack (1995).
Original Routine Tasks	The industry intensity of routine task occupations as of 1973. *Source*: Current Population Survey Merged Outgoing Rotation Group (CPS) and US Department of Labor's Dictionary of Occupational Titles (DOT).
Nonroutine tasks	The industry intensity of nonroutine task occupations measured as of May of the given year. This variable is transformed into percentile values corresponding to its rank in the 1973 distribution of nonroutine tasks. *Source*: CPS and DOT.
Analytical skill	The industry intensity of nonroutine tasks that require analytical skill to complete, measured as of May of the given year. This variable is transformed into percentile values corresponding to its rank in the 1973 distribution of analytical skill. *Source*: CPS and US Department of Labor's O*NET (O*NET).
Interpersonal skill	The industry intensity of nonroutine tasks that require interpersonal skill to complete, measured as of May of the given year. This variable is transformed into percentile values corresponding to its rank in the 1973 distribution of interpersonal skill. *Source*: CPS and O*NET.
RAM price shock	The unexpected change in annual computer RAM prices. For each year, prices are hind-casted using Moore's Law and price data beginning in 1950. The price shock is measured as the regression error. *Source*:
Ownership	The percent of firm equity owned by the CEO. *Source*: Execucomp and Yermack (1995).
Board Size	The number of directors on the firm's Board. *Source*: Thomson Reuters ownership database.
Pct. Indep. Directors	The percentage of the firm's directors that are classified as independent (i.e., neither inside nor grey directors). *Source*: Thomson Reuters ownership database.
CEO is Chair	A dummy variable equal to one if the CEO is also the chair of the board of directors. *Source*: Execucomp and Yermack (1995).
Institutional Ownership	The percent of firm equity owned by institutional investors as reported on Form 13F. *Source*: Thomson Reuters ownership database.

A.4 Additional Results

This appendix presents additional results and robustness tests. Table A.2 further investigates the channel driving the relationship between employee tasks and CEO pay. Using the successor to the DOT, O*NET, I am able to create a more up to date measure of occupation tasks. The O*NET task classifications allow me to separate nonroutine task intensity into analytic and interpersonal tasks. I estimate the model shown in Column 3 of Table 2.7 separately for each of these two measures. If the relationship between CEO pay and employee tasks is driven by an increased importance in the role of the CEO, I expect this to effect to be concentrated in interpersonal tasks. These are the types of tasks that require teamwork; consequently, the corporate culture is likely to have a significant influence on the productivity of these workers. The CEO, along with the executive team, is responsible for the corporate culture.

The results in Table A.2 are consistent with this channel. The coefficient on analytical skill is statistically indistinguishable from zero, but the coefficient on interpersonal skill is statistically significant at the 1% level. The point estimate of 10.2 implies that the change in interpersonal skill from the 1980s to 2010 led to a 2.3 times increase in the level of total CEO pay. The magnitude of this result is consistent with my earlier estimates, and suggests that the increase in CEO pay is driven primarily by managing interpersonal tasks.

While the previous result is broadly consistent with optimal contracting, it is inconsistent with agency problems driving the increase in pay. Analytic tasks are likely to be much more difficult for outsiders to understand than interpersonal tasks; as a result, agency conflict theories would suggest that managers would extract more rents from firms that are analytic task intense. Table A.2 suggests that this is not the case.

Table A.3 presents additional evidence that agency problems do not drive the results in this paper. In this table, I re-estimate the difference in difference regression shown in Table 2.4 and the 2SLS regression shown in Table 2.7 controlling for firm exposure to offshoring and firm governance characteristics. The offshoring measure is created using the O*NET definitions of occupation tasks and is designed to reflect the potential for moving a given occupation offshore. This measure includes both routine tasks such as textile production and nonroutine tasks such as customer service. Higher values of offshoring reflect an industry that can more easily substitute foreign workers for domestic workers. As shown in Columns 1 and 2, controlling for offshoring does not alter the estimated effect of nonroutine task workers on CEO pay. Intriguingly, exposure to offshoring has a negative and statistically significant effect on executive compensation.

In columns 3 and 4, I include several measures designed to proxy for agency conflicts. CEO ownership reflects the extent to which shareholder and manager interests are aligned. Higher institutional ownership is likely correlated with better monitoring. A CEO who is also chairman has increased power which might exacerbate agency issues. The size and independence of the board reflect the ability of shareholders to monitor and discipline the CEO. While some of these proxies suggest that agency issues matter for executive pay (e.g., CEOs with high ownership are paid less and dual CEO/chairman are paid more), other proxies are inconsistent with agency theories (e.g., more independent boards and firms with higher institutional ownership pay more). In any case, including these proxies does not alter the statistical or economical significance of nonroutine employee skill.

Table A.2
CEO pay and types of nonroutine tasks. I estimate the model shown in Column 3 of Table 2.7 with different measures of employee tasks. *Analytical skill* measures nonroutine tasks that require analytical skill to complete, while *Interpersonal skill* measures nonroutine tasks that involve relationships between people. I instrument for each task measure using *Original Routine Tasks* and *RAM price shock*. Definitions for each of the variables can be found in Table A.1 in the Appendix. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	ln(Total pay)		
	(1)	(2)	(3)
Analytical skill	4.76**		0.05
	(2.30)		(0.02)
Interpersonal skill		2.37***	2.34
		(2.81)	(1.48)
ln(Revenue)	0.26***	0.23***	0.23***
	(10.47)	(11.71)	(10.01)
Tobin's Q	0.02***	0.02***	0.02***
	(4.31)	(4.51)	(4.51)
Income to assets	0.92***	0.74***	0.75***
	(5.61)	(6.33)	(5.01)
Shareholder return	0.15***	0.15***	0.15***
	(12.54)	(13.73)	(13.73)
Std. dev. return	1.00	0.81	0.81
	(1.11)	(0.94)	(0.93)
Beta	−0.00	0.01	0.01
	(−0.18)	(0.32)	(0.30)
CEO Tenure	−0.01	−0.01***	−0.01***
	(−1.61)	(−2.62)	(−2.59)
Age	0.00	0.01	0.01
	(0.59)	(1.38)	(1.33)
t	0.03***	0.04***	0.04***
	(2.59)	(4.61)	(4.29)
Original Routine Tasks	−0.16	0.00	
	(−1.22)	(0.02)	
Number of CEOs	5,216	5,216	5,216
Observations	30,856	30,856	30,856
First-stage F statistic	22.62	98.95	1.39
Hansen J statistic	0.00	0.00	0.00
p-value for J-statistic			

Table A.3

Robustness tests. This table shows that the estimated effect of employee tasks on CEO pay is robust to controlling for firm exposure to offshoring and firm governance characteristics. Columns 1 and 3 estimate the difference in difference regression shown in Table 2.4; Columns 2 and 4 estimate the 2SLS regression shown in Table 2.7. Although the coefficients are not shown to conserve space, the specifications include all of the control variables used in the previous tables. Definitions for each of the variables can be found in Table A.1 in the Appendix. Standard errors are clustered at the industry-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	ln(Total Pay)			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
Interaction	0.67***		0.71***	
	(5.27)		(4.95)	
Nonroutine tasks		14.14***		6.11***
		(4.45)		(3.35)
Offshorability	−0.20**	−4.86***		
	(−2.22)	(−4.48)		
Ownership			−1.99***	−0.19
			(−6.31)	(−0.56)
Institutional Ownership			0.10*	−0.18***
			(1.87)	(−3.19)
CEO is Chair			0.24***	0.02
			(13.36)	(0.67)
Board Size			−0.00	−0.01
			(−1.11)	(−1.30)
Pct. Indep. Directors			0.13**	−0.04
			(2.17)	(−0.46)
Industry FE	Yes	No	Yes	No
5-year FE	No	Yes	No	Yes
Number of CEOs	6,090	5,216	4,714	3,755
Observations	31,730	30,856	19,904	18,945
R^2	0.46	−1.10	0.52	−0.11

APPENDIX B

Revenge of the Steamroller: ABCP as a Window on Risk Choices

B.1 Variable Definitions

Table B.1
Variable Definitions. This appendix defines the variables used throughout the paper.

Variable	Definition
Sponsor	Dummy variable equal to 1 if the bank sponsored at least one SIV, securities arbitrage, or hybrid ABCP vehicle during a given year. Source: Moody's quarterly program index.
Return on Assets	Net income divided by average assets. Source: Bankscope.
Dummy Intermediate Tercile	Dummy variable equal to 1 if the total assets of the bank are in the middle tercile of the sample for a given year. Source: Bankscope.
Dummy Top Tercile	Dummy variable equal to 1 if the total assets of the bank are in the top tercile of the sample for a given year. Source: Bankscope.
Equity to Assets	Ratio of book equity to total assets. Source: Bankscope.
Loans to Assets	Ratio of net loans to total assets. Source: Bankscope.
Deposits to Assets	Ratio of deposits to total assets. Source: Bankscope.
Non-Int. Op. Income to Assets	Ratio of non-interest operating income to total assets. Source: Bankscope.
High Yield Underwriting	Logistic transformation of the total face value (US$) of high yield debt underwritten by the bank during 2006, such that the resulting value on the (0,1) interval is 0.5 where the amount is $12.5 billion. Source: Bloomberg, Dealogic DCM Analytics.

Continued on next page

Table B.1 – *Continued*

Variable	Definition
Securitization Underwriting	Logistic transformation of the total face value (US$) of asset-backed securities and mortgage backed securities underwritten by the bank during 2006, such that the resulting value on the (0,1) interval is 0.5 where the amount is $12.5 billion. Source: Bloomberg, Dealogic DCM Analytics.
Dummy Landesbank	Dummy variable equal to 1 if the bank is a German Landesbank. Source: Bankscope.
Dummy US	Dummy variable equal to 1 if the bank is domiciled in the United States. Source: Bankscope.
Government Support	For each bank, Moodys long-term foreign currency deposit rating minus Moodys bank financial strength rating (see Brandão-Marques et al. (2013)). Source: Moodys Investors Services.
Compensation Index	The percentage of the bank's compensation and ownership attributes that satisfy thresholds specified by RiskMetrics Group's Corporate Governance Quotient, calculated as in Aggarwal et al. (2010). Source: RiskMetrics.
Inside Ownership	Percent of shares owned by individual insiders. Source: FactSet/Lionshares.
Board Independence	Share of independent directors on the bank's board of directors. Source: BoardEx.
Institutional Ownership	Percent of shares owned by institutions. Source: FactSet/Lionshares.
Widely Held	Dummy variable equal to 1 if the bank does not have a single owner with voting shares larger than 10 percent. Source: FactSet/Lionshares, Bankscope, Annual Reports.
Stock Market Cap.	Ratio of stock market capitalization to GDP. Source: Worldscope, Bank for International Settlements.
Private Bond Market Cap.	Ratio of private bond market capitalization to GDP. Source: Worldscope, Bank for International Settlements.
Real GDP Growth	Annual average percent change of real GDP. Source: OECD, Bureau of Economic Analysis.

B.2 Regulatory treatment

This appendix describes in more detail the regulatory capital requirements associated with credit arbitrage vehicles and rough estimates of the net profits required to cover the cost of incremental required equity capital. Our description is broadly similar to that in Acharya et al. (2013) except that they do not discuss capital requirements associated with credit enhancements, nor do they produce examples of costs of capital relative to profitability.

Capital requirements differed for SIVs and SAVs. Focusing first on SAVs, European banks incurred no incremental capital requirements by sponsoring a SAV through mid-2007. U.S. banks incurred regulatory risk-based capital requirements against credit enhancements beginning in 2002 and liquidity backstop lines of credits beginning in 2004, and beginning in 2004 their vehicles incurred costs associated with expected loss notes (ELNs) issued to avoid consolidation of vehicle assets onto the sponsor's balance sheet.

In a November 2001 rule that took effect as of January 1, 2002, U.S. bank regulators specified risk-based capital requirements for "direct credit substitutes," which included credit enhancements provided to ABCP vehicles (see Federal Register volume 66 page 59614).[1] In the Basel 1 scheme in effect at the time, requirements were specified in terms of "risk weights" with the risk weight for a standard corporate loan or bond being 100 percent. Risk weights were then multiplied by a standard requirement to obtain dollars of capital per dollar of assets. Though 8 percent is the most widely reported standard requirement (8 cents of "total" capital per dollar of 100% risk weight assets), a more relevant U.S. requirement was 6 percent of Tier 1 capital to be "well capitalized" (because Tier 1 more closely approximated common equity). Risk weights for credit-arb vehicle credit enhancements were applied to the face amount of the enhancement, which was commonly 10 percent of vehicle assets. The risk weight depended on the agency ratings of assets in the vehicle, with a 20 percent weight for AAA and AA assets and 50 percent weight for A rated assets. For example, for a vehicle with 80 percent of assets rated AAA or AA and the remainder A, the risk weight for the credit enhancement would be 0.8*0.2 + (1-0.8)*0.5 or 26 percent. Applying the weight to the credit enhancement size (10 percent of vehicle assets) and then multiplying by the standard 6 percent requirement yields required capital of 0.26*0.10*0.06=15.6 basis points of Tier 1 capital per dollar of vehicle assets. Such capital is costly in that it has a higher required return than debt and it must be held in addition to the ABCP that comprises all of SAV liabilities (since the Tier 1 capital is not on the books of the SAV). Using a back-of-the-envelope 15 percent required return on equity capital, 0.00156*.15 = 2.3 basis points of vehicle net profit is needed to compensate the sponsor for this regulatory requirement.

In a July 2004 rule to take effect September 2004, U.S. bank regulators specified risk-based capital regulatory treatment of ABCP liquidity facilities (see Federal Register volume 69 page 44908; see also Interagency Guidance issued August 4, 2005). This rule applied a 10 percent "credit conversion factor" (CCF) to "eligible" liquidity backstops and also specified that the face amount of any credit enhancement provided by the same bank could be subtracted from the face amount of the liquidity backstop before applying the CCF. The CCF is multiplied by the risk weight, which is rating-sensitive in a manner similar to that of credit enhancements. Continuing the example above, the risk weight for a typical liquidity backstop that equals 100 percent of the amount of vehicle assets, provided by the same bank that provides a 10 percent credit enhancement, would be

[1]The substance of the rule, which required capital beyond that in Basel 1, was not adopted by the Basel Committee on Banking Supervision (BCBS) and thus was not part of the Basel 1 agreement: U.S. regulators adopted it unilaterally, though it foreshadowed treatment in Basel 2 (which, as a practical matter, was not implemented by any relevant banks until around the time the crisis started).

CCF*(100-10 percent)*BlendedRiskWeight or 0.10*0.90*0.26=2.34 percent. Again multiplying by a 6 percent standard requirement and a 15 percent cost of Tier 1 capital yields 2.1 basis point of vehicle net profit needed to compensate the sponsor for the requirement, about the same as the cost for the credit enhancement.

In FIN 46R, the FASB required consolidation of ABCP vehicle assets onto sponsor balance sheets beginning in 2004. The U.S. leverage ratio capital requirements would have applied, requiring Tier 1 capital equal to 5 percent of vehicle assets. Although the same rule that specified risk-based capital requirements for liquidity backstops permitted banks not to consolidate vehicle assets when computing risk-based capital requirements, there was no exemption for calculation of leverage ratio capital requirements. However, as described in Bens and Monahan (2008), the FASB (and bank regulators) allowed deconsolidation in cases where a vehicle issued "expected loss notes" (ELNs) to third parties that would bear credit default losses on vehicle assets up to the estimated long-run average probability of default times loss given default on vehicle assets. Such deconsolidation permitted a bank to escape the leverage ratio requirement. Because the risk of principal losses on such notes was very high, required coupon payments were also high, reportedly approximately the same as the face amount of notes over the life of the notes. Thus, though the face amount of such notes was very small because of the low default risk on assets rated A or better, the cost was material. For example, PNC Bank's September 30, 2009 10Q states that its ELNs had a face value of about 14 basis points of vehicle assets. Assuming a five year life of the ELNs implies an annual cost of about 3 basis points.

Taken together, costs of risk-based capital requirements and of avoiding leverage ratio capital requirements for SAVs were probably around 7 basis points before the crisis (2 + 2 + 3). Of these, 3 basis points were expenses within the vehicle, and thus already netted from the Mellon Bank and Deutsche Bank examples which showed revenue before operating expenses of around 10 basis points. Though small in absolute terms, the remaining 4 basis points is substantial relative to 10 basis points and thus arguably represented a material disincentive for U.S. banks to sponsor SAVs. Two possible reasons why a few U.S. banks did so anyway are the agency problems discussed in the text and the possibility that some banks had "excess" Tier 1 capital and thus were not capital-constrained. Of course, the latter leaves open the question of why such banks did not return capital to shareholders.

Though the aforementioned estimates are back-of-the-envelope, the relevant features of regulation are known. Roughness in the estimates is from assumptions about the distribution of portfolio assets across rating categories and about banks' cost of capital. Though changes in such assumptions would change the estimates, moderate changes would not change the main points that regulatory capital costs of sponsorship were small in absolute magnitude but material relative to vehicle profitability.

Banks in all jurisdictions usually incurred no or almost no regulatory capital requirements from sponsoring SIVs because they usually did not own any of the SIV's subordinated notes and, if they provided liquidity backstop lines of credits, such lines totaled a modest fraction of total vehicle assets. SIVs were also structured to avoid consolidation.

B.3 Vehicle Size

This appendix examines the intensive margin of vehicle sponsorship. We report Tobit regressions of credit arbitrage vehicle assets as a fraction of the sponsor's total assets and of the sponsor's total equity. Table B.2 estimates vehicle size as a function of the predictors shown in Table 3.7. The main results for the extensive margin of sponsorship continue to hold for the intensive margin: more highly levered banks and German Landesbanks operated larger vehicles. These effects are both economically and statistically significant. For example, a one standard deviation decrease in the equity to assets ratio leads the risky CP to assets ratio to increase by about 6, which represents a 65% increase relative to the mean ratio of 9.5. The implied magnitude is similar when we scale by equity instead of assets. Note that in contrast to the results in Table 3.7 for sponsorship, securitization underwriting does not predict vehicle size.

Table B.3 uses the government support measure from Table 3.9 to predict vehicle size. The results are quantitatively and qualitatively similar to Table 3.9; increased levels of government support lead banks to sponsor larger vehicles.

Finally, Table B.4 examines if and how agency problems affect vehicle size using the measures shown in Table 3.12. To conserve space, we only tabulate the results using the risky CP to assets measure; the results are similar if we scale by equity. Finally, Table B.4 presents some evidence that agency problems contributed to vehicle size; however, the results are much weaker than the corresponding analysis for sponsorship. Since vehicle size was modest relative to bank assets in most cases, we view the existence of sponsored vehicles as the main signal that a bank had taken systematic bad-tail risk; consequently, our analysis focuses on the extensive margin of sponsorship.

Table B.2
Full-sample results from tobit models of sponsorship. This table shows the results from estimating a tobit regression in which the dependent variable is a measure of vehicle size that is censured at zero. We measure size in two ways: the total value of risky CP assets (sponsored assets at SIV, securities arbitrage, or hybrid ABCP vehicles) divided by total sponsor assets and divided by total equity.[2] We examine vehicle size as of June 2007. Most independent variables are measured as of December of the previous year. The values in the table represent average partial effects on the censured value of vehicle size. Definitions for the independent variables are found in Appendix B Section B.1. Standard errors (shown in parenthesis) are clustered at the country level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	(1) Risky CP Assets to Total Assets	(2) Risky CP Assets to Equity
Return on Assets	−0.673	−0.154
	(0.497)	(0.158)
Dummy Intermediate Tercile	0.295	0.089
	(0.632)	(0.136)
Dummy Top Tercile	1.226**	0.371*
	(0.640)	(0.213)
Equity to Assets	−0.185**	−0.042**
	(0.064)	(0.016)
Loans to Assets	−0.006	−0.000
	(0.013)	(0.003)
Deposits to Assets	−0.002	−0.002
	(0.007)	(0.003)
Non-Interest Operating Income to Assets	0.331***	0.078*
	(0.109)	(0.046)
High Yield Underwriting	−0.399	−0.102
	(0.490)	(0.105)
Securitization Underwriting	0.316	0.045
	(0.404)	(0.087)
Dummy Landesbank	1.795***	0.360***
	(0.234)	(0.046)
Dummy US	1.183	0.302
	(0.941)	(0.251)
Stock Market Cap.	0.003	0.000
	(0.004)	(0.001)
Private Bond Market Cap.	0.002	−0.000
	(0.010)	(0.002)
Real GDP Growth	−0.300	−0.055
	(0.335)	(0.083)
Observations	144	140
Countries	17	17
Psuedo R^2	0.119	0.166

Table B.3
Vehicle size and government support. This table shows the results from estimating a tobit regression in which the dependent variable is a measure of vehicle size that is censured at zero. We measure size in two ways: the total value of risky CP assets (sponsored assets at SIV, securities arbitrage, or hybrid ABCP vehicles) divided by total sponsor assets and divided by total equity. We examine vehicle size as of June 2007. Most independent variables are measured as of December of the previous year. The values in the table represent average partial effects on the censured value of vehicle size. *Government Support* is a ratings-based measure from Brandão-Marques et al. (2013) that captures the bank-specific value of explicit and implicit government support. Definitions for the other independent variables are found in Appendix B Section B.1. Standard errors (shown in parenthesis) are clustered at the country level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

	(1) Risky CP Assets to Total Assets	(2) Risky CP Assets to Equity
Government Support	0.215***	0.080***
	(0.033)	(0.014)
Return on Assets	−1.525	−0.284
	(0.940)	(0.273)
Dummy Intermediate Tercile	1.039	0.216
	(0.907)	(0.195)
Dummy Top Tercile	1.428*	0.474*
	(0.883)	(0.281)
Equity to Assets	−0.174	−0.037
	(0.180)	(0.041)
Loans to Assets	−0.027*	−0.001
	(0.018)	(0.004)
Deposits to Assets	−0.000	−0.005
	(0.019)	(0.008)
Non-Interest Operating Income to Assets	0.379	0.105
	(0.293)	(0.076)
High Yield Underwriting	−1.121	−0.258
	(0.861)	(0.170)
Securitization Underwriting	0.410	0.085
	(0.421)	(0.134)
Dummy US	1.584	0.390
	(1.432)	(0.355)
Stock Market Cap.	0.003	0.000
	(0.005)	(0.001)
Private Bond Market Cap.	0.006	0.000
	(0.012)	(0.003)
Real GDP Growth	−0.571	−0.102
	(0.417)	(0.109)
Observations	91	87
Countries	16	16
Psuedo R^2	0.117	0.164

Table B.4
Vehicle size and agency problems. This table shows the results from estimating a tobit regression in which the dependent variable is a measure of vehicle size that is censured at zero. This table measures vehicle size as the total value of risky CP assets (sponsored assets at SIV, securities arbitrage, or hybrid ABCP vehicles) divided by total sponsor assets. We examine vehicle size as of June 2007. Most independent variables are measured as of December of the previous year. The values in the table represent average partial effects on the censured value of vehicle size. Higher values of *Compensation Index* represent better firm-level compensation practices as defined by RiskMetrics Corporate Governance Quotient. *Insider ownership* is the percentage stake held by individual insiders. *Board Independence* is the share of independent directors on the bank's board of directors, *Institutional Ownership* is the percent of shares owned by institutions, and *Widely Held* is a dummy variable equal to 1 if the bank does not have a single owner with voting shares larger than 10%. While not shown to conserve space, these estimations include the same control variables used in Table B.2. Note that with the exception of column (5), this estimation sample does not include Landesbanks. Standard errors (shown in parenthesis) are clustered at the country level. Standard errors (shown in parenthesis) are clustered at the country level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Dependent Variable	Risky ABCP Assets to Total Assets					
	(1)	(2)	(3)	(4)	(5)	(6)
Compensation Index	−0.027***					−0.016*
	(0.008)					(0.009)
Board Independence		−1.192*				0.235
		(0.642)				(1.123)
Insider Ownership			−2.581			−2.270
			(3.283)			(4.608)
Institutional Ownership				1.748		1.513
				(1.330)		(1.422)
Widely Held					0.281	−0.131
					(0.422)	(0.472)
Observations	89	96	84	85	144	74
Countries	17	17	17	17	17	17
Psuedo R^2	0.120	0.100	0.119	0.118	0.101	0.122

BIBLIOGRAPHY

Acemoglu, Daron, and David Autor, 2010, Skills, tasks and technologies: Implications for employment and earnings, in Orley C. Ashenfelter, and David Card, eds., *Handbook of Labor Economics*, volume 4 (Elsevier).

Acharya, Viral V, Ramin P Baghai, and Krishnamurthy V Subramanian, 2012, Wrongful discharge laws and innovation, Working Paper 18516, National Bureau of Economic Research.

Acharya, Viral V, Philipp Schnabl, and Gustavo Suarez, 2013, Securitization without risk transfer, *Journal of Financial Economics* 107, 515–536.

Acharya, V.V., T.F. Cooley, M.P. Richardson, and I. Walter, 2010, Manufacturing tail risk: A perspective on the financial crisis of 2007-09, *Foundations and Trends in Finance* 4.

Admati, Anat R, Peter M DeMarzo, Martin Hellwig, and Paul Pfleiderer, 2011, Fallacies, irrelevant facts, and myths in the discussion of capital regulation: Why bank equity is not expensive, Research Paper 2065, Stanford Graduate School of Business.

Aggarwal, Reena, Isil Erel, René Stulz, and Rohan Williamson, 2010, Differences in governance practices between us and foreign firms: Measurement, causes, and consequences, *Review of Financial Studies* 23, 3131–3169.

Agrawal, Ashwini K, and David A Matsa, 2012, Labor unemployment risk and corporate financing decisions, *Journal of Financial Economics* .

Ai, Chunrong, and Edward C Norton, 2003, Interaction terms in logit and probit models, *Economics letters* 80, 123–129.

Arteta, C., M. Carey, R. Correa, and J. Kotter, 2013, Revenge of the Steamroller: ABCP as a Window on Risk Choices, Unpublished working paper, Federal Reserve Board.

Asli, D.K., E.J. Kane, and L. Laeven, 2008, Determinants of deposit-insurance adoption and design, *Journal of Financial Intermediation* 17, 407–438.

Autor, D.H., F. Levy, and R.J. Murnane, 2003, The Skill Content of Recent Technological Change: An empirical exploration, *Quarterly Journal of Economics* 118, 1279–1333.

Bannier, Christina E, Eberhard Feess, and Natalie Packham, 2013, Competition, bonuses, and risk-taking in the banking industry, *Review of Finance* 17, 653–690.

Barth, J.R., G. Caprio, et al., 2004, Bank regulation and supervision: what works best?, *Journal of Financial Intermediation* 13, 205–248.

Barth, J.R., G. Caprio Jr, and R. Levine, 2001, The regulation and supervision of banks around the world-a new database, *Policy Research Working Paper Series* .

Bebchuk, L., and Y. Grinstein, 2005, The growth of executive pay, *Oxford review of economic policy* 21, 283.

Bebchuk, L.A., and J.M. Fried, 2003, Executive compensation as an agency problem, *Journal of Economic Perspectives* 71–92.

Bebchuk, L.A., J.M. Fried, and D.I. Walker, 2002, Managerial power and rent extraction in the design of executive compensation, *University of Chicago Law Review* 69, 751–846.

Bebchuk, Lucian, Yaniv Grinstein, and Urs Peyer, 2010, Lucky CEOs and Lucky Directors, *The Journal of Finance, forthcoming* .

Becker, Gary S, 1962, Investment in human capital: A theoretical analysis, *The Journal of Political Economy* 70, 9–49.

Beltratti, Andrea, and René M Stulz, 2012, The credit crisis around the globe: Why did some banks perform better?, *Journal of Financial Economics* 105, 1–17.

Bens, Daniel A, and Steven J Monahan, 2008, Altering investment decisions to manage financial reporting outcomes: Asset-backed commercial paper conduits and fin 46, *Journal of Accounting Research* 46, 1017–1055.

Berger, Allen, Björn Imbierowicz, and Christian Rauch, 2012, The role of corporate governance in bank failures during the recent financial crisis, Unpublished working paper, University of South Carolina.

Berk, Jonathan B, Richard Stanton, and Josef Zechner, 2010, Human capital, bankruptcy, and capital structure, *The Journal of Finance* 65, 891–926.

Bertrand, M., E. Duflo, and S. Mullainathan, 2004, How much should we trust differences-in-differences estimates?, *Quarterly Journal of Economics* 119, 249–275.

Bertrand, M., and S. Mullainathan, 2001, Are CEOs rewarded for luck? The ones without principals are, *Quarterly Journal of Economics* 901–932.

Black, Sandra E, and Lisa M Lynch, 2001, How to compete: the impact of workplace practices and information technology on productivity, *Review of Economics and statistics* 83, 434–445.

Bloom, Nicholas, Benn Eifert, Aprajit Mahajan, David McKenzie, and John Roberts, 2013, Does management matter? evidence from india, *The Quarterly Journal of Economics* 128, 1–51.

Bloom, Nick, and John Van Reenen, 2007, Measuring and explaining management practices across firms and countries, *Quarterly Journal of Economics* .

Blum, Jürg, 1999, Do capital adequacy requirements reduce risks in banking?, *Journal of Banking & Finance* 23, 755–771.

Bolton, Patrick, Markus K Brunnermeier, and Laura Veldkamp, 2013, Leadership, coordination, and corporate culture, *The Review of Economic Studies* 80, 512–537.

Brandão-Marques, Luís, Ricardo Correa, and Horacio Sapriza, 2013, International evidence on government support and risk taking in the banking sector, International Finance Discussion Papers 1086, Board of Governors of the Federal Reserve System.

Brown, Jennifer, and David A Matsa, 2012, Boarding a sinking ship? an investigation of job applications to distressed firms, Working Paper 18208, National Bureau of Economic Research.

Brunnermeier, Markus K, 2009, Deciphering the liquidity and credit crunch 2007–2008, *Journal of Economic Perspectives* 23, 77–100.

Buser, Stephen A, Andrew H Chen, and Edward J Kane, 1981, Federal deposit insurance, regulatory policy, and optimal bank capital, *The Journal of Finance* 36, 51–60.

Calomiris, C.W., 2009, Financial innovation, regulation, and reform, *Cato Journal* 29.

Caprio, G., L. Laeven, and R. Levine, 2007, Governance and bank valuation, *Journal of Financial Intermediation* 16, 584–617.

Carbo-Valverde, Santiago, Edward J Kane, and Francisco Rodriguez-Fernandez, 2013, Safety-net benefits conferred on difficult-to-fail-and-unwind banks in the us and eu before and during the great recession, *Journal of Banking & Finance* 37, 1845–1859.

Chan, Nicholas, Mila Getmansky, Shane M Haas, and Andrew W Lo, 2007, Systemic risk and hedge funds, in Mark Carey, and René M Stulz, eds., *The Risks of Financial Institutions*, 235–338 (University of Chicago Press).

Chen, Hui, Scott Joslin, and Ngoc-Khanh Tran, 2012, Rare disasters and risk sharing with heterogeneous beliefs, *Review of Financial Studies* 25, 2189–2224.

Ciccone, Antonio, and Elias Papaioannou, 2009, Human capital, the structure of production, and growth, *The Review of Economics and Statistics* 91, 66–82.

Coval, Joshua, Jakub Jurek, and Erik Stafford, 2009, The economics of structured finance, *The Journal of Economic Perspectives* 3–26.

Covitz, Daniel, Nellie Liang, and Gustavo Suarez, 2013, The evolution of a financial crisis: Collapse of the asset-backed commercial paper market, *The Journal of Finance* 68, 815–848.

Cremers, Martijn, and Yaniv Grinstein, 2011, The Market for CEO Talent: Implications for CEO Compensation, Unpublished working paper, Yale School of Management.

Crotty, J., 2009, Structural causes of the global financial crisis: a critical assessment of the 'new financial architecture', *Cambridge Journal of Economics* 33, 563.

Custódio, C., M. Ferreira, and P. Matos, 2010, Generalists versus specialists: Managerial skills and CEO pay, Unpublished working paper, Arizona State University.

De Jonghe, Olivier, 2010, Back to the basics in banking? a micro-analysis of banking system stability, *Journal of financial intermediation* 19, 387–417.

Dechow, P.M., L.A. Myers, and C. Shakespeare, 2010, Fair value accounting and gains from asset securitizations: A convenient earnings management tool with compensation side-benefits, *Journal of Accounting and Economics* 49, 2–25.

Demirguc-Kunt, Asli, Enrica Detragiache, and Ouarda Merrouche, 2013, Bank capital: Lessons from the financial crisis, *Journal of Money, Credit and Banking* 45, 1147–1164.

DeYoung, Robert, and Gökhan Torna, 2013, Nontraditional banking activities and bank failures during the financial crisis, *Journal of Financial Intermediation* 22, 397–421.

Diamond, Douglas W, and Philip H Dybvig, 1983, Bank runs, deposit insurance, and liquidity, *The Journal of Political Economy* 91, 401–419.

Diamond, D.W., and R.G. Rajan, 2009, The credit crisis: Conjectures about causes and remedies, *American Economic Review* 99, 606–610.

Dinç, I Serdar, 2005, Politicians and banks: Political influences on government-owned banks in emerging markets, *Journal of Financial Economics* 77, 453–479.

Duarte, Jefferson, Francis A Longstaff, and Fan Yu, 2007, Risk and return in fixed-income arbitrage: Nickels in front of a steamroller?, *Review of Financial Studies* 20, 769–811.

Edmans, Alex, 2011, Does the stock market fully value intangibles? employee satisfaction and equity prices, *Journal of Financial Economics* 101, 621–640.

Edmans, Alex, Xavier Gabaix, and Augustin Landier, 2009, A multiplicative model of optimal ceo incentives in market equilibrium, *Review of Financial Studies* 22, 4881–4917.

Edmans, Alex, Itay Goldstein, and John Y Zhu, 2011, Contracting with synergies, Working Paper 17606, National Bureau of Economic Research.

Erkens, David H, Mingyi Hung, and Pedro Matos, 2012, Corporate governance in the 2007–2008 financial crisis: Evidence from financial institutions worldwide, *Journal of Corporate Finance* 18, 389–411.

Fahlenbrach, Rüdiger, and René M Stulz, 2011, Bank CEO incentives and the credit crisis, *Journal of Financial Economics* 99, 11–26.

Falato, Antonio, Dan Li, and Todd Milbourn, 2011, To Each According to His Ability? CEO Pay and the Market for CEOs, Unpublished working paper, Federal Reserve Board.

Fee, C Edward, Charles J Hadlock, and Joshua R Pierce, 2013, Managers with and without style: Evidence using exogenous variation, *Review of Financial Studies* 26, 567–601.

Finkelstein, Sydney, Donald C. Hambrick, and Albert A. Cannella, 2008, *Strategic leadership: theory and research on executives, top management teams, and boards* (Oxford University Press).

Frydman, C., and R.E. Saks, 2010, Executive compensation: A new view from a long-term perspective, 1936–2005, *Review of Financial Studies* 23, 2099–2138.

Frydman, Carola, 2005, Rising Through the Ranks. The Evolution of the Market for Corporate Executives, 1936-2003, Unpublished working paper, MIT.

Fung, William, and David A Hsieh, 2001, The risk in hedge fund strategies: Theory and evidence from trend followers, *Review of Financial studies* 14, 313–341.

Furlong, Frederick T, and Michael C Keeley, 1989, Capital regulation and bank risk-taking: A note, *Journal of Banking & Finance* 13, 883–891.

Gabaix, X., and A. Landier, 2008, Why has CEO pay increased so much?, *The Quarterly Journal of Economics* 123, 49–100.

Garicano, Luis, and Thomas N Hubbard, 2007, The return to knowledge hierarchies, NBER Working Paper 12815, National Bureau of Economic Research.

Gatev, Evan, Til Schuermann, and Philip E Strahan, 2009, Managing bank liquidity risk: How deposit-loan synergies vary with market conditions, *Review of Financial studies* 22, 995–1020.

Gennaioli, Nicola, Rafael La Porta, Florencio Lopez-de Silanes, and Andrei Shleifer, 2013, Human capital and regional development, *The Quarterly Journal of Economics* 128, 105–164.

Gennotte, Gerard, and David Pyle, 1991, Capital controls and bank risk, *Journal of Banking & Finance* 15, 805–824.

Glaeser, Edward L., Rafael La Porta, Florencio Lopez-de Silanes, and Andrei Shleifer, 2004, Do institutions cause growth?, *Journal of Economic Growth* 9, 271–303.

Gonzalez, Francisco, 2005, Bank regulation and risk-taking incentives: An international comparison of bank risk, *Journal of Banking & Finance* 29, 1153–1184.

Gordon, Robert J., 2012, Is U.S. economic growth over? Faltering innovation confronts the six headwinds, NBER Working Paper 18315, National Bureau of Economic Research.

Gorton, G., and R. Rosen, 1995, Corporate control, portfolio choice, and the decline of banking, *Journal of Finance* 50, 1377–1420.

Gorton, Gary, and Andrew Metrick, 2010, Regulating the shadow banking system., *Brookings Papers on Economic Activity* 261–312.

Gorton, Gary B, 2010, *Slapped by the invisible hand: The panic of 2007* (Oxford University Press).

Gorton, Gary B, and Nicholas S Souleles, 2007, Special purpose vehicles and securitization, in *The risks of financial institutions*, 549–602 (University of Chicago Press).

Greene, William, 2004, Fixed effects and bias due to the incidental parameters problem in the tobit model, *Econometric Reviews* 23, 125–147.

Gropp, Reint, Hendrik Hakenes, and Isabel Schnabel, 2011, Competition, risk-shifting, and public bail-out policies, *Review of Financial Studies* 24, 2084–2120.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales, 2013, The value of corporate culture, Working Paper 19557, National Bureau of Economic Research.

Hanushek, Eric A, and Dennis D Kimko, 2000, Schooling, labor-force quality, and the growth of nations, *American economic review* 1184–1208.

Hart, Stuart L., and Robert E. Quinn, 1993, Roles executives play: Ceos, behavioral complexity, and firm performance, *Human Relations* 46, 543–574.

Hau, Harald, and Marcel Thum, 2009, Subprime crisis and board (in-) competence: private versus public banks in germany, *Economic Policy* 24, 701–752.

Heckman, James J, Hidehiko Ichimura, and Petra Todd, 1998, Matching as an econometric evaluation estimator, *The Review of Economic Studies* 65, 261–294.

Hirsch, Barry T, and David A Macpherson, 2002, Union membership and coverage database from the current population survey: Note, *Industrial and Labor Relations Review* 56, 349.

Hovakimian, Armen, and Edward J Kane, 2000, Effectiveness of capital regulation at US commercial banks, 1985 to 1994, *The Journal of Finance* 55, 451–468.

Iannotta, Giuliano, and George Pennacchi, 2012, Bank regulation, credit ratings, and systematic risk, Unpublished working paper, University of Illinois.

Jensen, M.C., and W.H. Meckling, 1976, Theory of the firm: Managerial behavior, agency costs and ownership structure, *Journal of Financial Economics* 3, 305–360.

John, Kose, Anthony Saunders, and Lemma W Senbet, 2000, A theory of bank regulation and management compensation, *Review of Financial Studies* 13, 95–125.

John, Teresa A, and Kose John, 1993, Top-management compensation and capital structure, *The Journal of Finance* 48, 949–974.

Kacperczyk, M., and P. Schnabl, 2010, When safe proved risky: Commercial paper during the financial crisis of 2007-2009, *Journal of Economic Perspectives* 24, 29–50.

Kaplan, S.N., and J. Rauh, 2010, Wall street and main street: What contributes to the rise in the highest incomes?, *Review of Financial Studies* 23, 1004.

Kashyap, Anil, Raghuram Rajan, and Jeremy Stein, 2008, Rethinking capital regulation, *Maintaining stability in a changing financial system* 431–471.

Keeley, M.C., 1990, Deposit insurance, risk, and market power in banking, *The American Economic Review* 80, 1183–1200.

Keeley, Michael C, and Frederick T Furlong, 1990, A reexamination of mean-variance analysis of bank capital regulation, *Journal of Banking & Finance* 14, 69–84.

Khanna, Vikramaditya S, E Han Kim, and Yao Lu, 2013, Ceo connectedness and corporate frauds, University of Michigan Public Law Research Paper 283.

Kim, Daesik, and Anthony M Santomero, 1988, Risk in banking and capital regulation, *The Journal of Finance* 43, 1219–1233.

Kim, Hyunseob, 2011, Does human capital specificity affect employer capital structure? Evidence from a natural experiment, Unpublished working paper, Duke University.

Koehn, Michael, and Anthony M Santomero, 1980, Regulation of bank capital and portfolio risk, *The Journal of Finance* 35, 1235–1244.

Kroft, Kory, and Devin G Pope, 2013, Does online search crowd out traditional search and improve matching efficiency? Evidence from craigslist, *Journal of Labor Economics, forthcoming* .

Laeven, Luc, and Ross Levine, 2009, Bank governance, regulation and risk taking, *Journal of Financial Economics* 93, 259–275.

Lazear, Edward P, Kathryn L Shaw, and Christopher T Stanton, 2012, The value of bosses, NBER Working Paper 18317, National Bureau of Economic Research.

Li, Feng, 2010, The information content of forward-looking statements in corporate filings—a naìve bayesian machine learning approach, *Journal of Accounting Research* 48, 1049–1102.

Li, Feng, Michael Minnis, Venky Nagar, and Madhav Rajan, 2012, Knowledge, compensation, and firm value: An empirical analysis of firm communication, *Rock Center for Corporate Governance at Stanford University Working Paper No* 83.

Loughran, Tim, and Bill McDonald, 2011, When is a liability not a liability? textual analysis, dictionaries, and 10-ks, *The Journal of Finance* 66, 35–65.

Lustig, H., C. Syverson, and S. Van Nieuwerburgh, 2011, Technological change and the growing inequality in managerial compensation, *Journal of Financial Economics* 99, 601–627.

Merton, R.C., 1974, On the pricing of corporate debt: The risk structure of interest rates, *Journal of Finance* 29, 449–470.

Merton, R.C., 1977, An analytic derivation of the cost of deposit insurance and loan guarantees An application of modern option pricing theory, *Journal of Banking & Finance* 1, 3–11.

Murphy, K.J., and J. Zabojnik, 2004, CEO pay and appointments: A market-based explanation for recent trends, *The American Economic Review* 94, 192–196.

Petersen, Mitchell A, 2009, Estimating standard errors in finance panel data sets: Comparing approaches, *Review of Financial Studies* 22, 435–480.

Philippon, Thomas, and Ariell Reshef, 2012, Wages and human capital in the us finance industry: 1909–2006*, *The Quarterly Journal of Economics* 127, 1551–1609.

Pozsar, Zoltan, Tobias Adrian, Adam Ashcraft, and Hayley Boesky, 2010, Shadow banking, Staff Report 458, Federal Reserve Bank of New York.

Pratt, Ryan, 2011, A structural model of human capital and leverage, Unpublished working paper, Brigham Young University.

Rajan, Raghuram G, and Luigi Zingales, 1998, Power in a theory of the firm, *The Quarterly Journal of Economics* 113, 387–432.

Rajan, R.G., 2006, Has finance made the world riskier?, *European Financial Management* 12, 499–533.

Rochet, Jean-Charles, 1992, Capital requirements and the behaviour of commercial banks, *European Economic Review* 36, 1137–1170.

Rosen, S., 1981, The economics of superstars, *The American Economic Review* 71, 845–858.

Saunders, Anthony, Elizabeth Strock, and Nickolaos G Travlos, 1990, Ownership structure, deregulation, and bank risk taking, *The Journal of Finance* 45, 643–654.

Schultz, Theodore W, 1961, Investment in human capital, *The American Economic Review* 51, 1–17.

Shin, H.S., 2009, Securitisation and financial stability, *The Economic Journal* 119, 309–332.

Smith, Clifford W, and René M Stulz, 1985, The determinants of firms' hedging policies, *Journal of Financial and Quantitative Analysis* 20, 391–405.

Stevenson, Betsey, 2010, Beyond the classroom: Using title IX to measure the return to high school sports, *The Review of Economics and Statistics* 92, 284–301.

Yermack, D., 1995, Do corporations award CEO stock options effectively?, *Journal of financial economics* 39, 237–269.

Zingales, Luigi, 2000, In search of new foundations, *The Journal of Finance* 55, 1623–1653.