EPFL

Thèse n°7882

# Essays in Financial Economics

Présentée le 10 janvier 2020

au Collège du management de la technologie
Chaire du Prof. associé Fahlenbrach
Programme doctoral en finance

pour l'obtention du grade de Docteur ès Sciences

par

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École
polytechnique
fédérale
de Lausanne

2020

To my family and friends.

# Acknowledgements

This thesis would not have been possible without the guidance and support of my adviser Rüdiger Fahlenbrach. I would also like to thank the members of my jury committee, Erwan Morellec, Pierre Collin-Dufresne, Philip Valta, and Laurent Frésard, for their time and support. I am further indebted to Christoph Herpfer for his advice and our collaboration on the paper that forms the third chapter of this thesis.

A German proverb states that "shared suffering is half the suffering, shared happiness is twice the happiness". In the spirit of the proverb, I would like to thank my fellow PhD students, especially Kornelia Fabisik and Maxime Couvert.

The first part of this thesis is the accepted manuscript of an article published in the *Journal of Financial Economics* (DOI: https://doi.org/10.1016/j.jfineco.2019.03.014). The second and third parts are co-authored work that came to be in collaboration with Rüdiger Fahlenbrach and Christoph Herpfer, respectively.

*Lausanne, October 15, 2019* M. F.

# Abstract

This thesis consists of three chapters that study separate subjects in the area of corporate governance and financial intermediation.

In the first chapter, I study a protectionist anti-takeover law introduced in 2014 in France that covers a subset of all firms in the economy. The law decreased affected firms' likelihood of becoming the target of a merger or acquisition and had a negative impact on shareholder value. There is no evidence that management of those firms subsequently altered firm policies in its interest. Investment, employment, wages, profitability, and capital structure remain unchanged. The share of annual CEO compensation consisting of equity instruments increased by 8.4 percentage points, suggesting that boards reacted to the loss in monitoring by the takeover market by increasing the pay-for-performance sensitivity.

In the second chapter, which is co-authored work with Rüdiger Fahlenbrach, we conduct a detailed analysis of investors in successful initial coin offerings (ICOs). The average ICO has 4,700 contributors. The median participant contributes small amounts and many investors sell their tokens before the underlying product is developed. Large presale investors obtain tokens at a discount and flip part of their allocation shortly after the ICO. ICO contributors lack the protections traditionally afforded to investors in early stage financing. Nevertheless, returns nine months after the ICO are positive on average, driven mostly by an increase in the value of the Ethereum cryptocurrency.

In the third chapter, which is joint work with Christoph Herpfer, we investigate how bankers use information from lending relationships to help borrowers combine resources in strategic alliances. Firms that have borrowed from the same banker or share an indirect connection through a network of bankers are significantly more likely to enter an alliance. Consistent with bankers overcoming informational frictions, their ability to facilitate alliances decreases with network distance, and is stronger for opaque borrowers. Firms connected to more potential partners via banker networks enter more alliances. These alliances are associated with positive announcement returns and brokering banks are more likely to receive future underwriting mandates.

**Key words:** Corporate governance, mergers and acquisitions, protectionism, banking, financial intermediation, strategic alliances, initial coin offerings, individual investors, FinTech

# Zusammenfassung

Diese Doktorarbeit besteht aus drei Kapiteln welche verschiedene Themen im Bereich Corporate Governance und Finanzintermediation untersuchen.

Im ersten Kapitel untersuche ich ein protektionistisches Anti-Übernahmegesetz, welches 2014 in Frankreich eingeführt wurde und nur einen Teil aller Firmen betrifft. Das Gesetz reduzierte die Wahrscheinlichkeit, dass betroffene Firmen übernommen werden, und hatte einen negativen Einfluss auf deren Unternehmenswert. Ich finde keine Indizien dafür, dass die Geschäftsleitung betroffener Firmen die Operationen der Firma darauf in Ihrem Interesse verändert haben. Investitionen, Personalbestand, Mitarbeitersaläre, Rentabilität und Kapitalstruktur blieben unverändert. Der Anteil der jährlichen CEO-Vergütung bestehend aus Eigenkapitalinstrumenten stieg um 8.4 Prozentpunkte, was suggeriert dass Vorstände auf den Verlust an Aufsicht durch die Kapitalmärkte reagierten indem sie die Leistungsabhängigkeit der Vergütung erhöhten.

Im zweiten Kapitel, welches aus einer Zusammenarbeit mit Rüdiger Fahlenbrach stammt, unternehmen wir eine detaillierte Analyse von Investoren in Initial Coin Offerings (ICOs). Das durchschnittliche ICO hat 4,700 Anleger. Der Medianteilnehmer steuert einen vergleichsweise kleinen Betrag bei und verkauft seine Tokens bevor das Produkt, das dem ICO unterliegt, fertig entwickelt ist. Grosse Presale-Investoren erhalten Rabatte auf die Tokens und verkaufen einen Teil ihrer Allokation bald nach dem ICO. ICO-Teilnehmern fehlen die Schutzbestimmungen welche Investoren in Jungunternehmen normalerweise erhalten. Trotzdem sind die Renditen neun Monate nach dem ICO im Durchschnitt positiv, getrieben hauptsächlich durch einen Anstieg im Wert der Kryptowährung Ethereum.

Im dritten Kapitel, welche aus gemeinsamer Arbeit mit Christoph Herpfer stammt, untersuchen wir wie Banker Informationen aus Ihrer Kreditvergabetätigkeit nutzten um ihre Kreditnehmer bei der Kombination von Ressourcen in strategischen Allianzen zu unterstützen. Firmen die vom selben Banker Geld aufgenommen haben oder indirekt über ein Netzwerk von Bankern verbunden sind gehen eher eine Allianz ein. Übereinstimmend mit einer Fähigkeit, Informationsfriktionen zu überkommen, ist der Einfluss von Bankern auf die Formierung von Allianzen abnehmend in deren Netzwerkdistanz, und stärker für undurchsichtige Schuldner. Firmen, die mit einer grösseren Anzahl potenzieller Partner über ein Bankernetzwerk verbunden sind, gehen mehr Allianzen ein. Diese Allianzen führen zu positiven Kursentwicklungen wenn sie angekündigt werden, und involvierte Banker erhalten eher zukünftige Underwritingaufträge.

**Key words:** Corporate Governance, Fusionen und Übernahmen, Protektionismus, Bankwirtschaft, Finanzintermediation, Strategische Allianzen, Initial Coin Offerings, Einzelinvestoren, FinTech

# Contents

# Introduction

The past two decades have seen rapid changes in the structure of global equity markets. Today, the Berle and Means (1932) model of the widely held public corporation is becoming increasingly elusive. Instead, most developed financial markets are populated by institutional shareholders, a lot of them passive index funds, that own large blocks of shares in publicly listed firms. Governance scandals such as Enron and Parmalat during the early 2000s and the financial crisis of 2007-2008 have led to an unprecedented regulatory density affecting both public corporations and financial institutions. The international integration of capital markets has started to experience push-back from rising national security concerns and protectionist sentiment, and new technologies and financial innovations have given rise to new and innovative funding options for privately held companies. These developments lead to new questions about the optimal governance of joint-stock corporations. The core questions always remain the same, however: how to protect investors' interests without disproportionally decreasing firms' efficiency.

The first and second chapters of this dissertation deal directly with corporate governance questions arising from current events. The first chapters asks whether protectionism in cross-border mergers and acquisitions allows management to entrench itself at the expense of shareholders. The second chapter, which is co-authored work with Rüdiger Fahlenbrach, investigates initial coin offerings (ICOs), a new type of startup financing that has recently become popular with companies developing blockchain applications. The chapter investigates who invests in ICOs and why, and contrasts the protections that participants in ICOs receive with those common in traditional forms of early stage financing such as venture capital.

The third chapter, which is joint work with Christoph Herpfer, expands the discussion to financial intermediaries. Banks obtain detailed, private information about their corporate clients through lending and advisory relationships. Such privileged access to information can create agency conflicts when banks use information from a lending relationship in other areas to their own advantage. There is wide anecdotal evidence of banks allegedly passing on information to the opposing party in M&A transactions or using it for insider trading, with a number of cases resulting in high profile lawsuits. As a consequence, much of the academic literature on information spillovers in the banking sector has focused on possible negative consequences for clients. The third chapter of my thesis identifies a potentially beneficial side to information spillovers for clients of commercial banks in the U.S. syndicated loan market, resulting in collaborations between clients of the same or acquainted commercial bankers.

# Part I

# Does Protectionist Anti-Takeover Legislation Lead to Managerial Entrenchment?

# 1 Introduction

Over the last few years, governments worldwide have intervened in a significant number of cross-border mergers and acquisitions, often citing national security concerns. The Committee on Foreign Investment in the United States, the agency charged with evaluating the national security implications of foreign investments in the US, for example, conducted investigations into 66 transactions in 2015 (Committee on Foreign Investment in the United States, 2017). Elected officials from France, Italy and Germany have been advocating the introduction of a similar approval process in the European Union, and various European governments have recently given national agencies additional powers to intervene in inbound cross-border mergers and acquisitions (France in 2014 and Germany in 2017) or are currently contemplating doing so (e.g. the United Kingdom).[1] As these protectionist interventions are likely to continue, it is important to ask if and to what extent they affect companies and their shareholders.

Finance theory suggests that any action that reduces the threat of a takeover has the potential to entrench management at the expense of shareholders (Manne, 1965). I test whether protectionist anti-takeover legislation leads to managerial entrenchment based on the *Alstom Decree*, a protectionist law introduced in France in 2014. I use the Alstom Decree as a quasi-natural experiment to estimate the impact of protectionist anti-takeover legislation on firms' investment and employment policies, operating performance, capital structure, cash distributions to shareholders and executive compensation.

Although there is a large literature studying the connection between managerial entrenchment and anti-takeover legislation, it is unclear whether its results should apply to today's protectionist interventions.[2] Protectionism decreases affected firms' likelihood of receiving a takeover bid but does not necessarily affect management's bargaining power if a bid is received. The anti-takeover laws studied by the literature on the other hand were explicitly designed to improve target management's bargaining position in corporate transactions.

In addition, most of the published results are based on data and events from the 1980s. But corporate governance standards worldwide have become significantly stricter over the last two decades in the wake of the collapse of, for example, Enron and WorldCom in the US and Parmalat in Europe, making it unclear whether takeovers as a governance mechanism should still have the same relevance today.[3]

The Alstom Decree designates the five industry sectors energy, water supply, transportation,

[1] For the European Union, see Financial Times (2017a). For Germany, see Federal Ministry for Economic Affairs and Energy (2017). For France, see Legifrance (2014). For the United Kingdom, see Financial Times (2018b).

[2] The second generation of state-level anti-takeover laws enacted during the 1980s in the United States have been a particularly popular subject of empirical studies (e.g., Garvey and Hanka, 1999; Bertrand and Mullainathan, 2003).

[3] An example for stricter corporate governance standards in the US is the Sarbanes-Oxley Act. Virtually all countries have since passed a corporate governance codex. France, the country that is the subject of this study, has passed the AFEP-MEDEF Code (named after two industrial associations serving as self-regulatory organizations in the matter, the *Association française des entreprises privées*, short AFEP, and the *Mouvement des entreprises de France*, short MEDEF) to improve governance in 2002.

electronic communications and public health, which together account for around 30 percent of all publicly traded French firms, as strategic to the country's interest and enables the secretary of commerce to veto M&A transactions targeting companies operating in them if the bidder originates from abroad. Since the introduction of the Alstom Decree until early 2018, over a hundred transactions have been the subject of an investigation. In addition, the law also has the potential to deter M&A transactions ex ante (also see Dinc and Erel, 2013), either because it increases the costs for the bidder through delays or less favorable deal terms or because the acquirer suspects the transaction might not find approval.

I begin my investigation of the Alstom Decree by conducting a difference-in-differences analysis for firms' probability of being acquired on a panel of both publicly listed and privately held French firms. I find that treated firms face a 0.8 percentage point lower annual probability of being acquired following the legislation, amounting to 40% of the unconditional probability, and that this effect is driven by a decrease in the probability of a cross-border acquisition. I further conduct an event study around the Alstom Decree's announcement on May 14, 2014 to study its impact on shareholder value. The results indicate that a portfolio holding a long position in treated firms (i.e. firms subject to the Decree) and a short position of the same size in non-treated French firms would have generated statistically significant cumulative abnormal returns of between -0.59% and -0.98% over the event window.

I then test whether the measurable decrease in the threat of a takeover affected corporate policies and monitoring at affected firms through additional difference-in-differences estimates. The theory on the agency cost of free cash flow (Jensen, 1986) suggests that managers prefer to retain rather than distribute excess cash absent positive net present value investment opportunities. The literature has developed two competing hypotheses on managers' preferred use for such cash: *empire building* (e.g., Baumol, 1959) and the *quiet life* (Bertrand and Mullainathan, 2003). The empire building hypothesis suggests that executives have an interest in increasing the size of their firm, as it is positively correlated with their prestige and compensation. The quiet life hypothesis on the other hand states that managers prefer to avoid difficult decisions and are just as unlikely to aggressively grow a business as they are to restructure it when they are protected. Furthermore, it suggests that they have a preference for paying higher wages and growing the size of their staff, which results in lower productivity and profitability when they are at liberty to do so. I attempt to find evidence for empire building or quiet life behavior at firms subject to the Alstom Decree by testing for changes in firm characteristics that should be affected according to the two theories of managerial preferences. I do not find that affected firms increase capital expenditures, research and development (R&D) expenses or the number or volume of mergers and acquisitions they engage in. Therefore, I do not find any evidence for empire building. Inconsistent with quiet life behavior, I do not find that affected firms increase wages or employment following the Alstom Decree, and operating performance as measured by the return on assets (ROA) and return on sales (ROS) remains unchanged.

Based on Jensen's (1986) theory on the agency cost of free cash flow, I also test whether the Alstom Decree is associated with changes in protected firms' capital structure and payout policies. Following the theory, an entrenched manager will seek to decrease the amount of financial leverage, as debt financing commits part of the firm's cash flow to interest payments, or decrease distributions to shareholders in the form of cash dividends and stock buybacks. But as for the theories on managerial preferences above, I do not find any evidence that the Alstom Decree had an impact on firms' capital structure decisions. Financial leverage, whether it is measured in book or market terms, and distributions to shareholders in the form of dividends or stock buybacks as a fraction of book equity, are unaffected.

The final firm policy I examine in the light of the Alstom Decree is executive compensation. The most direct way in which an entrenched manager can extract value from a firm is through his or her own compensation contract. In case the Alstom Decree contributed to managerial entrenchment it might have caused an increase in the CEO's total compensation. In addition, the executive's compensation contract is one of the most important tools for aligning incentives between shareholders and management. If shareholders or the board of directors were concerned that the law would lead to a decrease in managerial discipline, they might have taken measures to increase the performance-sensitivity of executive compensation in an attempt to substitute for the loss in monitoring by the takeover market (Bertrand and Mullainathan, 1999a; Fahlenbrach, 2009). I find limited evidence for an increase in total executive compensation and robust evidence for an increase in the pay-for-performance sensitivity of executive compensation following the Alstom Decree. The increase in the pay-for-performance sensitivity, which I measure by the share of annual CEO compensation paid out in equity instruments, is economically significant at 8.4 percentage points.

In summary, my results suggest that a loss in efficiency stemming from a decrease in managerial discipline cannot explain the negative announcement returns of the Alstom Decree. Furthermore, the potential increase in executive compensation I find is too small to explain the full extent of the abnormal returns measured in the event study.[4] I suggest an alternative explanation for the stock market reaction to the Alstom Decree that does not build on managerial discipline: The incumbent shareholders frequently receive a large premium over the pre-offer share price during a takeover, and efficiency gains from removing bad management are only one reason for why the acquirer might be willing to pay such a premium. As protected firms are less likely to be acquired, the Alstom Decree caused a decrease in the expected value of future takeover premiums accruing to affected firms' shareholders (see Bennett and Dam, 2017).

My work contributes to the literature on the firm-level consequences of protectionism. Existing studies show that protectionist interventions into corporate transactions and related laws can substantially decrease the number of inbound cross-border mergers and acquisitions in a country (Dinc and Erel, 2013; Godsell et al., 2018). Researchers have also investigated the

[4]The mean CEO in the sample earns 1.32m euros a year. I find a 24.6% increase in executive compensation following the Alstom Decree, which would amount to 0.32m euros annually, or a present value of 4.06m euros if discounted at a hypothetical 8% cost of capital in perpetuity. A -0.98% abnormal return as measured in the event study corresponds to a loss of 43m euros in market value for the mean firm, exceeding the cost of increased executive compensation more than tenfold.

impact of other types of protectionism such as import tariffs on firm policies and outcomes (e.g. Valta, 2012; Valta and Frésard, 2016). However, to the best of my knowledge, this study is the first to examine the consequences of protectionist anti-takeover legislation for the policies and profitability of affected firms.

# 2 Literature review and hypotheses development

One of the ways in which the Alstom Decree differs from traditional anti-takeover legislationis that it does not give management the option to block an impending merger or acquisition but instead assigns this capacity to the government, which may or may not be using it very frequently. But even if the law does not lead to frequent interventions into cross-border transactions at the hands of the government, Dinc and Erel (2013) show that a single such intervention can discourage foreign companies from launching a takeover bid in the intervening country in the future. The Alstom Decree therefore has the potential to affect firms' likelihood of being acquired, either through its application or because it serves as a deterrent. Accordingly, I formulate the takeover probability hypothesis below.

***Takeover probability hypothesis:*** *The Alstom Decree reduces affected firms' likelihood of becoming the target of a merger or acquisition.*

There is a substantial literature connecting legislation and charter provisions deterring takeovers to shareholder value. Examining 600 charter amendments by US firms, Jarrel and Poulsen (1987), for example, find an average loss of 1.25% in market value. Malatesta and Walkling (1988) find similar abnormal returns of -1.13% using a sample of 61 poison pill adoptions. Examining the second generation of anti-takeover laws in the US, Karpoff and Malatesta (1989) find abnormal returns of -0.29% in a two-day window starting on the day before the first announcement, or -0.47% for the subset of business combination laws, although the abnormal returns are concentrated in firms with no pre-existing firm-level defenses.

The aforementioned studies, regardless of whether they examine the voluntary adoption of anti-takeover provisions by firms or their introduction through legislation, have in common that they find a relatively small, negative impact on stock returns. Based on the existing literature, I expect the Alstom Decree to have a negative impact on stock returns if it is an effective deterrent to takeovers, which is summarized in the shareholder value hypothesis below.

***Shareholder value hypothesis:*** *The Alstom Decree leads to a decrease in the market value of affected firms.*

A long tradition of corporate governance research has seen takeovers as a mechanism through which the acquirer can create value by removing unproductive management. This view has existed at least since Manne (1965), who discusses different motives for corporate transactions and argues that "[...] the lower the stock price, relative to what it could be with more efficient management, the more attractive the take-over becomes to those who believe that they can manage the company more efficiently." Hence the threat of a takeover is one of several possible devices to overcome the agency problem created by the separation of ownership and control in firms. One explanation for the negative impact on shareholder value that studies on anti-takeover laws have found is therefore that management implements policies that are inefficient from shareholders' perspective when the threat of a takeover decreases. Testing this hypothesis requires making assumptions about executives' preferences. Two competing theories of managerial preferences have been developed in the literature: the quiet life (Bertrand and Mullainathan, 2003) and empire building (e.g., Baumol, 1959; Jensen, 1986).

Quiet life preferences suggest that managers prefer to avoid difficult decisions and conflicts. Studies favouring the existence of quiet life preferences are those by Bertrand and Mullainathan (1999b, 2003) and Cronqvist et al. (2009). Studying plant-level data from the United States, Bertrand and Mullainathan (2003) find that manufacturing plants protected by a business combination law experience an increase wages and employment and a decrease in productivity and profitability. They also find that the closure of existing manufacturing plants as well as the opening of new plants both become less likely in states that have passed a business combination law. Cronqvist et al. (2009) study a panel of firms with associated CEO stock ownership data to investigate the relation between managerial entrenchment and wages. If managers have quiet life preferences, CEO stock ownership and wages could a priori be correlated either way: on the one hand, higher CEO ownership means the CEO is potentially more entrenched, implying higher wages, but on the other hand, it also means that the CEO has stronger financial motives to keep wages low. Because a portion of the firms in their sample has a dual-class share structure, leading to a separation of cash flow and voting rights, Cronqvist et al. are able to disentangle these two opposing effects. In support of quiet life preferences, they find that more entrenched CEOs tend to pay their workers more, but that financial incentives mitigate such behavior. The quiet life hypothesis below summarizes the predictions of quiet life preferences in the context of the Alstom Decree.

***Quiet life hypothesis:*** *Firms affected by the Alstom Decree increase wages and employment, experience a decrease in profitability and reduce their spending on M&A.*

Empire building on the other hand suggests that managers have a preference for heading as large a firm as possible, as their compensation and prestige is increasing in firm value. Empirical evidence for this type of preferences comes mostly from the study of firms' M&A decisions. One set of studies relates companies' investment decisions to their amount of disposable resources and investment opportunities. Harford (1999), for example, finds that firms with high cash holdings engage in value-destroying acquisitions, while Lang et al. (1991) find that firms that generate high free cash flows but have poor investment opportunities engage in acquisitions with lower announcement returns. Another set of studies has related the incidence of perceived empire building to a firm's governance characteristics and environment. Masulis et al. (2007), for example, find that acquirers protected by more anti-takeover provisions engage in transactions with lower announcement returns. Datta et al. (2001) study the relation between firms' acquisition behavior and the fraction of CEOs' annual compensation paid out in equity instruments and find that managers with high equity-based compensation engage in more value creating transactions and pay lower acquisition premiums, while the opposite is true for managers with low equity-based compensation. The predictions of empire-building preferences are summarized in the empire building hypothesis below. I generalize the results from the study of M&A decisions to capital expenditures and R&D, both of which management can use to grow a firm as well.

***Empire building hypothesis:*** *Firms affected by the Alstom Decree increase their investment into physical capital, R&D expenses and spending on M&A.*

A consequence of the imperfect alignment of interests between managers and shareholders is that managers will be inclined to spend the firm's excess cash on negative net present value projects - such as unprofitable acquisitions, in the case of empire building preferences - rather than return it to shareholders. Jensen (1986) calls this conflict the *free cash flow problem* and argues that management can commit to returning excess cash to investors by issuing debt in exchange for equity. Numerous studies have since attempted to find an empirical connection between managerial entrenchment and the firm's capital structure. Berger et al. (1997), for example, find that financial leverage is lower when CEOs hold relatively little equity in the firm and are compensated in a performance-insensitive manner, and that leverage tends to increase following entrenchment-reducing shocks. Garvey and Hanka (1999) find that US firms protected by business combination laws reduce financial leverage by substituting for debt financing with equity. The agency cost of free cash flow theory and the empirical findings cited above imply that managers of firms protected by the Alstom Decree might seek to retain a larger fraction of the free cash flow generated by their firm, which is summarized in the hypothesis below.

***Agency cost of free cash flow hypothesis:*** *Firms affected by the Alstom Decree reduce cash distributions to shareholders and their use of debt financing.*

Because of the potentially undesirable consequences of managers' preferences, shareholders and the board of directors might attempt to substitute for a loss in oversight caused by protectionist legislation by strengthening other governance mechanisms. For example, they could increase the fraction of independent directors on the board or increase the pay-for-performance sensitivity of executive compensation as a reaction. In the following, I will focus on executive compensation as the channel for substitution, the reason being that shareholders can influence compensation on relatively short notice and face little cost for doing so compared to other potential mechanisms. Furthermore, the studies by Cronqvist et al. (2009) and Berger et al. (1997) cited above conclude that financial incentives in particular mitigate executives' preference for paying relatively high wages and reducing financial leverage. To date, evidence for governance substitution is provided by Bertrand and Mullainathan (1999a) and Fahlenbrach (2009). Bertrand and Mullainathan (1999a) find that the CEO's pay-for-performance increased at firms covered by second-generation anti-takeover legislation, but that this increase was concentrated in firms with at least one large shareholder. Fahlenbrach (2009) shows that executives' levels of equity incentives and stock ownership are positively correlated with various measures of managerial entrenchment such as the length of the CEO's tenure, the fraction of internal directors and whether the CEO chairs the board, and negatively correlated with institutional ownership concentration, which has been associated with increased monitoring (Hartzell and Starks, 2003). Such governance substitution could therefore lead to an increase in the pay-for-performance sensitivity of executive compensation at firms protected by the Alstom Decree.

On the other hand, some researchers have argued that executives' influence in designing their own compensation contracts is material to a degree that shareholders might have very little impact on the design of executive pay (Bebchuck and Fried, 2004). Furthermore, the most immediate way for an entrenched manager to extract value from the firm is to adjust his or her own compensation contract. Therefore, if the Alstom Decree entrenches management, it might cause an increase in total compensation. The executive compensation hypothesis below summarizes possible consequences of the Alstom Decree for the level and composition of executive compensation, based on the literature on governance substitution and the influence of entrenched managers on their own compensation.

***Executive compensation hypothesis:*** *The Alstom Decree leads to an increase in the pay-for-performance sensitivity of executive compensation and/or an increase in total executive compensation.*

# 3 Institutional background

This section illustrates the institutional background for this study. I begin by describing the Alstom Decree in detail and then present an overview of the French market for corporate control in general.

## 3.1 The Alstom Decree

### 3.1.1 Contents

The Decree number 2014-479 concerning foreign investments subject to approval *(Décret n°2014-479 relatif aux investissements étrangers soumis à autorisation préalable)*, nicknamed the *Alstom Decree* by the press, was announced on May 14, 2014 and became legally binding the day after. It owed its swift introduction to an existing law on the approval of foreign investments in defense-related industries (article L.151-3 of the French Monetary and Financial Code) that allows the government's executive branch to extend the protection to additional industry sectors by means of a particular type of executive order (the *Décret en Conseil d'État*), which it did in May 2014.

The Decree marks the following five industry sectors as strategic to the country's interests and makes inbound mergers and acquisitions of French companies operating in them subject to prior approval by the secretary of commerce if the acquirer originates from abroad:

1. Integrity and security of and continuity in the supply of electricity, gas, hydrocarbons and other sources of energy.
2. Integrity, security and continuity of the water supply under the norms laid out in the interest of public health.
3. Integrity and security of and continuity in the operation of transportation networks and services.
4. Integrity and security of and continuity in the operation of electronic communication networks and services.
5. The protection of public health.

In addition to the above, the defense sector and other activities related more closely to national security had already been subject to the same approval rules under the existing law and continued to be so afterwards. I therefore exclude firms in the defense sector from any analysis; as they were already subject to special takeover rules before May 2014, it is unclear whether the Alstom Decree should have had any additional effect on them (although the defense industry is unaffected from a legal point of view, the Decree could have had an impact if market participants interpreted it as a signal for increased scrutiny by the government). The Decree also states that firms involved in activities of vital importance as defined by articles 1332-1 and 1332-2 of the French Code of Defense are subject to the same approval rules. This affects a list of around 150 private as well as public companies kept secret by the French administration which can therefore not be taken into account when identifying affected firms. It is worth noting, however, that these 150 firms should have a large overlap with the five industry sectors mentioned by the Alstom Decree. A publication by the General Secretariat for Defense and National Security lists the twelve industry sectors to which the firms belong, of which three are local and federal authorities and therefore not in the sample of public companies, one is the financial sector, which I exclude from the analysis, and five are the sectors mentioned by the Alstom Decree; this then leaves just the three sectors food, space and research, and general manufacturing, with the space sector having a large overlap with defense and therefore being largely excluded as well (General Secretariat for Defence and National Security, 2014).

### 3.1.2 History

The immediate cause of the Alstom Decree were competing acquisition offers for Alstom's power and grid division made by General Electric (GE) and Siemens (which later submitted a revised bid jointly with Mitsubishi Heavy Industries) in April 2014. Alstom at the time was one of the largest manufacturing companies in France, with many of its products being used in critical infrastructure such as nuclear and gas-fired power plants and railways.

The Alstom Decree was signed into law jointly by Manuel Valls, then prime minister of France, and Arnaud Montebourg, then secretary of commerce. Montebourg was the Decree's principal public proponent and also the person charged with its application in his capacity as the secretary of commerce. He was also responsible for the negotiations between the French government and the would-be acquirers GE and Siemens.

Montebourg used the newly-created Decree to block the acquisition offers from both parties. At a press conference on June 20, 2014 he stated publicly that he had submitted a list of conditions to GE under which he would allow it to acquire parts of Alstom (Directorate for Legal and Administrative Information, 2014a). Those conditions were:

1. Alstom's activities in the sectors nuclear power, steam turbines, power grid and renewable energies are to be held in a joint-venture co-owned by the French government
2. The French government is to be given a 'golden share' in the above joint-venture, granting it additional powers such as a veto right
3. All of Alstom's patents on nuclear technologies are to be retained by a French state-owned enterprise
4. The European headquarters of Alstom's gas turbine business are to remain in France
5. GE is to sell its railway signaling business to Alstom's transport unit
6. GE is to protect the existing jobs at Alstom in France and to create 1,000 additional ones

GE and the board of Alstom ended up publicly agreeing to a partial acquisition under Montebourg's terms on June 21. GE therefore agreed to conditions that were neither part of its initial offer nor of the competing offer from Siemens, and that might have proved prohibitive to acquirers in other transactions.

### 3.1.3 Discussion

In this section, I will argue that the Alstom Decree is well suited for use as a quasi-natural experiment. To this end, I will address four potential concerns regarding its validity. These potential concerns are that first, the Decree's primary motivation might have been to intervene in corporate affairs more generally rather than to prevent a transfer of control abroad, secondly, that the set of companies it affects might not be restricted to the one designated explicitly by the Decree, third, that the Decree might not actually pose a credible threat to foreign acquirers and fourth, that there is reverse causality because the Decree's introduction might have been the consequence of lobbying or efforts to prevent general structural changes in the industries it covers.

During the press conference on June 20, 2014, Montebourg stated that the government's motivation for intervening was "to guarantee our independence in terms of energy, the creation of jobs in the country and the preservation of decision centers in France". François Hollande, then-president of France, repeated the same three reasons one day later during a different press conference (Directorate for Legal and Administrative Information, 2014b). Both the retention of control as well as a specific firm policy, employment, were therefore mentioned as the rationale. What speaks for national security being the primary motivation is that four out of the six demands made during the press conference were focused on control and seemingly related to French national security interests. For example, the government's terms allowed GE to take full control of Alstom's gas turbine unit, but required it to enter a joint venture with French authorities in the nuclear sector; in 2014, gas-fired power plants accounted for only 2.7% of total electricity generation in France, whereas nuclear power accounted for 77.0% (Electricity Transmission Network, 2014). In contrast, the 1,000 additional jobs that were part of the terms only amount to slightly over 1% of Alstom's 2013 employee count.

A seeming contradiction in the national security rationale is that during the negotiations with General Electric, the government was rumored to be supporting the rival bid from Siemens, a German firm (The Guardian, 2014). Montebourg addressed this topic during the press conference, arguing that the competing offer had "demonstrated that Alstom was worth an alliance rather than an acquisition and absorption" and that it had "allowed, through discussion and competition, that France's interests be preserved". In his own words, his primary motivation for supporting Siemens seems to have been to use competition to extract control rights from the bidder, similar to how a competing offer might be used to extract a higher price. The fact that Siemens is a European company and General Electric is not, on the other hand, did not feature prominently in the discussions surrounding the Decree (in addition, the Siemens proposal was made jointly with Mitsubishi, also a non-European firm).

Another argument in favor of the national security rationale is that the Alstom Decree has not been undone by the successive government. At the time of its introduction, France was governed by the socialist party under François Hollande, and opposition parties received the Alstom Decree with skepticism (Le Parisien, 2014). Jean-François Copé, then president of the UMP (a centre-right party holding the second largest number of seats in parliament at the time), was quoted saying that the Decree was "in line with the philosophy that is his [Montebourg's], which is statism, interventionism and denial of the economic reality, [and] will obviously continue to discourage foreign investors from investing in France". Marine Le Pen, president of the far-right National Front (holding the third-most seats in parliament), on the other hand, called it a "smoke screen", saying that it did not do enough to prevent acquisitions of strategically important firms by foreign acquirers. Since then, however, the socialists have been replaced in government by the party "La République En Marche !" under president Emmanuel Macron. Both have been generally described as economically liberal and more in favor of free markets than the previous government. But instead of abolishing the Alstom Decree, president Macron doubled down by pushing for the introduction of a similar approval process on European level in 2017 and widening the scope of the Alstom Decree in 2018, to newly include the sectors space, semiconductors, electronic data storage and artificial intelligence (Financial Times, 2017b; Libération, 2018).

The second issue concerns whether the set of companies the Alstom Decree affects is clearly defined, which is important because the Decree's impact will be difficult to measure otherwise. If the government could apply it to firms at will, the definition of the treatment and control group would be unclear or at least imprecise. The main argument speaking in favor of a clearly defined treatment group are the European Union's rules on the free movement of capital and its provisions for mergers and acquisitions. Article 63 of the Treaty on the Functioning of the European Union (TFEU) specifies that all restrictions on the movement of capital between member states and between member states and third parties are prohibited, although Articles 65 and 346 of the same treaty provide exemptions for "measures which are justified on grounds of public policy or public security" (The Member States of the European Union, 2012). The European Commission examined the Alstom Decree upon its announcement and declared it to be compatible with the TFEU, but stated that it "will closely monitor any use of the law, i.e. systematically monitor any application of the investment screening legislation, and check in particular that it is not used to achieve purely economic objectives" (Reuters, 2014). Applications of the Alstom Decree are further limited by the EC Merger Regulation (The Council of the European Union, 2004). Following Article 21 of the former, the European Commission has sole jurisdiction in reviewing corporate transactions in which the combined turnover of all merging parties is at least 5 billion euros or in which at least two of the parties involved have a turnover in excess of 250 million euros. The EC Merger Regulation provides that EU member states can still intervene in transactions in case they have a "legitimate interest" for doing so, stating that public security, plurality of the media and (in the financial sector) prudential rules qualify as legitimate interests. Interventions based on any other public interest, however, have to be approved by the European Commission. Both the TFEU and the EC Merger Regulation thereby significantly constrain the leeway the French government has in applying the Alstom Decree to transactions involving firms in industries that would not commonly be considered to be of interest for national security.

The French ministry of the economy has historically not provided statistics on its examination of foreign investments. During the course of a parliamentary inquiry in early 2018, however, secretary of commerce Bruno Le Maire disclosed that the ministry of the economy had examined between 20 and 30 transactions in 2013 and 2014, and more than a hundred since the introduction of the Alstom Decree (Les Echos, 2018). Based on the historical number of inbound cross-border transactions in industries covered by the Alstom Decree (see the following Section), Le Maire's statement implies that essentially all such transactions are being investigated nowadays, although it is unclear what fraction of transactions subject to an investigation were actually prohibited. There are also public data suggesting that the approval process leads to delays even if a transaction is eventually permitted, and thereby imposes costs on the bidder. In 2015, for example, Finland-based Nokia Corporation made a public tender offer for the acquisition of Alcatel-Lucent, a French manufacturer of telecommunications equipment. Nokia's tender offer filed with the French financial market regulator AMF states that it had sought approval from the French authorities under articles L. 151-1 and R. 153-1 and following of the French Monetary and Financial Code (the latter being the section extended by the Alstom Decree) on May 18, 2015. According to the same document, Nokia received approval over five months later on October 21, 2015. It then published its tender offer only eight days after, on October 29.

Finally, another possible concern might be that the Alstom Decree was the product of successful lobbying efforts by a particular set of companies. For example, one could imagine that the CEOs of firms expecting a decline in performance prefer more protection from foreign acquisitions and would therefore engage in such lobbying activities. This manner of exercising influence would give rise to reverse causality in the form of certain firm characteristics and policies causing protectionist policies instead of the other way around. However, I deem it unlikely that the Alstom Decree was a product of successful lobbying efforts because the board of directors of Alstom itself had recommended shareholders to accept the first acquisition offer by General Electric before the government blocked it. Another possible reverse causality argument is that the Alstom Decree was introduced as a response to a secular decline in the industries it covers. The financial data for firms in industries protected by the Alstom Decree indicate, however, that they actually experienced a stable or increasing (depending on the measure of profitability) operating performance in the years leading up to 2014.

## 3.2 The French market for corporate control

The Alstom Decree can only be of interest in studying the importance of protectionism for corporate governance if there is an active market for corporate control in France. Table 3.1

below compares France to the other members of the G7 group of countries in terms of their market for mergers and acquisitions.

Table 3.1 – **Relative incidence of M&A across G7 countries**

M&A/GDP is the number of mergers and acquisitions per billion US Dollars of historical GDP. Vol./GDP is the annual volume of M&A transactions as a percentage of gross domestic product. The third column displays the total number of transactions whereas the fifth column only counts transactions for which the price was disclosed. Cross-border is the share of mergers and acquisitions with a foreign acquirer. The sample data range from 2004 to 2013. Number and volume (in historical USD at current exchange rate) of mergers and acquisitions have been retrieved from Capital IQ. Data for the GDP come from the OECD.

| Panel A: All targets | | | | | |
|---|---|---|---|---|---|
| Country | M&A/GDP | n | Vol./GDP [%] | n | Cross-border [%] |
| Canada | 1.04 | 13,743 | 6.21 | 6,825 | 27.95 |
| France | *0.64* | *14,486* | *2.02* | *3,296* | *22.90* |
| Germany | 0.40 | 12,852 | 1.51 | 3,277 | 38.47 |
| Italy | 0.20 | 4,092 | 1.57 | 1,531 | 35.87 |
| Japan | 0.01 | 278 | 0.20 | 177 | 22.30 |
| United Kingdom | 1.42 | 33,125 | 6.35 | 14,606 | 22.93 |
| United States | 0.74 | 109,557 | 5.85 | 45,342 | 11.90 |
| **Panel B: Targets in industries affected by the Alstom Decree** | | | | | |
| Country | M&A/GDP | n | Vol./GDP [%] | n | Cross-border [%] |
| Canada | 0.09 | 1,193 | 1.81 | 701 | 31.49 |
| France | *0.04* | *810* | *0.60* | *192* | *28.55* |
| Germany | 0.02 | 722 | 0.32 | 202 | 42.84 |
| Italy | 0.02 | 392 | 0.22 | 165 | 43.83 |
| Japan | 0.00 | 35 | 0.06 | 22 | 22.00 |
| United Kingdom | 0.07 | 1,563 | 1.44 | 845 | 35.56 |
| United States | 0.05 | 7,287 | 1.82 | 3,468 | 15.48 |

Panel A lists the average annual number and the total volume of mergers and acquisitions (both public and private), standardized by the gross domestic product, using data from Capital IQ (mergers and acquisitions) and the OECD (gross domestic product). Over the years 2004 to 2013, France had similar amount of M&A activity to the US when measured by the number of transactions (0.64 compared to 0.74 per billion USD of GDP), although the disclosed transaction volumes (2.02% of GDP compared to 5.85%) were lower. The French number is in line with that of the other G7 countries, however. Furthermore, in 23% of M&As targeting French companies, the acquirer was foreign (compared to only 12% in the US), leaving ample room for barriers to foreign investment to affect transactions. Panel B presents the same statistics for the subset of firms whose primary industry is one of the five mentioned by the Alstom Decree. Even though some of these industries are relatively concentrated and heavily regulated, corporate transactions in general and cross-border mergers and acquisitions in particular are frequent. In France, the M&A volume in now protected industries amounted to 0.60% of GDP between 2004 and 2013, representing 30% of the total volume, and in 29% of those cases the acquirer originated abroad. In conclusion, France has an active takeover market, including for firms that are now covered by the Alstom Decree.

# 4 Data

This section describes the data used in the analysis. It first describes the financial and compensation variables and then specifies how firms are assigned to the treatment and control group.

## 4.1 Financial and compensation variables

I construct a panel data set of publicly traded French firms spanning the years 2011 to 2016. Whereas three years of post-treatment data make for a relatively brief observation window, the time horizon at my disposal is not much shorter than that used, for example, by the literature on anti-takeover laws.[1]

I collect accounting, employment, and stock market data as well as historical four-digit SIC codes for publicly listed companies incorporated in France from the Compustat Global database. Data on executive compensation, government ownership, corporate transactions and firms' year of incorporation is retrieved from S&P's Capital IQ database. An exception with respect to the accounting data are cash dividends and repurchases of common stock, which I also retrieve from Capital IQ because the corresponding variables in Compustat (*dvc* and *prstkc*) are mostly missing. The two databases can be fully merged on Compustat's *gvkey* identifier. I exclude shell and holding companies (those belonging to Fama-French industry group 48, e.g. real estate investment trusts), banks and other lending institutions (Fama-French industry group 45) and non-operating establishments (SIC code 9995).[2] In addition, I exclude observations with a market capitalization below 75m euros (around $100m at the 2014 exchange rate) or less than 5m euros in sales. The reason for excluding these firms are twofold; first, the health sector contains a number of small, research intensive growth firms that disproportionally skew productivity and investment ratios such as the return on sales for the treatment group. Secondly, the sample contains a number of micro caps listed in Euronext's *Growth* and *Access* segments which are subject to limited transparency and disclosure requirements, which are removed by trimming.

Panel A of Table 4.1 above displays descriptive statistics for the financial variables. All continuous variables described in the table have been winsorized at the 1 and 99% level. The definition of financial variables from Compustat follows the literature and is detailed in A.1. For R&D expenses, I follow Himmelberg et al. (1999) and assume they are negligible when reported missing and set the corresponding observation to zero. The number of M&As is the number of M&A transactions listed in Capital IQ for a specific firm-year in which the firm is the acquirer. I exclude transactions in which the acquiring firm already owns a majority stake before the acquisition or in which the percentage sought is less than 100. The number of employees is the number reported by firms in their annual report and wage is the average wage calculated by dividing staff expenses by the number of employees. I follow the literature

[1] In the case of the US business combination laws, New York was the first state to pass a business combination law in 1985, but by 1989, 90% of all US firms (weighted by total assets) were subject to such a law (Cain et al., 2017).

[2] For the definition of the 49 Fama-French industry groups, see French (2018).

Table 4.1 – **Descriptive statistics**

Financial variables are in millions of euros. The number of employees, the average wage and total compensation for the CEO and median board member are in thousands. All continuous variables have been winsorized at the 1% and 99% level. Variables have been constructed from raw data retrieved from Compustat and Capital IQ as detailed in A.1. The sample consists of annual observations from 2011 to 2016. Statistics in Panels A and B are for publicly listed firms that have a record in Compustat. Data in Panel C is from Capital IQ and contains both publicly listed and privately held firms. CEO (board member (BM)) total is the total annual compensation of the CEO (median board member) for the fiscal year. CEO (median board member) equity-based is the fraction of annual compensation paid out in stock and option grants.

| Panel A: Compustat financials | | | | | | |
|---|---|---|---|---|---|---|
| | Mean | Median | Min | Max | SD | Obs. |
| Sales | 5,234.02 | 734.27 | 5.98 | 67,923.80 | 11,993.55 | 1,686 |
| Assets | 9,404.74 | 866.16 | 5.61 | 186,149.00 | 26,861.31 | 1,755 |
| MV of equity | 4,390.92 | 478.64 | 77.44 | 70,472.05 | 10,875.86 | 1,602 |
| Book-to-market | 0.83 | 0.67 | 0.09 | 3.71 | 0.60 | 1,571 |
| Firm age | 69.66 | 46.00 | 2.00 | 282.00 | 59.42 | 1,580 |
| Capex | 282.01 | 25.55 | 0.10 | 5,485.00 | 758.35 | 1,647 |
| R&D | 63.08 | 0.00 | 0.00 | 1,618.00 | 218.88 | 1,761 |
| R&D missing | 0.57 | 1.00 | 0.00 | 1.00 | 0.49 | 1,761 |
| (Capex+R&D)/assets | 0.06 | 0.04 | 0.00 | 0.37 | 0.06 | 1,646 |
| Capex/assets | 0.04 | 0.03 | 0.00 | 0.24 | 0.04 | 1,646 |
| PPE/assets | 0.19 | 0.13 | 0.00 | 0.86 | 0.18 | 1,716 |
| Employees | 19.42 | 3.23 | 0.01 | 379.14 | 53.27 | 1,168 |
| Avg. wage | 56.01 | 52.16 | 4.51 | 164.73 | 25.33 | 1,095 |
| ROA | 0.07 | 0.06 | −0.25 | 0.35 | 0.08 | 1,724 |
| ROS | 0.07 | 0.08 | −0.88 | 0.55 | 0.16 | 1,681 |
| M&A volume | 31.58 | 0.00 | 0.00 | 1,200.00 | 156.25 | 1,761 |
| M&A count | 0.47 | 0.00 | 0.00 | 11.00 | 0.97 | 1,761 |
| State ownership | 0.03 | 0.00 | 0.00 | 1.00 | 0.16 | 1,761 |
| Book leverage | 0.36 | 0.36 | 0.00 | 0.93 | 0.23 | 1,693 |
| Market leverage | 0.29 | 0.25 | 0.00 | 0.93 | 0.23 | 1,585 |
| Dividends/eq. | 0.04 | 0.03 | 0.00 | 0.44 | 0.06 | 1,688 |
| Buybacks/eq. | 0.01 | 0.00 | 0.00 | 0.20 | 0.03 | 1,534 |
| Panel B: Executive compensation | | | | | | |
| | Mean | Median | Min | Max | SD | Obs. |
| CEO total | 1,328.69 | 640.43 | 0.00 | 7,986.03 | 1,571.89 | 1,451 |
| CEO equity-based (%) | 18.12 | 0.00 | 0.00 | 80.14 | 22.45 | 856 |
| CEO stock-based (%) | 16.24 | 0.00 | 0.00 | 79.28 | 21.35 | 839 |
| CEO equity-based euros | 607.65 | 0.00 | 0.00 | 6,067.69 | 1,122.56 | 856 |
| BM total | 916.16 | 395.18 | 0.00 | 6,408.48 | 1,282.68 | 1,493 |
| BM equity-based (%) | 15.79 | 0.00 | 0.00 | 82.81 | 21.52 | 786 |
| BM stock-based (%) | 13.43 | 0.00 | 0.00 | 79.63 | 19.59 | 768 |
| BM equity-based euros | 431.02 | 0.00 | 0.00 | 4,938.24 | 880.49 | 786 |
| Panel C: Capital IQ financials | | | | | | |
| | Mean | Median | Min | Max | SD | Obs. |
| Firm is acquired | 0.02 | 0.00 | 0.00 | 1.00 | 0.14 | 42,087 |
| Firm is acquired cross-border | 0.01 | 0.00 | 0.00 | 1.00 | 0.08 | 42,087 |
| Revenues | 213.07 | 25.00 | 5.14 | 6,327.10 | 802.73 | 38,092 |
| Return on assets | 0.06 | 0.05 | −50.20 | 3.62 | 0.33 | 36,174 |
| Book leverage | 0.29 | 0.23 | 0.00 | 1.00 | 0.26 | 33,318 |
| PPE/assets | 0.13 | 0.06 | 0.00 | 1.00 | 0.17 | 37,476 |
| Firm is public | 0.06 | 0.00 | 0.00 | 1.00 | 0.24 | 38,800 |
| State Own. | 0.00 | 0.00 | 0.00 | 1.00 | 0.04 | 38,800 |

in dropping a firm's observations for the average wage and employment if the current divided by the previous year's wage is ever above 7/4 or below 4/7 (Bertrand and Mullainathan, 1999b). *State ownership* is an indicator variable equal to one if the French government holds a stake of at least 5% in the firm at the end of the fiscal year, which is the case for 3% of all firm-years in the sample.

Panel B of Table 4.1 gives descriptive statistics for CEO and board member (BM) compensation. Appendix A.1 provides additional details on how these variables are constructed. For the board of directors, I retrieve data for all board members listed in Capital IQ and then take the median across non-missing observations for every firm-year. CEO equity-based compensation is the fraction of annual compensation paid out in stock (the *restricted stock awards*, *director stock awards* and *long-term incentive plan* items in Capital IQ) and stock option grants, whereby I value stock option grants according to the Black and Scholes (1973) model as modified by Merton (1973) to account for cash dividends. I implement this valuation based on the approximation developed by Core and Guay (2002). CEO (BM) stock-based compensation is the fraction of annual compensation paid out in stock grants. Equity-based euros is the annual value of stock and option grants received by the executive in euros. Over the sample period, French CEOs (board members) receive on average 18.12% (15.79%) of their annual compensation in equity instruments, although during the median firm-year the CEO and median board member do not receive any compensation in equity instruments. It is worth noting, however, that the observations with zero equity-based compensation are mostly concentrated among small firms, and that restricted stock grants in particular are part of executive compensation at most large firms in the sample. The mean CEO (board member) in the sample earned 1.33m (0.92m) euros per year, with 0.61m (0.43m) euros of it paid out in equity instruments. That the average euro amount of equity far exceeds 18.12% (15.79%) of the average annual compensation confirms that it is mostly the high earning executives of large firms who are partly paid in equity instruments.

A part of the analysis in Section 5 is concerned with estimating the Alstom Decree's impact on firms' likelihood of being acquired. Because mergers and acquisitions are infrequent events and the number of publicly listed sample firms is limited, I assemble a larger sample containing accounting data for both publicly listed and privately held firms from Capital IQ and combine it with data on successful mergers and acquisitions, also from Capital IQ. The data on French private firms that Capital IQ provides is limited to the most important items of the balance sheet and income statement. I therefore only use it in the analysis of the acquisition probability and restrict the sample to public data everywhere else. Descriptive statistics for the Capital IQ sample are provided in Panel C of Table 4.1. The same filters are applied to M&A transactions as for the main sample and observations with annual revenues below 5m euros have also been excluded.

## 4.2 Treatment assignment

I assign treatment status (i.e. whether a firm is affected by the Alstom Decree or not) based on the historical 4-digit SIC code reported for the year 2014 in Compustat; the full list of treated SIC codes is given in Appendix A.1. Whenever the SIC code for 2014 is missing (in about 5% of cases), the current one is backfilled. To decide whether a 4-digit SIC code belongs to one of the five affected industry sectors, I use the product descriptions in the SIC manual of the United States Department of Labor (United States Departement of Labor, ated). Firms in the defense sector, which I wish to exclude from the analysis, cannot reliably be identified by SIC code alone. The reason is that they frequently manufacture goods that can be put to both civil and military use; for example, *Dassault Aviation SA*, a manufacturer of military aircraft, reports SIC code 3721 (aircraft). I therefore use the list of major defense contractors from the 2016 statistical yearbook of the French Ministry of Defense as an additional filter to identify and exclude firms in the defense sector (Economic Observatory of Defense, 2016). Figure 4.1 below displays the number of firms with non-missing data by year and treatment group. The sample consists of around 300 firms each year, of which about thirty percent are affected by the Alstom Decree.

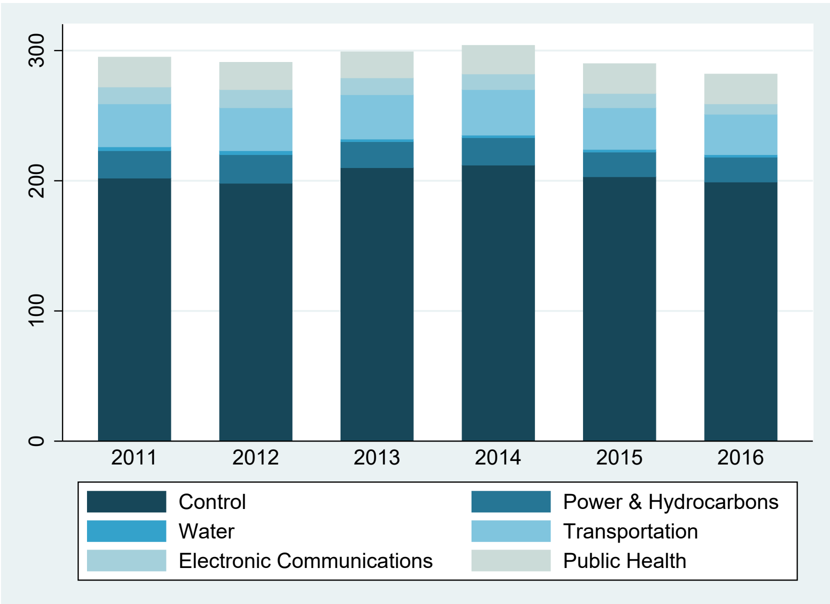



Figure 4.1 – Sample size by year.
The figure displays the number of sample firms by year and treatment status. Firms in the *Control* group are not subject to the Alstom Decree, those in the other industry groups are.

Descriptive statistics for firms in the treatment and control group as well as tests for equality of means are supplied in Table 4.2. Treated firms on average have significantly higher sales and more employees, are less profitable, have a higher ratio of fixed assets, higher capital expenditures and R&D expenses relative to their total assets, have higher higher leverage, pay their average employee more and are more likely to be partly owned by the government. These differences are similar to those in Bertrand and Mullainathan (2003), where manufacturing plants eventually subject to a business combination law have significantly higher sales, capital stock, employment and wages. To account for the differences in characteristics between treatment and control groups, I include sales, an indicator for government ownership, the PPE-to-assets ratio, ROA as well as market leverage as control variables into the regressions reported in Section 5.

Table 4.2 – **Descriptive statistics by treatment assignment**

The table displays the mean (standard deviation in parentheses) of financial variables for the treatment and control group over the sample period. All variables are measured in annual intervals. The third column gives the difference in means and the associated standard error. The number of employees and the average wage are in thousands. One, two and three asterisks denote statistical significance of the difference in means at the 10, 5 and 1% level respectively.

| | Control | Treatment | Difference |
|---|---|---|---|
| Sales | 3,665.209 | 8,609.176 | -4,943.967*** |
| | (9,352.77) | (15,773.24) | (735.55) |
| MV of equity | 3,556.194 | 6,319.080 | -2,762.886*** |
| | (9,485.49) | (13,366.40) | (670.53) |
| Book-to-market | 0.840 | 0.792 | 0.048 |
| | (0.62) | (0.56) | (0.03) |
| Firm age | 70.692 | 67.405 | 3.287 |
| | (62.29) | (52.59) | (3.03) |
| (Capex+R&D)/assets | 0.055 | 0.074 | -0.019*** |
| | (0.06) | (0.07) | (0.00) |
| PPE/assets | 0.163 | 0.241 | -0.078*** |
| | (0.16) | (0.20) | (0.01) |
| Employees | 17.693 | 24.098 | -6.404** |
| | (56.60) | (42.69) | (3.09) |
| Avg. wage | 54.777 | 59.167 | -4.390** |
| | (23.61) | (29.10) | (1.86) |
| ROA | 0.072 | 0.050 | 0.022*** |
| | (0.07) | (0.08) | (0.00) |
| ROS | 0.082 | 0.055 | 0.027** |
| | (0.12) | (0.23) | (0.01) |
| Book leverage | 0.341 | 0.399 | -0.058*** |
| | (0.22) | (0.24) | (0.01) |
| Market leverage | 0.277 | 0.327 | -0.050*** |
| | (0.23) | (0.24) | (0.01) |
| Dividends/eq. | 0.037 | 0.044 | -0.007** |
| | (0.05) | (0.07) | (0.00) |
| Buybacks/eq. | 0.010 | 0.009 | 0.001 |
| | (0.03) | (0.03) | (0.00) |
| M&A volume | 31.533 | 31.688 | -0.155 |
| | (157.28) | (154.01) | (8.02) |
| M&A count | 0.488 | 0.439 | 0.048 |
| | (1.00) | (0.91) | (0.05) |
| State ownership | 0.005 | 0.078 | -0.073*** |
| | (0.07) | (0.27) | (0.01) |
| Observations | 1,224 | 537 | 1,761 |

# 5 Results

Section 5.1 discusses the difference-in-differences methodology used to test most of the hypotheses in this paper. Section 5.2 then applies this methodology to test whether the Alstom Decree decreased affected firms' risk of becoming the target of a takeover. Section 5.3 reports what impact the Alstom Decree had on the market value of affected firms based on an event study. Sections 5.4 to 5.6 apply the difference-in-differences methodology to study firms' investment and employment policies, operating performance, capital structure and cash distributions to shareholders. Finally, Section 5.7 presents tests for changes in the level and composition of executive compensation.

## 5.1 Difference-in-differences methodology

Several sections hereafter use the same difference-in-differences methodology to assess the impact of the Alstom Decree on various variables of interest. The corresponding regression equation is

$$y_{ijt} = \alpha + \beta_1 Treatment_i \times Post_t + \beta_2 Treatment_i + \beta_3 Post_t + \Gamma' X_{ijt} + \delta_t + \theta_j + \varepsilon_{ijt} \quad (5.1)$$

where $i$ indexes firms, $j$ indexes industries and $t$ indexes time, $y_{ijt}$ is the variable of interest, $\alpha$ is the intercept, the $\delta_t$ are year fixed effects, the $\theta_j$ are industry fixed effects formed on 49 Fama-French industry groups, $X_{ijt}$ is a vector of control variables, and $\varepsilon_{ijt}$ is the error term. $Treatment_i$ is an indicator variable that takes a value of one if firm $i$ belongs to one of the five affected industry groups and zero otherwise. $Post_t$ takes a value of one if the date of the observation is later than May 14, 2014. I calculate robust standard errors and cluster them by firm.

The control variables are chosen to account for differences between the treatment and control group and generally include the natural logarithm of sales or revenues, an indicator variable for whether the government holds a stake in the firm, the return on assets, financial leverage and the PPE-to-assets ratio. These variables are also known determinants of some of the dependent variables, such as executive compensation and the average wage. Furthermore, some of the regressions contain additional control variables that the literature and economic theory have identified as important determinants of the dependent variable: the book-to-market ratio and firm age are included into regressions for investment, capital structure and payout policies. In addition, firm age is included into regressions for executive compensation and profitability. When they are included as control variables, I restrict the return on assets and the PPE-to-asset, book-to-market and leverage ratios in the post-treatment period to their last pre-treatment observation. The reason is that by the predictions of agency theory and the literature on managerial preferences they are potentially affected by the treatment themselves, which could introduce a sample bias also known as the *bad control* problem (Angrist and Pischke, 2009). The results and the conclusions derived from them do not change significantly when this adjustment is not made.

## 5.2 Impact on probability of being acquired

I investigate whether the Alstom Decree led to a decrease in affected firms' likelihood of being acquired by estimating a difference-in-differences regression in which the dependent variable is an indicator equal to one if the firm is successfully acquired in the year of observation and zero otherwise. The sample for this test is the one summarized in Panel C of Table 4.1, which includes privately held firms from the Capital IQ database. Therefore, the tests include an additional control variable indicating whether the firm is publicly listed. I estimate separate regressions for all takeover bids, cross-border bids only and domestic bids only. The results are presented in Table 5.1 below and indicate that firms affected by the Alstom Decree faced a significantly lower risk of being acquired in the post-treatment period.

The coefficients for $Treatment \times Post$ in columns one and two indicate that firms subject to the Alstom Decree have been about 0.8 percentage points less likely to be acquired each year in the post-treatment period, with statistical significance just above the 5% threshold. This effect is large in economic terms, amounting to 40% of the unconditional annual takeover probability. The tests for cross-border bids in columns four and five suggest that the reduction is driven by a 0.5 percentage point decrease in the probability of a cross-border transaction, statistically significant at the 5% level. Again, this effect is large in economic terms, amounting to 77% of the unconditional annual probability of becoming the target of a cross-border acquisition. While the economic magnitude of these results is large, they are in line with previous research. Dinc and Erel (2013), for example, find that the 50 largest listed companies in a country are between 58% and 93% less likely to become the target of a foreign acquisition bid over a time window from one half to two and a half years after a nationalist intervention into a previous foreign acquisition attempt. Similarly, Godsell et al. (2018) find that a 2007 law that made foreign acquisitions of certain US firms subject to increased scrutiny reduced those firms' likelihood of becoming the target of a foreign takeover by 74% relative to unaffected firms. Finally, the tests for domestic bids in columns seven and eight serve as a placebo test and do not provide any evidence for a change in the probability of being acquired domestically, which is consistent with the Alstom Decree being the cause of the observed reduction in overall takeover probability. The control variables suggest that larger, less profitable and publicly traded firms are less likely to become subject to any type of acquisition, be it domestic or cross-border. Since the independent variable for this test is a binary one, A.2 provides an alternative specification based on logistic regression; its results are both economically and statistically similar to the main specification.

The firms in the sample studied in this subsection are relatively small, with mean yearly revenues of only 213.1m euro compared to mean net sales of 5.2bn in the main sample. A potential concern could therefore be that, given their potentially bigger importance for national security, only large firms should be affected by protectionist legislation such as the Alstom Decree, and that the results of the test above might not apply to the average publicly listed firm studied in the remainder of this paper. But in case large firms were disproportionately affected by the law, the results of the difference-in-differences analysis above would *under*estimate

Table 5.1 – **The Alstom Decree's impact on firms' likelihood of becoming an acquisition target**

The coefficients displayed in the table have been estimated using ordinary least squares. The dependent variable is an indicator for whether the firm is acquired during the year of observation. Firm characteristics are lagged by one year. The sample ranges from 2011 to 2016 and contains publicly listed and privately held firms incorporated in France with revenues exceeding 5m euros. Financial firms and the defense industry have been excluded from the sample. Treated firms are firms active in one of the industries mentioned by the Alstom Decree. Large firms are those in the top revenue decile at the beginning of the sample period. Parentheses contain t-statistics calculated from robust standard errors clustered by firm. One, two and three asterisks denote statistical significance at the 10, 5 and 1% level respectively.

| | All bids | | | Cross-border | | | Domestic | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treatment×Post | -0.008* | -0.008* | -0.009* | -0.005** | -0.005** | -0.006** | -0.003 | -0.003 |
| | (-1.94) | (-1.92) | (-1.80) | (-1.98) | (-2.00) | (-2.09) | (-0.95) | (-0.93) |
| Treatment | -0.000 | 0.002 | 0.003 | 0.002 | 0.003 | 0.003 | -0.002 | -0.001 |
| | (-0.03) | (0.71) | (0.78) | (1.15) | (1.50) | (1.50) | (-0.87) | (-0.22) |
| Post | -0.001 | -0.000 | 0.000 | -0.003 | -0.003 | -0.003 | 0.002 | 0.003 |
| | (-0.13) | (-0.02) | (0.03) | (-0.59) | (-0.57) | (-0.51) | (0.39) | (0.52) |
| ln(Revenues) | | -0.004*** | -0.005*** | | -0.001*** | -0.001*** | | -0.003*** |
| | | (-9.82) | (-7.29) | | (-3.53) | (-3.13) | | (-9.43) |
| ROA | | 0.025*** | 0.025*** | | 0.014** | 0.013** | | 0.011* |
| | | (2.89) | (2.89) | | (2.43) | (2.42) | | (1.68) |
| Book leverage | | -0.001 | -0.001 | | 0.001 | 0.001 | | -0.002 |
| | | (-0.28) | (-0.27) | | (0.76) | (0.76) | | (-0.85) |
| PPE/assets | | 0.005 | 0.005 | | -0.001 | -0.001 | | 0.006 |
| | | (0.99) | (1.02) | | (-0.39) | (-0.38) | | (1.43) |
| Firm is public | | -0.007*** | -0.008*** | | -0.003** | -0.003** | | -0.004*** |
| | | (-3.73) | (-3.95) | | (-2.15) | (-2.27) | | (-3.08) |
| State own. | | 0.003 | 0.004 | | -0.002 | -0.002 | | 0.005 |
| | | (0.80) | (0.96) | | (-0.54) | (-0.62) | | (1.37) |
| Treatment×Post×Large | | | 0.007 | | | 0.006 | | |
| | | | (0.91) | | | (1.15) | | |
| Post×Large | | | -0.004 | | | -0.003 | | |
| | | | (-1.12) | | | (-1.34) | | |
| Treatment×Large | | | -0.004 | | | -0.002 | | |
| | | | (-0.77) | | | (-0.53) | | |
| Large | | | 0.007** | | | 0.003 | | |
| | | | (2.06) | | | (1.50) | | |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.00 | 0.01 | 0.01 | 0.00 | 0.00 | 0.00 | 0.00 | 0.01 |
| Observations | 30,610 | 30,610 | 30,610 | 30,610 | 30,610 | 30,610 | 30,610 | 30,610 |

the treatment effect for large firms because the treatment effect represents an average across firms of all sizes. In addition, there are several recent examples of governments intervening in acquisitions of relatively small firms on national security grounds, such as those of Aixtron SE (Germany, 2016, annual sales below 200m euros), Lattice Semiconductor Corporation (United States, 2017, annual sales below $400m) and UDC Finance Limited (New Zealand, 2018, annual revenues below $100m), which should somewhat alleviate these concerns.

To test formally whether the decrease in the probability of being acquired is concentrated in large firms, I estimate two additional specifications in which the variables identifying the treatment group, the post-treatment period and the *Treatment*×*Post* interaction term are again interacted with an indicator *Large* for firms in the top revenue decile. The coefficient on the triple interaction *Treatment*×*Post*×*Large* then captures the causal effect of the Alstom Decree on large firms net of its effect on small firms.[1] The results of these tests are displayed in columns three and six of Table 5.1. The coefficient estimates for the triple interaction are statistically insignificant in both columns. Therefore, these additional tests do not provide any evidence that large firms protected by the Alstom Decree are affected differently than small firms.

## 5.3 Impact on market value

This section tests the Alstom Decree's impact on shareholder value by means of an event study. Standard event study methodology assumes that abnormal returns are uncorrelated cross-sectionally, which implies covariances between different observations are zero and therefore allows for easy significance testing. This assumption clearly does not hold in the case of the Alstom Decree since the event window spans the same calendar dates for all securities. Therefore, I aggregate returns for treated and control firms respectively into equal-weighted portfolios and then analyze each portfolio's returns by calculating standard errors based on the time-series variation of returns as suggested by (Campbell et al., 1997). I closely follow Eckbo et al. (2016) in the setup of the event study.

In addition to testing whether abnormal returns for treated and control firms are different from zero, I also test whether there is a significant difference between the portfolio of treated and control firms. For this purpose, a third return series is calculated as the difference between the two portfolios (treated minus control), which is equivalent to the returns an investor would have experienced from holding a long position in the portfolio of treated firms and a short position of equal size in the portfolio of untreated firms.

The estimation window for the event study spans a maximum of 250 trading days extending

[1]Formally, the coefficient provides an estimate for the expression $\{(E[y|Treatment, Large, Post] - E[y|Treatment, Large]) - (E[y|Treatment, Post] - E[y|Treatment])\} - \{(E[y|Large, Post] - E[y|Large]) - (E[y|Post] - E[y])\}$ i.e. the difference in the change in probability of being acquired for large firms protected by the Decree relative to that for small firms, net of the concurrent difference in changes between large and small control firms.

backwards from the event day. The event day is May 15, 2014, as the Alstom Decree was published in the evening outside of trading hours on May 14. For robustness, the event study uses two different return generating processes, namely a constant mean return and a market model. The constant mean return model entails estimating the regression equation

$$r_{it} - r_{ft} = \alpha_i + AR_i \times d_t + \varepsilon_{it} \tag{5.2}$$

using ordinary least squares, where $r_{it}$ is the daily log-return on the securities portfolio $i$, $r_{ft}$ is the risk-free rate of interest proxied for by the Euro Interbank Offered Rate (EURIBOR) obtained from Capital IQ, $d_{it}$ is an indicator variable equal to one if $t$ is within the event window and zero otherwise and $\varepsilon_{it}$ is the error term. The market model I estimate is

$$r_{it} - r_{ft} = \alpha_i + \beta_i \times (MSCI_t - r_{ft}) + AR_i \times d_t + \varepsilon_{it} \tag{5.3}$$

where $MSCI_t$ is the gross return of the MSCI World Index in euros and the other variables are defined as above. I use the MSCI World Index rather than the French CAC40 because the treatment and control portfolios being analyzed together account for most of the French public equity market capitalization. When calculating portfolio returns, thinly traded observations with daily trading volume below 1,000 euros (which account for less than 2% of the sample) or missing trading volume are dropped. Furthermore, I require stocks to have at least 200 non-missing, non-thinly traded observations in the estimation window and no missing observations in the event window for inclusion in the respective portfolios. I calculate robust standard errors for all regression coefficients including $d_t$.

Table 5.2 displays cumulative abnormal returns (CAR) and their t-statistics for three different choices of the event window: a single-day window, a two-day window including one day before the event, and a three-day window containing both one pre- and one post-event day. The CAR for portfolio $i$ is calculated as $T \times AR_i$, where $T$ is the length of the event window.

For both return generating processes, both treated and control firms experience negative abnormal returns after the announcement of the Alstom Decree. Based on the market model, treated firms experience a cumulative abnormal return between -1.11% and -2.22%, whereas the CAR for control firms lies between -0.52% and -1.37%, depending on the event window. The cumulative abnormal return for the long-short portfolio, which represents the Alstom Decree's causal impact on firm value, amounts to -0.59% for the single-day event window and is statistically significant at the 1% level. When the event-window is extended to include one day before and one day after the event, the CAR rises to -0.85%, statistically significant at the 5% level. The constant mean return model delivers similar cumulative abnormal returns between -0.71% and -0.98% for the long-short portfolio. For both return generating processes, the inclusion of one pre-event day hardly affects the coefficient estimate for the abnormal return but leads to a loss in statistical significance. This is reassuring from an identification

Table 5.2 – **Cumulative abnormal stock returns following the Alstom Decree**

Firms have been assigned to the treatment and control group based on 4-digit SIC codes. The estimation window contains a maximum of 250 trading days of observations extending back from the event-day. The two return generating processes used in Panels A and B respectively are specified as in equations (5.2) and (5.3). The event window is specified in the form (pre-event days; post-event days) and the event day is May 15, 2014. Observations with daily trading volume below 1,000 euros have been excluded. Furthermore, I require stocks to have at least 200 non-missing observations in the estimation window and no missing observations in the event window for inclusion in the respective portfolios. The coefficients represent cumulative abnormal returns. t-statistics calculated from robust standard errors are given in parentheses. One, two and three asterisks denote statistical significance at the 10, 5 and 1% level respectively.

| Panel A: Constant mean return model | | | |
|---|---|---|---|
| Event window | Treatment | Control | Treatment-Control |
| (-0;0) | -0.0161*** | -0.0091*** | -0.0071*** |
| | (-40.97) | (-28.51) | (-40.25) |
| (-1;0) | -0.0180* | -0.0104* | -0.0076 |
| | (-1.77) | (-1.87) | (-1.65) |
| (-1;1) | -0.0276*** | -0.0178*** | -0.0098** |
| | (-2.74) | (-2.79) | (-2.03) |
| Panel B: Market model | | | |
| Event window | Treatment | Control | Treatment-Control |
| (-0;0) | -0.0111*** | -0.0052*** | -0.0059*** |
| | (-19.47) | (-10.47) | (-21.35) |
| (-1;0) | -0.0117 | -0.0056 | -0.0062 |
| | (-1.56) | (-1.57) | (-1.54) |
| (-1;1) | -0.0222*** | -0.0137** | -0.0085** |
| | (-2.79) | (-2.20) | (-2.19) |

perspective as it suggests that the market did not anticipate the Alstom Decree on the day before its announcement.

One way to put these results into perspective is to compare them to studies of anti-takeover laws. Karpoff and Malatesta (1989), for example, find two-day cumulative abnormal returns of -0.47% for firms incorporated in states that introduce a business combination law. The abnormal returns following the Alstom Decree are therefore slightly larger but of the same order of magnitude.

Possible reasons for the negative CAR on the portfolio of control firms are fourfold. First, market participants could have interpreted the Alstom Decree as a signal for more future government intervention, thereby affecting the value of all firms incorporated in France. Second, Dinc and Erel (2013) find that when governments block cross-border M&A for nationalist reasons, foreign companies become less likely to bid for domestic targets in the future; hence there is a potential spill-over effect from treated to control firms. Third, firms with several business segments are classified as untreated if their main activity (and therefore the SIC code they report) does not fall into a treated industry even if one of their smaller segments belongs into a treated industry and therefore makes them subject to the Alstom Decree. Fourth and perhaps most likely, because the market benchmark used is an international one, news about the macroeconomy arriving around the same time could have led to negative returns for both treated and control firms. However, such news should not fundamentally affect the abnormal return estimates for the difference portfolio, because the treatment and the control group have similar market betas with respect to the French public equity market.

For robustness, A.2 displays event study results from cross-sectional estimates. The resulting estimates are close to those presented above, and statistically significant at the 5% level or below for all three event windows and both return generating processes.

## 5.4 Impact on employment and investment policies

One of the main predictions of the quiet life hypothesis is that managers of firms protected from takeovers increase wages and employment. Table 5.3 below tests this relation by means of a difference-in-differences analysis for the average wage and employment (both in natural logarithms) around the Alstom Decree. Column two controls for the natural logarithm of total assets, sales, market capitalization and employment as in Bertrand and Mullainathan (1999b), while columns three and five include the standard set of controls. There is no evidence for a significant increase in either the average wage or the number of employees at affected firms. Therefore, the tests do not provide evidence in favor of the quiet-life hypothesis.

The control variables reveal that wages are decreasing in the return on assets, total employment and market leverage and increasing in sales and in the presence of government ownership. The positive relation between wages and sales is consistent with the literature (Bertrand and Mullainathan, 1999b). The negative relation between wages and employment in column two

Table 5.3 – **The Alstom Decree's impact on wages and employment**

The coefficients displayed in this table were estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France with more than 5m in sales and a market capitalization above 75m euros. Financial firms and the defense industry have been excluded. Treated firms are active in one of the industries mentioned by the Alstom Decree. Wage is the firm-wide average wage. Employment is the number of employees measured in thousands. Parentheses contain t-statistics calculated from robust standard errors clustered by firm. One, two and three asterisks denote statistical significance at the 10, 5 and 1% level respectively. Total assets, market value of equity and total employment (as in Column 2) are the control variables used by Bertrand and Mullainathan (1999b).

| | ln(Wage) | | | ln(Employment) | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treatment×Post | -0.029 | -0.024 | -0.019 | 0.136 | 0.029 |
| | (-0.21) | (-0.18) | (-0.14) | (0.99) | (0.39) |
| Treatment | -0.129 | -0.090 | -0.073 | 1.115*** | 0.020 |
| | (-0.82) | (-0.53) | (-0.47) | (2.67) | (0.12) |
| Post | -0.448*** | -0.441*** | -0.429*** | 0.189 | 0.014 |
| | (-3.23) | (-3.30) | (-2.97) | (0.47) | (0.08) |
| ln(MV) | | 0.023 | | | |
| | | (0.36) | | | |
| ln(Assets) | | -0.049 | | | |
| | | (-0.44) | | | |
| ln(Employment) | | -0.489*** | | | |
| | | (-7.03) | | | |
| ln(Sales) | | 0.495*** | -0.019 | | 0.982*** |
| | | (4.04) | (-0.66) | | (34.82) |
| State own. | | | 0.491* | | -0.206 |
| | | | (1.72) | | (-0.78) |
| ROA | | | -1.417** | | -0.051 |
| | | | (-2.02) | | (-0.11) |
| PPE/assets | | | -0.205 | | -0.089 |
| | | | (-0.69) | | (-0.24) |
| Market leverage | | | -0.453* | | 0.425* |
| | | | (-1.86) | | (1.93) |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.11 | 0.21 | 0.12 | 0.24 | 0.88 |
| Observations | 1,289 | 1,289 | 1,289 | 1,324 | 1,324 |

on the other hand likely arises from a division bias as the wage is being calculated as a fraction with employment in the denominator (Borjas, 1980). Wages being negatively related to the return on assets follows mechanically from the distribution of income between investors and employees. Furthermore, wages are higher at firms with partial state ownership, which could be the result of a political mandate. In addition, wages are negatively related to market leverage. Such a negative relation results for example if firms use debt financing as a bargaining tool in wage negotiations with organized labor because debt allows firms to commit future cash flows to interest payments, leaving less room for wage increases (Bronars and Deere, 1991; Matsa, 2010). Finally, the regression for employment in column five indicates that firms with higher sales employ more people, which is expected as producing more output generally requires more labor input.

Both the quiet life and empire building hypothesis make predictions for the Alstom Decree's impact on firm-level investment policies. Whereas empire building implies an increase in all measures of investments, quiet life preferences predict a negative impact on both the number and size of M&As treated firms engage in. Table 5.4 displays the results for a difference-in-differences analysis on several measures of investment and M&A spending. The dependent variables are the natural logarithm of capital expenditures (capex) and R&D expenses, capex and R&D scaled by total assets, the capex-to-asset ratio, and the number as well as the volume of mergers and acquisitions. An indicator variable *R&D missing* is included into regressions in which R&D is part of the dependent variable to prevent a bias in case firms that report R&D expenses are systematically different from firms that do not. Furthermore, both the natural logarithm of firm age and the book-to-market ratio are included as controls to proxy for firms' investment opportunity set.

All coefficients on $Treatment \times Post$ in Table 5.4 are statistically insignificant, which means firms' investment policies following the Alstom Decree did not change in accordance with either theory of managerial preferences.

The coefficients on the control variables suggest that older firms spend less on R&D and capital expenditures as a fraction of existing assets and have lower M&A volumes, which could be explained by slower growth or poorer investment opportunities as firms age. Consistent with the Q-theory of investment (e.g., Hayashi, 1982), book-to-market ratio is negatively related to all three measures of capital expenditures. Firms with a relatively low valuation (i.e. a high book-to-market ratio) are also less likely to engage in M&As, which can be interpreted both according to a misvaluation (Shleifer and Vishny, 2003; Dong et al., 2006) or a Q-theory of takeovers (Jovanovic and Rousseau, 2002). Firms with higher R&D expenses are less profitable; because the majority of R&D spending is generally expensed through the income statement, this type of relation between R&D expenses and ROA arises mechanically. Finally, firms partly owned by the government have higher investment ratios and engage less frequently in M&A compared to their peers, reflecting that most of them are active in capital intensive, mature and regulated sectors.

Table 5.4 – **The Alstom Decree's impact on investment and M&A**

The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France with more than 5m in sales and a market capitalization above 75m euros. Financial firms and the defense industry have been excluded. Treated firms are active in one of the industries mentioned by the Alstom Decree. The number of M&As is the number of transactions listed in Capital IQ for the firm-year in which the acquirer holds less than 50% before the transaction and 100% afterwards. The transaction volume is calculated from the subset of transactions fulfilling the same criteria and in which in addition the total consideration paid was disclosed. Parentheses contain t-statistics calculated from robust standard errors clustered by firm. One, two and three asterisks denote statistical significance at the 10, 5 and 1% level respectively.

| | ln(Capex+R&D) | | (Capex+R&D)/assets | | Capex/assets | | M&A count | | ln(1+M&A vol.) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treatment×Post | 0.000 | -0.107 | -0.006 | -0.007 | -0.003 | -0.003 | -0.142 | -0.160 | -0.033 | -0.072 |
| | (0.00) | (-1.40) | (-1.09) | (-1.23) | (-0.82) | (-0.96) | (-1.42) | (-1.58) | (-0.18) | (-0.39) |
| Treatment | 1.500*** | 0.332 | 0.008 | 0.008 | 0.007 | 0.005 | 0.248 | 0.092 | 0.369* | 0.013 |
| | (3.71) | (1.48) | (0.96) | (0.99) | (1.12) | (0.80) | (1.19) | (0.50) | (1.82) | (0.07) |
| Post | 0.280 | 0.044 | -0.016 | -0.009 | -0.004 | -0.002 | 0.021 | -0.034 | 0.249 | 0.228 |
| | (0.60) | (0.18) | (-0.88) | (-0.51) | (-0.51) | (-0.30) | (0.10) | (-0.16) | (0.88) | (0.84) |
| R&D missing | -1.719*** | -1.070*** | -0.039*** | -0.040*** | | | | | | |
| | (-6.69) | (-8.20) | (-5.37) | (-5.49) | | | | | | |
| ln(Firm age) | | -0.122 | | -0.009** | | -0.002 | | 0.051 | | -0.231*** |
| | | (-1.28) | | (-2.38) | | (-0.83) | | (0.60) | | (-3.39) |
| ln(Sales) | | 0.938*** | | -0.001 | | 0.000 | | 0.161*** | | 0.313*** |
| | | (21.58) | | (-0.75) | | (0.19) | | (6.65) | | (6.61) |
| State own. | | 0.673** | | -0.004 | | 0.004 | | -0.416** | | -0.462 |
| | | (2.33) | | (-0.26) | | (0.53) | | (-2.13) | | (-1.27) |
| ROA | | -2.305*** | | -0.097* | | 0.020 | | 0.159 | | -0.323 |
| | | (-2.73) | | (-1.67) | | (0.80) | | (0.32) | | (-0.46) |
| Book-to-market | | -0.337** | | -0.014** | | -0.009** | | -0.206** | | -0.290** |
| | | (-2.16) | | (-2.25) | | (-2.26) | | (-2.51) | | (-2.57) |
| Market leverage | | 0.568 | | 0.009 | | 0.019 | | -0.021 | | -0.216 |
| | | (1.29) | | (0.50) | | (1.65) | | (-0.09) | | (-0.63) |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.35 | 0.83 | 0.32 | 0.35 | 0.14 | 0.15 | 0.07 | 0.16 | 0.04 | 0.12 |
| Observations | 1,327 | 1,327 | 1,327 | 1,327 | 1,327 | 1,327 | 1,353 | 1,353 | 1,353 | 1,353 |

## 5.5 Impact on operating performance

The final prediction of the quiet life hypothesis is that managers' preferences for paying higher wages, increasing employment and avoiding restructuring measures will lead to a decrease in operating performance (Bertrand and Mullainathan, 2003). I test this relation in the context of the Alstom Decree through a difference-in-differences analysis measuring performance by the return on assets (ROA) and the return on sales (ROS). The results are displayed in Table 5.5 below and do not indicate that the Alstom Decree had a statistically significant impact on the profitability of affected companies.

The control variables suggest that firms with partial state ownership are less profitable than their peers, a fact that has been shown before and that is not unique to French state-owned enterprises (Dewenter and Malatesta, 2001). Furthermore, firms with higher sales are more profitable in terms of their return on sales, as one would expect for example in the presence of fixed cost. The PPE-to-asset ratio is positively correlated with the return on sales, possibly reflecting that firms with high fixed assets require a larger return to recover their depreciation expenses. Finally, highly leveraged firms are less profitable both in terms of ROA and ROS. A partial explanation for this fact might be that these firms are experiencing economic distress, causing both high leverage and low profitability.

## 5.6 Impact on capital structure and distributions

Following the agency cost of free cash flow hypothesis (Jensen, 1986), managers prefer to retain rather than distribute the free cash flow generated by the firm, leading to entrenched managers avoiding debt financing, cash dividends and stock buybacks. Table 5.6 tests these predictions in the context of the Alstom Decree using a difference-in-differences analysis. Columns one to four test whether the Alstom Decree led to a change in treated firms' book or market leverage ratio. Columns five to eight test whether the Decree led to a change in cash returned to shareholders either in the form of cash dividends or stock buybacks, measured in both cases as a fraction of total book value of common equity. The tests do not provide any evidence that the Alstom Decree had a statistically significant impact on firms' use of debt financing. Similarly, the coefficients in columns five to eight do not indicate a statistically significant impact on distributions to shareholders.

The coefficient estimates for the control variables are consistent with the literature on capital structure. There is a positive correlation between market leverage and the book-to-market ratio, which some researchers have attributed to market timing, i.e. managers issuing equity when market values are high in relation to book values and repurchasing shares when they are low (Baker and Wurgler, 2002; Rajan and Zingales, 1995). Alternative explanations are that high leverage firms are experiencing financial distress, leading to a higher book-to-market ratio (Fama and French, 1992), or that the correlation arises purely mechanically as an increase in the market value of equity simultaneously decreases market leverage while de-

Table 5.5 – **The Alstom Decree's impact on operating performance**

The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France with more than 5m in sales and a market capitalization above 75m euros. Financial firms and the defense industry have been excluded from the sample. Treated firms are active in one of the industries mentioned by the Alstom Decree. ROA is the return on assets defined as EBIT divided by total assets. ROS is the return on sales calculated as EBIT as a fraction of net sales. Parentheses contain t-statistics calculated from robust standard errors clustered by firm. One, two and three asterisks denote statistical significance at the 10, 5 and 1% level respectively.

| | ROA | | ROS | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatment×Post | -0.002 | -0.001 | -0.004 | -0.005 |
| | (-0.32) | (-0.16) | (-0.25) | (-0.33) |
| Treatment | -0.023** | -0.017 | -0.024 | -0.040 |
| | (-2.17) | (-1.62) | (-1.07) | (-1.56) |
| Post | 0.019 | 0.013 | 0.047 | 0.032 |
| | (0.88) | (0.65) | (1.32) | (0.85) |
| ln(Firm age) | | 0.005 | | 0.006 |
| | | (1.12) | | (0.56) |
| ln(Sales) | | 0.004 | | 0.018*** |
| | | (1.60) | | (2.75) |
| State own. | | -0.022** | | -0.085** |
| | | (-2.13) | | (-1.98) |
| PPE/assets | | 0.018 | | 0.174*** |
| | | (0.73) | | (2.69) |
| Market leverage | | -0.097*** | | -0.098*** |
| | | (-5.45) | | (-2.74) |
| Year FE | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |
| $R^2$ | 0.17 | 0.25 | 0.14 | 0.20 |
| Observations | 1,373 | 1,373 | 1,376 | 1,376 |

Table 5.6 – **The Alstom Decree's impact on capital structure and distributions**

The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France with more than 5m in sales and a market capitalization above 75m euros. Financial firms and the defense industry have been excluded. Treated firms are active in one of the industries mentioned by the Alstom Decree. Book leverage is book debt divided by the sum of book debt and book equity. Market leverage is book debt divided by the sum of book debt, market value of common stock and book value of preferred stock. Dividends/eq. is the fraction of book equity returned to shareholders in the form of cash dividends and Buybacks/eq. is the fraction of book equity returned to shareholders in the form of share repurchases. Parentheses contain t-statistics calculated from robust standard errors clustered by firm. One, two and three asterisks denote statistical significance at the 10, 5 and 1% level respectively.

| | Book leverage | | Market leverage | | Dividends/eq. | | Buybacks/eq. | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treatment×Post | 0.004 | -0.001 | 0.014 | 0.012 | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.26) | (-0.09) | (0.83) | (0.74) | (-0.09) | (-0.04) | (-0.00) | (-0.06) |
| Treatment | 0.093*** | 0.033 | 0.106*** | 0.049* | -0.018*** | -0.013** | -0.003 | -0.003 |
| | (2.74) | (1.03) | (2.95) | (1.96) | (-2.98) | (-2.35) | (-0.84) | (-1.04) |
| Post | -0.075* | -0.078** | -0.037 | -0.056** | 0.008 | 0.005 | 0.000 | 0.000 |
| | (-1.76) | (-2.10) | (-0.91) | (-2.01) | (0.90) | (0.59) | (0.02) | (0.03) |
| ln(Firm age) | | -0.024 | | -0.017 | | 0.003 | | 0.000 |
| | | (-1.60) | | (-1.34) | | (1.00) | | (0.25) |
| ln(Sales) | | 0.035*** | | 0.026*** | | -0.000 | | 0.002** |
| | | (5.27) | | (4.88) | | (-0.15) | | (2.29) |
| State own. | | 0.044 | | 0.114** | | 0.007 | | -0.009*** |
| | | (0.86) | | (2.25) | | (0.76) | | (-2.99) |
| ROA | | -0.468*** | | -0.404*** | | 0.244*** | | 0.000 |
| | | (-4.06) | | (-4.27) | | (5.07) | | (0.00) |
| Book-to-market | | 0.031* | | 0.168*** | | -0.016*** | | -0.005*** |
| | | (1.70) | | (7.93) | | (-5.21) | | (-2.90) |
| PPE/assets | | 0.143* | | 0.105 | | 0.000 | | -0.011* |
| | | (1.66) | | (1.45) | | (0.02) | | (-1.95) |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.34 | 0.44 | 0.36 | 0.60 | 0.16 | 0.31 | 0.07 | 0.09 |
| Observations | 1,342 | 1,342 | 1,347 | 1,347 | 1,339 | 1,339 | 1,216 | 1,216 |

creasing the book-to-market ratio. The positive relation between size and leverage is similarly well-documented, but poorly understood (Rajan and Zingales, 1995). A firm's book equity increases with its historical cash flows, implying a negative correlation between book leverage and ROA as is observable in column two and is known from the literature (e.g. Welch, 2004; Shyam-Sunder and Myers, 1999). ROA and market leverage are similarly negatively correlated. Partial state ownership is associated with higher market leverage, a tendency that has been documented for state-owned enterprises around the world. Dewenter and Malatesta (2001) attribute this fact to state-owned firms being unable to raise equity from third parties and using (sometimes implicit) government guarantees to borrow at below-market rates.

Regarding distributions to shareholders, the negative correlation between buybacks and the book-to-market value in column eight is inconsistent with explanations based on market timing (Baker and Wurgler, 2002), and might be due to a division bias as the dependent variable is scaled by the book value of equity. The relation between dividends and the book-to-market ratio on the other hand has been shown to be time-varying, by some accounts because investors value dividends differently at different times (Baker and Wurgler, 2004). Following this argument, the negative coefficient on the book-to-market ratio in column six suggests that French shareholders placed a premium on dividend-paying stocks over the sample period. Furthermore, cash dividends are positively related to profitability, consistent with the literature (e.g. Fama and French, 2002). Column eight indicates a positive correlation between buybacks and sales, suggesting that large firms are more likely to use share repurchases to return cash to shareholders, consistent with Dittmar (2000). Finally, there is a negative relation between buybacks and partial state ownership, which supports the above-mentioned hypothesis that state-owned firms are constrained in their ability to raise equity, and might therefore be more conservative in distributing retained earnings to shareholders.

## 5.7 Impact on executive compensation

This subsection tests the executive compensation hypothesis, which states that shareholders will attempt to substitute for the loss in oversight exercised by the takeover market by increasing the pay-for-performance sensitivity of executive compensation, and that entrenched executives will seek to increase their total compensation.

I implement the test for the pay-for-performance sensitivity by investigating whether the Alstom Decree has led to an increase in the percentage of annual compensation consisting of equity instruments. This measure of CEO incentive pay has been used previously in the corporate governance literature, for example by Mehran (1995) and Datta et al. (2001). Another outcome variable that has been used extensively in the literature is the dollar change in CEO wealth as a function of the change in the stock price (e.g., Jensen and Murphy, 1990; Core and Guay, 1999). I do not use this measure because new equity grants tend to be small compared to CEOs' existing holdings of common stock, implying that changes in the measure over time will primarily be driven by changes in stock prices and not CEO compensation policies; this importance of the stock price could be problematic in the present case, because the treatment group is defined along industry sectors which are subject to sector-wide trends in valuation.

Table 5.7 displays the results of the difference-in-differences analysis for the Alstom Decree's impact on the total compensation of CEOs and the fraction of it paid out in stock and option grants. There is evidence of an increase in the total compensation of CEOs, with the regression coefficient on $Treatment \times Post$ in column two being statistically significant at the 5% level. The result is also significant in economic terms, as the coefficient of 0.220 suggests an increase of approximately 24.6% in compensation as a consequence of the Alstom Decree. Columns three and four present evidence for an increase in equity-based compensation, also statistically significant at the 5% level. The treatment effect amounts to an 8.4 percentage point increase in the fraction of annual CEO compensation paid out in equity instruments.

I conduct a number of robustness tests for these results discussed in detail in Section 6. The results for equity-based compensation are robust to placebo-tests, several alternative specifications of the outcome variable, and a replacement of the control group by firms operating in the industries affected by the Alstom Decree but incorporated in other EU member states. The results for total compensation on the other hand are mixed, in particular do they not hold up when the control group is replaced in the same manner, allowing for the possibility that it is a trend specific to the firms in the five industries covered by the Decree that is driving the results.

The coefficients for the control variables indicate that the size of the firm as measured by its sales is a significant determinant of both total and equity-based compensation. Surprisingly, the firm's operating performance as measured by the ROA seems negatively correlated with the concurrent compensation of the CEO. However, unreported univariate regressions of the dependent variables in question on ROA lead to positive and statistically significant coefficients that switch signs after conditioning on sales. Hence the negative coefficient estimates for ROA in Table 5.7 are due to multicollinearity, and do not imply that executives of less profitable firms are paid more unconditionally. Finally, total compensation is negatively related to financial leverage. Alternative explanations for this relation could be either that highly paid, entrenched CEOs opt for a low-debt capital structure as predicted by the agency cost of free cash flow literature (e.g., Berger et al., 1997), or that high leverage firms are in economic distress.

Table 5.7 – **The Alstom Decree's impact on executive compensation**

The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France with more than 5m in sales and a market capitalization above 75m euros. Financial firms and the defense industry have been excluded. Treated firms are active in one of the industries mentioned by the Alstom Decree. CEO equity-based compensation is the fraction of the CEO's annual compensation paid out in stock and option grants. CEO total is the CEO's total compensation for the fiscal year. Parentheses contain t-statistics calculated from robust standard errors clustered by firm. One, two and three asterisks denote statistical significance at the 10, 5 and 1% level respectively.

| | ln(CEO total) | | CEO equity-based | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatment×Post | 0.206** | 0.220** | 0.078** | 0.084** |
| | (2.00) | (2.55) | (2.21) | (2.51) |
| Treatment | 0.104 | -0.269 | 0.121** | 0.080* |
| | (0.28) | (-0.86) | (2.28) | (1.76) |
| Post | 0.181 | -0.019 | 0.059 | 0.058 |
| | (0.82) | (-0.11) | (1.17) | (1.13) |
| ln(Firm age) | | 0.098 | | -0.022 |
| | | (1.13) | | (-1.07) |
| ln(Sales) | | 0.420*** | | 0.048*** |
| | | (12.36) | | (6.33) |
| State own. | | -0.319 | | -0.076 |
| | | (-1.26) | | (-0.93) |
| ROA | | -1.598** | | -0.095 |
| | | (-2.25) | | (-0.51) |
| PPE/assets | | -0.408 | | -0.055 |
| | | (-0.98) | | (-0.62) |
| Market leverage | | -0.692** | | -0.137* |
| | | (-2.54) | | (-1.73) |
| Year FE | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |
| $R^2$ | 0.18 | 0.50 | 0.19 | 0.29 |
| Observations | 1,166 | 1,166 | 693 | 693 |

# 6 Robustness

I conduct several additional tests to establish the robustness of the results presented in the preceding section. First, I discuss the potential for pre-treatment trends in the dependent variables under investigation. Second, I provide two sets of alternative specifications for the tests concerned with firm policies presented in Tables 5.3 to 5.7. The first set of specifications uses alternative measures for equity-based and total compensation, and the second one uses a control group of firms operating in the industry sectors specified by the Alstom Decree but incorporated outside of France.

## 6.1 Pre-treatment trends

Difference-in-differences assumes parallel pre-treatment trends for the treatment and control group. Time-series plots of the group means and associated 95% confidence intervals for all independent variables are provided in Figure 6.-1. Visual inspection of these plots suggests that the dependent variables, including total and equity-based CEO compensation, generally seem to follow approximately parallel pre-treatment trends. Potential exceptions are the capex-to-asset ratio and the return on sales; however, these two variables are only two out of a larger number of variables used to measure firms' investment policies and profitability, and should therefore not be driving the overall results and interpretation.

A more formal placebo-test for the takeover probability and executive compensation, being the variables for which the difference-in-differences analysis indicated a statistically significant change, is provided in A.2. The placebo test delivers supporting evidence for parallel pre-treatment trends: when an additional indicator variable based on a hypothetical treatment date one or two years earlier and an interaction term with the treatment group are added to the baseline specification in equation (5.1), both of these additional variables turn out to be statistically insignificant in all but one specification, while the actual treatment effect retains its size and statistical significance.

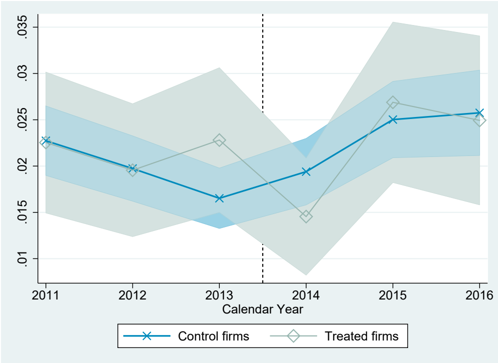
## 6.2 Additional difference-in-differences specifications

I test several additional specifications for total and equity-based executive compensation. The results of these tests are provided in A.2 and discussed below. The increase in CEOs' equity-based compensation persists when not counting shares granted under long term incentive plans towards equity-based compensation. The tests also indicate an increase in the euro amount of equity-based compensation in addition to the increase in the percentage measure shown earlier. When the fraction of stock-based compensation in CEOs' annual compensation is tested by itself on the other hand (i.e. having removed stock option grants from the measure of equity-based compensation), the resulting coefficient loses its statistical significance, implying that grants of stock options are an important part of the observed increase. Furthermore, I find that the median board member at affected firms receives a significantly larger portion of their annual compensation in the form of equity instruments following the Alstom Decree as well. The results indicate an increase of 7.5 percentage points of annual compensation and are close to the 8.4 percentage points identified for CEOs. To determine whether the increase in total CEO compensation is accompanied by an increase in the total compensation of other executives, I estimate the treatment effect for the total compensation of the median board member. The results do not indicate an increase in the total compensation of the median board member following the Alstom Decree. Finally, the results for equity-based and total compensation are robust to the exclusion of all firms in which the French government ever holds a significant equity stake over the sample period.

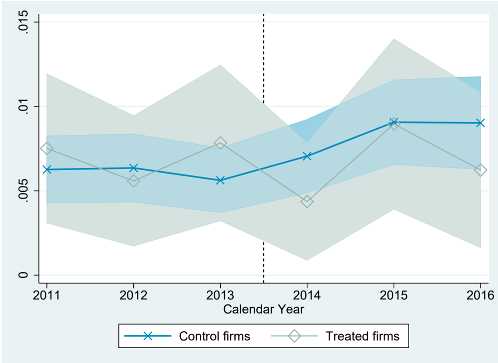
## 6.3 Control group of European firms operating in the same industries

Another potential concern is that firms in the industries affected by the Alstom Decree are different from the control group in some fundamental way, and that they are therefore subject to different trends in executive compensation, starting at the same time as the Alstom Decree. I might then mistakenly identify these trends as consequences of the Alstom Decree, leading to spurious results. If the way I assign treatment status by SIC code is too imprecise, on the other hand, the control group might contain a substantial number of firms affected by the treatment, which would bias the results of the tests for changes in firm policies towards zero. This section provides a set of tests intended to simultaneously address both these concerns by using a control group of non-French firms operating in the industry sectors covered by the Alstom Decree. Because the firms in the control group are not incorporated in France, they are unlikely to be affected by the Alstom Decree, and because they are active in the same industry sectors, industry-trends should not influence the results. For the control group, I use firms incorporated in the five original EU member states besides France, i.e. Germany, Belgium, the Netherlands, Italy and Luxembourg. The reason for this choice is that these countries are geographically and economically close to France and also have legal systems based on civil law.

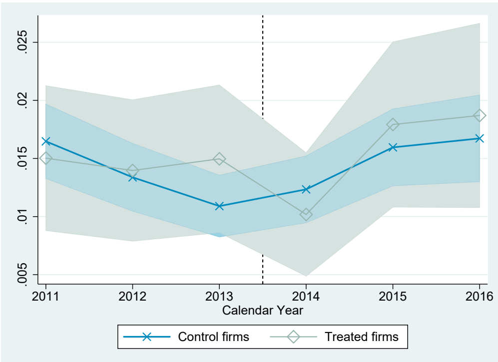
The results of the test are presented in A.3. As for the main specifications, they do not indicate any significant changes in wages, employment, investment policies, profitability, capital structure and distributions to shareholders following the Alstom Decree. The results for executive compensation, on the other hand, lose most of their statistical significance. The estimated treatment effect for total CEO compensation is negative and close to zero. This result allows for an alternative explanation for the increase in CEO compensation observed in the main specification, namely that it is due to an overall trend in the industry sectors covered by the Alstom Decree starting around the same as the treatment date, and not causally related to the legislation itself. The tests for CEO equity-based compensation are closer to the results of the main specification. They indicate a 7 percentage point increase in the share of annual CEO compensation paid out in equity instruments, which is of similar magnitude to the 8.4 percentage point increase estimated using a control group of French firms, although the corresponding coefficient is only statistically significant at the 10% level.



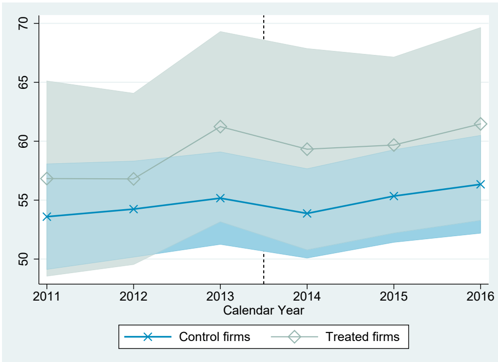

(a) Probability of being acquired



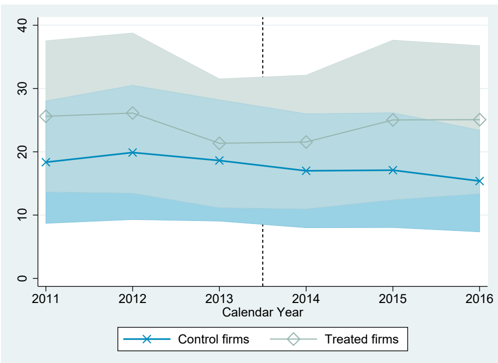

(b) Probability of being acquired cross-border



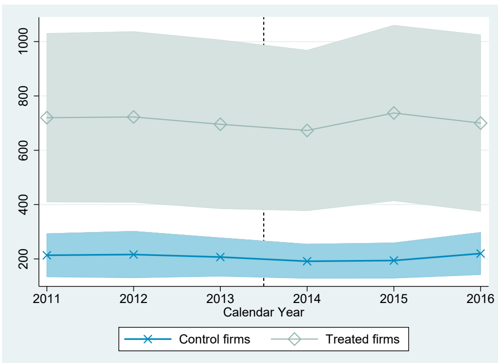

(c) Probability of being acquired domestically



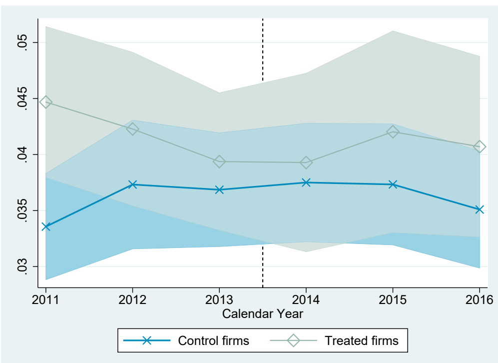

(d) Average wage



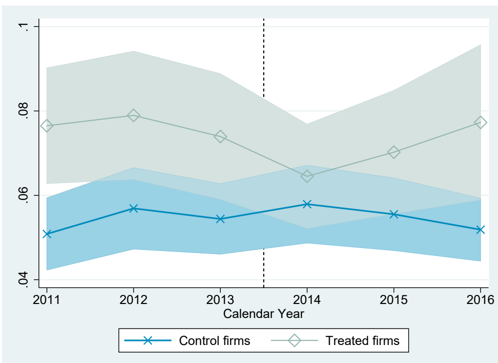

(e) Total employment



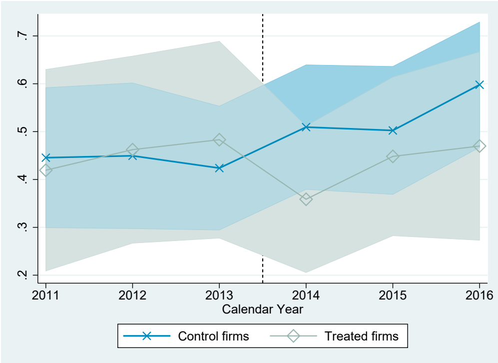

(f) Capex + R&D



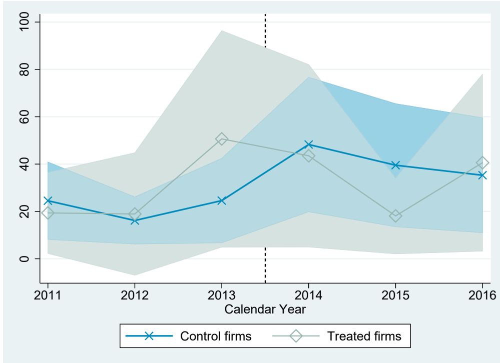

(g) Capex/assets



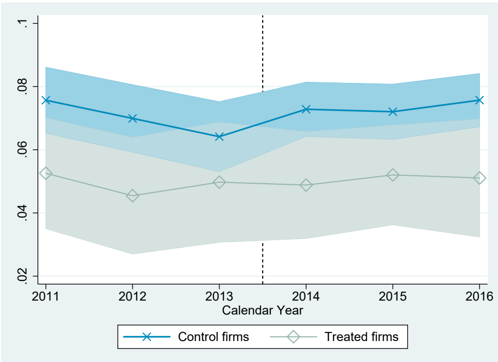

(h) (Capex + R&D)/assets



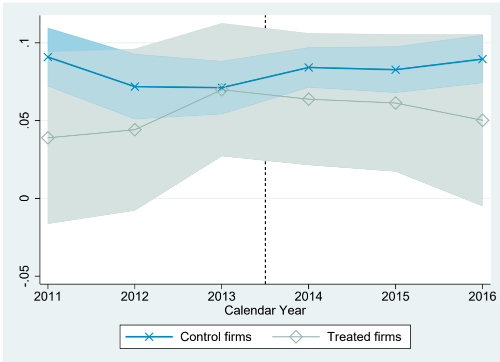

(i) M&A count



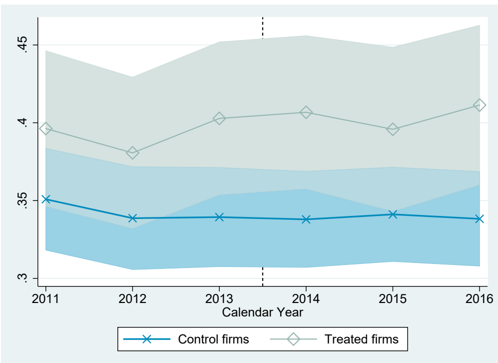

(j) M&A volume



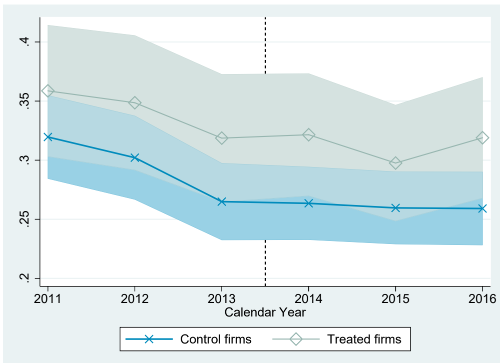

(k) ROA



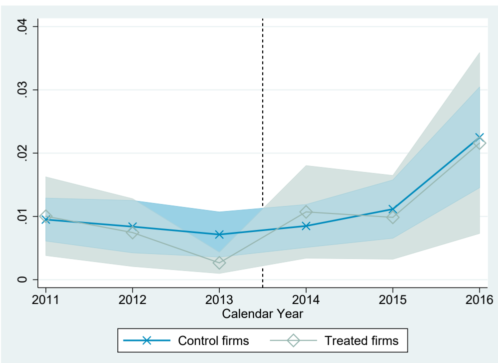

(l) ROS



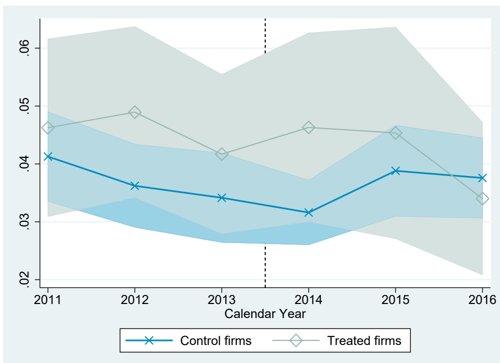

(m) Book leverage



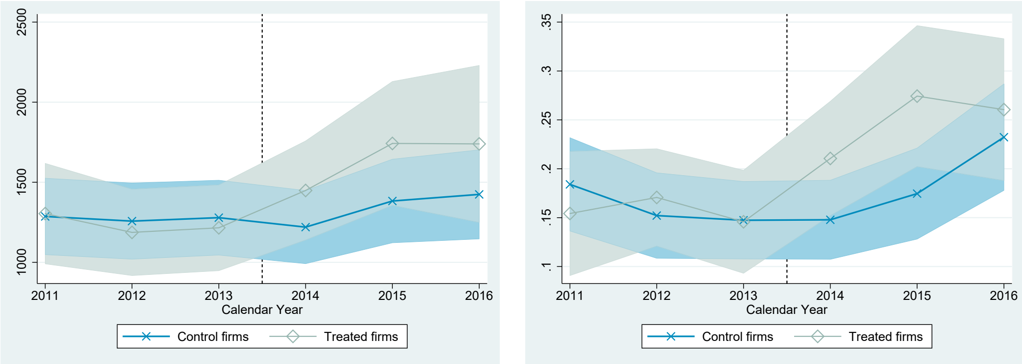

(n) Market leverage




(o) Buyback ratio




(p) Dividend ratio




(q) Total CEO compensation

(r) Equity-based CEO compensation

Figure 6.-1 – The figure displays time-series plots for the mean of the dependent variables for the treatment and control group. The shaded areas represent the 95% confidence interval for the mean. The dashed vertical line illustrates the border between pre- and post-treatment period.

# 7 Conclusion

The corporate governance literature has long described takeovers as a way through which a shareholder or a third party can remove unproductive management to create value. Therefore, recent protectionist interventions into cross-border mergers and acquisitions in the interest of national security have the potential side effect of entrenching management at the expense of shareholders. I investigate whether protectionist anti-takeover legislation leads to managerial entrenchment based on the Alstom Decree, a protectionist anti-takeover law introduced quickly and unexpectedly in France in 2014.

An analysis of M&A rates following the Alstom Decree suggests that the law made it significantly less likely for protected firms to be acquired in the post treatment period; the corresponding decrease in the annual probability of being acquired is large and amounts to 40% of the unconditional probability. Furthermore, firms protected by the Alstom Decree experienced statistically significant negative abnormal returns between -0.59% and -0.98% compared to a control group of unaffected firms.

Despite this measurable decrease in the probability of being acquired and the corresponding decrease in market value, I do not find that the Alstom Decree had an impact on tangible firm characteristics: employment, wages, investment, operating performance, capital structure and cash distributions to shareholders remain unchanged. There is no evidence for *quiet life* (Bertrand and Mullainathan, 2003) or *empire building* (e.g. Baumol, 1959) policies being implemented at protected firms. Furthermore, the managers of firms affected by the Alstom Decree have not retained a larger fraction of the free cash flow by changing the capital structure or payout policies in the period since the law's introduction, contrary to the predictions of the literature on the free cash flow problem (Jensen, 1986). I do find some evidence for an increase in total CEO compensation following the legislation. I also find robust evidence that executive compensation at firms covered by the Alstom Decree has become more equity-based, thereby improving the alignment of incentives between management and shareholders. I interpret this results as an attempt of the board of directors to substitute for the loss in monitoring exercised by the market for corporate control.

Despite the Alstom Decree's negative impact on shareholder value, I find few signs of increased managerial entrenchment at affected firms. Therefore, it is unlikely that increased manager-shareholder conflicts of interest were the main cause of the law's negative effect on market values. A possible alternative explanation is a decrease in the expected present value of the takeover premium included in the share prices of affected firms (Bennett and Dam, 2017).

# Part II

# ICO investors (co-authored with Rüdiger Fahlenbrach)

# 8 Introduction

In an initial coin offering (ICO) an entrepreneur raises capital by selling a newly-minted cryptographic token to the public. The token is usually listed on a specialized exchange quickly after the ICO, creating a secondary market. ICOs have become the prevalent source of financing for start-up companies that use the blockchain technology; more than $30bn have been raised so far through ICOs (Lyandres et al., 2019).[1] Entities conducting ICOs have unproven business models and are most often in the pre-product stage. There exists virtually no hard information on them and asymmetric information is large. The financing of such early stage companies has previously been the domain of highly specialized angel investors or venture capitalists (VCs) who acquire soft information by meeting with potential customers, suppliers, and the team, and by using sophisticated security design methods guaranteeing priority and control rights.

While a significant empirical and theoretical literature on the determinants of post-issue financial success of ICOs has developed, relatively little is known about ICO investors and their reasons to invest. We wish to fill this gap and analyze the composition and trading behavior of the ICO investor base. Most tokens sold in ICOs are "utility" tokens which can be spent to buy a product or service produced by the issuer but do not confer cash flow rights. Our analysis of investor trading behavior seeks to understand whether initial investors primarily buy utility tokens because they are interested in the product (and that therefore, ICOs are a good mechanism for entrepreneurs to understand the market's demand for the products or platform they develop) or for speculative purposes. We use primary sources (such as ICO whitepapers or an ICO's Medium, Twitter and Telegram pages as well as the Ethereum blockchain data) to construct a hand-collected sample of successful ICOs with information on the ICO, investors, governance characteristics, and products offered, to answer these questions.

The median investor in our sample of ICOs invests only $1,200 and each of our sample ICOs has approximately 4,700 investors. ICOs therefore appear to have succeeded in tapping a new type of investor to finance innovation, one that security market regulators typically seek to protect. The typical investor makes active use of the secondary market. He sells a substantial fraction of his tokens shortly following the ICO, when the product of the company is not yet developed, indicating that he is more interested in financial gain than the underlying product. Token returns have high variance and positive skewness; both are attributes that retail investors appreciate (e.g. Goetzmann and Kumar, 2008; Kumar, 2009). In our sample, investors do not hold a diversified portfolio of ICOs in the same wallet.

A key identifying assumption of our analysis is that ICO investors use one wallet to invest in ICOs and do not camouflage their true investment through multiple wallet strategies. We show through several formal tests that the identifying assumption is defendable for the typical ICO investor. Investors frequently use the same wallet with which they invested into the ICO for other transactions on the Ethereum network afterwards, which suggests that they use a wallet

[1] Also see PwC Switzerland, 2019, 5th ICO / STO Report, https://www.pwc.ch/en/publications/2019/ch-PwC-Strategy&-ICO-Report-Summer-2019.pdf.

for multiple purposes. We show that the value of tokens transferred out of investors' wallets is highly correlated with trading volume in secondary markets in the same token, implying that most of these tokens are not moved to another wallet belonging to the same investor but rather sold on an exchange. Finally, for ICOs that have a know your customer (KYC) policy, i.e. where the issuer knows the ultimate beneficial owners of tokens bought in the initial sale, the number of contributors disclosed by the issuer after the offering period is statistically indistinguishable from the number of wallets that contributed. The result suggests that most investors invest with one wallet in these ICOs.

ICOs typically happen in two stages. A majority of ICOs holds a closed presale round for larger investors and insiders, during which the participating investors receive a sizeable discount over regular investors. The second phase is the crowdsale stage during which regular investors participate. In our sample, the median discount to presale investors is an economically large 30%. Presale investors can therefore lock in a profit by selling immediately after the ICO if the prevailing secondary market price is at or above the presale price, which is lower than the "list price" paid by regular investors. We find evidence that they do. Large investors sell earlier if there was a presale and if the presale discount was high, and holding period returns to other investors are decreasing in the amount of funding raised in the presale as well as the presale discount. The analysis of the initial participation and subsequent trading patterns by presale investors illustrates a potential issue with the ICO model. Investments by presale investors provide important information to crowdsale investors who interpret the early investments as a signal of the quality of the ICO (e.g. Howell et al., 2018; Fisch, 2019), but the possibility of flipping the coins purchased at a discount reduces the information content of presale investor purchases.

We find little evidence that ICO investors receive downside protection or governance rights for their investment, as would be typical for VC or angel investors. Most ICOs do not confer residual cash flow rights to investors, let alone give them liquidation preferences or offer board representation. Only 4% of ICOs specify milestones for the release of funds, and only 4% leave an independent custodian in charge of the funds raised by the company. However, we find some evidence for incentive alignment between investors and entrepreneurs in that a majority of issuers lock up at least part of the tokens held by the issuing firm and its founders. The mean weighted average maturity of the tokens retained by the issuing firm and its founders is 1.1 years. We conclude with an analysis of secondary market returns. The single most important driver of ICO returns to investors is the concurrent return of Ethereum. Few other variables reliably predict returns nine months after the ICO. The average gross return (i.e. not adjusted for the returns on Bitcoin or Ethereum) on a token is positive nine months after the ICO. Average returns in excess of the return of Bitcoin or Ethereum are consistently below unadjusted returns nine months after the ICO but are, perhaps surprisingly in light of allegations of widespread fraud and pump-and-dump schemes, still positive. Our paper relates to the literature on the behavior of individual investors (for an overview, see Barber and Odean, 2013). In particular, Barber and Odean (2000) document that in their database of retail investors, investors hold on average an undiversified portfolio of only four stocks.

Goetzmann and Kumar (2008) show that retail investors hold highly volatile stocks with a high correlation, and Kumar (2009) finds that individuals like to hold stock with high idiosyncratic volatility and skewness. Several researchers have pointed out that investors like to gamble with lottery-like stocks (Dorn et al., 2014; Barber et al., 2009; Gao and Lin, 2015; Kumar, 2009). The results of these papers are broadly consistent with our findings on ICO investors and can potentially explain the attractiveness of the asset class to retail investors despite the lack of transparency and investor protection. Our paper is also related to the literature that examines apparently irrational investor behavior in public firms in new industries that promise high growth (e.g. Shiller, 2000). Cooper et al. (2001) document that firms that added ".com" to their name during the internet boom experienced abnormal returns of 53% over the following five days. Cheng et al. (2019) show that investors react positively to vague 8-K announcements of public firms that they are "going to use blockchain technology in the future". Lamont and Thaler (2003) demonstrate that investors irrationally bid up prices of equity carve-outs in U.S. technology stocks during the internet boom. Ofek and Richardson (2003) and Lamont and Thaler (2003) suggest that short sale restrictions may explain the persistence of the mispricing of tech stocks during that time. This literature could help explain investor's appetite for ICOs and the high market valuations, as ICO tokens too are difficult and risky to short.

Our work contributes to an emerging literature on ICOs. Most empirical papers on ICOs relate ICO characteristics collected by secondary sources to measures of ICO success. Contrary to those papers, we focus on the investors in ICOs instead of the issuers of ICOs. Of the large literature on ICOs, few papers have investigated ICO investors. The only academic analyses of investors in the ICO market so far are – to the best of our knowledge – Howell et al. (2018), Lee et al. (2018), and Boreiko and Risteski (2019). Howell et al. (2018) provide a case study of the investors in the Filecoin ICO, which is interesting but also fairly special because the Filecoin ICO allowed only accredited investors. Lee et al. (2018) use individual investor contribution data to study how quickly the ICO reaches its soft cap and to test the theory of the wisdom of the crowds, and Boreiko and Risteski (2019) analyze investor data to show that only large investors have some ability to time the market and select better ICOs. Many firms issuing ICOs develop a decentralized trading platform that promises network effects, and much of the emerging theoretical ICO literature has focused on the conditions under which ICOs can create value by solving coordination problems (Bakos and Halaburda, 2018; Catalini and Gans, 2018; Cong et al., 2018; Li and Mann, 2018; Sockin and Xiong, 2018). Other theoretical work includes Chod and Lyandres (2018) and Lee and Parlour (2019). The law literature has also started to discuss the legal and regulatory framework for ICOs (e.g. Kaal, 2018; Maas, 2019; Robinson, 2018; Rohr and Wright, 2017; Zetzsche et al., 2017).

The remainder of our paper proceeds as follows. Section 9 discusses the data collection procedure. Section 10 presents a brief overview of the ICO market. Section 11 presents the results of our analysis of the characteristics and behavior of ICO contributors. Section 12 contrasts investor protection provisions in venture capital and angel financing with those in ICOs. Section 13 presents regression estimates for whether investor and ICO characteristics matter as determinants of secondary market returns and Section 14 concludes.

# 9 Data collection

## 9.1 Primary market data

We hand-collect data on token sales from primary sources. Our reasons for hand-collecting data are twofold: concerns about data quality and the amount of data items available from secondary sources. Secondary sources often diverge substantially in their assessment of an ICO (see Boreiko and Sahdev, 2018; Lyandres et al., 2019, for a systematic analysis of these concerns). Hand-collection also allows the inclusion of important characteristics that are not available from secondary sources but are important for our study of ICO investors and investor protection. We collect information on the exact split of funds raised from presale and crowdsale investors, the pricing schedules for both, founder token vesting schedules and whether a venture capitalist has invested into the issuer prior to the ICO. The pricing schedules in particular are important to gain an accurate picture of returns to investors, as discounts given to early and large investors are often sizeable.

To construct our sample, we first create a list of completed ICOs from four secondary sources (icorating.com, smithandcrown.com, icowatchlist.com and coinschedule.com).[1] Appendix B.1 provides the full list of sample ICOs. For the characteristics of those ICOs, we rely exclusively on primary sources such as whitepapers or other documents published by issuers, archived issuer websites kept by the Internet Archive (web.archive.org), company announcements on social media (primarily on Medium, Twitter and Telegram), source code on Github, company announcements on the bitcointalk.org message boards and various national commercial registers. Furthermore, we sometimes consult the Crunchbase database for information on venture funding. Appendix B.1 defines all collected attributes in detail.

Our final sample consists of 306 ICOs that collectively raised over $6.2b in funding between March 2016 and March 2018.[2] In 2017 alone, they raised $5 billion.

## 9.2 Secondary market data

We retrieve secondary market prices in US dollars from coinmarketcap.com. The webpage aggregates traded prices from all cryptocurrency exchanges that provide data on prices and trading volumes through a public application programming interface, and then calculates volume-weighted average daily open, high, low and closing prices. To prevent a survivorship bias that might arise if coinmarketcap deleted information for bankrupt or fraudulent ICOs, we downloaded bi-weekly snapshots of the data since the start of the research project in February 2018 and consecutively merged those snapshots to present a picture of secondary market

[1] We retain only records for which the secondary sources indicate that total ICO funding exceeded $1m. The reason for truncating the sample in this manner is that primary source data on the smaller ICOs are frequently scarce or unavailable.

[2] Many ICOs only allow contributions in cryptocurrencies, primarily Ethereum and Bitcoin. Because the dollar value of such cryptocurrencies is volatile, we collect the amounts of funding raised in cryptocurrencies where available. We then calculate the value of total funding raised, in US dollars, using closing prices on the last day of the contribution period. We only rely on totals in US dollars disclosed by issuers where the detailed breakdown into cryptocurrencies is not available.

prices that is as accurate as possible. We observe secondary market prices for 276 out of 306 sample ICOs (90%).[3]

We calculate continuously compounded returns in US dollars based on the average price paid by crowdsale investors. Where the average price is unavailable (which is the case for 24% of ICOs), we base returns on the mid-price, i.e. the average between the highest and the lowest price paid by investors in the crowdsale. We use continuous compounding because most ICOs trade continuously.

## 9.3 Ethereum blockchain data

Over 90% of our sample ICOs sell crypto tokens hosted on an existing blockchain, most commonly Ethereum. The publicly available Ethereum data enable us to provide statistics such as the median contribution per wallet (we use the terms address and wallet interchangeably) and the number of sample ICOs to which each wallet contributes. We can also follow the issued tokens through time and analyze how quickly investors sell their tokens.

All data we observe only identify parties by their Ethereum address, and multiple Ethereum addresses belonging to the same person or organization cannot be easily reconciled. The main assumption underlying our investor analysis is that the representative ICO investor only controls a single Ethereum address and that we can equate wallets with investors. We believe and provide several formal pieces of evidence in Section 9 that our main assumption can be maintained for many investors.

An Ethereum account consists of a public key, part of which (after a mathematical transformation called hashing) forms an address, representing the equivalent of a bank account number to which transactions can be sent. A corresponding private key (the equivalent of a password) controls transfers from the account. All transactions and token transfers made between different addresses on the Ethereum blockchain are publicly available and downloadable.[4]

Ethereum addresses can either be controlled by a human being or a smart contract. The latter is a piece of computer code that interacts with other parties on the Ethereum network according to a set of rules. The ERC20 contract is a popular smart contract for ICOs that contains a ledger that tracks the number of tokens held by each address. When tokens are sold or spent, the ledger is modified to reflect their new owner. Every change in token ownership requires interacting with the token contract to change the ledger.

During an initial coin offering on Ethereum, contributors send Ether to an address controlled

[3] We base our calculations on prices instead of total returns because we do not observe interest and dividend payments made by the 22% of the sample composed of security tokens. For robustness, we repeat – but do not show – all calculations on the subsample of ICOs that issue utility-tokens and that therefore cannot make any cash distributions. The results closely resemble those of the full sample, implying that security tokens do not affect the fundamental conclusions of our analysis.

[4] We thank Evgeny Medvedev for providing computer code to export data from the Ethereum blockchain (see https://github.com/medvedev1088/ethereum-etl).

by the promoter (the "token sale address") with the promise of being allocated tokens in an ERC20 contract in return. Deriving comprehensive information on the investor base from the transactions associated with contributions is typically not possible because of a number of challenges, which are visualized in Figure 9.1 . The presale and crowdsale stages usually use different contracts and transactions made towards the token sale address are not always limited to ICO contributions (the promoter will usually send some Ether to the address to pay for transaction costs, for example). Because the presale stage is usually private, the Ethereum address used during the presale is often not public knowledge. In addition, contributions made using means of payment other than Ether (e.g. US dollars or Bitcoin) will not show up as transactions on the blockchain. We therefore decided not to analyze the contributions made by investors, but instead focus on the distribution of tokens to investors following the ICO. Knowing the token prices from our manually collected dataset, we can infer the approximate investment per Ethereum address from the number of tokens allocated following the ICO.

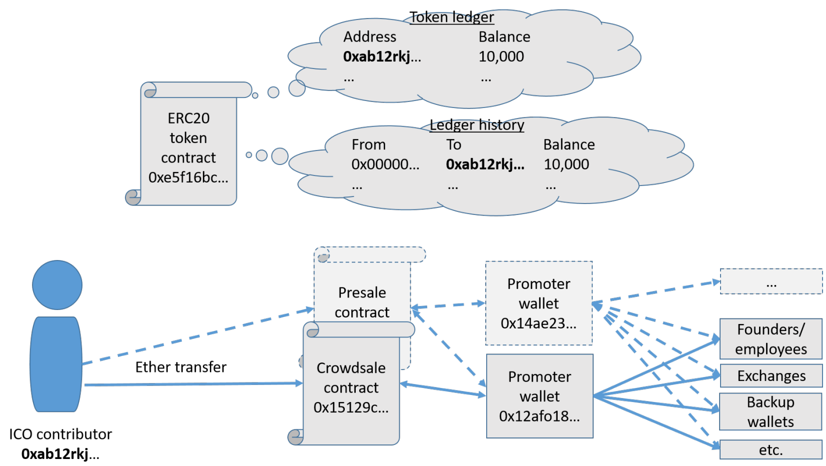



Figure 9.1 – Illustration of contribution flows during an ICO on the Ethereum platform

ICO promoters can distribute tokens in two ways. The initial balance can be allocated to the crowdsale contract or one or more addresses controlled by the ICO's promoter, from which the tokens are then reallocated to contributors. In that case, we observe one or more ERC20 token transfers from the initial address to the contributor's address. Alternatively, the token can be made mintable, in which case there is no initial balance but tokens are "created" from nothing for every contributor. In that case, we observe a token transfer from the "zero address" to the contributor.

We generally do not know from which address the initial token distribution is made. We address this challenge by analyzing the first 100 transfers made for each token in the sample. If at least 98 of them have the same source, we assume that the most common source within those 100 transactions is the unique address from which token distributions originate.[5] Second, some

[5]We only require 98 out of 100 transactions because sometimes a token is mintable, but the entirety of the token distribution is first minted to (i.e. transferred from the zero address) one or more addresses controlled by the ICO's organizers and then redistributed from those secondary addresses to investors. If no address is at least 98 times

transfers are not made in exchange for a financial contribution but represent an allocation to the founding team or free, promotional distributions to the general public ("air drops") to publicize the new token. We exclude transfers where the amount of tokens sent is worth less than 50 USD or where the receiving address receives more than 10% of the total token supply in all transactions to avoid such token transfers contaminating our sample.

The Ethereum platform hosts 264 out of the 306 sample ICOs. We are able to identify the token contract address and the token transfers for 247 of those ICOs. We further know the average price or average crowdsale price paid by investors for a subset of 181, and unambiguously identify the Ethereum address from which the initial token allocation occurred for 98 of those ICOs. These 98 ICOs received over $2.3b in funding and represent about a third of all money raised in our total sample. From now, we will call this subsample of our data "the investor sample".

the source of initial distribution, we say that we cannot identify the origin and do not analyze the token further.

# 10 Description of the ICO market

We briefly describe the typical structure of an initial coin offering and summarize the characteristics that are important for our subsequent analysis in Table 10.1.[1] In an ICO, an issuer sells a newly-minted cryptocurrency or cryptographic token to the public. The ICO ends once the contribution period is over or once it reaches the maximum amount of funding (if applicable). A decentralized ledger (blockchain) tracks token ownership thereafter, and tokens trade in secondary markets shortly following the ICO. The ICO can either be based on a new, standalone blockchain ledger or be implemented as a smart contract on an existing platform (which is the case for 91% of the sample). The Ethereum platform typically hosts the cryptographic tokens.[2]

The majority of firms in our sample of successful ICOs raised between $1m and $40m through their ICO. Often, the ICO comprises two stages. In our sample, 68% of ICOs begin with a presale (also known as pre-ICO or private sale) stage, in which larger investors can purchase tokens at discounted prices. In a subsequent crowdsale (also known as public sale) stage, the general public can acquire tokens. The mean ICO received $24.2m over all rounds, and $18.0m during the crowdsale stage. Hence, the crowdsale investors contribute the majority of funds.

ICOs frequently have a soft cap (45%) and/or a hard cap (95%). If the ICO contributions do not reach the soft cap, the company returns funds to the sender (ensured by an escrow arrangement or smart contract). The soft cap is therefore similar to the threshold model applied by popular crowdfunding websites such as Kickstarter (see, e.g., Mollick (2014)). The hard cap is the maximum amount of funding the issuer will accept. On average, sample ICOs raised 70.2% of their hard cap, including the presale stage.

It is rare that all investors pay the same price for the tokens. The presale usually takes place at heavily discounted prices, and early and/or large investors in the crowdsale obtain a discount as well. On average, presale investors receive a 34% discount over the "list price", whereas the earliest (or largest) crowdsale investors receive a 17% discount. The issuer on average offers 47% of the total token supply for sale during the crowdsale. Presale investors hold an average of 11% of the anticipated post-ICO token supply as of the time of the crowdsale, while the founders hold 39%. On average, a mere 2% of tokens are reserved for miners (the parties carrying out the verification of transactions on the blockchain), reflecting that most ICOs issue non-mineable tokens on the Ethereum blockchain. More than half (55%) of ICO issuers destroy unsold tokens after the offering period.

Only 51% of ICO issuers have a product or prototype. A minority of ICO promoters has decided to avoid securities regulations by only offering tokens to accredited or qualified investors (3%), or only to foreign investors and accredited US investors (51%). Such restrictions remove an important advantage of an ICO: to gauge demand for the product by future users. Issuers

[1] We refer the reader to Amsden and Schweizer (2018) and Howell et al. (2018) for more detailed descriptive statistics of the ICO market. Appendix B.3 features more extensive summary statistics on our sample.

[2] Some sources refer to assets issued on a standalone blockchain as cryptocurrencies and to those implemented through smart contracts as cryptographic tokens. In the remainder of this document, we will refer to all cryptographic assets sold in ICOs as *tokens*, regardless of their technical implementation.

Table 10.1 – **Descriptive statistics**

The table shows summary statistics for a hand-collected sample of 306 ICOs that took place between March 2016 and March 2018 and raised at least $1m according to secondary sources. All variables are defined in Appendix B.2.

| Panel A: ICO characteristics | | | | | | |
|---|---|---|---|---|---|---|
| | Mean | Median | Min | Max | SD | N |
| Is cryptographic token | 0.91 | 1 | 0 | 1 | 0.29 | 306 |
| Has a presale | 0.68 | 1 | 0 | 1 | 0.47 | 306 |
| Total amount raised (USDm) | 24.16 | 15.07 | 1.01 | 233 | 33.16 | 228 |
| Amount raised in crowdsale (USDm) | 18.03 | 10.76 | 0.5 | 218.84 | 26.7 | 262 |
| Amount raised in presale (USDm) | 6.02 | 1.12 | 0 | 193.65 | 15.01 | 246 |
| Fundraiser has minimum ('soft cap') | 0.44 | 0 | 0 | 1 | 0.5 | 306 |
| Fundraiser has maximum ('hard cap') | 0.95 | 1 | 0 | 1 | 0.22 | 306 |
| Percentage of hard cap raised (%) | 70.16 | 81.39 | 2.34 | 180.65 | 38.89 | 204 |
| Presale discount (%) | 34.18 | 30 | -16.5 | 96.88 | 23.17 | 152 |
| Crowdsale max. discount (%) | 17.36 | 15 | 0 | 98.57 | 18.76 | 288 |
| Token share crowdsale investors (ex ante) | 0.47 | 0.49 | 0.01 | 1 | 0.27 | 248 |
| Token share presale investors (ex ante) | 0.11 | 0.04 | 0 | 0.7 | 0.15 | 247 |
| Token share team (ex ante) | 0.39 | 0.38 | 0 | 0.96 | 0.22 | 292 |
| Token share producers/miners (ex ante) | 0.02 | 0 | 0 | 0.88 | 0.12 | 300 |
| Unsold tokens 'burnt' or proportional alloc. | 0.55 | 1 | 0 | 1 | 0.5 | 306 |
| Product or prototype developed | 0.51 | 1 | 0 | 1 | 0.5 | 306 |
| Qualified investors only | 0.03 | 0 | 0 | 1 | 0.18 | 306 |
| US retail investors excluded | 0.51 | 1 | 0 | 1 | 0.5 | 306 |
| High quality advisory team | 0.41 | 0 | 0 | 1 | 0.49 | 306 |
| Use of proceeds mentioned | 0.71 | 1 | 0 | 1 | 0.45 | 306 |
| Legal advisor disclosed | 0.25 | 0 | 0 | 1 | 0.44 | 306 |
| Has VC backing | 0.26 | 0 | 0 | 1 | 0.44 | 306 |
| **Panel B: Investor protection** | | | | | | |
| | Mean | Median | Min | Max | SD | N |
| Is a security | 0.22 | 0 | 0 | 1 | 0.41 | 306 |
| Legal form and jurisdiction known | 0.88 | 1 | 0 | 1 | 0.33 | 306 |
| Legal entity is corporation or LLC | 0.9 | 1 | 0 | 1 | 0.31 | 269 |
| Registered in offshore financial center | 0.2 | 0 | 0 | 1 | 0.4 | 306 |
| Funding milestones | 0.04 | 0 | 0 | 1 | 0.2 | 306 |
| Independent custodian for ICO funds | 0.04 | 0 | 0 | 1 | 0.19 | 306 |
| Team tokens locked up | 0.58 | 1 | 0 | 1 | 0.49 | 306 |
| Team lockup period (weighted avg.) | 1.1 | 0.75 | 0.02 | 5.5 | 0.99 | 179 |
| Presale tokens locked up | 0.14 | 0 | 0 | 1 | 0.34 | 207 |
| Presale lockup period (weighted avg.) | 0.53 | 0.27 | 0.02 | 2 | 0.52 | 28 |
| Investors have governance rights | 0.18 | 0 | 0 | 1 | 0.38 | 306 |

often disclose their advisory team, 41% of which we judge to be "high quality" advisory teams consisting of venture capitalists, researchers, executives and entrepreneurs. In general, the level of disclosure varies substantially in the cross-section; 29% of ICOs do not even disclose their intended use of the money raised (e.g. by category of expenses), and 25% of issuers disclose the name of the legal advisor that assists them with the transaction to the public. At the time of the ICO, 26% of issuers have received VC funding.

ICO tokens can help launder money gained in illicit ways. To comply with anti-money-laundering legislation, 48% of sample ICOs have adopted AML (anti-money-laundering) or KYC procedures, which verify the identity of an investor before accepting an investment. The awareness of regulatory issues has been increasing among ICO issuers. The fraction of ICOs with a KYC policy has been steadily increasing, from 0% in the first quarter of 2017, to 80% during the first quarter of 2018.

Panel B of Table 10.1 describes characteristics related to investor protection. The fraction of security token (i.e. tokens for which the issuer promises to make payments to their owner in the future) in the sample is 22% but has been falling, from a high of 40% during the first quarter of 2017 to only 14% a year later. We were able to identify the jurisdiction and legal form for 88% of all entities organizing ICOs using the material provided by the issuer and publicly searchable commercial registers. Among the identifiable subset, 90% are either joint-stock or limited liability companies (or their international equivalents), i.e. entities typically associated with for-profit commercial activity. Offshore financial centers, using the definition of the International Monetary Fund (IMF), host 20% of all ICOs. Only 4% of ICOs specify milestones for the release of funds and 4% specify an independent custodian for the funds raised. A majority (58%) of ICOs implement vesting periods for the tokens allocated to the company and its founders. The weighted-average vesting period for locked up tokens is 1.10 years. Only 14% of ICOs specify a lockup period for tokens owned by presale investors, on the other hand. Those that do lock them up for 0.53 years on average. Only 18% of ICOs give investors governance rights, usually by allowing them to vote on certain topics.

# 11 Analysis of ICO investors

We now turn to the main analysis of the characteristics and trading patterns of ICO investors, using the investor sample. In Section 11.1 we first address the central question of whether our key identifying assumption that we can approximately equate the number of cryptographic wallets holding a token with the number of investors in an ICO is defendable. In Section 11.2, we provide evidence that an aggregation of all distributed coins multiplied with the price per coin from our Ethereum data approximately equals the total amount of funds raised during the ICO. We also show summary statistics along several key ICO characteristics for the investor sample and compare it to the overall sample to analyze how different the investor sample is from the overall sample. Section 11.3 then analyzes the average contribution size, Section 11.4 examines the determinants of investor participation in the crowd sale, and Section 11.5 analyzes the fraction of repeat contributors. Finally, Section 11.6 attempts to identify crowdsale and presale investors' motivation for participating in ICOs.

## 11.1 Is the assumption that the typical investor invests with only one address per ICO defendable?

Investors can open wallets at no costs (although it is costly to send funds and tokens from one Ethereum wallet to another even if they have the same owner) and wallets are pseudonymous, i.e. it is impossible for a researcher to link wallets to identities. Throughout the analysis in Section 11, we equate wallets with investors. Investors may want to use multiple wallets for at least two reasons. They may want to hide from the issuing firm that they are a large investor or they may want to hide this information from the general public. One potential concern with our analysis is that we overestimate the number of investors and underestimate the contribution amount because investors use multiple wallets for the same ICO. A second concern relates to our analysis of investor trading behavior post-ICO. We may overestimate the trading activity of ICO investors, if investors move tokens from one of their wallets to another one.

We conduct several tests to reduce concerns about our main assumption. Our first piece of evidence comes from a comparison of movements of tokens out of ICO investors' wallets with trading volume for that token on cryptocurrency exchanges. This test seeks to establish that the majority of investors who move tokens out of their wallet do so to sell them on an exchange rather than to move them to another of their own wallets. If a significant number of original ICO investors did not sell their tokens post-ICO, but rather moved them from one of their wallets to another, exchange-reported trading volume on a given day would not correlate highly with changes in the tokens held by the wallets participating in the ICO. The correlation between exchange-reported trading volume and our implied (from Ethereum) sales by ICO investors is, however, very strong. We calculate daily implied sales for the first 90 days after the ICO as the gross number of tokens moved out of ICO investors' wallets multiplied with the average between the daily opening and closing price. We aggregate implied sales by ERC20 token and day. We then estimate a regression of the actual daily trading volume reported by coinmarketcap on daily implied sales by ICO investors (having winsorized both at the 1% and

99% levels) and time and token fixed effects. The coefficient on implied sales is 0.92 (t=9.30), so for every USD in implied sales the actual volume increases by 0.92 USD. Hence, when the token balance of an ICO investor drops, the tokens are most often traded on an exchange and not moved to a different wallet of the same investor.

Second, we also examine how often addresses are used for sending and receiving Ether following their investment in an ICO. If investors created a new wallet for every ICO, it is unlikely that they would frequently be using these special-purpose wallets for transactions afterwards. We find that in the first 270 days following a contribution to an ICO, the median address is used for two transactions, outgoing or incoming, with a total volume of $210.11 valued at the Ether prices of those dates. We interpret this number as evidence that investors use the wallets with which they participate in ICOs also for other purposes. Note that the total volume we analyze would only include proceeds from the sale of ERC20 tokens if the investor explicitly transferred the sales proceeds from their exchange account to the same Ethereum wallet. In addition, the total volume is also larger than what investors would typically keep in their wallet to pay for transaction cost.[1]

Our third and final set of tests relies on the existence of a KYC policy at the ICO. If an ICO has a KYC policy, investors have no incentive to use multiple addresses to hide their identity from the issuing firm (although they may still do so to hide their identity from the public). Our first test uses the existence of a KYC policy together with the voluntary disclosure of the number of contributors to the offering by some issuing firms. Because these firms know the individuals associated with each address, their self-reported number of contributors should reflect the actual number of investors rather than the number of contributing addresses. In particular, if many ICO participants use multiple wallets to hide their true investments, the number of self-reported contributors should be much lower than the number of wallets that we identify. Using a simple t-test, we find that for ICOs with a KYC policy, the self-reported number of contributors actually slightly exceeds our estimate for the number of investors, but insignificantly so.[2] The result means it is unlikely that a large fraction of investors is using multiple wallets to hide their identity from the public; if they did, our estimate for the number of investors would significantly exceed the self-reported number in this subsample.

We also test whether our estimate for the number of contributors for ICOs with a KYC policy is different from our estimate for the subset without one. If investors systematically use multiple wallets to hide their identity from the issuer, our estimate for the number of contributors should be higher for those ICOs that do not have a KYC policy than for those that do. However, a two-sample t-test indicates that our estimate for the number of contributors for ICOs with a KYC policy actually exceeds that of ICOs without one, with marginal statistical significance.[3]

[1] According to data from etherscan.io, the average fee for sending Ether from one address to another was $0.13 between the start of the first and the end of the last ICO in our sample. Fees for sending ERC20 tokens depend on the token and are higher than those for sending Ether, but they are of the same order of magnitude.

[2] The mean self-reported number of investors is 5,555.6, the mean of our estimated number is 5,169.4 (n=18, t=1.08).

[3] The mean of the estimated number of contributors is 5,656.8 (n=58) for ICOs with a KYC policy and 3,310.0

Therefore, we do not find any evidence indicating that investors are systematically using multiple addresses to hide their identity from the issuing firm.

## 11.2 Data quality and representativeness of the investor sample

Table 11.1 compares the actual amount of funding and the amount implied by our analysis of token distributions for the investor sample. The mean of the implied amount of funding is \$26.0m and is statistically indistinguishable from the mean of the actual amount, which is \$23.1m. The medians are similarly close but reversed in order, with \$12.7m for the implied total and \$14.6 for the actual. Some ICOs also disclose the number of unique contributors. We collect such disclosures for the investor sample and compare them to the number of contributors derived from our analysis in Panel B of Table 11.1. The two means are statistically indistinguishable.

Table 11.2 compares the investor sample to the remaining ICOs based on several characteristics. The two samples differ along two dimensions: the fraction of security tokens and the fraction of ICOs with a KYC procedure. 59.2% of ICOs in the investor sample have KYC verification against 42.8% of the remaining ICOs. Similarly, only 14.3% of tokens in the investor sample are unambiguously securities, compared to 25.0% of the remaining ICOs. Importantly, ICOs in the investor sample are not any more or less likely to restrict participation by retail investors. Based on these results, we conclude that there is sufficient overlap in characteristics between the two subsamples and that the investor sample is representative of the typical ICO in our overall sample.

## 11.3 Average contribution size

We analyze the contribution per investor in Table 11.3. The mean of the median contribution per investor is \$1,203.35. The small dollar amount suggests that the majority of investors are not like the accredited investors that would typically participate in angel financing rounds.[4] Hellmann et al. (2017) for example examine data from British Columbia's Investment Capital Program and find that Canadian angel investors invest on average \$440'000 in first rounds. Goldfarb et al. (2014) examine data on 182 Series A U.S. financings and find that the mean investment by an angel investor is \$150,375, while the median investment size is \$25,000.

Additional evidence for the frequent participation of retail investors in ICOs comes from the average number of investors, which at 4,698.91 is three orders of magnitudes larger than the number of investors in a typical angel financing round. The number of ICO contributors and the amount of financing per contributor also significantly exceed the number of backers and

(n=40) for ICOs without one, with a t-stat of -1.73 for the difference.

[4] Wong et al. (2009) formally define angels as those that are "accredited investors" according to SEC Regulation D, Rule 501. Rule 501 states that accredited investors must have a net worth of over \$1m or annual income of over \$200,000.

Table 11.1 – **Comparing disclosed and calculated amounts of funding and number of contributors**

The table compares the actual amount of funding and the number of contributors with the corresponding amounts implied by our analysis of token distributions for 98 ICOs conducted on the Ethereum blockchain (the 'investor sample'). We exclude ICOs for which we cannot identify with certainty the Ethereum address from which the tokens have been initially distributed. Furthermore, transfers where the amount transferred is worth less than 50 USD or where the receiving address holds more than 10% of the total token supply are excluded. Contribution amounts are only calculated for ICOs where the average prices for presale and crowdsale are less than 50% apart. The implied total is calculated as the mean US dollar contribution per ICO participant times the number of participants implied by token distributions following the ICO.

| Panel A: Funding | | | | | | |
|---|---|---|---|---|---|---|
| | Mean | Median | Min | Max | SD | N |
| Total amount raised (USDm) | 23.12 | 14.57 | 1.25 | 159.28 | 30.15 | 74 |
| Implied total calculated (USDm) | 26.01 | 12.72 | 0.13 | 240.92 | 41.02 | 74 |
| t-test for difference in means | 1.50 | p-value | 0.14 | | | |
| **Panel B: Number of contributors** | | | | | | |
| | Mean | Median | Min | Max | SD | N |
| Self-reported number of contributors | 4,687.94 | 2,950.00 | 500.00 | 25,000.00 | 4,970.68 | 32 |
| Implied number of contributors calculated | 4,220.53 | 1,698.00 | 505.00 | 21,297.00 | 4,713.63 | 32 |
| t-test for difference in means | 1.24 | p-value | 0.22 | | | |

Table 11.2 – **Descriptive statistics for the 'investor sample' and other ICOs**

The table compares the means of select attributes for the subsample of 98 ICOs conducted on the Ethereum blockchain for which we can calculate descriptive statistics for investors' contributions with those of all other sample ICOs. All variables are defined in Appendix B.2. Parentheses in the first two columns contain standard deviations. The third column displays the difference in means and, in parentheses, the associated standard error. One, two and three asterisks indicate statistical significance at the ten, five and one percent level, respectively.

| | Investor sample | Other ICOs | Difference |
|---|---|---|---|
| Total amount raised (USDm) | 23.185 | 21.617 | -1.568 |
| | (27.14) | (32.94) | (3.67) |
| Amount raised in presale (USDm) | 6.130 | 5.961 | -0.168 |
| | (9.92) | (17.34) | (1.74) |
| Has VC backing | 0.245 | 0.274 | 0.029 |
| | (0.43) | (0.45) | (0.05) |
| US retail investors excluded | 0.582 | 0.476 | -0.106* |
| | (0.50) | (0.50) | (0.06) |
| Qualified investors only | 0.020 | 0.038 | 0.018 |
| | (0.14) | (0.19) | (0.02) |
| Registered in offshore financial center | 0.173 | 0.216 | 0.043 |
| | (0.38) | (0.41) | (0.05) |
| Is a security | 0.143 | 0.250 | 0.107** |
| | (0.35) | (0.43) | (0.05) |
| KYC/AML procedure | 0.592 | 0.428 | -0.164*** |
| | (0.49) | (0.50) | (0.06) |
| Investors have governance rights | 0.204 | 0.168 | -0.036 |
| | (0.41) | (0.38) | (0.05) |
| Observations | 98 | 208 | 306 |

Table 11.3 – **Contribution amount per address in Ethereum ICOs**

The table displays summary statistics for the ICO contributions made per address on the Ethereum platform. US dollar amounts are calculated as the number of tokens transferred to the investor times the average price per token over the entire ICO, including the presale. We exclude ICOs for which we cannot identify with certainty from which Ethereum address the tokens have been initially distributed. Furthermore, transfers where the amount transferred is worth less than 50 USD or where the receiving address holds more than 10% of the total token supply are excluded. Contribution amounts are based on the average price over both presale and crowdsale only calculated for ICOs where the average prices for presale and crowdsale are less than 50% apart.

| | Mean | Median | Min | Max | SD | N |
|---|---|---|---|---|---|---|
| Minimum contribution (USD) | 65.00 | 50.60 | 50.00 | 464.86 | 65.31 | 74 |
| Maximum contribution (USDm) | 3.23 | 1.05 | 0.00 | 37.11 | 6.10 | 74 |
| Mean contribution (USD) | 10,093.88 | 4,355.20 | 809.42 | 128,301.83 | 17,516.03 | 74 |
| Median contribution (USD) | 1,203.35 | 697.95 | 158.95 | 13,976.73 | 1,965.85 | 74 |
| SD of contribution (USD) | 87,907.93 | 41,707.62 | 1,524.86 | 1025779.56 | 153,958.89 | 74 |
| Number of contributors | 4,698.91 | 2,312.50 | 81.00 | 39,356.00 | 6,672.74 | 98 |

contributed amounts in the average successful Kickstarter crowdfunding project. Mollick (2014) uses the universe of Kickstarter projects from its inception in 2009 to July 2012. He estimates that on average 122 individuals contribute $80.55 each to a typical Kickstarter project in his sample. The data suggest that ICO promoters tapped a new type of startup investor.

The skewness of the ICO contribution amount distribution is positive, with the mean of the average contribution per investor amounting to $10,093.88, suggesting that a small number of larger investors exists, with contributions likely often made during the presale.[5]

## 11.4 Determinants of investor participation in the crowdsale

Next, we ask whether retail investors are drawn to ICOs with certain characteristics. For this purpose, we regress our estimate for the (natural logarithm of the) number of contributors on ICO characteristics.

Ex ante, we expect that the number of investors will be increasing in the level of disclosure, the number of investor protections, and the presence of presale investors and venture capitalists that might fulfill a monitoring or certification function for the ICO. We therefore include these characteristics in the regression. We also control for an ex ante measure of size (the hard cap) and several core ICO attributes such as whether the issuer has developed a product or prototype, whether it is advised by a high quality advisory team, and whether there is a KYC procedure. These variables provide a proxy for the quality of the ICO and its demand for funding. In addition, the tests contain fixed effects for the month of the first day of the ICO.

The regression results are presented in Table 11.4. In the first Column, we include all ICOs and control for the existence of a presale through an indicator variable. In Column two, we condition on the ICO having had had a presale and include a control for the natural logarithm of the amount of money raised in the presale. Column 1 shows that ICOs with a presale attract 87.8% more investors, statistically significant at the 5% level. However, an increase in the amount raised in the presale does not have a significant impact on the number of investors (Column 2).[6] Most of the variables describing ICO attributes and disclosure are insignificant as well. An exception is the existence of a KYC policy, which is associated with a 74.5% increase in the number of investors.

Many of the characteristics related to investor protection are statistically significant at the 10 to 5 percent level. Tokens that are unambiguously securities, i.e. grant their holders cash flow rights, surprisingly get 68.0% fewer investors. One possible explanation for this fact might be that such tokens are associated with more legal uncertainty. A one standard deviation increase in the founder lockup period on the other hand increases the number of investors by 29.5%,

[5]Table 10.1 shows however that 75% of the total contribution come from the crowdsale event, so that most of the money raised in an ICO comes from crowdsale investors.

[6]The dependent variable is log-transformed, the marginal effect of having a presale is therefore exp(0.63)-1 = 0.878.

Table 11.4 – **Determinants of the number of contributors**

The table shows regression results of an ordinary least squares regression of the number of ICO contributors on ICO characteristics. We exclude ICOs for which we cannot identify with certainty from which Ethereum address the tokens have been initially distributed. Furthermore, transfers where the amount transferred is worth less than 50 USD or where the receiving address holds more than 10% of the total token supply are excluded. All variables are defined in Appendix B.2. Dependent and independent variables have been winsorized at the 1 and 99% level. T-statistics calculated from robust standard errors are listed in parentheses below the coefficients. One, two, and three asterisks indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) |
|---|---|---|
| Has a presale | 0.630** | |
| | (2.22) | |
| Ln(presale amount) (USDm) | | 0.040 |
| | | (0.20) |
| Use of proceeds mentioned | -0.251 | 0.123 |
| | (-0.99) | (0.34) |
| Offshore incorporation | 0.229 | -0.319 |
| | (0.70) | (-0.50) |
| Legal form and jurisdiction known | -0.061 | -1.251 |
| | (-0.12) | (-1.35) |
| Legal advisor disclosed | 0.018 | -0.052 |
| | (0.06) | (-0.12) |
| Is a security | -0.819** | -1.139** |
| | (-2.05) | (-2.32) |
| Token share team (ex ante) | -1.553* | -2.899* |
| | (-1.98) | (-1.92) |
| Team lockup period | 0.287* | 0.278* |
| | (1.91) | (1.81) |
| Product or prototype | 0.343 | 0.248 |
| | (1.39) | (0.78) |
| High quality advisory team | 0.346 | 0.136 |
| | (1.59) | (0.40) |
| KYC/AML procedure | 0.641*** | 0.557* |
| | (2.73) | (1.93) |
| Has VC backing | 0.169 | 0.343 |
| | (0.50) | (0.84) |
| Ln(Hard cap size) (USDm) | 0.745*** | 0.744** |
| | (4.69) | (2.32) |
| Month FE | Yes | Yes |
| N | 92 | 57 |
| R2 | 0.57 | 0.55 |

whereas a one standard deviation increase in the fraction of tokens retained by the founders is associated with a 46.5% decrease in the number of investors. A likely explanation is that a large founder share increases the risk of dilution for the investors in case the founders decide to sell the tokens in the secondary market.[7] Overall, the alignment of incentives between founders and investors seems to matter more for contributors' investment decision than the level and quality of disclosure. Finally, ex ante larger ICOs attract more investors as every one percent increase in the hard cap is associated with a 0.7% increase in the number of investors.

## 11.5 Fraction of repeat contributors

Perhaps surprisingly, the vast majority of addresses in our investor sample only contributes to one ICO. Only 19.1% of addresses contribute more than once, and only 1.1% of addresses participate at least five times.[8] To address the possible concern that our limited sample of ICOs is the reason for this result, we also analyze a comprehensive sample of primary and secondary market token purchases using a sample of all ERC20 tokens listed on coinmarketcap.[9] The results for this extended sample are very similar and indicate that only 19.6% of investors hold more than one type of token over the sample period, and only 1.3% hold five or more.

There is evidence suggesting that more professional investors contribute more frequently, however. Table 11.5 displays the results from regressing the average size of the contributions made by an Ethereum address on the number of ICOs in which the address participates over the sample period. The logarithmic specification in Column 1 implies that the average contribution increases by 27.1% when the number of ICOs the address has contributed to increases by one, statistically significant at the 1% level. Column 2 presents a linear specification. The coefficient estimates indicate that the mean investment made by an address increases by 264.6$ when the total number of ICOs invested in is increased by one, also significant at the 1% level.

Studying the portfolio allocation decisions of individual investors in the stock market, Goetzmann and Kumar (2008) find that the average investor holds only four stocks in his account at a large online brokerage firm. Our sample investors use the same address to invest in 1.3 different tokens through the primary market on average. If we count Ether as a separate financial asset and add secondary market purchases, the average tokenholder invests into 2.4 different assets on the Ethereum blockchain over the sample period. It is likely that ICO investors also hold cryptocurrencies on other blockchains such as Bitcoin that we cannot link

[7] The regression includes the ICO's hard cap. Therefore, the coefficient estimate for the fraction of tokens retained by founders represents the effect on the number of investors given a constant number of tokens offered for sale. The correlation between the founder allocation and the number of investors is therefore not purely mechanical.

[8] Using a different methodology, Boreiko and Risteski (2019) find that 24.3% of addresses contribute to more than one ICO on Ethereum.

[9] For this analysis, we only retain tokens with a mean daily trading volume of at least $1,000 and mean market cap of at least $100,000 during their first two weeks of trading. This filter leaves us with a sample of 449 ERC20 tokens.

Table 11.5 – **Number of contributions by address and mean investment amount**

The unit of observation is an Ethereum address that has contributed to at least one of the ICOs in the investor sample. Coefficients are estimated using ordinary least squares. Dependent and independent variables have been winsorized at the 1 and 99% level. T-statistics calculated from robust standard errors are listed in parentheses below the coefficients. One, two, and three asterisks indicate statistical significance at the ten, five and one percent level, respectively.

| | Ln(mean investment in USD) | Mean investment in USD |
|---|---|---|
| | (1) | (2) |
| Number of ICOs invested in | 0.240*** | 264.598*** |
| | (89.61) | (19.74) |
| Constant | 6.108*** | 1830.200*** |
| | (1241.49) | (81.07) |
| N | 257,073 | 257,073 |
| R2 | 0.02 | 0.00 |

to their Ethereum wallet. So the number of cryptoassets the average investor holds appears to resemble the number of assets that individual investors have been found to own in the stock market. A caveat regarding this conclusion is that ICO investors might be using different wallets for different ICOs, which we cannot rule out completely (similar to the concern that clients of the online brokerage studied byGoetzmann and Kumar (2008) may have multiple security accounts at different banks). But given the evidence presented in Section 11.1 and the fact that repeat contributors invest larger amounts, we deem it possible but unlikely.

## 11.6 What motivates investors to participate in ICOs?

### 11.6.1 Are contributors motivated primarily by financial returns?

Ex ante, we see two primary reasons why people might participate in ICOs. The first is to make a financial profit and the second to pre-purchase the product or service the issuer is developing.

The majority of our sample tokens are either utility tokens or security tokens. For the 22% of tokens that we classify as security tokens because they pay dividends, interest or make other financial distributions, the nature of the token makes it likely that investors are motivated primarily by financial gains. For utility tokens, which represent 61% of our sample, the answer requires more investigation.

To determine what motivates investors to buy utility tokens, we study the frequency of trading in secondary markets, which we see as an indication of investors having a financial motivation rather than mainly pre-purchasing a product. We calculate the fraction of ICO investors that sell at least one token within 90 days of the ICO, as well as the fraction of the ICO allocation resold by investors over the same time period. We explicitly restrict our sample to platforms that are in the pre-launch phase by manually collecting platform launch dates and dropping observations for tokens in the post-launch period. While token transfers from one address to another are publicly visible on the Ethereum blockchain, it is more difficult to infer the purpose of such transfers from the data. There are three main reasons for token transfers: investors spend tokens to consume the product, investors move tokens to a different wallet, or investors sell tokens in the secondary market. We exclude the first reason by restricting our sample to token transfers that occur before the launch of the service or product. We have shown in Section 11.1 that daily token transfers correlate very highly with exchange trading volume of the same token, so token transfers between wallets belonging to the same investor do not make up a significant fraction of token transfer either. Hence, the main purpose for token transfers in this sample are sales of tokens on exchanges.

We find that a substantial fraction of investors sells their allocation soon after the ICO. We estimate that, for utility tokens, on average 49.3% of all investors sell some or all of their tokens within 90 days of the ICO.[10] Over the same time window, the mean number of tokens, net of new purchases, sold by the original ICO investors in the secondary market following the crowdsale, scaled by the total number of tokens distributed in the ICO, amount to 41.8%. Therefore, a substantial fraction of contributors who purchase utility tokens sell a sizeable portion of their holdings before the product is developed and usable. We observe similar behavior in the full sample of tokens, which includes securities and cryptocurrencies. There, 47.9% of investors sell at least one token within 90 days, and net token sales by ICO contributors over the same time window amount to 42.3% of the total ICO allocation.

Our results are consistent with the survey evidence provided by Fisch et al. (2018). Out of a sample of 517 ICO investors, 50.7% of participants answered that a "future sale of the token at a higher price (shortly after the ICO)" was an "important" or "very important" reason for their investment decision.

### 11.6.2 What properties of ICOs make them attractive to investors?

Having established that ICO participants are often small investors motivated by the prospect of financial returns, a natural follow-up question to ask is what features might make ICOs so attractive to retail investors. One potential explanation is that they have "lottery features": high idiosyncratic volatility, high skewness and a low absolute price. Individual investors in stock markets have been shown to have a preference for stocks with such characteristics (e.g., Kumar, 2009). In our sample of ICOs, the average annualized volatility of returns in excess of Ethereum for the nine months following the ICO is 173%. Returns are also positively skewed (5.13) in the cross-section, and the median token has a price of only $0.16 during the crowdsale.

[10] The average daily trading volume for the median ICO is $193,976 over the first 90 days following the ICO. Therefore, secondary markets seem to be liquid enough to allow investors to liquidate their positions relatively easily should they wish to do so.

In addition, researchers have shown that investors fear to miss out in new industries with large growth potential and uncertainty, and do not necessarily carry out the required due diligence. A substantial body of evidence comes from the last period of technological revolution, the internet boom. Cooper et al. (2001) examine 95 firms that changed their names to a ".com" firm. These small firms, mostly traded on the OTC Bulletin Board, experienced 53% five-day announcement returns on the news of the name change.Lamont and Thaler (2003) show that investors valued carve-outs of technology stocks irrationally high during the same boom period. Griffin et al. (2011) show that during the technology stock reversal in March 2000, institutional investors sold technology stocks to retail investors (especially those without financial advisors). It seems that retail investors in ICOs could be driven by the same motivation that drove retail investors during the internet boom.

Shiller (2000) uses the term "new era economic thinking" to describe the tendency of technological innovation to lead to financial expansions. In Shiller's words, "stock market expansions have often been associated with popular perceptions that the future is brighter or less uncertain than it was in the past." This thinking is often linked to the emergence of new technologies, as "the public is interested in expansive descriptions of future technology–for example, in what amazing new capabilities computers will soon have–not in gauging the level of U.S. corporate earnings in coming years." The emergence of blockchain technology and its potential to disrupt the financial system presents a potential trigger for such new era thinking, which could provide an additional explanation for the large number of retail investors participating in ICOs.[11]

### 11.6.3 Do the large discounts to pre-sale investors impact their trading behavior?

There is substantial heterogeneity among ICO investors. Some invest larger amounts and do so more frequently, and may behave in a different way. Presale investors in particular usually invest more, receive a significant discount over crowdsale investors, and can thus lock in a profit by selling their allocation in the secondary market directly after the ICO. This situation is akin to flipping IPO share allocations on the first trading day to benefit from underpricing (e.g., Aggarwal, 2003; Krigman et al., 1999). As long as the secondary market price lies at or above the presale price, and the market is sufficiently deep, investing in the presale could thereby be profitable regardless of the issuer's fundamentals. We therefore expect presale investors to have a particularly short time-horizon.

If it is common for presale investors to "flip" their investment in this manner, the correlation between the size of the investment and the holding period should be negative.[12] We estimate a regression of the number of days until the first sale of tokens by an investor, measured from

[11] Cheng et al. (2019) show that publicly listed firms experience positive stock market returns when they announce that they are going to use blockchain technology, although these companies are not experts in blockchain technology and do not offer any specifics of their projects. In this case however, the stock market returns reverse shortly after, possibly because it is easier to take short positions in the large and mature stocks of their sample.

[12] Krigman et al. (1999) show that large and informed traders flip the IPO allocations that perform the worst in the future.

the last day of the ICO, on the amount contributed. At the time of the analysis, we have nine months of post-ICO data for the last sample ICO, therefore the dependent variable for this test is right-censored at 270 days.

Table 11.6 – **Token holding period as a function of the investment amount**

The table presents results of Tobit regressions of the number of days until the first sale of tokens by an ICO contributor on the size of the contributor's investment in US dollars, both in natural logarithms. The unit of observation is an investor in an ICO. The sample used for this test is the 'investor sample' consisting of 98 ICOs conducted on the Ethereum platform. The number of days is measured from the last day of the crowdsale period and is right-censored at 270. All continuous variables have been winsorized at the 1 and 99% level. Presale is an indicator variable equal to one if the ICO had a presale, and zero otherwise. The presale discount is defined as the difference between the maximum crowdsale price and the minimum presale price, measured as a fraction of the former. Standard errors are clustered by ICO. T-statistics are reported in parentheses below the coefficient. One, two, and three asterisks indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Ln(contribution in USD) | -0.454*** | -0.240*** | -0.189*** | -0.257*** |
| | (-153.87) | (-88.33) | (-43.74) | (-32.98) |
| Ln(contribution in USD) * Presale | | | -0.084*** | |
| | | | (-15.08) | |
| Ln(contribution in USD) * Presale discount | | | | -0.103*** |
| | | | | (-4.08) |
| Constant | 7.672*** | 6.432*** | 6.661*** | 6.776*** |
| | (384.48) | (73.28) | (74.72) | (68.40) |
| Token FE | No | Yes | Yes | Yes |
| N | 264,439 | 264,439 | 264,439 | 158,575 |
| Pseudo $R^2$ | 0.03 | 0.12 | 0.12 | 0.09 |

Table 11.6 displays results from Tobit regressions where both the dependent and independent variables are in natural logarithms. The specifications in Columns one and two suggest that there is a negative relationship between the size of the contribution and the holding period, statistically significant at the 1% level, implying that larger investors sell earlier. The specification in Column one implies that a one percent increase in the contribution decreases the (latent, uncensored) holding period by 0.5%. The specification in Column two adds ICO fixed effects that control for observable and unobservable ICO-level characteristics.[13] The estimate from the fixed effects specification suggests that a one percent increase in the investment amount decreases the holding period by 0.2% on average. In Column three, we interact the size of the contribution with an indicator variable equal to one if the ICO had a presale, and zero otherwise. The interaction term is negative and statistically significant at the 1% level, suggesting that the relationship between size and holding period is stronger in

[13] Greene (2004) finds that the incidental parameter problem, which commonly affects nonlinear regression models with fixed effects, usually does not impact the coefficient estimates in Tobit models.

ICOs that have a presale. The coefficient for the interaction term amounts to about a third of the magnitude of the relationship between the size and holding period estimated in Column two. While the coefficient on the contribution amount by itself decreases by around 20% in Column three, it retains its statistical significance, suggesting that larger investors still sell earlier in ICOs that do not have a presale. A partial explanation for the negative correlation in those ICOs might be that some issuers grant volume-based discounts to crowdsale investors. Finally, Column four provides an additional specification in which we interact the size of the contribution with the presale discount, based on the subsample of ICOs that had presale. The coefficient estimate for the interaction term is negative and statistically significant at the one percent level, implying that large investors sell earlier if the presale discount was larger, i.e. when it is more likely that the secondary market price after the initiation of trading lies above the presale price. The impact of the presale discount is meaningful in economic terms as well; the marginal effect of the contribution size on the holding period is roughly 14% larger for an ICO with a presale discount at the mean compared to the marginal effect for an ICO with a presale discount of zero.

Overall, Table 11.6 provides evidence that some large presale investors tend to flip their allocations to realize the windfall profits generated by their discount. They display a behavior that is similar to IPO investors that flip their IPO allocations during the first trading days to benefit from IPO underpricing (e.g., Aggarwal, 2003). Our analysis has important consequences for crowdsale investors who rely on presale investors for ICO certification. Unlike the investments of early stage investors in typical seed rounds that are illiquid, presale investors can obtain liquidity on the secondary market. The value of their certification may be less than crowdsale investors believe, especially when presale investors obtain large discounts.

# 12 Investor protection

As illustrated by the extended summary statistics in Appendix B.3, our average sample firm was founded only 1.6 years prior to the ICO, has 11 employees, and does not have a finished product. Hence it is at a stage in its life cycle when it would typically seek angel or venture capital funding instead of going to public markets.

Asymmetric information and moral hazard problems between entrepreneurs and financiers are a prominent issue in early stage financing. Therefore, investment contracts between venture capitalists or angel investors and entrepreneurs usually provide numerous protections to investors, such as cash flow rights, board and voting rights and liquidation rights (Kaplan and Strömberg, 2003). Our goal in this section is to determine whether the retail investors who participate in ICOs receive some of the protections that professional investors typically ask for.

## 12.1 Cash flow rights

Residual cash flow rights in ICOs are rare, and are only present among a subset of the 22% of ICOs that issue security tokens. For the vast majority of ICOs, investors will only receive financial gains from their token holdings if the product developed by the issuer gains in popularity. In addition to the lack of dividends, there is also a more subtle point with selling utility tokens. Whether and how much the price of a utility token increases with the popularity of the product depends on the issuer not accepting alternative means of payments in the future (e.g., Catalini and Gans, 2018). Accepting other means of payment decreases the demand for tokens sold in the ICO and subsequently decreases the value of the token. Interestingly, token sales terms rarely expressively prohibit the issuer from introducing additional means of payments.

## 12.2 Liquidation preferences

Liquidation preferences are an important element of term sheets between venture capitalists and entrepreneurs, most commonly in the form of convertible preferred stock. Liquidation preferences reduce moral hazard concerns: Should the company fail, merge or be sold, VC investors receive the first proceeds, typically up to their initial investment. (Kaplan and Strömberg, 2003) study a sample of VC financing rounds and find that over 96% use preferred stock. Token sales agreements on the other hand typically state that the firm will make a "best effort" attempt to deliver the promised product, but investors have no additional rights in case of failure and liquidation.

## 12.3 Voting rights, board of directors, and staggered distribution of ICO proceeds

Investors only have voting rights in 18% of ICOs, and votes are usually non-binding in nature and limited to approving major investment decisions or updates of software protocols. We are not aware of any firm that allows ICO investors to participate in director elections. VCs, on the other hand, control 41.4% of board seats and a majority of the shareholder votes following the average financing round (Kaplan and Strömberg, 2003). According to the same source, 14.6% of venture funding rounds place restrictions on the release of committed funds. In contrast, only 4% of ICOs specify milestones for the release of funds, and only 4% leave an independent custodian in charge of the funds raised by the company.

## 12.4 Lockup periods

Firms lock up at least part of the tokens held by them and their founders in a majority (59%) of ICOs, compared to 41% of VC contracts containing vesting clauses for founders (Kaplan and Strömberg, 2003). The mean weighted average lockup period of the tokens retained by the issuing firm and its founders is 1.1 years.

Another concern for investors should be that presale investors, who usually purchase tokens at a substantial discount, could realize a profit by selling the tokens directly following the ICO in the secondary market, which would put downward pressure on prices. Investors in initial public offerings are exposed to a similar risk, because of early investors and insiders who typically own a large share of the company going public and might be looking to sell soon after the IPO. For this reason, most IPOs feature a lockup period that typically lasts for 180 days during which pre-IPO shareholders are barred from selling (Field and Hanka, 2001; Brav and Gompers, 2003). ICOs rarely address this concern, although investors would probably benefit given our finding that presale investors often quickly sell their allocation in secondary markets after the ICO is over. Only 14% of ICOs impose a lockup period on presale investors. For those that do, presold tokens remain locked up for 0.53 years on average following the ICO.

## 12.5 Control rights in angel investments

ICOs fund projects in the early stages of product development. Contractual protections of angel investors are therefore perhaps a better benchmark than protections of venture capitalists. Goldfarb et al. (2014) and Wong et al. (2009) examine the contractual provisions that angel investors request, and compare them with the provisions of venture capitalists. They generally find that the angel market is more informal than the venture capital market and has fewer control rights. However, both papers demonstrate that angel investors do receive control rights. For example, Goldfarb et al. (2014) show that in their sample, most angels get preferred stock with liquidation preferences. Wong et al. (2009) show that in their sample, angel investors get board seats in slightly less than 50% of deals and that they take straight equity without liquidation preferences in about one third of deals.

Angels make up for the lack of more detailed control rights by geographical proximity and deep industry experience. It is unlikely that ICO investors have the same geographical proximity; ICOs are typically marketed globally and the whitepaper (a document that illustrates the product, the team and the ICO in broad strokes) provided by the issuer is often translated into multiple languages. We do not know the level of industry experience of the typical ICO investor, but speculate that it is lower than for the typical angel investor, given the low contribution amount and large number of contributors in ICOs relative to angel investments.

# 13 Empirical analysis of ICO secondary market returns

We now examine how ICO investors fared in secondary markets. Did investors obtain a positive return on their ICO investments, despite the risks inherent in investing in ICOs and the lack of investor protection? Do measures that could reduce information asymmetries and substitute for the oversight typically provided by financial intermediaries have explanatory power for ICO returns and could they serve as a guidelines for investors to choose ICOs? Do contributor characteristics such as number of contributors or average contribution size help predict returns?

## 13.1 Return summary statistics

Figure 13.1 displays four graphs of the secondary market performance of all sample ICOs. The left-hand side of the figure shows equal-weighted returns, and the right-hand-side funding-weighted returns. The top two graphs show absolute returns, and the bottom two graphs show returns in excess of the return on Ether. We choose a period of 270 days (nine months) post-ICO, because it is the longest period that is complete for all sample ICOs as of the time of writing. Secondary market and crowdsale prices are available for 250 out of 306 ICOs. We exclude thinly traded observations with daily trading volume below $1,000. Furthermore, we use the last observed cumulative return for the remainder of the sample period in case a token is delisted. Delistings happen for twelve sample ICOs. If price data for a token is missing intermittently, we treat the cumulative return for the period without price data as missing as well.

Crowdsale investors gain on average 117.4% over a period of 270 days following the ICO. The figure further displays a weighted average return based on the total amount of funding raised during the ICO's crowdsale stage. The results indicate a 104.8% return over nine months.[1] We isolate the performance of individual ICOs from that of the market for cryptocurrencies in general by calculating returns in excess of the Ethereum cryptocurrency (results are comparable when we use the return on Bitcoin for reference instead). Excess returns amount to -1.5% for the full sample using equal weights and 37.0% using value weights. The results suggest that the underlying value of Ether drives much of the returns of ICO investors. Furthermore, the distribution of ICO returns is positively skewed. Figure 13.2 displays medians for absolute and excess returns. Both are negative for the median ICO after 270 days, implying that a minority of ICOs is driving the positive average returns shown in Figure 2. Our result emphasizes the lottery-like features of ICOs.

Overall, our estimates are more conservative than those of existing research on the market performance of ICOs, in particular Dittmar and Wu (2018) and Benedetti and Kostovetsky (2018). Dittmar and Wu find raw returns of 362.21% and Bitcoin-adjusted returns of 92.08%

[1] A caveat with the result that investors experience positive returns on average is that there is evidence of price manipulation in cryptocurrency markets (Gandal et al., 2018; Xu and Livshits, 2018; Li et al., 2018). The literature shows that manipulative trading in cryptocurrencies can lead to inflated prices. Gandal et al. (2018) in particular show that these inflated prices can persist for prolonged periods. If the ICO tokens we study are subject to such manipulation, it is possible that the positive returns we find will not last beyond our sample horizon.

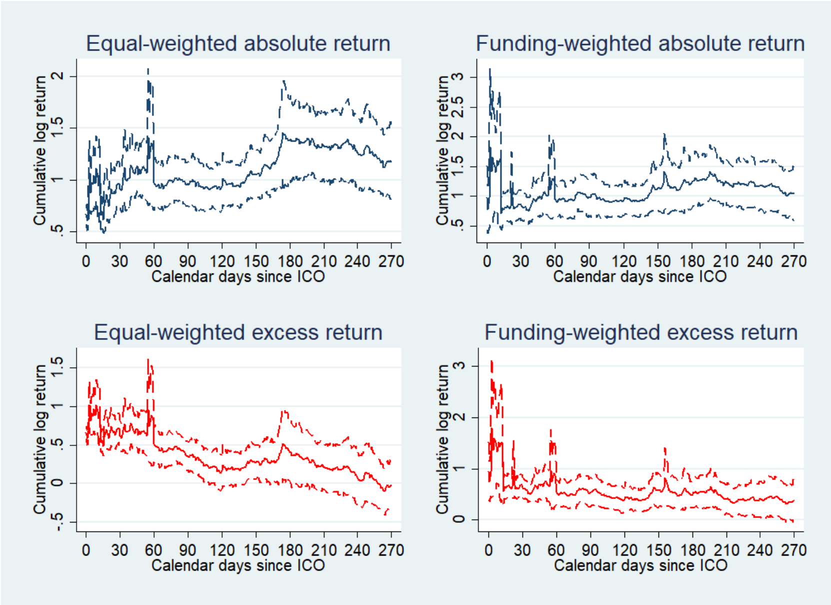



Figure 13.1 – Secondary market performance of all ICOs.
The figure is based on the secondary market prices for 250 ICOs. Returns are continuously compounded price returns based on the average price paid by investors in the crowdsale. If the average crowdsale price is unavailable, returns are based on the mid-price (average between highest and lowest price paid in the crowdsale). Observations with daily trading volume below $1,000 have been excluded. Funding-weighted returns have been weighted by total funding received during the crowdsale. Excess returns are in excess of the return on the Ethereum cryptocurrency. Dashed lines indicate the 95% confidence interval for the mean; confidence intervals have been bootstrapped using 250 replications

over a window of 180 calendar days for 570 ICOs. Benedetti and Kostovetsky find raw returns of 430.9% and Bitcoin-adjusted returns of 242.5% over the same window for a sample of 293 ICOs. It is possible that our sample of large successful ICOs is less prone to price manipulation or microstructure effects, which would explain the different findings.

Given the overall lack of disclosure and investor protection and the large number of likely uninformed retail investors, it is surprising that returns for the average ICO are positive after nine months. Lamont and Thaler (2003) argue that both frictions such as short sales constraints and irrational investors were needed for mispricing of technology stocks to persist during the tech bubble. They show that it was very difficult to short the overpriced carved out technology stocks of their sample so that the arbitrage opportunity could persist. Ofek and Richardson (2003) use a model with short sale restrictions to explain the internet bubble. Interestingly, Cheng et al. (2019) show that the positive short-term announcement returns to the usage of blockchain technology eventually reverse for publicly listed firms that do not have any expertise in the technology and for which short sales are much easier than for ICO tokens.

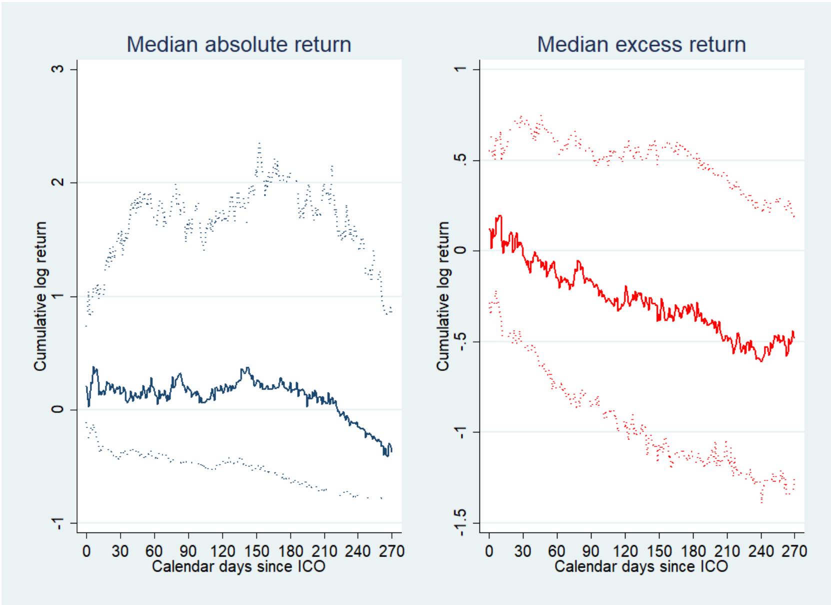



Figure 13.2 – Median secondary market performance
The figure is based on the secondary market prices for 250 ICOs. Returns are continuously compounded price returns based on the average price paid by investors in the crowdsale. Where the average crowdsale price is unavailable, returns are based on the mid-price (average between highest and lowest price paid in the crowdsale). Obervations with daily trading volume below $1,000 have been excluded. Solid lines represent the median absolute return and the return in excess of the Ethereum cryptocurrency, respectively. Dotted lines indicate the 25th and 75th percentile.

At this point in time, we cannot assert with certainty if the value of tokens is justified, perhaps due to the technological advances of the platform and products offered, or whether token valuation is a speculative bubble that may deflate in the future.

## 13.2 Determinants of returns

Table 13.1 presents regression results of the (continuously compounded) financial return to crowdsale investors 270 days following the ICO on investor and ICO characteristics. Returns are based on the average crowdsale price or the mid-price (average between the maximum and minimum crowdsale price) where the average is not known. In addition, all specifications control for the return on the Ethereum and Bitcoin cryptocurrencies over the same 270 days to isolate the performance of the individual ICO from overall market trends. We also add time fixed effects for the month of the last day of the ICO, when trading in secondary markets typically starts. We acknowledge that absent a risk model, we are unable to distinguish initial mispricing (either by the issuer, or where an auction mechanism is used, by investors) from compensation for risk in the secondary market regressions.

Table 13.1 – **Determinants of return 270 days after the ICO**

The table shows OLS regressions of ICO returns on ICO characteristics. The dependent variable is the log return based on the average crowdsale price 270 calendar days following the completion of the ICO. If the average price is unavailable, the return is calculated based on the mid-price (average between the maximum and minimum crowdsale price). If an ICO is delisted before 270 days of trading, the return is based on the last price before delisting. All variables are defined in Appendix B.2. All continuous variables are winsorized at the 1 and 99% level, respectively. One, two, and three asterisks indicate statistical significance at the ten, five, and one percent level, respectively.

| | Fundraising | | Business | Governance | All characteristics | | Investor base | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Has VC backing | 0.407* | 0.340 | | | 0.269 | 0.271 | | 0.622** |
| | (1.90) | (1.39) | | | (1.19) | (1.05) | | (2.40) |
| Unsold tokens burnt | -0.055 | -0.001 | | | -0.067 | -0.046 | | 0.472* |
| | (-0.27) | (-0.00) | | | (-0.32) | (-0.19) | | (1.74) |
| Ln(1+presale amount raised) | -0.149* | -0.241* | | | -0.197** | -0.394*** | | 0.053 |
| | (-1.80) | (-1.71) | | | (-2.16) | (-2.73) | | (0.40) |
| Ethereum return | 1.313*** | 0.744 | 0.898** | 0.979** | 1.149** | 0.883 | | 0.351 |
| | (2.73) | (1.10) | (2.23) | (2.34) | (2.26) | (1.13) | | (0.46) |
| Bitcoin return | -0.880 | -0.051 | 0.024 | 0.073 | -0.616 | -0.171 | | 1.613 |
| | (-1.11) | (-0.05) | (0.03) | (0.10) | (-0.78) | (-0.15) | | (1.66) |
| Presale discount | | -1.253** | | | | -1.068 | | |
| | | (-2.08) | | | | (-1.60) | | |
| Presale lockup period | | 0.582 | | | | -0.452 | | |
| | | (0.61) | | | | (-0.50) | | |
| Product or prototype | | | -0.023 | | -0.180 | -0.155 | | 0.538* |
| | | | (-0.14) | | (-0.91) | (-0.56) | | (1.95) |
| Experienced team | | | 0.103 | | 0.029 | -0.047 | | 0.316 |
| | | | (0.59) | | (0.14) | (-0.18) | | (1.12) |
| High quality advisory team | | | 0.128 | | 0.239 | 0.291 | | 0.204 |
| | | | (0.73) | | (1.14) | (1.26) | | (0.59) |
| Project code available | | | 0.539*** | | 0.490** | 0.341 | | 0.453 |
| | | | (3.21) | | (2.41) | (1.14) | | (1.62) |
| Use of proceeds mentioned | | | -0.151 | | -0.039 | 0.177 | | -0.261 |
| | | | (-0.76) | | (-0.14) | (0.53) | | (-0.95) |
| Offshore incorporation | | | | -0.208 | 0.000 | -0.084 | | 0.445 |
| | | | | (-0.92) | (0.00) | (-0.22) | | (1.22) |
| Legal form and jurisdiction known | | | | 0.295 | 0.356 | 0.129 | | -0.021 |
| | | | | (0.85) | (0.91) | (0.27) | | (-0.04) |
| KYC/AML procedure | | | | 0.319 | 0.172 | 0.193 | | 0.597** |
| | | | | (1.54) | (0.71) | (0.65) | | (2.41) |
| Token share team (ex ante) | | | | -0.224 | -0.384 | 0.128 | | -1.666* |
| | | | | (-0.48) | (-0.68) | (0.17) | | (-1.97) |
| Team lockup period | | | | 0.221** | 0.247** | 0.450*** | | 0.270* |
| | | | | (2.54) | (2.14) | (4.50) | | (1.77) |
| Legal advisor disclosed | | | | 0.022 | 0.109 | 0.269 | | 0.293 |
| | | | | (0.12) | (0.55) | (1.09) | | (0.97) |
| Ln(number of contributors) | | | | | | | 0.103 | -0.002 |
| | | | | | | | (0.80) | (-0.02) |
| Ln(median contribution size) | | | | | | | -0.156 | -0.385** |
| | | | | | | | (-0.83) | (-2.29) |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 207 | 108 | 261 | 251 | 199 | 106 | 71 | 70 |
| $R^2$ | 0.59 | 0.57 | 0.58 | 0.58 | 0.63 | 0.62 | 0.69 | 0.85 |

The coefficients in Columns four to six indicate that a one standard deviation increase in the lockup period increases holding period returns by 27.0 to 54.9 percentage points, with statistical significance at the 5% level or below in Columns four and six. A possible explanation for this result is that a longer lock up period improves the alignment of incentives between investors and the team. Additionally, Columns three and five indicate that ICOs which disclose the project's source code ex ante produce 49.0 to 53.9 percentage points higher holding period returns. Obvious explanations based on mispricing are that the disclosure makes it more likely that the firm will be able to deliver a viable product, or less likely that the ICO is a scam.

Columns five and six indicate that holding period returns are negatively correlated with the presale amount, statistically significant at the five and one percent level. The corresponding estimates in Columns one and two are statistically significant at the 10% level. The coefficient estimates are economically meaningful as well, implying that a one percent increase in the amount raised in the presale leads to a 0.2 to 0.4 percentage point decrease in the holding period return. An explanation for this result based on mispricing could be that because presale investors receive a discount, they can lock in a profit by selling their tokens in the secondary market directly after the ICO, thereby putting downward pressure on prices. Consistent with this explanation, the coefficient estimates for the presale discount are negative, with statistical significance at the 5% level in Column two. A one standard deviation increase in the presale discount is associated with a 42.8% decrease in the holding period return for crowdsale investors.

Overall, however, the holding period return of the Ethereum cryptocurrency has largest explanatory power for nine-month ICO returns, both in terms of statistical and economic significance. The corresponding coefficient estimates are statistically significant at the 5% level in Columns one, three, four and five and imply that a one percentage point increase in the return on Ethereum is associated with a 0.9 to 1.3 percentage point increase in the holding period return of an ICO.

There is reason to believe that the holding period return might depend on the composition of the investor base. The results presented in Section 11 show that larger investors sell their tokens sooner, at least partially due to presale investors who can lock in a profit by selling their tokens right after the ICO, and the tests in this section have established a negative correlation between secondary market returns and the amount of funding raised in the presale. Column seven therefore presents the results of additional specifications regressing the nine month holding period return on the number of contributors and the size of the median contribution, both in natural logarithms. Neither variable is statistically significant. But when we add the full set of ICO-level controls to the regression in Column eight, the coefficient for the median contribution size is negative with statistical significance at the five percent level. The coefficient suggests that a one percent increase in the size of the median contribution leads to a 0.4 percentage point decrease in the holding period return. This result provides further evidence that the presence of larger investors eventually leads to lower secondary market returns.

# 14 Conclusion

Initial coin offerings are a novel fundraising mechanism for start-up companies, in particular those focusing on applications of the blockchain technology. Our paper characterizes the typical ICO investor and seeks to understand his primary motives to participate in the ICO market.

Based on an analysis of ICOs hosted on the Ethereum platform, we conclude that most contributors are likely to be retail investors. The average ICO has over 4,000 contributors. The median contributor invests a relatively small amount. The ICO market appears to have successfully given access to the financing of innovation to a new class of investors, which is a long standing public policy issue (e.g., the Jumpstart Our Business Startups Act, or JOBS Act, passed in 2012 in the US, also wishes to encourage the financing of startups by smaller investors).

For at least half of all primary market investors, the goal of participating in the ICO appears not to be the pre-purchase of a product that they intend to use but rather speculation, as they sell the tokens before the product is developed. Large presale investors who certify ICOs and whose participation is monitored and relied upon by crowdsale investors (e.g., Howell et al., 2018; Fisch, 2019) are potentially conflicted. They buy tokens at a significant discount of 34% average, and can lock in a profit by selling their allocation in the secondary market right after the ICO. We show that large investors indeed sell quickly after the ICO, and we find that holding period returns for crowdsale investors are significantly lower in ICOs with a large presale and/or a large presale discount.

ICO returns have features akin to lottery stocks, and most projects feature a new technology that has the potential to lead to dramatic efficiency improvements and new applications. Both of these characteristics have been shown to be of interest to retail investors (e.g., Kumar, 2009; Cooper et al., 2001). These characteristics could explain why retail investors purchased ICOs despite lack of detailed information on the funded projects and why ICO returns are on average positive nine months after the ICO. Because blockchain technology is a recent development that has not yielded many economically viable applications, it seems impossible to assert with certainty whether the returns we find are justified, or whether ICO tokens are currently experiencing a speculative bubble that may deflate in the future.

# Part III

# Information Intermediaries: How Commercial Bankers Facilitate Strategic Alliances (co-authored with Christoph Herpfer)

# 15 Introduction

Banks obtain detailed, private information about their corporate clients through lending (Fama, 1985; Diamond, 1984; Petersen and Rajan, 1994) and advisory relationships (Lowry et al., 2019). Such privileged access to information can create agency conflicts when banks use information from a lending relationship in other areas to their own advantage. There is wide anecdotal evidence of banks allegedly passing on information to the opposing party in M&A transactions or using it for insider trading, with a number of cases resulting in high profile lawsuits.[1] As a consequence, much of the academic literature on information spillovers in the banking sector has focused on possible negative consequences for clients.[2]

Our paper identifies a potentially beneficial side to information spillovers for clients of commercial banks in the U.S. syndicated loan market. We investigate how commercial bankers create value for borrowers by brokering collaborations between them. We focus on a broad set of collaborations in the form of strategic alliances, formalized collaborations that are somewhere between arm's length, market based transactions and intra firm relationships.[3] Our analysis documents that banks, and individual bankers in particular, act as information intermediaries between potential partners, thereby facilitating alliances, and creating value for borrowers.

These collaborations are an ideal laboratory to study information transmission through banks since they are publicly observable forms of collaboration that are sensitive to information asymmetries and create value for firms (Chan et al., 1997). Most alliances are formed to benefit from specific knowledge or capabilities of the partner firm (Mariti and Smiley, 1983), therefore requiring partners to possess specific, potentially non-public information about each other's capabilities ex ante. One potential source to obtain this information are capital providers associated with both firms (e.g., Lindsey, 2008; He and Huang, 2017). For example, Greg Becker, CEO of Silicon Valley Bank, describes his bank's advantage as its "ability to make an introduction to a potential partnership, because we understand that business better than maybe one of our competitors would", as well as "the value added we give to our clients, whether it is making an introduction to a potential client or making an introduction to a potential partnership".[4]

[1] A prominent lawsuit involving M&A transactions is Dana Corporation v. UBS *(Dana Corporation v. UBS Securities LLC, New York Southern District Court, Case No. 1:03-cv-05820).* In 2018, there were similar allegations in an M&A transaction advised by Goldman Sachs (The New York Times, 2018). Possibly as a result of these lawsuits, some large advisory clients now require banks to enter non-compete agreements (Financial Times, 2018a). Examples for cases involving insider trading include ASIC v. Citigroup (Financial Times, 2006) and the SEC against Barclay's (Securities and Exchange Commission, 2007).

[2] E.g., Bodnaruk et al. (2009), Acharya and Johnson (2007), Asker and Ljungqvist (2010), Griffin et al. (2012), Kedia and Zhou (2014) and Ivashina et al. (2009).

[3] The literature on strategic alliances sometime focuses on a more narrow set of research oriented alliances, for example in the biotechnology sector. While we use the term strategic alliances, we look into collaborations more broadly, including marketing and production alliances. As an illustration, consider supplier customer relationships. At the arm's length level, a firm can purchase input material on a transaction-by-transaction basis. Alternatively, it can formalize the relationship in a customer supplier agreement, a specific type of alliance. The closest form of collaboration would be a takeover of the supplier to internalize the relationship.

[4] See interview "Meet your partner: The bank as matchmaker" in the 2016 PwC US CEO survey, starting at 4:04, available at https://youtu.be/t3wAOBeG81o?t=244. Appendix C.1 provides a more extensive transcript of this interview and presents additional anecdotal evidence from news stories and our own conversations with

To link borrowers to specific commercial bankers, we use data from the signature pages of loan contracts. These data allow us to identify connections between bankers and borrowers and to assess whether two firms have borrowed not just from the same bank, but from the same specific banker in the past. We hypothesize that individual bankers are the specific economic channel through which information is transmitted. Commercial bankers play a key role in negotiating, structuring and monitoring loan agreements, which allows them to form a close relationship with firms' management and gives them access to private information (Esty, 2001; Uzzi, 1999; Uzzi and Lancaster, 2003).

We first test directly whether strategic alliances between pairs of firms are more likely if the pair is connected through a network of bankers using a simple univariate t-test. The results show that firms are significantly more likely to enter strategic alliances with partners they are connected to (either directly or indirectly) as compared to the overall universe of potential partner firms.

We then estimate panel regressions that allow us to control for time-invariant firm-pair characteristics such as geographic proximity, industry, firm quality and compatible corporate culture and strategy through firm-pair fixed effects. In addition, these regressions include industry-year fixed effects for both firms, which absorb time-varying confounding factors at the industry-level. These specifications also allow us to separate the effect of connections through bankers as people from that of banks as institutions by directly controlling for whether a potential alliance pair has borrowed from the same bank in the past. We find that sharing the same banker significantly increases the likelihood of entering a strategic alliance at a rate that is economically about five times as large as that of sharing the same bank. In additional robustness tests, we control for time-varying firm-level unobservables by including firm-year fixed effects for both firms in a potential alliance pair. The results persist in this heavily saturated specification.

Since each banker has only a limited set of direct borrowers, it can be hard for them to match firms within their own portfolio of borrowers. We therefore also investigate whether indirect connections between borrowers through a network of bankers can facilitate alliances. For our purposes, connected bankers are defined as two or more individuals who have previously syndicated loans together. Previous co-syndication is a good proxy for personal connections since the bankers involved in a lending syndicate interact with each other repeatedly during the origination process (Esty, 2001). After origination, bankers stay in touch over the life of the loan for the purpose of monitoring covenants and renegotiating terms.[5] We hypothesize that bankers can use these connections to find suitable collaboration partners for their portfolio of borrowers, similar to board members connecting firms in M&A transactions (Cai and Sevilir, 2012). We then test whether these indirect network connections between bankers can help broker collaborations for borrowers in the same way as direct ones from sharing the same

practitioners suggesting that banks actively arrange collaborations for their borrowers.

[5]The average loan is modified five times (Roberts, 2015) and more than 90% of loans undergo at least one such renegotiation (Roberts and Sufi, 2009).

banker. We find that even indirectly connected borrowers are significantly more likely to engage in a strategic alliance, albeit at a lower rate than directly connected firms.

Brokering alliances between borrowers requires coordination and effort on the part of bankers. Therefore, the ability of bankers to facilitate alliances between clients should decrease as more links in the banker network are needed to connect the firms. This prediction is borne out in the data, where we find that the likelihood that two firms enter an alliance is monotonically decreasing in the network distance between their bankers. Our results are robust to a wide range of alternative definitions and estimation techniques of banker networks, firm-bank relationship, and fixed effects. We further perform a number of tests that rule out that our results are driven by firms initiating collaborations first, before starting to borrow from the same banker later.

Firms have limited use and capacity for alliances. Once a firm has decided to collaborate with a certain partner, it is less likely to engage in another alliance for the same purpose with another firm. Because the level of observation in our sample is a pair of firms, the result is a correlation structure that conventional clustering of standard errors in panel regressions cannot fully account for (Cameron and Miller, 2014). To account for the interconnected nature of alliance formation, we estimate a sequenced conditional logit model. This discrete choice model developed by Lindsey (2008) allows us to explicitly model firms' choice of alliance partners over time. The tests confirm that firms sharing a banker or an indirect connection through a network are significantly more likely to enter an alliance and that the influence of indirect connections is decreasing in the network distance between bankers.

If bankers facilitate collaborations due to their knowledge of borrowers, their role should be more pronounced when information asymmetries are large. We investigate this conjecture in cross-sectional tests and find that banker connections are more important for informationally opaque borrowers, in particular those that lack a public credit rating or have a high share of intangible assets.

In our final set of results, we investigate whether commercial bankers' involvement in the formation of strategic alliances benefits borrowers and their banks. First, we document that firms with well-connected bankers form a larger number of alliances than those with less well-developed networks. In an event study, we find that the average strategic alliance increases market value by 0.7% (consistent with Chan et al., 1997; Allen and Phillips, 2000; Bodnaruk et al., 2013), but that strategic alliances in which firms are connected through the banker network create as much value as those without such a connection. Together, our results suggest that banker networks benefit firms on the extensive rather than the intensive margin when forming strategic alliances. Better connected firms form a larger number of alliances, which are valuable on average. The individual alliances of well-connected firms, however, are not any more valuable than those of less well-connected firms.

We also find that borrowers reward banks for facilitating collaborations by awarding them additional business. After a firm initiates a strategic alliance with another firm it is connected to through the banker network, the connecting banks are substantially more likely to be chosen as the lead arranger for an additional syndicated loan by those firms in the next five years. Similarly, borrowers are significantly more likely to choose such banks as the underwriter for a bond or seasoned equity offering, albeit to a lower extent.

Our paper contributes to two different strands of the literature. The first one is concerned with the impact of investors and financial intermediaries on different forms of collaboration between firms. Ivashina et al. (2009) and Fee et al. (2017) show that banks use private information about borrowers in merger transactions. We add to those findings by showing that information transmission through banks does not just lead to M&A transactions, but also less intense forms of collaborations. Similarly, our paper extends the work of Lindsey (2008) and He and Huang (2017) who illustrate the importance of capital providers other than banks in facilitating collaborations between firms.[6] He and Huang (2017) find that strategic alliances are more likely between firms that have a high degree of institutional cross-ownership. Lindsey (2008) shows that venture capital funds broker strategic alliances within their portfolio of startup firms as long as at least one of them is private. We add to this literature by documenting that banks can act as matchmakers as firms grow and switch from venture capital to bank funding. Our results are perhaps more surprising than those of the previous literature, because commercial banks generally neither have board seats nor equity stakes in the companies they arrange alliances for. Our findings imply that they nevertheless have both the ability and incentives to provide these services to borrowers. In addition, we demonstrate that even indirect connections through a network of bankers can transmit information and facilitate collaborations.

The second literature we contribute to relates to the importance of personal relationships in bank lending. We find that individual bankers are the primary conduit through which information is transmitted, and document the importance of professional networks beyond executives (Engelberg et al., 2012; Karolyi, 2017). We also add to a growing number of studies that investigates the role of individual commercial bankers in the lending process to large, publicly traded corporations (Herpfer, 2018; Gao et al., 2017, 2018a).

[6]Additional evidence for the role of banks in shaping collaborations between firms can be found in Coiculescu (2018), who finds that firms sharing the same bank are more likely to enter a customer-supplier relationship, and Saidi and Streitz (2019), who find evidence that firms sharing the same lender compete less aggressively.

# 16 Hypotheses development

Being connected through lenders can help borrowers looking for a collaboration partner to overcome asymmetry in both public and private information. First, selecting the right alliance partner can be difficult if alliance success relies on private information. Second, even if all relevant information is public, search costs can impede the formation of collaborations. Bankers play a role in overcoming both these challenges. First, since they interact with a number of different borrowers, bankers likely have access to public and private information regarding potential partners which can speed up searches. In addition, if an alliance requires a certain non-publicly observable (e.g. managerial or technological) capability, bankers can identify potential partners using private information obtained through their lending. One banker interviewed by Uzzi and Lancaster (2003) describes the process through which bankers form connections between borrowers: "You happen to find out that a firm is having problems sourcing a certain raw material, and the banker happens to know someone that provides that material. [..] the banker happens to know someone that they can trust that can help out. On and on, that's a network." Another banker states that "there are costs to the entrepreneur to gather [select] information. A relationship can set me apart if I deliver the information. That's the concept of value-added provider." We therefore formulate

*Hypothesis 1:* Two firms are more likely to enter a strategic alliance if they share the same banker.

The ability of bankers to find matching alliance partners is limited by the number of firms about which they have information. One way a banker can increase the number of potential partners she has access to is by reaching out to her network. If alliances are beneficial to borrowers, bankers might be willing to assist in arranging an alliance even if one of the partners is not their own client but somebody else's (e.g. because improved borrower performance aids bankers' career, see Gao et al., 2018b). Bankers can facilitate alliances even if none of their own borrowers are directly involved in it, by connecting other bankers to each other. Such transmission of information across two degrees of separation would imply that bankers can trade favors to each other. Transmitting private information over longer network paths (i.e. a larger number of bankers) likely increases the cost of coordination. We therefore formulate

*Hypothesis 2:* Firms are more likely to enter an alliance if they deal with different bankers that know each other, either directly or through one or several acquaintances. The magnitude of this effect decreases as the number of links required to connect the bankers increases.

Figure 16.1 illustrates a simplified example of how firms are connected through the banker network. Consider three bankers (1 to 3) and four firms (A to D). At time t = 0, each firm has borrowed from one banker each. Both banker 1 and banker 3 have previously co-syndicated one loan each with banker 2. If firm A was to consider a potential collaboration at this point, it could obtain information about its three potential partners from its banker, banker 1. Since banker 1 has previously worked with banker 2, the network distance between firms A and B takes the value 1. It would be relatively easy to obtain information about banker 2's client, firm B. The network distance between firm A and firm C takes the value 2, since their bankers have not previously co-syndicated loans and are only indirectly connected through banker 2. Finally, there is no way for firm A to obtain information about firm D through the banker network.

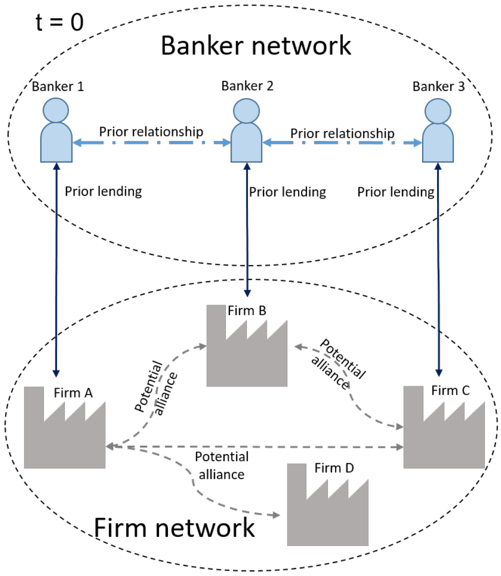



Figure 16.1 – Illustration of the banker network
The figure presents a simplified illustration of the multilayer network structure. The upper bubble represents the banker network between three bankers. The lower bubble represents the firm network of borrowers. Connections between bankers exist if the bankers have co-syndicated loans in the past. Connections between firms and bankers are established when the banker signs a syndicated loan contract with the firm, but only when serving as lead arranger. At time 0, firms A, B, and C borrow from bankers 1, 2, and 3, respectively. Firm D is unconnected to the banker network. Banker 2 has co-syndicated separate loans with both banker 1 and banker 3 in the past. The network distance between firm A to its potential collaboration partners is therefore 1 to firm B, and 2 to firm C. Its network distance to firm D is undefined. At time 1, firm B takes out a new loan from banker 1. The network distance between firm A and firm B therefore shrinks to 0. Dotted (full) gray lines between firms denote potential (realized) alliances. For clarity we only display the potential alliances for firm A.

At time t = 1, firm B has taken out a new loan from banker 1. Accordingly, the network distance between firms A and B has decreased to 0. In the context of this example, hypotheses 1 and 2 suggest that firm A is more likely to engage in a strategic alliance with firm B than with firm C, both at t=0 and at t=1. Our main specification includes firm-pair fixed effects, and hence identifies correlations between network distances and the likelihood of entering an alliance only based on *changes* in network distance, such for firms A and B from t=0 to t=1 in the example above.

Anecdotal evidence from news stories and our conversations with practitioners detailed in

Appendix C.1 suggest that bankers mainly aid firms through lowering search frictions, leading to faster and more efficient outcomes for finding partners. We hypothesize that the help in finding a collaboration partner is most valuable when search costs for borrowers are highest, which leads to our next hypothesis.

*Hypothesis 3:* The role of bankers in facilitating alliances is more pronounced in circumstances with high information asymmetries.

Finally, we ask why firms would want bankers to facilitate alliances for them, and why bankers would exert effort to do so. To explain these behaviors, alliances arranged through bankers should benefit both the alliance partners as well as the bank(s) brokering the alliance. More well connected banks can add value both on the intensive and the extensive margin. First, if bankers indeed lower search costs for firms looking for alliance partners, firms with more well connected bankers should enter a larger number of alliances even if the value of the alliance is unaffected by the involvement of a banker. The reasons is that the potential benefit from an alliance will exceed the cost of arranging the alliance in a larger number of cases when the search cost are lower. We therefore formulate the following hypothesis:

*Hypothesis 4a:* Firms with more well-connected bankers enter a larger number of strategic alliances.

Second, if the alliances brokered by bankers are value enhancing, firms' market value should increase upon the announcement of such an alliance. We explicitly do not have a prior on whether strategic alliances arranged through a bank should create more, less or the same amount of value as the average alliance. Accordingly, we formulate the next hypothesis.

*Hypothesis 4b:* Alliances facilitated by bankers are associated with an increase in participating firms' market value.

One reason for why bankers might assist firms in finding partners for strategic alliances is an expectation of being compensated through lucrative mandates in the future. While there is little academic research on the topic (with the exception of Bharath et al., 2007), there is ample anecdotal evidence of banks providing free services to their corporate customers in the hope of building relationships.[1] We hypothesize that future mandates are the primary motivation

[1] In 2018 a consortium of banks underwrote a $1.3bn bond offering by three Indian state owned companies for free. The Wall Street Journal (2018) commented that "banks that agree to arrange bond offerings for ultralow fees are generally hoping to build relationships with corporate clients for future deals." Similarly, observers have speculated that banks who provide certain types of loans to corporate clients primarily do so to build client loyalty (Financial Times, 2016a; The Wall Street Journal, 2017). As a final example, in the course of a parliamentary investigation in the United Kingdom, Goldman Sachs stated that it "often carries out unpaid work for longstanding clients", listing a total of 25 unpaid assignments it had carried out for one particular client over a period of 12 years (Financial Times, 2016b). Finally, we want to note that an increase in firm value benefits banks not just through

for bankers to get involved in the facilitation of strategic alliances.

*Hypothesis 4c:* Borrowers reward banks for brokering alliances by giving them additional business.

potential future business, but also directly as they hold the firm's debt. This effect through a lower likelihood of bankruptcy is, however, likely a smaller incentive than the promise of additional business.

# 17 Data

## 17.1 Data on bankers

We follow a number of recent papers (e.g. Gao et al., 2018b; Herpfer, 2018) and obtain data from the signature pages of publicly available loan contracts to link individual bankers to specific corporations. All U.S. companies with publicly traded securities are obliged to file "material contracts" with the securities and exchange commission (SEC). The SEC makes these filings available to the public through its electronic archive system EDGAR.[1] The majority of loan contracts contains a signature page featuring the names and functions of all banks involved in the deal and the names of all bankers representing those banks.

We use a search algorithm to identify loan contracts from EDGAR and extract the name of each banker involved in the deals. Figure 17.1 shows the layout of such a signature page and marks the data items extracted by the algorithm. Most loans to large, publicly traded borrowers are syndicated between multiple banks. Since the algorithm extracts the names of all bankers involved in a syndicated loan, our data do not just allow us to track individual bankers, but also to construct a network of linkages between bankers based on whether they have syndicated a loan in the past. A more detailed description of the extraction procedure, the resulting data set and various quality controls can be found in both Herpfer (2018) and Gao et al. (2018b).

To formally model the effect of bankers on the formation of strategic alliances, we employ a rudimentary multilayer network approach. The first network consists of firms, which form the nodes of that network. Connections between firms, the intra-layer edges, represent strategic alliances between firms. The network's second layer consists of bankers in the syndicated loan market. Each banker is a node, and links are constructed through bankers' joint appearance on loan contracts (i.e. we assume two bankers are acquainted after they show up as signatories on the same loan contract). The inter-layer edges, representing connections between bankers and firms, are created when a banker signs a loan contract with the firm, but only while representing the loan syndicate's lead arranger. In this case, the syndicate's lead banker has a professional relationship with the borrowing firm.[2] In our sample, bankers have personal relationships with between 1 and 13 distinct borrowers. The relatively small number of relationships makes it more likely that bankers have intense relationships with each borrower.[3]

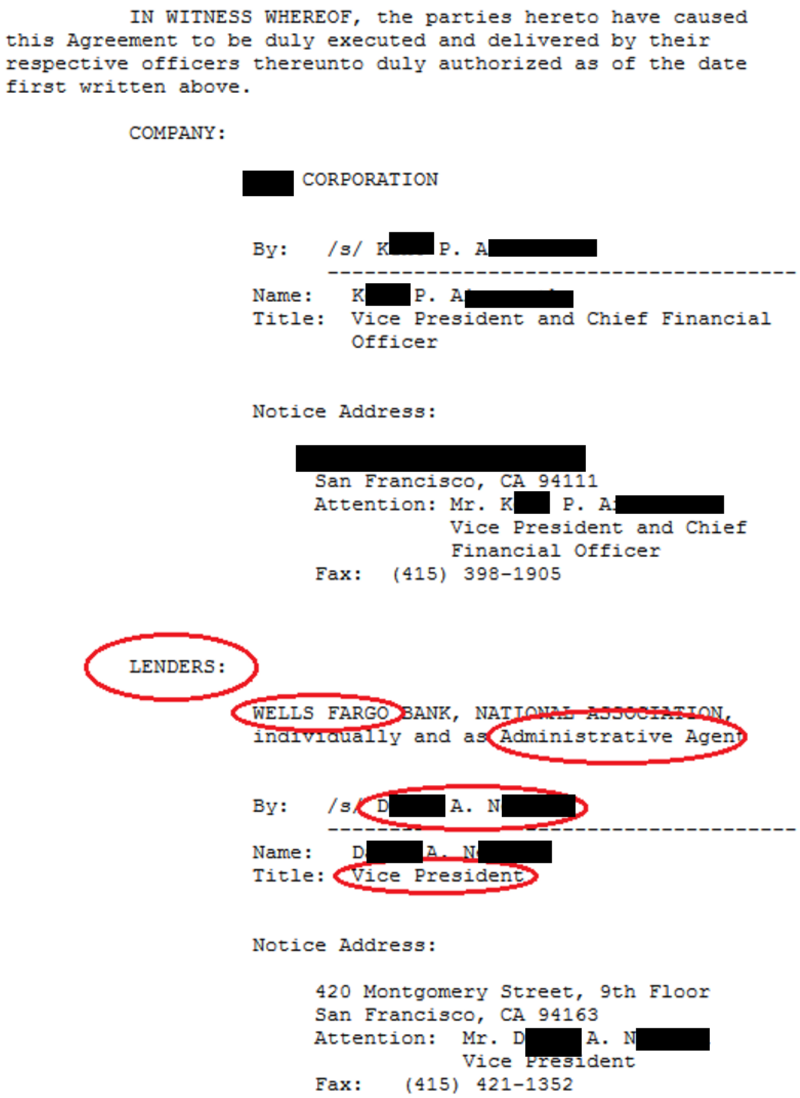
Existing work provides evidence that these signatures correctly identify the bankers involved in the lending decision process, and that the data is of high quality (Herpfer, 2018; Gao et al., 2018b). To the degree that there is measurement error, e.g. because bankers make loans to private firms which are unobservable, we will tend to underestimate the degree to which

[1] Since loan contracts are considered material under item 601(b) of Regulation S-K, EDGAR provides a comprehensive list of all loan contracts since the inception of mandatory electronic filing in 1996. Information from these contracts is also a primary source for DealScan (see Chava and Roberts, 2008).

[2] See Esty (2001) for a case study on the syndication process and the relationship formation between lead banks and borrowers.

[3] We likely understate the true number of clients since our dataset limits us to publicly traded borrowers. Uzzi (1999) finds that bankers in the mid-market segment have between 6 and 50 clients, using proprietary data from a mid-market lender.

Figure 17.1 – Example of simple signature page with a single bank
The red circles indicate information extracted by the text search algorithm. This information includes the name and role of the bank, as well as the name and title of the signatory. The names of the banker, corporation, and corporate executive are anonymized for the sake of privacy. The prior literature offers additional, detailed descriptions of the data as well as extensive quality checks (e.g. Herpfer, 2018; Gao et al., 2018b).



IN WITNESS WHEREOF, the parties hereto have caused this Agreement to be duly executed and delivered by their respective officers thereunto duly authorized as of the date first written above.

COMPANY:

[redacted] CORPORATION

By: /s/ K[redacted] P. A[redacted]
Name: K[redacted] P. A[redacted]
Title: Vice President and Chief Financial Officer

Notice Address:

[redacted]
San Francisco, CA 94111
Attention: Mr. K[redacted] P. A[redacted]
Vice President and Chief Financial Officer
Fax: (415) 398-1905

LENDERS:

WELLS FARGO BANK, NATIONAL ASSOCIATION, individually and as Administrative Agent

By: /s/ D[redacted] A. N[redacted]
Name: D[redacted] A. N[redacted]
Title: Vice President

Notice Address:

420 Montgomery Street, 9th Floor
San Francisco, CA 94163
Attention: Mr. D[redacted] A. N[redacted]
Vice President
Fax: (415) 421-1352

borrowers are connected through the banker network, which biases our analysis against finding an effect of banker networks on alliance formation.

One potential concern with the estimation is reverse causality: Two firms might enter a strategic alliance and subsequently both start borrowing from the same bank, e.g. due to word of mouth recommendations or to raise funding for a joint project. To rule out that strategic alliance precede connections through the banker network, we lag the network characteristics by one period in all estimations.[4]

## 17.2 Data on strategic alliances

Data on strategic alliances comes from Standard and Poor's (S&P's) Capital IQ and SDC Platinum. Importantly, both databases classify a wide range of collaborations as "strategic alliances", including collaborations in marketing, production and customer-supplier agreements. Capital IQ covers announcements regarding the initiation or modification of strategic alliances between two or more firms since 2002. A database entry consists of the names and identifiers of the firms involved, a headline that briefly mentions the participating firms and the alliance's content and purpose, a detailed description and a reference to the source of the information. Capital IQ does not classify database entries by their timing (i.e. whether the announcement concerns the initiation of a new alliance or the termination of an existing alliance). Since we are only interested in initiations we apply pattern-matching programs to the database entries' headlines to filter out items referring to the termination of an existing alliance. SDC Platinum lists announcements of strategic alliances ranging back to the 1960s, covering the initiation of strategic alliances and a multitude of attributes such as the alliance's purpose and announcement date.

We collect strategic alliances announced between 2002 and 2013 from both databases and merge the resulting data sets. We aggregate all strategic alliances by the ultimate parent of the announcing firm and retain only those alliances where all parties involved have an ultimate parent that is publicly listed and incorporated in the United States. For every firm-pair, we only retain the first alliance announcement over the sample period. Note that our data covering bankers goes back to 1996, which gives us six years prior to the sample to let the banker network build up. Given an average loan maturity of about four years, our network should sufficiently approximate the underlying, unobservable connections between individuals at the start of our estimation sample in 2002. We treat alliances between more than two firms as a set of bilateral alliances between all parties involved.

Finally, we merge the strategic alliances with financial data from Compustat and the personal relationship measures discussed above.[5] The final sample covers 3,189 strategic alliances

[4] In un-tabulated results we confirm that both the OLS and sequenced conditional logit estimates are robust to increasing this lag to two years.

[5] Data from Capital IQ can be directly merged on Compustat's *gvkey*, whereas firms in the SDC data are identified by their CUSIP code.

between publicly listed, non-financial US firms with non-missing accounting data.

## 17.3 Sample characteristics

Table 17.1 displays summary statistics for alliance pairs in the year they are first observed. All variables are calculated as defined in Appenix C.2.

Table 17.1 – Summary statistics for observed initial alliance pairs
The table presents descriptive statistics for firm-pairs at the time they form an alliance. Variables are defined as defined in Appendix C.2.

| Panel A: Bank loan characteristics | | | | | |
|---|---|---|---|---|---|
| | Obs. | Mean | SD | Min | Max |
| Same bank | 3,189 | 0.18 | 0.39 | 0.00 | 1.00 |
| Same banker | 3,189 | 0.03 | 0.18 | 0.00 | 1.00 |
| Banker network connection | 3,189 | 0.11 | 0.31 | 0.00 | 1.00 |
| Banker network distance | 348 | 0.91 | 0.77 | 0.00 | 3.00 |
| One has a syndicated loan | 3,189 | 0.88 | 0.32 | 0.00 | 1.00 |
| Both have a syndicated loan | 3,189 | 0.44 | 0.50 | 0.00 | 1.00 |
| **Panel B: Firm-pair characteristics** | | | | | |
| | Obs. | Mean | SD | Min | Max |
| Same state | 3,189 | 0.17 | 0.38 | 0.00 | 1.00 |
| One high intangibles | 2,938 | 0.32 | 0.47 | 0.00 | 1.00 |
| One unrated | 3,189 | 0.69 | 0.46 | 0.00 | 1.00 |
| Previous alliances | 3,189 | 17.13 | 27.35 | 0.00 | 220.00 |

The syndicated loan market is a common source of funding for the firms in our sample: for 88% of observed alliances, at least one firm has borrowed in the syndicated loan market before entering the alliance, and for 44% of alliances both have done so. At the time they enter a strategic alliance, firms are substantially more likely to have borrowed from the *same bank* (mean = 0.18) than from the *same banker* (mean = 0.03) at any point in the past. About 11% of all firm-pairs are connected through the banker network at the time an alliance is initiated (*banker network connection = 1*). Note that our sample is limited to formalized collaborations between firms, because arm's length transactions are usually unobservable. Because smaller, informal collaborations are unobservable, our analysis provides a lower bound for the role of banker connections in facilitating collaboration between borrowers. Banker network distance is expressed as the number of connections between bankers needed to connect two firms. Accordingly, a network distance of 0 corresponds to two firms sharing the same banker. The firm-pairs that are connected via the banker network have a mean distance of only 0.91, with the modal distance being one. Low distances are therefore most common. Because distances exceeding two are rare (less than two percent of the sample), we censor the banker network distance at three (i.e. we pool all distances exceeding two).[6]

[6]Our results are both statistically and economically similar when we do not make this change.

# 18 Results

This section presents various specifications estimating the impact of shared banker connections on the formation of alliances between borrowers.

## 18.1 Univariate test and OLS results

We begin our analysis with a simple, univariate estimate for whether firms' connections through bankers affect their propensity to enter strategic alliances. For this test, we consider all network connections and alliances established over the entire sample period. The sample consists of all publicly listed US firms in Compustat between 2002 and 2013 that enter at least one strategic alliance over that same period. We implement the univariate test on two different levels: by firm and by banker portfolio. The firm-level test compares firms' propensity to enter alliances with potential partners they are connected to through the banker network to their unconditional propensity to ally. For this purpose, we calculate two ratios; a firm's *within-network alliance ratio*, intended to capture the firm's propensity to enter strategic alliances with other firms it is connected to via the banker network, is defined as:

$$within\text{-}network\ alliance\ ratio_j = \frac{C_j}{n_j} \tag{18.1}$$

where $C_j$ is the number of firms $j$ is connected to and enters a strategic alliance with and $n_j$ is its total number of connections. This ratio is compared to its *total alliance ratio*, which is designed to capture a firm's unconditional propensity to enter strategic alliances, defined as:

$$total\ alliance\ ratio_j = \frac{A_j}{n-1} \tag{18.2}$$

where $A_j$ is the total number of firms that $j$ enters a strategic alliance with and $n$ is the number of sample firms. The two ratios are then compared to each other by means of a simple t-test.

For illustration, consider the situation in Figure 16.1 at time $t = 1$. Firm A has entered only one strategic alliance, the partner for that alliance being firm B. Firm A's within-network alliance ratio as defined by equation (18.1) is then $\frac{1}{2}$; there are two firms it is connected to via its banker network, B and C, and it has entered an alliance with one of them. Its total alliance ratio as defined by equation (18.2), on the other hand, is $\frac{1}{3}$. It has still only entered one alliance – with firm B – but the total number of potential alliance partners across network boundaries is three (firms B, C and D).

The results of this test in our sample are displayed in Panel A of Table 18.1. There are 669 observations, equal to the number of sample firms. Means and standard errors in Table 18.1 have been scaled by 100 to improve readability. If firms are as likely to enter collaborations with firms they are connected to through the banker network as they are with those they are not connected to, the two ratios should be identical. The average within-network alliance ratio is 0.27%. Firms are almost ten times as likely to form alliances within their banker network compared to the overall sample, and the t-test rejects the null hypothesis of equality in means at the 1% level.[1]

The banker portfolio test compares firms' propensity to enter strategic alliances with other firms they share a banker with to their unconditional propensity to ally. For this purpose, we calculate two statistics for every banker in the sample, similar to the firm-level test above (also see Lindsey, 2008). The *within-portfolio alliance ratio* for banker $i$ is defined as:

$$within\text{-}portfolio\ alliance\ ratio_i = \frac{W_i}{n_i(n_i - 1)} \tag{18.3}$$

where $W_i$ is the number of nodes (i.e. firms in an observed alliance) in alliances between firms that both belong to banker $i$'s portfolio and $n_i$ is the total number of firms in the portfolio. The denominator represents the total number of potential alliance nodes that could be formed within a banker's portfolio. This ratio therefore captures firms' propensity to form strategic alliances conditional on sharing the same banker. We compare it to the banker's total alliance ratio, defined as:

$$total\ alliance\ ratio_i = \frac{A_i}{n_i(n - 1)} \tag{18.4}$$

where $A_i$ is the total number of alliance nodes in the banker's portfolio, regardless of whether only one or both alliance partners are part of the banker's portfolio. $n$ is the total number of sample firms, so the denominator represents the maximum number of alliance nodes that *could* form in banker i's portfolio if each of her borrowers entered an alliance with every other sample firm. This second ratio is again designed to capture firms' unconditional propensity to form alliances.

As a numeric example, consider once more the situation in Figure 16.1 at t=1. Firms A and B and firms B and C have entered pairwise alliances. Firms A and B have borrowed from banker 1, the others have not. The number of nodes in alliances formed between firms that are both within banker 1's portfolio, $W_1$, is equal to 2 (because firms A and B have entered an alliance), which is also the total number of such nodes possible, $n_1(n_1 - 1)$. Banker 1's *within portfolio alliance ratio*, captured by equation (18.3), is therefore 1. For the same banker, the total number of alliances nodes in the portfolio is $A_1 = 3$ (firm A once and firm B twice), while

[1] To illustrate this test further, assume that a firm has an unconditional propensity of forming an alliance with any firm of $p_1$. If there are $n$ potential partners in the world, the expected number of alliances $A_j$ equals $n \times p_1$. Similarly, if there are $n_j$ firms inside a firm's network, the expected within-network number of alliances $C_j$ is $n_j \times p_2$, where $p_2$ is the propensity to form within-network alliances. Equations (1) and (2) form the sample analogues of $p_2$ and $p_1$, respectively and we then test the null hypothesis of $p_1 = p_2$. One misconception could be that, as $n$ goes up, $\frac{A_j}{n-1}$ falls and the test mechanically rejects the null. This intuition is misleading for two reasons: First, as $n$ increases, firm j's network $n_j$ expands. Second, even if $n_j$ stayed constant, $C_j$ should decrease as $n$ increases, if alliances are being entered completely independently from firms' banker networks.

the maximum number of nodes possible is $n_1(n-1) = 6$. Therefore, banker 1's *total alliance ratio*, captured by equation (18.4), is equal to $\frac{1}{2}$.

For our sample, we compare the two ratios by means of a t-test for equal means. The results are displayed in Panel B of Table 18.1. The number of observations is 4,632, equal to the number of bankers that are connected to at least two sample firms (the within-portfolio alliance-ratio is undefined for loan officers with less than two connections). The t-test rejects the null hypothesis at the 1% level, implying that firms are significantly more likely to form alliances if they share a banker. The difference between the two ratios is large, with the mean within-portfolio alliance ratio of 0.3% being almost 50 times the mean total alliance ratio.

Table 18.1 – Univariate tests for propensity to ally given network connections
Panel A tests whether firms are more likely to enter strategic alliances with counterparties that they are connected to through the banker network. Panel B tests whether firms are more likely to enter strategic alliances with potential partners that they share a banker with. Reported means and standard errors have been multiplied by 100 for legibility.

| Panel A: By firm | | | |
|---|---|---|---|
| Variable | Mean | Standard Error | Observations |
| Within-network alliance ratio | 0.2765 | 0.0241 | 669 |
| Total alliance ratio | 0.0282 | 0.0023 | 669 |
| $t$-statistic | 10.2507 | $p$-value | 0.0000 |
| **Panel B: By banker portfolio** | | | |
| Variable | Mean | Standard Error | Observations |
| Within-portfolio alliance ratio | 0.2937 | 0.0527 | 4632 |
| Total alliance ratio | 0.0062 | 0.0002 | 4632 |
| $t$-statistic | 5.4517 | $p$-value | 0.0000 |

There are numerous reasons why firms sharing the same banker should be more likely to initiate a strategic alliance, such as bankers specializing in certain industries and regions, combined with a higher propensity of firms to ally with others in their own industry and geographic proximity. These attributes are likely partly responsible for the large economic magnitudes of the results of the univariate tests above. In our next step, we therefore extend our analysis to a panel setting which allows us to control for alternative drivers of the propensity to ally, such as sharing the same bank, industry or location.

We assemble a panel data set where the unit of observation is a pair of publicly listed, non-financial US firms during 2002 to 2013. The panel consists of all possible firm pairs, subject to two restrictions. First, we only consider firms that enter at least one alliance over the whole sample period. Second, we only consider firm pairs in two industries if there is at least one reported alliance between firms in those two industries in the data. We define a firm's industry based on the 30 Fama-French industry portfolios. This choice is a compromise between trying not restrict firms' choice of alliance partners too much while also avoiding numerical issues that would arise in the estimation of the conditional logit model in the next section if the number of observations per industry-pair becomes too large. These two conditions restrict the size of the panel to a manageable dimension and ensure that only firm-pairs that could realistically have formed an alliance enter the estimation. The panel then consists of 6.4 million firm-pair-years. Firms are not allowed to self-match and we eliminate duplicates from permutations of the same pair of firms. The main dependent variable of interest, an indicator variable labeled $alliance_{it}$, equals one in case a pair of firms has entered a strategic alliance during the reference year or any preceding year. We then estimate the linear probability model (LPM):

$$alliance_{it} = \beta network\ connection_{it} + \gamma same\ bank_{it} + \lambda_{it}\delta + \theta_i + \varepsilon_{it} \tag{18.5}$$

where $i$ indexes firm-pairs and $t$ years. The main explanatory variables – different measures of network connectivity between firms – are represented by $network\ connection_{it}$. The variable $same\ bank_{it}$ controls for whether the two potential alliance partners have borrowed from the same (lead) bank in the past, since not just bankers as individuals but also banks as institutions can transmit information between borrowers (Ivashina et al., 2009).

In addition, there might be time-varying factors, potentially at the industry-level (e.g. technological developments or changes in the competitive landscape) that affect both borrowing and the rate of alliance formation. Our specification therefore includes industry-year fixed effects for both firm one and firm two, represented by the vector $\lambda_{it}$. In addition, the likelihood of alliance formation can vary along a number of observable (e.g. higher alliance propensity between related industries) and unobservable dimensions such as the compatibility of two companies' corporate culture. We therefore control for time invariant, firm-pair specific variation in the propensity to form alliances by adding firm-pair fixed effects ($\theta_i$). Finally, $\varepsilon_{it}$ is the error term. We double-cluster standard errors by firm one and firm two in all specifications.

We begin our investigation by testing hypothesis 1, which states that two firms should be more likely to engage in a strategic alliance if they share the same banker, as measured by the indicator variable *same banker* which takes the value of one if a pair of firms has ever shared a banker. The results are presented in Column 1 of Table 18.2 and show that two firms are about 0.67 percentage points more likely to engage in a strategic alliance if they share the same banker, even after controlling for the effect of sharing the same bank, time variation in the overall number of alliances and connections at the industry level, and time invariant observable and unobservable firm-pair characteristics. We therefore only draw inference from observations that change from not sharing the same banker to doing so during our sample period. We also find that firms are 0.12 percentage points more likely to ally if they have at some point shared the same bank. One potential concern might be that bankers are a more granular unit of observation than banks. Two firms sharing the same banker are, for example significantly more likely to be in the same industry. Our firm-pair fixed effects capture such similarities as long as they are time invariant. Both of these estimates are statistically

Table 18.2 – Influence of banker networks on the formation of strategic alliances: OLS results
The table displays estimates for firms' likelihood of entering a strategic alliance given whether and how closely they are connected through the network of bankers. The unit of observation is a firm-pair-year and the dependent variable is an indicator variable equal to one if a certain firm-pair has entered a strategic alliance before or during the year of observation. The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. *Same banker* is equal to one if the firm-pair has a banker in common. *Banker network distance* measures how many banker to banker connections are required to establish a connection between the two firms, zero indicating none (i.e. the firms share the same banker). *Banker connection* is an indicator equal to one if the two firms are connected through the network of bankers. Parentheses contain t-statistics calculated from standard errors double-clustered by firm one and firm two. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Same banker | 0.0067*** | | | |
| | (4.56) | | | |
| Banker network connection | | 0.0021*** | | |
| | | (3.81) | | |
| Banker network distance | | | 0.0003 | |
| | | | (0.78) | |
| Distance = 0 | | | | 0.0078*** |
| | | | | (4.76) |
| Distance = 1 | | | | 0.0023*** |
| | | | | (3.13) |
| Distance = 2 | | | | 0.0011** |
| | | | | (2.46) |
| Distance > 2 | | | | 0.0007 |
| | | | | (1.60) |
| Same bank | 0.0012*** | 0.0012*** | 0.0013 | 0.0010*** |
| | (3.88) | (3.69) | (1.62) | (3.34) |
| Firm-pair FE | Yes | Yes | Yes | Yes |
| Industry 1-year FE | Yes | Yes | Yes | Yes |
| Industry 2-year FE | Yes | Yes | Yes | Yes |
| N | 6,370,758 | 6,370,758 | 359,668 | 6,370,758 |
| $R^2$ | 0.7444 | 0.7444 | 0.8360 | 0.7444 |

significant at the one percent level. Given that the effect of sharing a banker is five times the effect of sharing a bank, the economic magnitude of our estimate of the impact of sharing the same banker is high in both absolute and relative terms.

Hypothesis 2 states that two firms should be more likely to ally even if they do not share the same banker, but are indirectly connected through a banker network. In Column 2 we estimate the same model as in Column 1 but replace *same banker* with *banker network connection*, an indicator that takes the value of one if the two firms in a pair are in any way connected through their banker network from past loans. The estimated coefficient on this indicator is 0.21 percentage points and highly statistically significant, consistent with our prediction.

Hypothesis 2 also predicts that the effect of an indirect banker connection should become weaker as the distance between bankers increases. We explicitly test this conjecture in Column 3, where our main explanatory variable is *banker network distance*, a measure of the shortest network path between all bankers associated with the two firms. A distance of zero therefore corresponds to two firms sharing the same banker and a distance of one indicates that the shortest connection between two firms involves two bankers that have worked together on loans to other companies.

We test hypothesis 2 in two ways. First, we limit our sample to only those firms that do share a connection through the banker network, and run a regression of our alliance indicator on *banker network distance*. Note that the sample shrinks significantly in this specification, since we can only consider pairs of firms that are in any way connected through a banker network, as the distance between two firms that are unconnected is undefined. While hypothesis 2 would predict a negative and significant effect of network distance on the propensity to form an alliance, the estimated coefficient on *banker network distance* is both statistically and economically insignificant in this specification. Since firm-pair fixed effects absorb any time invariant firm pair level characteristics, these specifications can only draw inference from firm bank pairs that are connected through the banker network with different levels of distance. The power of this test is significantly lowered since we cannot draw inference from firms that move from being unconnected to being connected.

To overcome this limitation and increase the power of our test, we instead treat *banker network distance* as a discrete variable and estimate coefficients for each level of distance separately in Column 4 of Table 18.2. In that way, we are able to use unconnected firms as the reference group, and draw inference from firm pairs that move from being unconnected to being connected. The magnitude of the coefficient estimates is monotonously decreasing in the distance in these specifications. The coefficients on $distance = 0$ (0.0078), $distance = 1$ (0.0023) and $distance = 2$ (0.0011) are all statistically significant at the 5% and 1% level. The coefficient estimate for distances larger than two (which we pool into a single group due to the small number of such observations) is still positive (0.0007), but statistically insignificant.

These result suggests that, while sharing the same banker is the strongest predictor of two firms entering into a strategic alliance, even indirect connections still increase the likelihood of two firms to ally. At the same time, larger network distance between bankers reduces their matchmaking ability, with the estimated coefficient monotonically decreasing in network distance. Once the chain of bankers exceeds three people there is very little impact on alliance formation. Across all specifications, the estimated effect of sharing the same bank has a positive and statistically significant impact on the likelihood of alliance formation.[2]

## 18.2 Additional robustness tests for the OLS specification

For robustness, we re-estimate the specification in Table 18.2 with additional firm-year fixed effects for both firms. We thereby control for both observable and unobservable time-varying characteristics on the firm level that might introduce an omitted variable bias. The results are displayed in Table 18.3. Even in this heavily saturated fixed effect specification, sharing the same banker remains a statistically and economically significant predictor for whether two firms enter a strategic alliance. The coefficients on indirect connections retain their sign, albeit their slightly smaller magnitude means they lose most of their statistical significance in this specification. The exception is the coefficient for sharing any network connection in Column 2, which remains on the margin of statistical significance.

Another potential concern with the fixed effect specification in Table 18.2 is that it cannot fully distinguish whether a network connection precedes a strategic alliance or whether the opposite is the case. Strategic alliances could therefore systematically precede network connections, in which case the results could be driven by reverse causality. To alleviate this concern, Appendix C.3 presents results from first difference regressions that relate changes in alliance status to concurrent changes in network connections. The first difference setup is substantially more conservative than the baseline OLS results because it identifies the impact of banker networks on alliance formation only based on alliances entered in the first period after the network connection is first established.[3] All results retain their statistically significance in the first difference specifications. As expected, the economic magnitude of the estimated coefficients is lower, reflecting that they only represent the increase in the probability that two firms enter an alliance immediately after becoming connected through a banker network. Since, in addition, the sequenced conditional logit results in Section 18.3 hereafter are unaffected by this issue by construction, the overall evidence strongly suggests that our results are not driven by alliances preceding connections through the banker network.

An additional robustness test presented in Appendix C.3 is concerned with the time-dimension of the network. The main specification in Table 18.2 assumes that connections between firms, bankers and banks last forever. The robustness test introduces time-phased connections by limiting the lifetime of all connections (bank to firm, banker to banker and banker to firm) to five years. The results of this specification are both economically and statistically close to

[2]In un-tabulated results, we estimates the regression based only on the control variables to determine the impact of sharing the *same bank*. The resulting coefficient resembles those in the main specification both in terms of size and statistical significance.

[3]As mentioned in section 17.1, network connections are lagged by one period in all estimations.

Table 18.3 – Linear probability model with firm-year fixed effects
The table displays estimates for firms' likelihood of entering a strategic alliance given whether and how closely they are connected through the network of bankers. The tests follow Table 18.2 but are augmented with firm 1-year and firm 2-year fixed effects. The unit of observation is a firm-pair-year and the dependent variable is an indicator variable equal to one if a certain firm-pair has entered a strategic alliance before or during the year of observation. The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. *Same banker* is equal to one if the firm-pair has a banker in common. *Banker network distance* measures how many banker to banker connections are required to establish a connection between the two firms, zero indicating none. *Banker connection* is an indicator equal to one if the two firms are connected through the network of bankers. Parentheses contain t-statistics calculated from standard errors double clustered by firm one and firm two. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Same banker | 0.0042*** | | | |
| | (3.26) | | | |
| Banker network connection | | 0.0008* | | |
| | | (1.93) | | |
| Banker network distance | | | 0.0005 | |
| | | | (0.90) | |
| Distance = 0 | | | | 0.0048*** |
| | | | | (3.40) |
| Distance = 1 | | | | 0.0008 |
| | | | | (1.51) |
| Distance = 2 | | | | 0.0002 |
| | | | | (0.62) |
| Distance > 2 | | | | 0.0000 |
| | | | | (0.01) |
| Same bank | -0.0000 | -0.0000 | 0.0001 | -0.0000 |
| | (-0.06) | (-0.04) | (0.17) | (-0.21) |
| Firm 1-year FE | Yes | Yes | Yes | Yes |
| Firm 2-year FE | Yes | Yes | Yes | Yes |
| Firm-pair FE | Yes | Yes | Yes | Yes |
| N | 6,370,712 | 6,370,712 | 359,605 | 6,370,712 |
| $R^2$ | 0.7493 | 0.7493 | 0.8436 | 0.7493 |

those in Table 18.2 as well.

Finally, there are relatively few alliances (about 3000) compared to the overall sample size. We are unaware of any evidence indicating that the skewed nature of the dependent variable in the estimation above could render our coefficient estimates biased or inconsistent. Nevertheless, we repeat the LPM analysis on a reduced sample consisting of all firm-pairs that enter an alliance over the sample period and a single control pair for each one in a final robustness test. We select the control firm pair by matching both firms in an observed alliance to their nearest neighbor given a number of observable characteristics including industry, size and age and construct the control pair from the two nearest neighbors. The results displayed in Appendix C.3 confirm that sharing a banker, or being connected through the banker network, are associated with a higher likelihood of forming a strategic alliance.[4]

## 18.3 A dynamic model of alliance formation based on the sequenced conditional logit model

The fundamental unit of observation in our data is that of a firm-pair-year. Because a firm's choice of entering a strategic alliance might affect its decision to enter additional alliances in the future, observations for a particular firm-pair are potentially correlated with all other observations involving either of the two firms forming the pair. The result is a complicated correlation structure that conventional clustering of standard errors cannot fully account for.[5] Robust inference in the presence of such *dyadic* data, where the unit of observation is a pair, is still an active area of research (Fafchamps and Gubert, 2007; Cameron and Miller, 2014; Tabord-Meehan, 2018). Unfortunately, the size of our data set and the large number of corresponding fixed effects means implementing the existing estimators for dyadic data is impossible for computational reasons. According to the results of Monte-Carlo simulations in Cameron and Miller (2014), however, our choice of clustering standard errors twice both along the first and second dimension of the dyad is the most conservative among the alternatives and provides the closest approximation to full dyadic clustering.

To account for the firm-level dependence in alliance choice more comprehensively, we instead apply the sequenced conditional logit model developed by Lindsey (2008), a discrete choice model based on the standard conditional logit model (e.g., Chamberlain, 1980) but different in that it allows the set of conditioning outcomes to vary over time. This approach allows us to explicitly model the sequential way in which alliances form over time while also incorporating

[4] The main difference is that the effect does not fall in network distance in these specifications. The failure to pick up on this nuanced effect might be due to these specifications drawing inference from a sample comprising less than 1% of our main sample.

[5] For example, consider a sample consisting of the firms A, B and C. Possible pair-wise combinations are {A,B}, {A,C} and {B,C}; at least one firm (in this case, B) will show up once as the first and once as the second entry, no matter how the combinations are chosen. Therefore, the observations {A,B} and {B,C} are possibly correlated, but even standard errors double-clustered by firm one and firm two will not account for this fact.

the group structure of the data.[6]

The probability of an observed alliance under the sequenced conditional logit model is parameterized as

$$pr(alliance = 1) = \frac{e^{X_s^t \beta}}{\sum_{s \in S} e^{X_s^t \beta}} \tag{18.6}$$

where $X$ is a vector of explanatory variables, $\beta$ is the coefficient vector to be estimated, $t$ indexes time, $s$ indexes firm-pairs and $S$ is the set of feasible alliances constructed from firms in the two alliance partners' industries. The set of conditioning outcomes $S$ varies over time as alliances are formed. Lindsey (2008) develops two different implementations of the model, the *variable capacity* and the *fixed capacity* version, which differ in the way in which $S$ is restricted over time. In both versions of the model, when an alliance between a particular pair of firms is realized, the pair is removed from $S$ in subsequent years.

The variable capacity model places no additional restrictions on $S$, therefore it assumes that firms could have entered any number of alliances. Hence the variable capacity model does not account for the possibility that the realization of one alliance can affect the same firm's probability of entering additional alliances in the future, but has the benefit of not imposing any additional restrictions on the estimation. The fixed capacity version of the model, on the other hand, assumes that firms have a maximum alliance capacity corresponding to the total number of alliances they enter over the sample period. Once a firm has reached its alliance capacity, all firm-pairs containing it are removed from the set of conditioning outcomes $S$ in subsequent periods, thereby accounting for the dynamic way in which the realization of one alliance can preclude others in the future.

The likelihood $L^p$ for industry-pair $p$, with $N_p$ realized alliances between time 1 and $T$ is then the product of the probability of all realized alliances, i.e.

$$L^p = \left( \frac{e^{X_{s_1}^1 \beta}}{\sum_{s \in S^p} e^{X_s^1 \beta}} \right) \left( \frac{e^{X_{s_2}^2 \beta}}{\sum_{s \in S^{pf(s_1)}} e^{X_s^2 \beta}} \right) \dots \left( \frac{e^{X_{s_{N_p}}^T \beta}}{\sum_{s \in S^{pf(s_1, s_2, \dots, s_{N_p - 1})}} e^{X_s^T \beta}} \right) \tag{18.7}$$

And the overall likelihood, multiplied across industry pairs, can be expressed as

$$L = \prod_{p \in P} L^p(s_1, \dots, s_{N_p}) \tag{18.8}$$

Appendix C.4 illustrates the sequenced conditional logit model in detail using examples. We apply the two versions of the sequenced conditional logit model to our estimation of the effect of banker network connections on alliance propensity. We first present the results of the less

[6] While the sequenced conditional logit allows us to model firms' choices in more detail, including the group structure of the data, it also comes with drawbacks. The reported coefficients are logit coefficients and can therefore not be economically interpreted (except in the form of an odds ratio). Unlike in standard logit models, it is not possible to directly calculate margins in conditional logit models due to the different reference group for each firm pair.

restrictive variable capacity model in Table 18.4.

As in the OLS specification, we include controls for sharing the same bank. Furthermore, we include a control *previous alliances* for the number of alliances the two firms in each pair have previously entered. Note that the sequenced conditional logit estimation setup controls for industry-year effects by construction since the industry-pair-year is used as the reference group.

The specification in Column 1 estimates the sequenced conditional logit model in its variable capacity version with the *same banker* as the main explanatory variable. The estimated coefficient of *same banker* on initiating a strategic alliance is 0.380 and statistically significant at the 1% level. As in the OLS analysis we therefore conclude that having shared the same banker increases the likelihood of two firms initiating a strategic alliance. In Column 2, we replace *same banker* with *banker network connection*, an indicator of whether two firms are in any way connected. As in the OLS setting, the estimated coefficient is positive at 0.290 and statistically significant at the 1% level. In the next column, we limit the sample to those firms that are connected through the banker network and estimate the effect of an increase in network distance on the likelihood of alliance formation. The coefficient estimate is -0.175 and statistically significant at the 10% level. The sequenced conditional logit model therefore finds that greater network distance between bankers reduces their ability to broker strategic alliances. When we include each distance level individually in our final specification – with unconnected firm-pairs forming the base category – we find that the propensity of a banker network connection to broker a strategic alliance decreases monotonously as the distance increases, from 0.427 for a distance of zero to 0.256 for a distance of one (both significant at the 1% level), with all additional coefficients being statistically insignificant.

Unlike in the OLS analysis, there are no firm-pair fixed effects subsuming time invariant firm-pair features in the sequenced conditional logit regressions. This allows us to include an indicator whether two firms are headquartered in the same state to specifically test for the effect of geographic proximity between firms. Consistent with the results in Reuer and Lahiri (2013), we find that firms headquartered in the same state are significantly more likely to form alliances. The coefficient for *same bank* is positive but statistically insignificant in the variable capacity model.

The conditional logit model, in general, does not allow for the unconditional marginal effects associated with individual regression coefficients to be recovered, but the exponential of the estimated coefficients can be interpreted as an odds ratio. If a pair of firms shares a banker (*same banker=1*) it is 1.462 times as likely to enter a strategic alliance in any given year as it would be if it did not. Similarly, the odds ratio for being connected through the banker network in any manner (*banker connection=1*) is 1.336, so a firm-pair is 1.336 as likely to enter an alliance if it is connected every year. The base case for the interpretation of the odds ratio in Column 3 is a firm-pair that shares the same banker. Hence a firm pair connected indirectly with *distance=1* is only 0.839 times as likely to enter a strategic alliance as it would be if it

Table 18.4 – Influence of banker networks on the formation of strategic alliances: Variable capacity model
The table displays results from a maximum likelihood estimation of the variable capacity sequenced conditional logit model. The unit of observation is a firm-pair-year and the dependent variable is an indicator variable equal to one if a certain firm-pair has entered a strategic alliance during the year of observation. The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. A firm's maximum alliance capacity is assumed to be unlimited. *Same banker* is equal to one if the firm-pair has a banker in common. *Banker network distance* measures how many banker to banker connections are required to establish a connection between the two firms, zero indicating none (i.e. the firms share the same banker). *Banker connection* is an indicator equal to one if the two firms are connected through the network of bankers. The (omitted) base category for the indicator variables in Column 4 is two firms *not* being connected through the network. Parentheses contain z-statistics. Industry-pair-year fixed effects are implicitly embedded in the conditional logit estimation procedure. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Same banker | 0.380*** | | | |
| | (3.31) | | | |
| Banker network connection | | 0.290*** | | |
| | | (4.28) | | |
| Banker network distance | | | -0.175* | |
| | | | (-1.92) | |
| Distance = 0 | | | | 0.427*** |
| | | | | (3.69) |
| Distance = 1 | | | | 0.256*** |
| | | | | (2.81) |
| Distance = 2 | | | | 0.244 |
| | | | | (1.63) |
| Distance > 2 | | | | 0.071 |
| | | | | (0.24) |
| Same bank | 0.042 | 0.019 | 0.017 | 0.006 |
| | (0.71) | (0.31) | (0.12) | (0.11) |
| Same state | 0.382*** | 0.390*** | 0.452*** | 0.387*** |
| | (7.57) | (7.72) | (2.72) | (7.65) |
| Previous alliances | 0.025*** | 0.025*** | 0.019*** | 0.025*** |
| | (30.17) | (30.21) | (7.92) | (30.19) |
| N | 529,323 | 529,323 | 24,844 | 529,323 |
| Prob > $\chi^2$ | 0.000 | 0.000 | 0.000 | 0.000 |

shared the same banker, decreasing further to 0.705 for *distance=2*, 0.592 for *distance=3* and so on. Finally, in the discrete specification in Column 4 the base case is that of a firm-pair unconnected through the network, implying a pair of firms connected directly (*distance=0*) is 1.533 times as likely to enter a strategic alliance than it would be if it was unconnected, decreasing to 1.292 times for an indirect connection of order 1 (*distance=1*).[7]

In summary, Table 18.4 shows that our results hold in the sequenced conditional logit specification. Because our unit of observation is a firm-pair, we do not have a clear prior on the impact of individual firms' financial characteristics on a pair's propensity to enter an alliance and therefore do not control for them in our main specification. A robustness test in Appendix C.3 adds controls for sales, tangibility of assets and financial leverage, and shows that our results remain economically and statistically very similar.

We next estimate the sequenced conditional logit model in its more restrictive fixed capacity specification. The corresponding results are presented in Table 18.5.

The specifications presented follow those from Table 18.4. The *previous alliances* control is absent in the fixed capacity version of the model since the estimation already controls for it by design. While our power shrinks significantly due to the 40% lower sample size in the fixed capacity setting, the coefficient estimates are both economically and statistically very similar to the variable capacity model. The coefficient estimate on sharing the same banker (Column 1) is about 0.3 and statistically significant at the 1% level. The coefficient estimate on the indicator of sharing any connection through the banker network (Column 2) is 0.181 and equally statistically significant. The estimate for the relationship between banker network distance and the propensity to form strategic alliances is -0.156 and marginally statistically significant in the continuous setting (Column 3).

As in our prior specifications, the ability of bankers to broker alliances between their clients is monotonously decreasing in the discrete specification (Column 4), with the coefficient for $distance = 0$ remaining statistically significant at the 1% level. The coefficient estimate for *same bank* is positive and statistically significant in all specifications. Taken together, the results from this section show that our main result, that bankers broker strategic alliances both between their own portfolio firms and those of connected bankers, holds even in the most restrictive regression settings.

## 18.4 Bankers are more important when information asymmetry is high

Our third hypothesis predicts that bankers' ability to broker alliances should exhibit cross-sectional differences based on borrower characteristics. We test the prediction that greater

[7] Note that the odds ratio for *same banker* in Column 1 and $distance = 0$ in column four are different because the base case is a different one; in Column 1 the base case is not sharing the same banker, in Column 4 it is not having any connection, even an indirect one, through a banker network.

Table 18.5 – Influence of banker networks on the formation of strategic alliances: Fixed capacity model

The table displays results from a maximum likelihood estimation of the fixed capacity sequenced conditional logit model. The unit of observation is a firm-pair-year and the dependent variable is an indicator variable equal to one if a certain firm-pair has entered a strategic alliance during the year of observation. The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. A firm's maximum alliance capacity is assumed to be fixed and equal to the number of strategic alliances the firm enters over the sample period. Once firms have exhausted their alliance capacity they are excluded from the panel in subsequent periods. *Same banker* is equal to one if the firm-pair has a banker in common. *Banker network distance* measures how many banker to banker connections are required to establish a connection between the two firms, zero indicating none (i.e. the firms share the same banker). *Banker connection* is an indicator equal to one if the two firms are connected through the network of bankers. The (omitted) base category for the indicator variables in Column 4 is two firms *not* being connected through the network. Parentheses contain z-statistics. Industry-pair-year fixed effects are implicitly embedded in the conditional logit estimation procedure. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Same banker | 0.298*** | | | |
| | (2.59) | | | |
| Banker network connection | | 0.181*** | | |
| | | (2.63) | | |
| Banker network distance | | | -0.156* | |
| | | | (-1.67) | |
| Distance = 0 | | | | 0.327*** |
| | | | | (2.81) |
| Distance = 1 | | | | 0.156* |
| | | | | (1.69) |
| Distance = 2 | | | | 0.080 |
| | | | | (0.52) |
| Distance > 2 | | | | 0.008 |
| | | | | (0.03) |
| Same bank | 0.176*** | 0.167*** | 0.008 | 0.153** |
| | (2.94) | (2.77) | (0.06) | (2.50) |
| Same state | 0.319*** | 0.324*** | 0.398** | 0.321*** |
| | (6.26) | (6.37) | (2.35) | (6.30) |
| N | 308,459 | 308,459 | 12,866 | 308,459 |
| Prob > $\chi^2$ | 0.000 | 0.000 | 0.031 | 0.000 |

opacity should amplify the role of bankers in brokering alliances in Table 18.6.

The results in Columns 1 and 2 of Table 18.6 are for the variable capacity version of the sequenced conditional logit model.[8] For robustness, we repeat the same tests using a linear probability model in Columns 3 and 4. The specifications in Table 18.6 interact the independent variable *same banker* with two measures of opacity: lack of credit ratings and high intangibility of assets. In Column 1, we interact *same banker* with *one unrated*, an indicator variable that takes the value one for pairs in which at least one firm has no domestic long-term issuer credit rating from S&P's, Moody's or Fitch. We find that the coefficient estimate on the interaction of sharing the same banker and *one unrated* is 0.660 and statistically significant at the 5% level. Interestingly, we find that the un-interacted variable *one unrated* enters the regression negative and statistically significant at the 1% level, which implies that firm pairs in which one is unrated indeed are less likely to join a strategic alliance. This result shows that sharing the same banker has a significantly more positive impact on the formation of strategic alliances when there is less publicly available information about the participants.

Similarly, Column 2 tests whether the effect of bankers on alliance formation is larger when at least one of the potential partners has a particularly high (i.e. in the top quintile) fraction of intangible assets. We find that *one high intangibles* indeed interacts positively with *same banker*, with a coefficient of 0.615 and statistical significance at the 1% level.[9] The main effect for *one high intangibles* on the other hand is statistically insignificant.

Because the coefficients are from a conditional logit model, they again cannot be interpreted as a marginal effect without imposing unduly strict assumptions on the (unidentified) fixed effects. However, an interpretation in terms of odds ratios is possible. The exponential of the interaction term in Column 1 indicates that when two firms share the same banker and do not have a credit rating, the odds they will subsequently enter an alliance increase by 1.935 times as much as they would if the firms did have a credit rating. In other words, unrated firms benefit almost twice as much from sharing the same banker as rated firms. The economic impact of a high share of intangible assets in Column 2 is of a similar magnitude.

Columns 3 and 4 repeat the same tests based on the linear probability model. Inconsistent with the main specification, the coefficient for the interaction with *one unrated* is negative and marginally statistically significant. The interaction term for *one high intangibles* on the other hand is positive and statistically significant at the 5% level, consistent with the main specification.

[8] In unreported analyses, we repeat all tests in this table using the fixed capacity model. All estimates are both statistically and economically very close to the variable capacity estimates.

[9] Another cross-sectional dimension on which to measure opacity might be firm size. In unreported results we find no statistically significantly different effect of network connection across small and large firms. That finding is consistent with Ivashina et al. (2009) who demonstrate that banks have sensitive inside information even for the largest, most transparent firms.

Table 18.6 – Banker networks and firm opacity
The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. Estimates for the sequenced conditional logit model are based on the variable capacity implementation. *One unrated* means either one or both firms do not have a domestic long-term issuer credit rating from either S&P, Moody's or Fitch. *One high intangibles* means either one or both firms have an intangibles-to-assets ratio in the top quintile. Parentheses contain z-statistics for the conditional logit model and t-statistics for the LPM. Industry-pair-year fixed effects are implicit in the sequenced conditional logit model. Standard errors for the LPM have been double clustered by firm one and firm two. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | Sequenced cond. logit | | LPM | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Same banker × one unrated | 0.660** | | -0.005* | |
| | (2.52) | | (-1.88) | |
| Same banker × one high intangibles | | 0.615*** | | 0.004** |
| | | (2.67) | | (2.03) |
| One unrated | -0.439*** | | -0.001** | |
| | (-8.05) | | (-2.26) | |
| One high intangibles | | -0.040 | | 0.000 |
| | | (-0.87) | | (1.33) |
| Same banker | 0.174 | 0.039 | 0.008*** | 0.005*** |
| | (1.37) | (0.22) | (4.37) | (3.91) |
| Same bank | -0.130** | 0.052 | 0.001*** | 0.001*** |
| | (-2.07) | (0.86) | (3.69) | (3.88) |
| Same state | 0.394*** | 0.359*** | | |
| | (7.79) | (6.71) | | |
| Previous alliances | 0.024*** | 0.025*** | | |
| | (27.97) | (28.61) | | |
| Firm-pair FE | No | No | Yes | Yes |
| Industry 1-year FE | No | No | Yes | Yes |
| Industry 2-year FE | No | No | Yes | Yes |
| N | 529,323 | 480,006 | 6,370,758 | 5,846,834 |
| Prob > $\chi^2$ | 0.000 | 0.000 | | |
| $R^2$ | | | 0.744 | 0.756 |

## 18.5 More well-connected bankers allow borrowers to forge more alliances

We now test Hypothesis 4a, whether banker networks increase the number of alliances firms form. To test our hypothesis, we aggregate data on the firm-year level and run regressions of the number of (new) alliances on measures of the aggregate connectedness of each firm to its potential alliance partners in Table 18.7. The samples for the sequenced conditional logit regressions and the OLS regressions differ both in terms of sample construction and how they treat realized alliances. We therefore run these tests both on the data structure of the sequenced conditional logit panel (Columns 1 and 2) as well as the OLS panel (Columns 3 and 4).

In the sequenced conditional logit model we remove firm-pairs in the year following a realized alliance, so the dependent variable in the first two columns is the log of the number of newly realized alliances plus one in each firm year. In Column 1, we measure each firm's average level of connectedness through the banker network as the mean of the *same banker* variable, i.e. the fraction of other firms it could have entered an alliance with and with which it shares the same banker. We control for unobservable firm-level characteristics through firm fixed effects, and time variation in the propensity to form alliances through year fixed effects. Our specifications therefore only draw inference from variation in each firm's network connections over time. We also control for time varying firm level characteristics such as firm size, age, leverage, profitability, and the number of potential alliance partners. In addition, we control for the fraction of potential alliance partners the firm shares the same bank with *(mean(same bank))*. We find that the coefficient estimate on *mean(same banker)* is 0.125, and statistically significant at the 10% level. Evaluated at the mean of all independent variables, this number implies that a one standard deviation increase in *mean(same banker)* leads to an additional 0.015 alliances for a particular firm-year, all else equal. The remaining coefficients imply that firms that share the same bank with more potential alliance partners initiate more new alliances, as do larger firms and those with a larger set of potential partners.

In Column 2, we replace our main explanatory variable *mean(same banker)* with *mean(banker connection)*, the mean of the *banker connection* indicator that captures whether a firm pair shares any direct or indirect links through the banker network. The coefficient estimate is 0.067 and statistically significant at the 5% level, implying that a one standard deviation increase in *mean(banker connection)* leads to an additional 0.017 alliances for a particular firm-year, evaluated at the means of all independent variables. Consistent with our earlier findings that indirect connections through the banker network have a lower impact on the formation of strategic alliances, we find that the coefficient estimates for *mean(same banker)* exceeds both that for indirect connections *(mean(banker connection))* and sharing the same bank *(mean(same bank))*.

In Columns 3 and 4, we repeat this analysis using the OLS panel as the basis for the firm-year aggregation. In this sample, we do not remove firm pairs in the years after an alliance is first

Table 18.7 – Banker networks and firms' number of strategic alliances
The unit of observation for the tests displays in the table is a firm-year and the independent variable an indicator for the number of strategic alliances the firm enters in the current year (Columns 1 and 2) or has entered over the sample period (Columns 3 and 4). The set of potential alliance partners for each firm is constructed analogously to the tests in Table 18.2 to 18.4. The network characteristics (*same banker*, *banker network connection* and *same bank*) have then been averaged across this set of potential partners for each firm. In Columns 1 and 2, potential alliance partners have been eliminated from a firm's set of possible matches in the first year after an alliance is first realized, analogous to the sequenced conditional logit sample. In Columns 3 and 4 those pairs remain in the sample, analogous to the OLS sample. Standard errors have been clustered by firm. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | Ln(1+new alliances) | | Ln(1+total alliances) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Mean(same banker) | 0.1249* | | 1.8552*** | |
| | (1.87) | | (3.60) | |
| Mean(banker connection) | | 0.0673** | | 0.3939*** |
| | | (2.09) | | (4.03) |
| Mean(same bank) | 0.0570** | 0.0534** | 0.2033** | 0.2097** |
| | (2.31) | (2.15) | (2.13) | (2.21) |
| Ln(total assets) | 0.0388*** | 0.0386*** | 0.0329*** | 0.0304*** |
| | (6.66) | (6.61) | (3.42) | (3.16) |
| Ln(firm age) | -0.0109 | -0.0098 | 0.0129 | 0.0086 |
| | (-0.52) | (-0.46) | (0.40) | (0.27) |
| Market leverage | -0.0423 | -0.0420 | 0.0403 | 0.0393 |
| | (-1.61) | (-1.60) | (1.17) | (1.13) |
| ROA | -0.0064 | -0.0064 | 0.0008 | 0.0011 |
| | (-1.33) | (-1.32) | (0.15) | (0.21) |
| Ln(no. of potential alliances) | 0.3970*** | 0.3989*** | -0.1343 | -0.1189 |
| | (17.25) | (17.29) | (-0.65) | (-0.58) |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| N | 13,589 | 13,589 | 14,020 | 14,020 |
| $R^2$ | 0.3776 | 0.3777 | 0.8474 | 0.8472 |

realized. Therefore, the appropriate dependent variable in this analysis is the total number of alliances since the beginning of the sample period for each firm and year. The results confirm those from Columns 1 and 2. The coefficient estimates on both *mean(same banker)* and *mean(banker connection)* are positive, and statistically significant at the 1% level.

Overall, the results are consistent with Hypothesis 4a, which states that more banker connections lead to firms engaging in more strategic alliances.

## 18.6 Alliances facilitated by bankers are valuable for firms

To investigate Hypothesis 4b, whether strategic alliances arranged by bankers are beneficial for firms, we perform an event study around their announcement. The dependent variable in these regressions is the cumulative abnormal return (CARs) for every alliance announcement over a three-day event window centered on the announcement date. We then relate the CAR to the firm pair's network characteristics in OLS regressions. Cumulative abnormal returns are calculated based on the market model with a 250 day estimation period and winsorized at the 1 and 99% level. We require at least 220 observations in the estimation window to be non-missing and use the value-weighted return of all CRSP firms as the market benchmark and the 1-month US treasury bill for the risk-free rate. The estimated market beta has been shrunk towards the cross-sectional mean based on the Vasicek (1973) estimator. We use the value-weighted return of all US-incorporated stocks in CRSP and the one-month US treasury bill rate provided by Kenneth French on his website[10] as proxies for the market return and the risk-free rate, respectively. For robustness, we repeat the same tests on alliance (instead of firm) level, where the CAR for an observed alliance is the market value weighted average CAR of all participating firms. The results of these regressions are presented in Table 18.8.

If alliances facilitated by bankers create more value than other alliances, the coefficient estimate for sharing the same banker should be positive. If they create less value, the coefficient should be negative, and if there is no difference the coefficient should be zero. Consistent with the prior literature (e.g. Chan et al., 1997) we find that strategic alliances are generally valuable for firms. In all model specifications, the intercept, which captures the general effect of alliances on firm value, is positive and statistically significant at the 1% level. This result implies that a strategic alliance adds between 0.6 and 0.7% to a firm's market value on average. The intercepts for the weighted average CAR by alliance in Columns 3 and 4 are lower at 0.2%, implying that small firms, in relative terms, benefit disproportionately from strategic alliances. The specifications in Columns 1 and 3 control for whether the firms in an announced alliance share either the *same banker* or the *same bank*, Columns 2 and 4 do the same for whether there exists any *banker network connection*. The estimated coefficients for all of the network characteristics are statistically insignificant at the 5% level, therefore not providing any evidence that alliances facilitated through banker networks are either better or worse than the

[10] http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html

Table 18.8 – Do alliances brokered through banker networks increase firm value?
The table displays coefficient estimates from regressions of cumulative abnormal returns (CARs) over a [-1;1] event window around alliance announcements on network characteristics. The sample consists of all initial strategic alliances entered by publicly listed non-financial US firms that are listed in SDC Platinum or Capital IQ for the period from 2002 to 2013. CARs have been calculated according to the market model with market betas estimated from 250 daily observations and shrunk towards the cross-sectional mean based on the Vasicek (1973) estimator. Standard errors have been clustered by alliance. The unit of observation in Columns 1 to 2 is a firm in an observed alliance. The unit of observation in Columns 3 to 4 is a strategic alliance, with the CAR having been calculated by taking the market value weighted average of the alliance members' CARs. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | Firm-level CAR | | Alliance-level CAR | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Intercept | 0.006*** | 0.007*** | 0.002*** | 0.002*** |
| | (8.10) | (8.09) | (3.31) | (3.20) |
| Same banker | -0.001 | | 0.002 | |
| | (-0.56) | | (0.95) | |
| Banker network connection | | -0.002 | | 0.001 |
| | | (-1.49) | | (0.59) |
| Same bank | -0.002* | -0.002 | -0.001 | -0.001 |
| | (-1.79) | (-1.40) | (-1.14) | (-1.11) |
| N | 5,535 | 5,535 | 2,993 | 2,993 |
| $R^2$ | 0.000 | 0.001 | 0.000 | 0.000 |

average alliance.[11] These results suggest that banker networks benefit firms on the extensive rather than the intensive margin in the formation of alliances: better connected networks allow firms to enter more alliances. These alliances are valuable but not of higher quality than the average strategic alliance.

## 18.7 Banks are compensated through additional mandates

One reason why a bank might be interested in helping a borrower enter a strategic alliance is that it strengthens the lending relationship. Bharath et al. (2007) find that stronger lending relationships benefit banks through their ability to cross-sell other financial services, and Hellmann et al. (2007) find that banks which build a venture capital relationship to borrowers are more likely to be chosen as lenders later. We therefore ask whether banks that broker strategic alliances get rewarded through additional mandates, for example when raising debt or equity capital, or engaging in M&A transactions.[12]

We test for the existence of compensation through additional mandates explicitly on an annual panel of firm-bank pairs. For each firm in year $t$, we record all banks that served as lead arrangers on a loan in the past. The dependent variable of interest is an indicator whether the bank is given a particular mandate from this borrower over the subsequent five-year period, i.e. until $t+4$. We consider three types of mandates: arranging an additional syndicated loan ("bank-based financing"), serving as the underwriter in a bond or seasoned equity offering ("market-based financing") or advising in an M&A transaction ("M&A advisory"). Data on seasoned equity offerings, bond issues, and advisory mandates in mergers and acquisitions comes from Capital IQ, data on syndicated loans from LPC DealScan.[13] Our main explanatory variable is the number of strategic alliances the firm has entered with a partner it shared the bank with ex ante (the underlying assumption is that the shared bank connection played a role in brokering the strategic alliance). For robustness, we perform all tests both using a logit model as well as a linear probability model. Standard errors for the latter are clustered by firm. The linear probability model further contains firm-year and bank-year fixed effects. To avoid the incidental parameter problem, the logit specification only contains year fixed effects. In addition, the logit model controls for the firm's number of strategic alliances announced during the year of reference and its total number of mandates (e.g. M&A advisory mandates) of a particular type for the year.[14]

Table 18.9 displays the results of these tests. For the logistic regressions we report marginal effects rather than the direct coefficient estimates. Both the OLS and logit estimates indicate a

[11] While the coefficient for sharing the same bank in Column 1 is on the margin of statistical significance, it is only a third of the size of the intercept, i.e. even if it was statistically significant, alliances between partners sharing the same bank would still have a positive overall impact on market value.

[12] A bank that holds a borrower's debt also experiences a small benefit through the rise in firm (and therefore debt) value from increased firm performance after brokering an alliance. This more direct channel of how facilitating alliances benefits banks is, however, likely to be small.

[13] The two databases are linked by matching banks on names.

[14] These controls are absorbed by the firm-year fixed effects in the linear probability specification.

positive impact of the number of facilitated alliances on the probability of being selected to arrange a syndicated loan or underwrite a securities offering, statistically significant at the 1% level. The result for M&A advisory services are similar. The coefficient in the logit model is positive and statistically significant at the 1% level. The LPM estimate is also positive, but not statistically significant.

Table 18.9 – Are relationship banks compensated for brokering alliances?
The unit of observation for the tests displays in the table is a relationship bank-firm-year and the independent variable an indicator for whether the relationship bank is chosen at least once as the lead arranger of a loan syndicate in Columns 1 and 2, the underwriter for a bond or seasoned equity offering in Columns 3 and 4 or the adviser in an M&A transaction in Columns 5 and 6 by the firm over the next five years, starting with the year of reference. For the logistic regressions, marginal effects are displayed. Parentheses contain z-statistics for logistic regressions and t-statistics for the LPM. Standard errors for the LPM estimates have been clustered by firm. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | Bank-based financing | | Market-based financing | | M&A advisory | |
|---|---|---|---|---|---|---|
| Probability model | Logit<br>(1) | LPM<br>(2) | Logit<br>(3) | LPM<br>(4) | Logit<br>(5) | LPM<br>(6) |
| No. of alliances facilitated by bank | $0.336^{***}$ | $0.193^{***}$ | $0.033^{***}$ | $0.046^{***}$ | $0.002^{***}$ | 0.005 |
| | (30.02) | (14.20) | (12.36) | (4.92) | (3.58) | (1.02) |
| Number of syndicated loans | $0.112^{***}$ | | | | | |
| | (87.32) | | | | | |
| Number of bond issues and SEOs | | | $0.000^{***}$ | | | |
| | | | (15.40) | | | |
| Number of M&A transactions | | | | | $0.001^{***}$ | |
| | | | | | (19.33) | |
| Number of alliances | $0.010^{***}$ | | $0.009^{***}$ | | $0.000^{***}$ | |
| | (14.64) | | (37.99) | | (9.87) | |
| Year FE | Yes | No | Yes | No | Yes | No |
| Firm-year FE | No | Yes | No | Yes | No | Yes |
| Bank-year FE | No | Yes | No | Yes | No | Yes |
| N | 255,556 | 212,235 | 255,556 | 212,235 | 255,556 | 212,235 |
| (Pseudo) $R^2$ | 0.044 | 0.556 | 0.048 | 0.481 | 0.034 | 0.225 |

The estimated coefficients are not only of statistical, but also economic significance. The average marginal effect for an increase of one in the number of alliances brokered by the bank increases the probability of that bank becoming the lead arranger for at least one syndicated loan over the following five years by 33.6 percentage points (the corresponding LPM estimate suggests a 19.3 percentage point increase). The marginal effect for securities underwriting services is lower at only 3.3 percentage points (the corresponding LPM estimate being 4.6 percentage points). This difference in magnitude might have both economic and mechanical reasons, as the firm-bank relationships for our tests are formed based on syndicated lending. The economic argument is that a bank's syndicated loan department and its employees are likely more directly and visibly compensated through an additional syndicated loan than through security underwriting or M&A advisory services.

The control variables in the logit specifications indicate that, as expected, the likelihood of a bank receiving any mandate (syndicated loan, securities underwriting, M&A advisory) is positively related to the firm's number of such mandates for the period and the number of alliances, which could potentially be explained by an increased need for financing and/or investment following the announcement of an alliance, adding another reason for why a bank might be interested in facilitating alliances.

# 19 Conclusion

We investigate how individual bankers facilitate collaboration between firms in the form of strategic alliances by helping them overcome asymmetric information. Bankers can use their knowledge of borrowers obtained from prior lending transactions to help match firms to an alliance partner. Consistent with this intuition, we find that two firms are significantly more likely to enter a strategic alliance if they share the same banker. The role of bankers in transmitting information extends beyond firms inside a single banker's portfolio. We show that two firms are significantly more likely to engage in a strategic alliance even if they borrow from two different bankers, as long as those have a connection through joint prior lending. The impact of sharing a banker on the likelihood of entering a strategic alliance is strongest for informationally opaque firms. Consistent with costs to transmitting information between multiple bankers, the ability of bankers to facilitate alliances decreases as the network distance between them increases.

We find that both firms and banks profit from their involvement in strategic alliances. Firms that have a larger number of network connections to potential alliance partners enter a larger number of strategic alliances. These alliances lead to positive abnormal stock returns upon their announcement, and banks that were likely involved in their arrangement are more likely to be chosen to underwrite loans, bonds and seasoned equity offerings in the future.

Our results are robust to a range of controls and estimation techniques. They highlight a novel way through which banking relationships benefit borrowers besides providing access to capital: positive information spillovers that create value for borrowers by helping them combine resources in strategic alliances.

# A Appendix to "Does Protectionist Anti-Takeover Legislation Lead to Managerial Entrenchment?"

## A.1 Variable definitions and data sources

This appendix provides additional information on the sample construction. The executive compensation data for this study come from Capital IQ. Capital IQ provides a list of individuals that have held the CEO position at a certain firm and the compensation they have received from the firm on an annual basis. However, it does not provide information on CEO tenure. Therefore, when a former CEO stays on the board of the firm after resigning, or when a future CEO is already a high-ranking executive before being appointed CEO, the database may list several executives with nonzero compensation for a given firm-year. For those years, I retain only the information for the highest-paid executive (based on the *Total Calculated Compensation* item). I verify the accuracy of this approach on a random 10% sample by comparing it with information on the chief executive officer sourced from reference documents filed with the French Financial Markets Regulator AMF and firms' annual reports. Choosing the highest-paid executive leads to an accuracy of 97% in the chosen sample. Most errors are due to the chairperson of the board being selected instead.

I value stock option grants based on the approximation developed by Core and Guay (2002). For this purpose, I retrieve the number and exercise value of newly granted options per CEO from Capital IQ. I use the one-year Euro Interbank Offered Rate as proxy for the risk-free rate and the realized dividend yield of the previous year as a proxy for the expected dividend yield. The volatility is calculated as the annualized standard deviation of the previous 36 months of stock returns. Unfortunately, the Capital IQ database does not provide data on the maturity of granted stock options. I therefore always use the default (because most common) value of nine years suggested by Core and Guay (2002). These items together with the stock price are then sufficient to calculate the approximate Black-Scholes option price. Values for the median board member are calculated the same way.

## Appendix A. Appendix to "Does Protectionist Anti-Takeover Legislation Lead to Managerial Entrenchment?"

Table A.1.1 – **Variable definitions**

This table displays how the variables used in the paper are defined. Database items are referenced by their names in Compustat and Capital IQ. Items in italics are from Compustat. Items with an underscore are intermediate results. All other items are from Capital IQ.

| Variable | Definition |
|---|---|
| **Firm attributes (Compustat)** | |
| Book-to-market | $(ceq+txditc)/$MV if $ceq+txditc>0$; assume $txditc=0$ if missing |
| Book leverage | $(dltt+dlc)/(dltt+dlc+seq)$ |
| Buybacks/eq. | (Repurchase of Common Stock)/$ceq$ if $ceq>0$ |
| Capex/assets | $capx/at$ |
| (Capex+R&D)/assets | $(capx+xrd)/at$; assume $xrd=0$ if missing |
| Dividends/eq. | (Common Dividends Paid)/$ceq$ if $ceq>0$ |
| Employment | $emp$ if $emp_t/emp_{t-1}$ always in $[4/7,7/4]$ |
| Firm is public | Equal to 1 if there is a Compustat record for the firm-year, else 0 |
| Market leverage | $(dltt+dlc)/(dltt+dlc+$MV$+pstk)$; assume $pstk=0$ if missing |
| Market value (MV) | $prccd\times cshoc$ |
| PPE/assets | $ppent/at$ |
| ROA | $ebit_t/((at_{t-1}+at_t)/2)$ |
| ROS | $ebit/sale$ |
| Sales | $sale$ |
| Wage | $xstfws/emp$ if $emp_t/emp_{t-1}$ always in $[4/7,7/4]$ |
| **Firm attributes (Capital IQ)** | |
| Book Leverage | Total Debt/(Total Debt+Total Equity) if Total Equity>0 and Total Debt>0 |
| Firm age | $fyear$ - Year Founded |
| Firms is acquired | Equal to 1 if the firm becomes the target of a successful merger or acquisition during the year of observation, else 0. Transactions are screened as for the variable "M&A count" below. |
| Firms is acquired cross-border | Same as "firm is acquired", but the acquirer, in addition, has to be incorporated outside of France. |
| M&A count | Number of M&As in which the firm is the acquirer, owns a stake smaller than 50% before the acquisition and 100% after. A transaction is coded as cross-border if Capital IQ identifies it as such explicitly or if the acquirer and the target have a different country of incorporation. |
| M&A volume | Sum of reported transaction volumes for M&As selected as above. |
| PPE/assets | Net Property, Plant and Equipment/Total Assets |
| Return on assets | $\text{EBIT}_t/((\text{Total Assets}_{t-1}+\text{Total Assets}_t)/2)$ |
| State own. | 1 if State Owner - % Owned ≥ 5; else 0 |
| **Event study** | |
| Daily trading volume | $cshtrd\times prccd$ |
| Daily stock return | $\ln(prccd_t/ajexdi_t\times trfd_t)-\ln(prccd_{t-1}/ajexdi_{t-1}\times trfd_{t-1})$ |
| MSCI | MSCI World Index - Total Return Gross - EUR (MXWO) (log returns) |
| Risk-free rate | $\ln(1+\text{EURIBOR}^{(weekly)}/250)$ |
| **Option grants** | |
| Annual dividend yield | $\sum_{s=t-364}^{t}(divd_s)/prccd_t$ |
| Option grants | Using the Black-Scholes model and the inputs in this table section, assuming 9 years to maturity (see Core and Guay, 2002). |
| Risk-free rate | $\ln(1+\text{EURIBOR}^{(annual)})$ |
| Stock return volatility | $\sqrt{12}$ times the standard deviation of the last 36 monthly returns. |
| Strike price | $prccd-$(Granted Options Value)/(Granted Options Amount) if > 0; else $prccd$ |
| **Compensation** | |
| Equity-based compensation | (Restricted Stock Awards + Director Restricted Stock Awards + LTIP + Option Grants)/Total Compensation |
| Equity-based ex. LTIP | (Restricted Stock Awards + Director Restricted Stock Awards + Option Grants)/(Total Compensation - LTIP) |
| Stock-based compensation | (Restricted Stock Awards + Director Restricted Stock Awards + LTIP)/(Total Compensation - Option Grants) |
| Total compensation | Salary + Bonus + All Other Comp. + Non-Eq. Incentive Plan Comp. + Non-Eq. Annual Incentive Plan + Non-Eq. LTIP + Director Fee + Director Bonus + LTIP + Director Non-Eq. Incentive Plan Comp. + Director All Other Comp. + Restricted Stock Awards + Director Restricted Stock Awards + Change in Pension Plan + Director Change in Pension Plan + Option Grants |

Table A.1.2 – Treatment assignment by SIC code
The table displays how four-digit SIC codes are mapped into the five industries covered by the Alstom Decree and the defense sector, which is excluded from the analysis. SIC codes are assigned to the treatment group based on the product descriptions in the SIC manual of the US Department of Labor.

| | |
|---|---|
| Panel A: Treated SIC codes | |
| Power and hydrocarbons | 1094, 1200, 1220, 1221, 1222, 1241, 1300, 1311, 1321, 1381, 1382, 1389, 1623, 1731, 2860, 2911, 2990, 2999, 3357, 3511, 3533, 3612, 3620, 3621, 3675, 3691, 4610, 4612, 4613, 4619, 4900, 4910, 4911, 4922, 4923, 4924, 4925, 4931, 4932, 4939, 4991, 5063, 5171, 5172, 5541, 9631 |
| Water | 1781, 4941, 9511 |
| Transportation | 1600, 1611, 1622, 1629, 2531, 3452, 3465, 3510, 3519, 3537, 3629, 3694, 3700, 3711, 3713, 3714, 3715, 3720, 3721, 3724, 3728, 3730, 3731, 3743, 3790, 4011, 4013, 4100, 4111, 4119, 4131, 4141, 4142, 4173, 4200, 4210, 4212, 4213, 4214, 4215, 4220, 4221, 4222, 4225, 4231, 4311, 4400, 4412, 4424, 4432, 4449, 4453, 4481, 4482, 4489, 4491, 4492, 4499, 4512, 4513, 4522, 4581, 4700, 4722, 4729, 4731, 4741, 4785, 4789, 5012, 5014, 5088, 7513, 9621 |
| Communication | 3576, 3613, 3660, 3661, 3663, 3669, 4810, 4811, 4812, 4813, 4822, 4830, 4832, 4833, 4841, 4888, 4890, 4899, 7383, 7385 |
| Public health | 2833, 2834, 2835, 2836, 3800, 3826, 3842, 3843, 3844, 3845, 3851, 5048, 5122, 7352, 7391, 8000, 8011, 8021, 8031, 8041, 8042, 8043, 8049, 8050, 8051, 8052, 8059, 8060, 8062, 8063, 8069, 8071, 8072, 8080, 8082, 8090, 8092, 8093, 8099, 8300, 8731, 9431 |
| Panel B: Excluded SIC codes | |
| Defense | 2892, 3480, 3482, 3483, 3484, 3489, 3760, 3761, 3764, 3769, 3795, 3810, 3812, 7381, 9661, 9711 |

## A.2 Robustness tests

Table A.2.1 – The Alstom Decree's impact on firms' likelihood of becoming an acquisition target—logistic regression

The coefficients in the table have been estimated using logistic regression. The dependent variable is an indicator for whether the firm is acquired during the year of observation. Firm characteristics are lagged by one year. The sample ranges from 2011 to 2016 and contains publicly listed and privately held firms incorporated in France with revenues exceeding five million euros. Financial firms and the defense industry have been excluded from the sample. Treated firms are firms active in one of the industries mentioned by the Alstom Decree. Parentheses contain *t*-statistics. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively. The difference-in-differences estimate is the marginal effect at the means and has been calculated as suggested by Puhani (2012) for the case of nonlinear models. The corresponding standard error has been derived using the delta method.

| | All bids | | Cross-border | | Domestic | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treatment×Post | -0.394* | -0.387* | -0.746** | -0.749** | -0.227 | -0.214 |
| | (-1.91) | (-1.87) | (-2.04) | (-2.05) | (-0.91) | (-0.86) |
| Post | -0.064 | -0.034 | -0.415 | -0.406 | 0.202 | 0.242 |
| | (-0.15) | (-0.08) | (-0.67) | (-0.66) | (0.34) | (0.40) |
| Treatment | 0.309** | 0.430*** | 0.505** | 0.606** | 0.224 | 0.353** |
| | (2.30) | (3.18) | (2.09) | (2.50) | (1.39) | (2.18) |
| ln(Revenues) | | -0.325*** | | -0.206*** | | -0.384*** |
| | | (-8.65) | | (-3.38) | | (-8.11) |
| ROA | | 1.177*** | | 1.854*** | | 0.831* |
| | | (3.00) | | (2.72) | | (1.75) |
| Book leverage | | -0.084 | | 0.055 | | -0.149 |
| | | (-0.48) | | (0.18) | | (-0.70) |
| PPE/assets | | 0.254 | | -0.288 | | 0.469 |
| | | (0.97) | | (-0.56) | | (1.54) |
| Firm is public | | -1.105*** | | -0.555 | | -1.641*** |
| | | (-3.05) | | (-1.18) | | (-2.81) |
| Constant | -3.893*** | -2.945*** | -5.366*** | -4.813*** | -4.161*** | -3.018*** |
| | (-38.88) | (-17.92) | (-26.64) | (-15.93) | (-36.25) | (-15.38) |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Pseudo-$R^2$ | 0.00 | 0.03 | 0.01 | 0.02 | 0.00 | 0.03 |
| Observations | 30,699 | 30,699 | 30,699 | 30,699 | 30,699 | 30,699 |
| Difference-in-differences | -0.0077* | -0.0071* | -0.0039 | -0.0038 | -0.0035 | -0.0030 |
| SE | 0.0045 | 0.0043 | 0.0025 | 0.0024 | 0.0041 | 0.0036 |

Table A.2.2 – Cumulative abnormal stock returns following the Alstom Decree—tests based on cross-sectional variation

Statistical significance for the cumulative abnormal returns on display has been calculated based on the cross-sectional variation in firm-level CAR around the Alstom Decree. The sample consists of all publicly listed firms incorporated in France, excluding financial firms and the defense industry. Firms have been assigned to the treatment and control groups based on four-digit SIC codes. Firms in the treatment group are those subject to the Alstom Decree. The estimation window contains a maximum of 250 trading days of observations extending back from the event-day. The market benchmark is the market value-weighted average daily return on the common stock of all publicly listed French firms in Compustat. The event window is specified in the form (pre-event days; post-event days) and the event day is May 15, 2014. Observations with daily trading volume below one thousand euros have been dropped. Furthermore, I require stocks to have at least 200 non-missing observations in the estimation window and no missing observations in the event window for inclusion in the treatment or control group. The coefficients on display represent cumulative abnormal returns. *t*-statistics are given in parentheses. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively.

| Panel A: Constant mean return model | | | |
|---|---|---|---|
| Event window | Treatment | Control | Treatment-Control |
| (-0;0) | -0.0085*** | -0.0029*** | -0.0056*** |
| | (-4.05) | (-2.61) | (-2.62) |
| (-1;0) | -0.0104*** | -0.0042*** | -0.0062** |
| | (-4.16) | (-2.28) | (-1.95) |
| (-1;1) | -0.0212*** | -0.0127*** | -0.0085** |
| | (-6.65) | (-5.68) | (-2.20) |
| **Panel B: Market model** | | | |
| Event window | Treatment | Control | Treatment-Control |
| (-0;0) | -0.0158*** | -0.0088*** | -0.0070*** |
| | (-7.05) | (-7.88) | (-3.14) |
| (-1;0) | -0.0172*** | -0.0098** | -0.0074** |
| | (-6.53) | (-5.25) | (-2.30) |
| (-1;1) | -0.0263*** | -0.0168*** | -0.0095** |
| | (-8.06) | (-7.42) | (-2.41) |

Table A.2.3 – Placebo tests for takeover probability
The coefficients displayed below have been estimated using ordinary least squares. The dependent variable is an indicator for whether the firm is acquired during the year of observation. The sample ranges from 2011 to 2016 and contains publicly listed and privately held firms incorporated in France with revenues exceeding five million euros. Financial firms and the defense industry have been excluded from the sample. Treated firms are firms active in one of the industries mentioned by the Alstom Decree. The tests assume an additional treatment occurred one or two years before the Alstom Decree. This assumption is implemented by adding a time fixed effect $Post^{(Placebo)}$ equal to one for all dates following the hypothesized alternative treatment date and zero otherwise to the baseline specification, together with an interaction term for the treatment group ($Treatment \times Post^{(Placebo)}$). Controls include the natural logarithm of firm age, the natural logarithm of sales, ROA, market leverage, the PPE-to-assets ratio and an indicator variable for whether the French state holds a stake in the firm. Parentheses contain $t$-statistics calculated from robust standard errors clustered by firm. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively.

| | All bids | | Cross-border | |
|---|---|---|---|---|
| | year t-1 | year t-2 | year t-1 | year t-2 |
| | (1) | (2) | (3) | (4) |
| Treatment×Post | -0.017*** | -0.009** | -0.008** | -0.005* |
| | (-2.64) | (-2.00) | (-2.19) | (-1.80) |
| Treatment | -0.002 | -0.000 | 0.001 | 0.003 |
| | (-0.50) | (-0.05) | (0.70) | (1.01) |
| Post | 0.001 | 0.000 | -0.002 | -0.003 |
| | (0.17) | (0.01) | (-0.47) | (-0.58) |
| Treatment×Post$^{(Placebo)}$ | 0.013** | 0.004 | 0.005 | -0.000 |
| | (2.02) | (0.66) | (1.36) | (-0.06) |
| Post$^{(Placebo)}$ | 0.005 | -0.002 | -0.004 | -0.010 |
| | (0.80) | (-0.27) | (-0.83) | (-1.59) |
| Controls | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |
| $R^2$ | 0.01 | 0.01 | 0.00 | 0.00 |
| Observations | 30,610 | 30,610 | 30,610 | 30,610 |

Table A.2.4 – Placebo tests for equity-based compensation
This table presents placebo tests for the impact of the Alstom Decree on the compensation of CEOs. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France with more than five million in sales and a market capitalization above 75 million euros. Financial firms and the defense industry have been excluded. CEO total refers to the CEO's total annual compensation. CEO equity-based is the fraction of annual CEO compensation paid out in equity instruments. The coefficients displayed below were estimated using ordinary least squares. The tests assume an additional treatment occurred one or two years before the Alstom Decree. This assumption is implemented by adding a time fixed effect $Post^{(Placebo)}$ equal to one for all dates following the hypothesized alternative treatment date and zero otherwise to the baseline specification, together with an interaction term for the treatment group ($Treatment \times Post^{(Placebo)}$). Controls include the natural logarithm of sales, ROA, market leverage, the natural logarithm of firm age, the PPE-to-assets ratio, and an indicator variable for whether the French state holds a stake in the firm. Parentheses contain $t$-statistics calculated from robust standard errors clustered by firm. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively.

| | CEO total | | CEO equity-based | |
|---|---|---|---|---|
| | year t-1 | year t-2 | year t-1 | year t-2 |
| | (1) | (2) | (3) | (4) |
| Treatment×Post | 0.199** | 0.211*** | 0.086** | 0.083** |
| | (2.44) | (2.72) | (2.23) | (2.40) |
| Treatment | -0.275 | -0.298 | 0.083* | 0.078 |
| | (-0.86) | (-0.89) | (1.69) | (1.40) |
| Post | -0.021 | -0.014 | 0.052 | 0.057 |
| | (-0.12) | (-0.08) | (1.00) | (1.11) |
| Treatment×Post$^{(Placebo)}$ | 0.025 | 0.039 | -0.005 | 0.003 |
| | (0.29) | (0.34) | (-0.14) | (0.08) |
| Post$^{(Placebo)}$ | -0.203 | 0.162 | -0.083 | -0.018 |
| | (-0.91) | (0.86) | (-1.21) | (-0.34) |
| Controls | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |
| $R^2$ | 0.50 | 0.50 | 0.29 | 0.29 |
| Observations | 1,166 | 1,166 | 693 | 693 |

Table A.2.5 – The Alstom Decree's impact on executive compensation—alternative measures for executive compensation

The coefficients displayed below have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France with more than five million in sales and a market capitalization above 75 million euros. Financial firms and the defense industry have been excluded. Treated firms are active in one of the industries mentioned by the Alstom Decree. CEO EB ex. LTIP is the fraction of annual compensation paid out in stock and option grants except for shares granted under a long term incentive plan. CEO equity euros is the absolute amount of compensation for the year paid out in common stock and stock options. CEO stock-based compensation is the percentage of annual compensation paid out in common stock. BM (CEO) total is the total annual compensation of the median board member (CEO). BM EB is the fraction of the median board member's annual compensation paid out in equity instruments. Columns 6 and 7 exclude firms in which the French government ever has an ownership stake of over 5% during the sample period. Parentheses contain $t$-statistics calculated from robust standard errors clustered by firm. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively.

| | CEO EB ex. LTIP | ln(1+CEO equity €) | CEO stock-based | ln(BM total) | BM EB | CEO EB | ln(CEO total) |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Treatment×Post | 0.084** | 1.021** | 0.050 | 0.045 | 0.075* | 0.082** | 0.245*** |
| | (2.32) | (2.01) | (1.48) | (0.29) | (1.95) | (2.42) | (2.75) |
| Treatment | 0.090* | 1.480** | 0.070 | 0.566* | 0.089** | 0.070 | -0.321 |
| | (1.90) | (2.06) | (1.44) | (1.71) | (2.02) | (1.58) | (-1.02) |
| Post | 0.055 | 0.655 | 0.051 | 0.553 | 0.046 | 0.054 | -0.021 |
| | (1.02) | (0.68) | (1.08) | (1.18) | (0.85) | (1.05) | (-0.12) |
| ln(Firm age) | -0.016 | -0.124 | -0.016 | 0.015 | -0.034** | -0.027 | 0.081 |
| | (-0.74) | (-0.41) | (-0.85) | (0.12) | (-2.19) | (-1.33) | (0.90) |
| ln(Sales) | 0.044*** | 0.822*** | 0.047*** | 0.360*** | 0.045*** | 0.050*** | 0.431*** |
| | (5.45) | (7.18) | (6.29) | (6.57) | (6.07) | (6.70) | (12.73) |
| State own. | -0.057 | -1.989 | -0.090 | -0.488 | -0.066 | | |
| | (-0.68) | (-1.47) | (-1.26) | (-1.27) | (-0.94) | | |
| ROA | -0.081 | -0.094 | -0.008 | -3.461** | -0.361** | -0.089 | -1.602** |
| | (-0.41) | (-0.03) | (-0.04) | (-2.22) | (-2.04) | (-0.47) | (-2.25) |
| PPE/assets | -0.040 | -1.025 | 0.005 | -0.921 | -0.043 | -0.038 | -0.333 |
| | (-0.44) | (-0.78) | (0.05) | (-1.48) | (-0.54) | (-0.44) | (-0.79) |
| Market leverage | -0.139 | -2.076* | -0.112 | -0.213 | -0.164** | -0.133 | -0.736*** |
| | (-1.61) | (-1.91) | (-1.39) | (-0.45) | (-2.18) | (-1.64) | (-2.66) |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.26 | 0.37 | 0.29 | 0.34 | 0.28 | 0.29 | 0.50 |
| Observations | 653 | 693 | 682 | 1,180 | 661 | 673 | 1,126 |

## A.3 Alternative control group

Table A.3.1 – The Alstom Decree's impact on wages and employment—affected industries in France compared to those in other EU member states
The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France, Germany, Italy, Belgium, the Netherlands, and Luxembourg with over five million in sales and a market capitalization above 75 million euros that belong to one of the five industry sectors mentioned in the Alstom Decree. Treated firms are those incorporated in France. Wage is the firm-wide average wage. Employment is the number of employees measured in thousands. Parentheses contain $t$-statistics calculated from robust standard errors clustered by firm. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively. Total assets, market value of equity and total employment are the control variables used by Bertrand and Mullainathan (1999b).

| | ln(Wage) | | | ln(Employment) | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treatment×Post | -0.040 | -0.010 | -0.025 | 0.067 | 0.048 |
| | (-0.25) | (-0.06) | (-0.16) | (0.51) | (0.66) |
| Treatment | 0.313** | 0.348** | 0.312** | 0.219 | 0.108 |
| | (2.02) | (2.32) | (1.98) | (0.74) | (1.03) |
| Post | 0.113 | 0.008 | 0.067 | -0.106 | -0.183 |
| | (0.58) | (0.04) | (0.32) | (-0.15) | (-1.01) |
| ln(MV) | | 0.116 | | | |
| | | (1.09) | | | |
| ln(Assets) | | -0.116 | | | |
| | | (-0.82) | | | |
| ln(Employment) | | -0.503*** | | | |
| | | (-5.64) | | | |
| ln(Sales) | | 0.461*** | -0.042 | | 0.919*** |
| | | (3.57) | (-0.80) | | (24.79) |
| State own. | | | 0.431* | | 0.257 |
| | | | (1.92) | | (1.27) |
| ROA | | | -0.626 | | 0.392 |
| | | | (-0.69) | | (0.57) |
| PPE/assets | | | -0.307 | | -0.688** |
| | | | (-0.63) | | (-2.05) |
| Market leverage | | | -0.509 | | 0.110 |
| | | | (-1.38) | | (0.42) |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.16 | 0.23 | 0.18 | 0.26 | 0.89 |
| Observations | 983 | 983 | 983 | 1,017 | 1,017 |

Table A.3.2 – The Alstom Decree's impact on investment and M&A—affected industries in France compared to those in other EU member states
The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France, Germany, Italy, Belgium, the Netherlands, and Luxembourg with over five million in sales and a market capitalization above 75 million euros that belong to one of the five industry sectors mentioned in the Alstom Decree. Treated firms are those incorporated in France. The number of M&As is the number of transactions listed in Capital IQ for the firm-year in which the acquirer holds less than 50% before the transaction and 100% afterwards. The transaction volume is calculated from the subset of transactions fulfilling the same criteria and in which in addition the total consideration paid was disclosed. Parentheses contain *t*-statistics calculated from robust standard errors clustered by firm. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively.

| | ln(Capex+R&D) | | (Capex+R&D)/assets | | Capex/assets | | M&A count | | ln(1+M&A vol.) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Treatment×Post | -0.030 | -0.059 | -0.000 | 0.001 | 0.001 | 0.001 | -0.044 | -0.041 | 0.160 | 0.158 |
| | (-0.22) | (-0.69) | (-0.02) | (0.13) | (0.28) | (0.33) | (-0.43) | (-0.41) | (0.80) | (0.79) |
| Treatment | 0.501* | 0.072 | -0.004 | -0.003 | -0.001 | -0.000 | 0.282** | 0.225* | 0.105 | 0.030 |
| | (1.85) | (0.47) | (-0.47) | (-0.37) | (-0.12) | (-0.00) | (2.19) | (1.87) | (0.63) | (0.18) |
| Post | 0.238 | -0.169 | -0.015 | -0.012 | -0.015 | -0.015 | -0.200 | -0.207 | -1.333 | -1.271 |
| | (0.41) | (-0.73) | (-0.70) | (-0.68) | (-0.57) | (-0.61) | (-0.54) | (-0.56) | (-1.49) | (-1.38) |
| R&D missing | -1.890*** | -0.698*** | -0.025*** | -0.032*** | | | | | | |
| | (-6.27) | (-4.38) | (-3.85) | (-4.30) | | | | | | |
| ln(Firm age) | | -0.196** | | -0.006 | | 0.001 | | -0.023 | | -0.110* |
| | | (-2.01) | | (-1.58) | | (0.20) | | (-0.51) | | (-1.65) |
| ln(Sales) | | 0.885*** | | -0.004* | | -0.001 | | 0.109*** | | 0.154*** |
| | | (18.69) | | (-1.90) | | (-0.84) | | (5.11) | | (4.86) |
| State own. | | 0.602** | | 0.008 | | 0.005 | | -0.132 | | -0.065 |
| | | (2.53) | | (0.93) | | (0.81) | | (-1.39) | | (-0.38) |
| ROA | | -1.531** | | -0.074 | | 0.027 | | 0.232 | | 0.892* |
| | | (-2.33) | | (-1.21) | | (1.19) | | (0.81) | | (1.72) |
| Book-to-market | | -0.008 | | 0.000 | | -0.000* | | 0.004** | | 0.009*** |
| | | (-1.60) | | (1.04) | | (-1.92) | | (2.20) | | (3.11) |
| Market leverage | | 0.165 | | -0.035** | | -0.004 | | -0.298** | | -0.131 |
| | | (0.45) | | (-2.48) | | (-0.40) | | (-1.99) | | (-0.50) |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.28 | 0.79 | 0.31 | 0.37 | 0.13 | 0.14 | 0.09 | 0.13 | 0.04 | 0.07 |
| Observations | 1,120 | 1,120 | 1,120 | 1,120 | 1,120 | 1,120 | 1,129 | 1,129 | 1,129 | 1,129 |

Table A.3.3 – The Alstom Decree's impact on operating performance—affected industries in France compared to those in other EU member states
The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France, Germany, Italy, Belgium, the Netherlands, and Luxembourg with over five million in sales and a market capitalization above 75 million euros that belong to one of the five industry sectors mentioned in the Alstom Decree. Treated firms are those incorporated in France. ROA is the return on assets defined as EBIT divided by total assets. ROS is the return on sales calculated as EBIT as a fraction of net sales. Parentheses contain $t$-statistics calculated from robust standard errors clustered by firm. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively.

| | ROA | | ROS | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatment×Post | 0.005 | 0.006 | 0.025 | 0.022 |
| | (0.65) | (0.81) | (0.71) | (0.71) |
| Treatment | -0.004 | -0.014 | -0.028 | -0.065* |
| | (-0.41) | (-1.38) | (-0.70) | (-1.70) |
| Post | -0.020 | -0.033 | -0.046 | -0.079 |
| | (-1.26) | (-1.65) | (-1.32) | (-1.40) |
| ln(Firm age) | | 0.007* | | 0.011 |
| | | (1.69) | | (0.79) |
| ln(Sales) | | 0.015*** | | 0.062*** |
| | | (5.00) | | (4.63) |
| State own. | | -0.027*** | | -0.106** |
| | | (-2.81) | | (-2.08) |
| PPE/assets | | 0.067*** | | 0.392*** |
| | | (2.74) | | (4.09) |
| Market leverage | | -0.092*** | | -0.083 |
| | | (-4.48) | | (-1.43) |
| Year FE | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes |
| $R^2$ | 0.10 | 0.23 | 0.14 | 0.28 |
| Observations | 1,140 | 1,140 | 1,141 | 1,141 |

Table A.3.4 – The Alstom Decree's impact on capital structure and distributions—affected industries in France compared to those in other EU member states
The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France, Germany, Italy, Belgium, the Netherlands, and Luxembourg with over five million in sales and a market capitalization above 75 million euros that belong to one of the five industry sectors mentioned in the Alstom Decree. Treated firms are those incorporated in France. Book leverage is book debt divided by the sum of book debt and book equity. Market leverage is book debt divided by the sum of book debt, market value of common stock, and book value of preferred stock. Dividends/eq. is the fraction of book equity returned to shareholders in the form of cash dividends and buybacks/eq. is the fraction of book equity returned to shareholders in the form of share repurchases. Parentheses contain $t$-statistics calculated from robust standard errors clustered by firm. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively.

| | Book leverage | | Market leverage | | Dividends/eq. | | Buybacks/eq. | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treatment×Post | 0.009 | 0.010 | 0.023 | 0.021 | 0.002 | 0.002 | 0.005 | 0.006 |
| | (0.54) | (0.58) | (1.27) | (1.10) | (0.34) | (0.35) | (1.18) | (1.25) |
| Treatment | -0.004 | -0.027 | -0.012 | -0.030 | -0.010 | -0.007 | -0.002 | -0.001 |
| | (-0.13) | (-0.95) | (-0.40) | (-1.08) | (-1.21) | (-1.07) | (-0.60) | (-0.22) |
| Post | -0.116*** | -0.119** | -0.059 | -0.088* | -0.007 | -0.001 | 0.002 | 0.006 |
| | (-2.64) | (-2.12) | (-0.98) | (-1.68) | (-0.73) | (-0.04) | (0.53) | (1.38) |
| ln(Firm age) | | -0.026 | | -0.001 | | 0.002 | | -0.002 |
| | | (-1.59) | | (-0.07) | | (0.41) | | (-0.70) |
| ln(Sales) | | 0.032*** | | 0.032*** | | -0.002 | | -0.002 |
| | | (3.57) | | (3.96) | | (-0.73) | | (-1.00) |
| State own. | | 0.034 | | 0.068 | | 0.005 | | -0.001 |
| | | (0.81) | | (1.43) | | (0.52) | | (-0.14) |
| ROA | | -0.107 | | -0.441*** | | 0.355*** | | 0.097 |
| | | (-0.75) | | (-4.45) | | (3.76) | | (1.59) |
| Book-to-market | | -0.003*** | | 0.007*** | | -0.001*** | | -0.000 |
| | | (-2.98) | | (3.72) | | (-2.85) | | (-0.71) |
| PPE/assets | | 0.212*** | | 0.243*** | | -0.012 | | -0.021* |
| | | (2.80) | | (3.00) | | (-0.52) | | (-1.71) |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.27 | 0.34 | 0.31 | 0.42 | 0.16 | 0.34 | 0.05 | 0.10 |
| Observations | 1,120 | 1,120 | 1,122 | 1,122 | 1,120 | 1,120 | 994 | 994 |

Table A.3.5 – The Alstom Decree's impact on executive compensation—affected industries in France compared to those in other EU member states

The coefficients displayed in the table have been estimated using ordinary least squares. The sample ranges from 2011 to 2016 and contains all publicly listed firms incorporated in France, Germany, Italy, Belgium, the Netherlands, and Luxembourg with over five million in sales and a market capitalization above 75 million euros that belong to one of the five industry sectors mentioned in the Alstom Decree. Treated firms are those incorporated in France. CEO equity-based compensation is the fraction of annual compensation paid out in stock and option grants. CEO total is the executive's total compensation for the fiscal year. Parentheses contain *t*-statistics calculated from robust standard errors clustered by firm. All continuous variables have been winsorized at the 1% and 99% levels. Variables are defined in Appendix A.1. One, two, and three asterisks denote statistical significance at the 10%, 5%, and 1% level, respectively.

| | ln(CEO total) | CEO equity-based |
|---|---|---|
| | (1) | (2) |
| Treatment×Post | -0.014 | 0.070* |
| | (-0.12) | (1.71) |
| Treatment | 0.093 | -0.250*** |
| | (0.68) | (-6.61) |
| Post | -0.070 | 0.051 |
| | (-0.22) | (0.71) |
| ln(Firm age) | 0.017 | -0.039* |
| | (0.23) | (-1.87) |
| ln(Sales) | 0.469*** | 0.011 |
| | (10.13) | (1.00) |
| State own. | -0.508*** | -0.096 |
| | (-2.65) | (-1.37) |
| ROA | -0.187 | 0.174 |
| | (-0.21) | (0.64) |
| PPE/assets | 0.113 | -0.043 |
| | (0.27) | (-0.49) |
| Market leverage | -0.309 | -0.055 |
| | (-0.98) | (-0.60) |
| Year FE | Yes | Yes |
| Industry FE | Yes | Yes |
| $R^2$ | 0.46 | 0.33 |
| Observations | 876 | 529 |

# B Appendix to "ICO investors"

## B.1 Sample constituents

Table B.1.1 – Sample constituents
The table lists all 306 ICOs in the sample, ordered by the amount raised, in descending order. The total amount raised is specified in millions of US dollars. Where the total is listed as n/a, secondary sources indicated a total exceeding $1m but we were unable to establish the exact amount raised in the presale and/or crowdsale using primary sources. The end date is specified in the format month/day/year. The column Inv. sample indicates which of the ICOs are part of the "investor sample".

| Name | Date ended | Total raised | Industry | Inv. sample |
|---|---|---|---|---|
| Filecoin | 9.7.17 | 233.0 | Data storage | no |
| Tezos | 7.14.17 | 219.7 | Blockchain Infra. | no |
| Hdac | 12.22.17 | 210.2 | Blockchain Infra. | no |
| Bancor | 6.12.17 | 159.3 | Trading & exchanges | yes |
| SIRIN LABS | 12.25.17 | 157.9 | Communications | yes |
| Polkadot | 10.27.17 | 144.3 | Blockchain Infra. | no |
| TenX | 6.24.17 | 104.8 | Payments | no |
| Status | 6.21.17 | 101.0 | Blockchain Infra. | yes |
| Envion | 1.14.18 | 100.0 | Crypto mining | no |
| Kik | 9.26.17 | 98.5 | Blockchain Infra. | yes |
| Grid+ | 11.11.17 | 74.7 | Energy & utilities | yes |
| Bankex | 12.26.17 | 74.3 | Finance | no |
| WAX | 11.29.17 | 68.4 | Video games & VR | no |
| NAGA | 12.15.17 | 65.0 | Payments | yes |
| Kyber Network | 9.18.17 | 58.7 | Trading & exchanges | yes |
| Blockstack | 12.1.17 | 56.8 | Privacy & security | no |
| Storm | 12.7.17 | 56.2 | Video games & VR | no |
| Ambrosus | 10.20.17 | 56.1 | Provenance & notary | no |

Continued

| Name | Date ended | Total raised | Industry | Inv. sample |
|---|---|---|---|---|
| Neuromation | 1.7.18 | 56.0 | Data analytics | yes |
| indaHash | 12.20.17 | 55.7 | Social networks | no |
| Crypterium | 1.5.18 | 55.3 | Payments | yes |
| MobileGo | 5.24.17 | 53.1 | Video games & VR | no |
| TraDove | 2.28.18 | 52.0 | Social networks | no |
| Pundi X | 1.21.18 | 49.4 | Payments | yes |
| Unikoin Gold | 10.22.17 | 48.3 | Gambling | no |
| SONM | 6.17.17 | 46.7 | Blockchain Infra. | yes |
| Enigma | 9.12.17 | 45.0 | Marketplaces | no |
| Bread | 12.24.17 | 44.0 | Payments | yes |
| Trade Token | 1.7.18 | 42.5 | Trading & exchanges | no |
| Electroneum | 10.19.17 | 41.5 | Blockchain Infra. | no |
| Finom | 12.30.17 | 41.3 | Finance | no |
| Bloom | 1.1.18 | 40.0 | Finance | no |
| Mobius | 1.18.18 | 39.0 | Communications | no |
| Ripio Credit Network | 11.10.17 | 37.8 | Finance | no |
| Basic Attention Token | 5.31.17 | 36.0 | Commerce & advertising | no |
| hero token | 2.28.18 | 35.9 | Finance | no |
| Centra | 10.2.17 | 35.6 | Payments | no |
| Aeternity | 6.9.17 | 35.0 | Blockchain Infra. | yes |
| Enjin Coin | 10.31.17 | 34.8 | Video games & VR | yes |
| CRYPTO20 | 11.30.17 | 34.8 | Finance | no |
| TokenPay | 12.26.17 | 34.2 | Payments | no |
| Etherparty | 10.29.17 | 33.6 | Blockchain Infra. | yes |
| SingularityNET | 12.22.17 | 33.3 | Data analytics | yes |
| Jibrel Network | 12.27.17 | 33.2 | Finance | no |
| Civic | 6.28.17 | 33.0 | Identity & reputation | no |
| Raiden Network Token | 11.1.17 | 31.9 | Payments | no |
| Polybius | 7.5.17 | 31.0 | Finance | no |
| Storiqa | 1.29.18 | 30.2 | Commerce & advertising | yes |
| Stox | 8.3.17 | 30.0 | Gambling | yes |
| Restart Energy | 3.14.18 | 30.0 | Energy & utilities | yes |
| Blackmoon Crypto | 9.13.17 | 30.0 | Finance | no |
| DADI | 2.28.18 | 29.0 | Cloud computing | no |
| ETHLend | 12.27.17 | 28.7 | Finance | no |
| Monetha | 9.30.17 | 28.6 | Payments | yes |
| Electrify.Asia | 3.2.18 | 27.3 | Marketplaces | yes |
| Spectre | 12.10.17 | 27.0 | Trading & exchanges | no |
| OmiseGO | 6.27.17 | 26.8 | Payments | no |
| Monaco | 6.18.17 | 26.5 | Payments | yes |
| Power Ledger | 10.6.17 | 26.4 | Energy & utilities | no |
| Everex | 8.31.17 | 26.4 | Finance | yes |
| FunFair | 6.23.17 | 26.0 | Gambling | yes |

Continued

| Name | Date ended | Total raised | Industry | Inv. sample |
|---|---|---|---|---|
| Decentraland | 8.17.17 | 26.0 | Video games & VR | no |
| BitClave | 12.29.17 | 25.7 | Search | yes |
| Neumark | 12.17.17 | 25.7 | Finance | no |
| 0x | 8.16.17 | 25.5 | Trading & exchanges | no |
| Tierion | 7.28.17 | 25.4 | Privacy & security | no |
| Aragon | 5.17.17 | 25.0 | Blockchain Infra. | no |
| Target Coin | 8.31.17 | 24.9 | Finance | no |
| Medicalchain | 2.28.18 | 24.0 | Drugs & healthcare | yes |
| SophiaTX | 12.17.17 | 23.5 | Blockchain Infra. | yes |
| FinShi | 10.6.17 | 23.0 | Finance | no |
| Pillar | 7.17.17 | 22.8 | Payments | yes |
| BitDegree | 12.29.17 | 22.5 | Education | yes |
| BLOCKv | 10.25.17 | 22.1 | Blockchain Infra. | no |
| KICKICO | 9.16.17 | 22.1 | Finance | yes |
| Simple Token | 12.1.17 | 21.8 | Commerce & advertising | no |
| Selfkey | 1.14.18 | 21.7 | Identity & reputation | yes |
| UTRUST | 11.20.17 | 21.4 | Payments | yes |
| Debitum Network | 2.26.18 | 20.9 | Finance | yes |
| Gladius | 2.12.18 | 20.8 | Privacy & security | no |
| QLINK | 1.19.18 | 20.6 | Communications | no |
| Aventus | 9.6.17 | 20.0 | Events & entertainment | yes |
| aXpire | 1.12.18 | 20.0 | Finance | no |
| Dock.io | 2.21.18 | 20.0 | Marketplaces | yes |
| Covesting | 12.31.17 | 19.9 | Finance | yes |
| TE-FOOD | 2.22.18 | 19.1 | Provenance & notary | yes |
| DMarket | 11.28.17 | 19.0 | Video games & VR | yes |
| Uptoken | 12.15.17 | 18.9 | Payments | yes |
| FintruX Network | 2.28.18 | 18.9 | Finance | no |
| OriginTrail | 1.17.18 | 18.5 | Provenance & notary | yes |
| Lympo | 2.28.18 | 18.2 | Health | yes |
| Insights Network | 2.14.18 | 18.1 | Data analytics | yes |
| Cosmos | 4.6.17 | 18.1 | Blockchain Infra. | no |
| Uquid Coin | 11.7.17 | 17.8 | Payments | no |
| Cryptopay | 10.30.17 | 17.5 | Payments | no |
| InsurePal | 1.16.18 | 17.4 | Insurance | no |
| BOScoin | 5.10.17 | 17.4 | Blockchain Infra. | no |
| Mysterium | 5.30.17 | 17.3 | Privacy & security | no |
| Latium | 1.18.18 | 17.2 | Marketplaces | no |
| MCAP | 5.27.17 | 17.1 | Finance | no |
| Change | 10.16.17 | 17.0 | Finance | yes |
| Cindicator | 10.12.17 | 16.9 | Finance | yes |
| Rivetz | 9.10.17 | 16.9 | Privacy & security | no |
| SmartMesh | 12.3.17 | 16.8 | Communications | yes |

Continued

| Name | Date ended | Total raised | Industry | Inv. sample |
|---|---|---|---|---|
| AppCoins | 12.20.17 | 16.7 | Marketplaces | yes |
| Bluzelle | 1.20.18 | 16.3 | Data analytics | no |
| AidCoin | 1.16.18 | 16.2 | Charity | yes |
| Arbidex | 2.28.18 | 16.0 | Trading & exchanges | yes |
| Gatcoin | 1.14.18 | 16.0 | Commerce & advertising | no |
| Waves | 5.31.16 | 15.8 | Blockchain Infra. | no |
| Qtum | 3.21.17 | 15.4 | Blockchain Infra. | no |
| Maecenas | 10.7.17 | 15.4 | Art & music | yes |
| Santiment Network Token | 7.5.17 | 15.3 | Trading & exchanges | no |
| COPYTRACK | 2.9.18 | 15.1 | Privacy & security | yes |
| Adhive | 2.28.18 | 15.0 | Commerce & advertising | no |
| AirToken | 10.7.17 | 15.0 | Finance | no |
| Ignis | 11.4.17 | 15.0 | Blockchain Infra. | no |
| Dynamic Trading Rights | 12.6.17 | 15.0 | Trading & exchanges | no |
| Red Pulse | 10.8.17 | 14.9 | Finance | no |
| Cofound.it | 6.7.17 | 14.6 | Finance | yes |
| Modum | 9.22.17 | 14.6 | Drugs & healthcare | yes |
| OAX | 7.4.17 | 14.4 | Trading & exchanges | yes |
| HelloGold | 10.5.17 | 14.3 | Finance | no |
| AdEx | 6.30.17 | 14.2 | Commerce & advertising | no |
| DIMCOIN | 8.28.17 | 14.0 | Trading & exchanges | no |
| Substratum | 9.14.17 | 13.8 | Blockchain Infra. | no |
| MicroMoney | 11.18.17 | 13.5 | Finance | no |
| DEEX | 2.28.18 | 12.9 | Trading & exchanges | no |
| TokenCard | 5.2.17 | 12.7 | Payments | yes |
| ETCWin | 11.6.17 | 12.4 | Blockchain Infra. | no |
| Qbao | 11.20.17 | 12.4 | Social networks | no |
| Science Blockchain | 11.22.17 | 12.3 | Finance | no |
| NapoleonX | 2.28.18 | 12.3 | Finance | yes |
| Gnosis | 4.22.17 | 12.1 | Data analytics | no |
| Bitbounce | 8.29.17 | 11.8 | Communications | no |
| WaBi | 1.28.18 | 11.8 | Provenance & notary | yes |
| iExec RLC | 4.19.17 | 11.7 | Cloud computing | yes |
| REAL | 9.30.17 | 11.2 | Real estate | yes |
| Po.et | 8.8.17 | 11.0 | Content mgmt. | no |
| Wagerr | 6.21.17 | 10.8 | Gambling | no |
| spectiv | 12.29.17 | 10.7 | Commerce & advertising | no |
| Iconomi | 9.26.16 | 10.7 | Finance | yes |
| Patientory | 6.28.17 | 10.7 | Drugs & healthcare | yes |
| Mercury Protocol | 11.24.17 | 10.5 | Communications | yes |
| Devery | 1.19.18 | 10.4 | Privacy & security | yes |
| ZrCoin | 6.9.17 | 10.4 | Commodities | no |

Continued

| Name | Date ended | Total raised | Industry | Inv. sample |
|---|---|---|---|---|
| adbank | 1.6.18 | 10.4 | Commerce & advertising | no |
| doc.ai | 10.12.17 | 10.0 | Drugs & healthcare | no |
| XPA | 8.30.17 | 10.0 | Events & entertainment | no |
| Blockchain Capital | 5.10.17 | 10.0 | Finance | no |
| district0x | 8.1.17 | 9.8 | Marketplaces | no |
| Rialto | 7.4.17 | 9.7 | Trading & exchanges | yes |
| B2BX | 11.17.17 | 9.5 | Trading & exchanges | yes |
| TIES Network | 10.18.17 | 9.2 | Social networks | yes |
| Indorse | 9.7.17 | 9.2 | Social networks | no |
| Starbase | 11.24.17 | 9.0 | Finance | no |
| BitDice | 9.15.17 | 8.7 | Gambling | yes |
| CarTaxi Token | 10.31.17 | 8.7 | Marketplaces | no |
| EBCoin | 2.13.18 | 8.5 | Tourism | no |
| Inspeer | 2.5.18 | 8.5 | Finance | no |
| Tomocoin | 3.1.18 | 8.4 | Blockchain Infra. | yes |
| Golem | 11.13.16 | 8.3 | Cloud computing | yes |
| IP Exchange | 3.5.18 | 8.1 | Marketplaces | yes |
| Primalbase Token | 7.26.17 | 7.9 | Real estate | no |
| GUTS | 12.13.17 | 7.7 | Events & entertainment | yes |
| NVO | 6.27.17 | 7.6 | Trading & exchanges | no |
| iXledger | 7.13.17 | 7.6 | Finance | yes |
| CrowdWiz | 1.31.18 | 7.2 | Finance | no |
| Peerplays | 5.14.17 | 7.2 | Gambling | no |
| TaaS | 4.27.17 | 7.2 | Trading & exchanges | no |
| Aditus | 12.20.17 | 7.1 | Commerce & advertising | yes |
| BlockCAT | 8.18.17 | 7.0 | Payments | no |
| Blocktix | 7.28.17 | 7.0 | Events & entertainment | yes |
| ATLANT | 10.31.17 | 7.0 | Real estate | yes |
| Datum | 11.29.17 | 6.8 | Commerce & advertising | yes |
| Hubii Network | 9.8.17 | 6.6 | Content mgmt. | no |
| Oxycoin | 10.1.17 | 6.3 | Finance | no |
| Lisk | 3.21.16 | 6.3 | Blockchain Infra. | no |
| Opus | 8.24.17 | 5.8 | Content mgmt. | yes |
| Sociall | 9.15.17 | 5.7 | Social networks | no |
| Matchpool | 4.4.17 | 5.6 | Social networks | no |
| DigixDAO | 3.30.16 | 5.5 | Finance | no |
| FirstBlood | 9.26.16 | 5.5 | Video games & VR | no |
| Synereo | 10.18.16 | 5.4 | Social networks | no |
| Chronobank | 2.14.17 | 5.4 | Marketplaces | yes |
| TrueFlip | 7.27.17 | 5.4 | Gambling | no |
| Musiconomi | 9.28.17 | 5.3 | Content mgmt. | yes |
| Exscudo | 5.31.17 | 5.3 | Payments | no |
| Betmaster | 12.31.17 | 5.2 | Gambling | no |

Continued

| Name | Date ended | Total raised | Industry | Inv. sample |
|---|---|---|---|---|
| DCORP | 6.29.17 | 5.1 | Finance | no |
| SportyCo | 12.10.17 | 5.1 | Finance | yes |
| WeTrust | 4.12.17 | 5.0 | Finance | yes |
| Flixxo | 11.23.17 | 4.7 | Content mgmt. | yes |
| Sharpe Capital | 2.5.18 | 4.2 | Finance | yes |
| DECENT | 11.6.16 | 4.2 | Content mgmt. | no |
| Starta | 7.5.17 | 4.1 | Finance | no |
| Hacken | 11.30.17 | 4.1 | Privacy & security | no |
| MiniApps | 12.19.17 | 3.9 | Blockchain Infra. | yes |
| Aigang | 12.15.17 | 3.9 | Insurance | no |
| ALIS | 9.29.17 | 3.8 | Social networks | no |
| Suretly | 8.11.17 | 3.5 | Finance | no |
| Lunyr | 4.28.17 | 3.4 | Content mgmt. | yes |
| Divi | 11.25.17 | 3.3 | Payments | no |
| Humaniq | 4.26.17 | 3.2 | Finance | no |
| SRG | 1.15.18 | 3.2 | Commerce & advertising | no |
| Proof Suite | 12.1.17 | 3.1 | Finance | no |
| Kibo Lotto | 11.9.16 | 3.1 | Gambling | no |
| Mirocana | 12.19.17 | 3.0 | Finance | no |
| Ethereum Movie Venture | 5.15.17 | 3.0 | Events & entertainment | yes |
| Privatix | 11.16.17 | 2.9 | Communications | yes |
| Melonport | 2.15.17 | 2.9 | Finance | no |
| SkinCoin | 7.21.17 | 2.8 | Video games & VR | yes |
| Lykke | 10.10.16 | 2.8 | Trading & exchanges | no |
| CryptoPing | 6.25.17 | 2.6 | Trading & exchanges | no |
| Genie | 2.28.18 | 2.5 | Finance | no |
| Sola | 12.25.17 | 2.2 | Social networks | no |
| Bounty0x | 12.16.17 | 1.9 | Search | no |
| vSlice | 12.12.16 | 1.8 | Gambling | no |
| Wings | 1.6.17 | 1.8 | Finance | no |
| SunContract | 7.25.17 | 1.8 | Energy & utilities | yes |
| Blockpool | 6.30.17 | 1.8 | Blockchain Infra. | no |
| Smart Investment Fund Token | 9.15.17 | 1.7 | Finance | yes |
| FundYourselfNow | 7.31.17 | 1.6 | Finance | no |
| FidentiaX | 12.6.17 | 1.6 | Insurance | yes |
| Incent | 11.30.16 | 1.4 | Commerce & advertising | no |
| Ethbits | 5.13.17 | 1.3 | Trading & exchanges | yes |
| Databits | 2.28.17 | 1.1 | Video games & VR | no |
| Adelphoi | 5.31.17 | 1.0 | Finance | no |
| CommerceBlock | 12.19.17 | n/a | Finance | no |
| EncryptoTel | 5.11.17 | n/a | Communications | no |
| ATBCoin | 7.12.17 | n/a | Payments | no |

Continued

| Name | Date ended | Total raised | Industry | Inv. sample |
|---|---|---|---|---|
| LATOKEN | 10.10.17 | n/a | Finance | no |
| Paragon | 10.15.17 | n/a | Real estate | no |
| DAO.Casino | 7.21.17 | n/a | Gambling | no |
| Bitquence | 7.16.17 | n/a | Finance | no |
| REALT | 10.31.17 | n/a | Commerce & advertising | no |
| Vezt | 12.3.17 | n/a | Art & music | no |
| Genaro Network | 11.30.17 | n/a | Blockchain Infra. | no |
| ChainLink | 9.19.17 | n/a | Blockchain Infra. | no |
| Time New Bank | 11.24.17 | n/a | Finance | no |
| Pally | 12.13.17 | n/a | Tourism | no |
| CoinStarter | 2.17.18 | n/a | Finance | no |
| Nimiq | 7.28.17 | n/a | Blockchain Infra. | no |
| Cobinhood | 10.22.17 | n/a | Trading & exchanges | no |
| MyBit Token | 8.26.17 | n/a | Finance | no |
| Storj | 6.7.17 | n/a | Data storage | yes |
| ICOS | 9.15.17 | n/a | Finance | no |
| MediBloc | 12.15.17 | n/a | Drugs & healthcare | no |
| Iungo | 1.31.18 | n/a | Communications | no |
| InvestFeed | 8.7.17 | n/a | Trading & exchanges | no |
| Ecobit | 6.15.17 | n/a | Agriculture | no |
| Hive | 7.31.17 | n/a | Finance | no |
| Block Array | 1.8.18 | n/a | Data analytics | yes |
| Chaintrade | 12.16.17 | n/a | Commodities | no |
| QASH | 11.8.17 | n/a | Finance | no |
| Viberate | 9.3.17 | n/a | Art & music | yes |
| Cashaa | 12.6.17 | n/a | Finance | no |
| Achain | 7.7.17 | n/a | Blockchain Infra. | no |
| bitJob | 10.12.17 | n/a | Marketplaces | no |
| DreamTeam | 12.14.17 | n/a | Video games & VR | no |
| LeadCoin | 3.1.18 | n/a | Commerce & advertising | no |
| Giga Watt Token | 7.31.17 | n/a | Crypto mining | no |
| Matryx | 11.20.17 | n/a | Video games & VR | no |
| CoinDash | 7.17.17 | n/a | Trading & exchanges | no |
| iDice | 6.26.17 | n/a | Gambling | no |
| Rentberry | 2.28.18 | n/a | Real estate | no |
| Fusion | 2.10.18 | n/a | Finance | yes |
| HADE | 1.26.18 | n/a | Data analytics | no |
| adToken | 6.26.17 | n/a | Commerce & advertising | no |
| Request Network | 10.15.17 | n/a | Payments | no |
| CyberMiles | 12.3.17 | n/a | Commerce & advertising | no |
| SwissBorg | 1.10.18 | n/a | Finance | no |
| Protos | 12.15.17 | n/a | Content mgmt. | no |
| EncrypGen | 7.18.17 | n/a | Drugs & healthcare | no |

Continued

| Name | Date ended | Total raised | Industry | Inv. sample |
|---|---|---|---|---|
| SingularDTV | 10.29.16 | n/a | Events & entertainment | no |
| Banca | 2.26.18 | n/a | Marketplaces | no |
| Dovu | 10.17.17 | n/a | Marketplaces | yes |
| Leverj | 12.6.17 | n/a | Trading & exchanges | no |
| Loopring | 8.16.17 | n/a | Trading & exchanges | no |
| Universa | 12.8.17 | n/a | Blockchain Infra. | no |
| Gameflip | 1.29.18 | n/a | Video games & VR | no |
| Publica | 12.1.17 | n/a | Content mgmt. | no |
| Agrello | 8.17.17 | n/a | Legal | no |
| Lamden | 1.4.18 | n/a | Blockchain Infra. | no |
| Telcoin | 12.30.17 | n/a | Payments | no |
| LOCIcoin | 12.31.17 | n/a | Search | no |
| Nitro | 12.26.17 | n/a | Video games & VR | no |
| Gifto | 12.14.17 | n/a | Payments | yes |
| AirSwap | 10.10.17 | n/a | Trading & exchanges | no |
| Corion Platform | 8.27.17 | n/a | Blockchain Infra. | no |
| Playkey | 11.30.17 | n/a | Video games & VR | no |
| Veritaseum | 5.26.17 | n/a | Finance | no |
| Crederoom | 11.13.17 | n/a | Finance | no |
| Propy | 9.15.17 | n/a | Real estate | no |
| DomRaider | 10.9.17 | n/a | Payments | no |
| Komodo | 11.20.16 | n/a | Blockchain Infra. | no |
| Tokenbox | 11.28.17 | n/a | Finance | no |
| Seratio Project | 10.31.17 | n/a | Charity | no |
| Chronologic | 9.4.17 | n/a | Blockchain Infra. | no |
| CanYa | 12.27.17 | n/a | Marketplaces | no |
| Sphre AIR | 6.30.17 | n/a | Identity & reputation | no |
| Moria | 2.25.18 | n/a | Commodities | no |
| Clout | 12.17.17 | n/a | Media | no |
| Aeron | 10.30.17 | n/a | Tourism | no |
| Mothership | 7.28.17 | n/a | Trading & exchanges | yes |
| ZenGold | 5.26.17 | n/a | Commodities | no |

## B.2 Definition of variables

Table B.2.2 – Definition of variables

| Variable name | Type | Definition | Source(s) |
|---|---|---|---|
| Amount raised in crowdsale | Continuous | Total amount of funds (in US dollars) raised during the ICO's crowdsale stage. Where possible, the total is calculated by multiplying the amounts of cryptocurrencies received by their closing price on the last day of the ICO. Where amounts in cryptocurrency are unavailable, the US dollar figures disclosed by the ICO's promoter are used. If the ICO conducts a presale without any effective restrictions (such as participation by invitation only, or a minimum investment requirement above USD 5,000) on participants, funds raised during the presale are counted towards the crowdsale. | Company website, ICO documentation, social media |
| Amount raised in presale | Continuous | Total amount of funds (in US dollars) raised during the ICO's presale stage. Where possible, the total is calculated by multiplying the amounts of cryptocurrencies received by their closing price on the last day of the ICO. Where amounts in cryptocurrency are unavailable, the US dollar figures disclosed by the ICO's promoter are used. | Company website, ICO documentation, social media |
| Business model available | Indicator | The documentation details the market opportunity the product financed by the ICO addresses and lays out how the company will eventually earn money. | Company website, ICO documentation, social media |
| Celebrity endorsement | Indicator | The ICO is being promoted by a popular entertainment or sports personality on social media. | Company website, social media |
| Crowdsale is auction | Indicator | The token price for crowdsale investors depends on the total amount of funds raised during the crowdsale. | Company website, ICO documentation, social media |
| Crowdsale max. discount | Continuous | Maximum discount given to (usually large or early) investors during the crowdsale stage. Calculated as Crowdsale max. discount = (maximum crowdsale price - minimum crowdsale price)/maximum crowdsale price | Company website, ICO documentation, social media |

Continued

| Variable name | Type | Definition | Source(s) |
|---|---|---|---|
| Development road map available | Indicator | The documentation contains a road map with dates and milestones for the development and commercialization of the product. | Company website, ICO documentation, social media |
| Experienced team | Indicator | The founding team has an average of at least ten years of experience in technology, management or entrepreneurship. | Company website, ICO documentation, social media |
| Financial advisor disclosed | Indicator | The financial/blockchain expert (either a company or an individual) who advised the company in arranging its ICO is disclosed. | Company website, ICO documentation, social media |
| Funding milestones | Indicator | The terms of funding lay out binding milestones (e.g. development of a working prototype) that need to be met in order for the funds raised in the ICO to be released to the firm. | Company website, ICO documentation, social media |
| Fundraiser has maximum ('hard cap') | Indicator | There is a maximum number of tokens the company will sell in its ICO. | Company website, ICO documentation, social media |
| Fundraiser has minimum ('soft cap') | Indicator | There is a minimum number of tokens to be sold or money to be raised for the ICO to be considered a success. | Company website, ICO documentation, social media |
| Has a presale | Indicator | The ICO has a dedicated presale stage reserved for large investors. Zero if the presale has no minimum investment requirement. | Company website, ICO documentation, social media |
| Has VC backing | Indicator | The company has received funding from a venture capitalist, in exchange for an equity stake or tokens, prior or during the ICO. | Company website, ICO documentation, social media, Crunchbase |
| High quality advisory team | Indicator | Advisory team is of high quality, i.e. mostly composed of individuals with significant experience as entrepreneurs, executives, venture investors or academics. | Company website, ICO documentation, social media |
| Independent custodian for ICO funds | Indicator | The funds raised in the ICO are held by an independent third party, e.g. a Swiss foundation where the majority of the foundation board is composed of individuals not presently in a business relationship with the promoter of the ICO. | Company website, ICO documentation, social media, commercial registers |

Continued

| Variable name | Type | Definition | Source(s) |
|---|---|---|---|
| Investors from other jurisdictions excluded | Indicator | Investors from jurisdictions other than the US are not allowed to participate in the ICO (most commonly countries that have banned ICOs such as China and South Korea and countries on the OFAC sanctions list). | Company website, ICO documentation, social media |
| Investors have governance rights | Indicator | Token holders have a right to vote on investment, business or governance decisions. Includes advisory votes. | Company website, ICO documentation, social media |
| Is a security | Indicator | The token likely qualifies as a financial security. Most commonly because it pays interest or dividends, because the issuing firm commits to buybacks using the firm's net income or because the token represents a physical asset or a share in an investment fund. | Company website, ICO documentation |
| Is a utility token | Indicator | The token is intended to be used primarily for consumption of a product or services and does not generate cash distributions to holders. | Company website, ICO documentation |
| Is cryptographic token | Indicator | The ICO takes the form of a smart contract on an existing blockchain (e.g. Ethereum, Waves, Qtum, Nxt). | Company website, ICO documentation |
| Is currency or general purpose blockchain | | The token is intended to be used primarily as a currency, replacing traditional fiat money, or as the unit of account for a new general purpose blockchain able to execute smart contracts. | Company website, ICO documentation |
| Issuer has customers for product | Indicator | The product or service underlying the ICO has users (regardless of whether they pay for the service or not). | Company website, ICO documentation, social media |
| KYC/AML procedure | Indicator | The ICO's promoter required participants to identify themselves by submitting personal documents such as a passport copy, utility bills, etc. | Company website, ICO documentation, social media |
| Legal advisor disclosed | Indicator | The legal expert (either a company or an individual) who advised the company in arranging its ICO is disclosed. | Company website, ICO documentation, social media |
| Legal form and jurisdiction known | Indicator | Type of legal entity (e.g. limited liability company or joint-stock corporation) and jurisdiction of incorporation of the entity conducting the ICO are disclosed. | Company website, ICO documentation, commercial registers |

Continued

| Variable name | Type | Definition | Source(s) |
|---|---|---|---|
| Legal form is foundation | Indicator | The issuing entity is a not-for-profit foundation (typically incorporated in Switzerland or Liechtenstein). | Company website, ICO documentation, commercial registers |
| Legal entity is corporation | Indicator | The issuing entity is a joint-stock corporation or its equivalent in non-US jurisdictions. | Company website, ICO documentation, commercial registers |
| Legal entity is LLC | Indicator | The issuing entity is a limited liability corporation (LLC) or limited liability partnership (LLP) or their equivalent in non-US jurisdictions. | Company website, ICO documentation, commercial registers |
| Length of ICO (calendar days, actual) | Discrete | Actual length of the crowdsale period in number of days. | Company website, ICO documentation, social media |
| Length of ICO (calendar days, planned) | Discrete | Planned maximum length of the crowdsale period in number of days. | Company website, ICO documentation, social media |
| Lock up period unsold tokens | Continuous | Weighted average of the period over which unsold tokens are locked up (i.e. cannot be sold). Equals zero if the tokens are not locked up. | Company website, ICO documentation, social media |
| Percentage of hard cap raised | Continuous | Fraction of the maximum amount the company manages to raise during its ICO. | Company website, ICO documentation, social media |
| Postal address known | Indicator | Physical postal address of the ICO promoter's headquarters is known. | Company website, ICO documentation, commercial registers |
| Presale discount | Continuous | Presale discount over the crowdsale "list price", based on original price quotes in cryptocurrencies where available for both presale and crowdsale, otherwise based on converted US dollar prices. Presale discount = (maximum crowdsale price – minimum presale price)/maximum crowdsale price | Company website, ICO documentation, social media |

Continued

| Variable name | Type | Definition | Source(s) |
|---|---|---|---|
| Presale lockup period (weighted avg.) | Continuous | Lockup period of tokens sold during the presale stage. Where tokens are subject to a vesting schedule or only part of the tokens is locked up, we track the weighted average maturity of all tokens sold at the presale stage. Where different fractions of presold tokens are subject to different lockup periods, but the size of those fractions is unclear, we calculate the weighted average maturity based on the minimum lockup period. | Company website, ICO documentation, social media |
| Presale tokens locked up | Indicator | Tokens sold during the presale stage cannot be sold for a certain period of time. | Company website, ICO documentation, social media |
| Product can be tried out | Indicator | Prospective investors can try the product or prototype. | Company website, ICO documentation, social media |
| Product or prototype developed | Indicator | The product for which funding is being raised or an early "alpha" or "beta" version of it has been developed. | Company website, ICO documentation, social media |
| Project code available | Indicator | The company provides original source code for the project it is raising money for on Github as of the first day of the ICO. | Github |
| Qualified investors only | Indicator | Only investors with accredited investor status or equivalent are allowed to participate in the ICO. | Company website, ICO documentation, social media |
| Registered in offshore financial center | Indicator | The jurisdiction of incorporation is an offshore financial center as per the definition of the International Monetary Fund. | Company website, ICO documentation, commercial registers |
| Simple agreement for future tokens (SAFT) | Indicator | The ICO employs a "Simple Agreement for Future Tokens" (SAFT) under which tokens are only issued once the platform on which they can be used has been released. | Company website, ICO documentation |
| Smart contract code available | Indicator | If the token sold during the ICO takes the form of a smart contract on another blockchain, is the source code for the smart contract available on Github prior to the ICO? | Github |
| Team business background missing | Indicator | Insufficient information to determine the value of the variable "team member with business background". | Company website, ICO documentation, social media |

Continued

| Variable name | Type | Definition | Source(s) |
|---|---|---|---|
| Team experience missing | Indicator | Insufficient information to determine the value of the variable “experienced team”. | Company website, ICO documentation, social media |
| Team lockup period (weighted avg.) | Continuous | Weighted average maturity of the tokens under control of the issuing company and the founding team. Includes all the tokens also included in “Token share team (ex ante)”. | Company website, ICO documentation, social media |
| Team member with business background | Indicator | At least one of the team members has significant experience in entrepreneurship, consulting or management. | Company website, ICO documentation, social media |
| Team size | Discrete | Number of full time team member at the time of the ICO, excluding advisors and contractors. | Company website, ICO documentation, social media |
| Team tokens locked up | Indicator | Some fraction of the tokens held by the issuing company and/or the founding team are subject to a vesting schedule. | Company website, ICO documentation, social media |
| Time to listing (calendar days) | Discrete | Number of days between the last day of the crowdsale period and the first day for which a closing price is listed on Coinmarketcap. | Company website, ICO documentation, social media, Coinmarketcap |
| Token share crowdsale investors (ex ante) | Continuous | Fraction of total token supply allocated to crowdsale investors following the crowdsale, assuming the crowdsale sells out. | Company website, ICO documentation, social media |
| Token share crowdsale investors (ex post) | Continuous | Fraction of tokens held by crowdsale investors after the crowdsale, after all tokens have been distributed and unsold tokens destroyed or allocated to the issuer. | Company website, ICO documentation, social media |
| Token share presale investors (ex ante) | Continuous | Fraction of total token supply allocated to presale investors following the crowdsale, assuming the crowdsale sells out. | Company website, ICO documentation, social media |
| Token share producers/miners (ex ante) | Continuous | Fraction of total token supply reserved for “miners” or producers on the platform following the crowdsale, assuming the crowdsale sells out. | Company website, ICO documentation, social media |

Continued

| Variable name | Type | Definition | Source(s) |
|---|---|---|---|
| Token share team (ex ante) | Continuous | Fraction of total token supply under control of the issuing firm and the founding team following the crowdsale, assuming the crowdsale sells out. Includes all tokens under the control of the firm, including tokens reserved promotional activities, “bounties” (compensation for promotional activities), compensation of suppliers, employees and advisors, and any other residual categories. | Company website, ICO documentation, social media |
| Token supply is fixed | Indicator | The total number of tokens stays fixed indefinitely, as opposed to tokens that allow for inflation or the creation of additional tokens under certain circumstances. | Company website, ICO documentation, social media |
| Total amount raised | Continuous | Total amount of funds (in US dollars) raised during the ICO. Includes funds raised during crowdsale and presale. Where possible, the total is calculated by multiplying the amounts of cryptocurrencies received by their closing price on the last day of the ICO. Where amounts in cryptocurrency are unavailable, the US dollar figures disclosed by the ICO's promoter are used. | Company website, ICO documentation, social media |
| Unknown or low quality advisors | Indicator | Advisory team is either unknown or of low quality (i.e. mostly composed of “crypto evangelists”, celebrities, or similar). | Company website, ICO documentation, social media |
| Unsold tokens 'burnt' or proportional allocation | Indicator | Unsold tokens are either destroyed or the token allocation is done proportionally (e.g. the team receives 20% of all tokens created following the crowdsale, regardless of its result). | Company website, ICO documentation, social media |
| Unsold tokens kept by issuer | Indicator | The issuer retains unsold tokens, either for future token sales or to be used for a different purpose. | Company website, ICO documentation, social media |
| US retail investors excluded | Indicator | Non-accredited investors from the United States are not allowed to participated in the ICO. | Company website, ICO documentation, social media |

Continued

| Variable name | Type | Definition | Source(s) |
|---|---|---|---|
| Use of proceeds disclosed in detail | Indicator | The issuer provides a detail breakdown for the use funds raised during the ICO (e.g. X software developers at Y dollars and hour are required to do Z hours of work to complete the product). | Company website, ICO documentation, social media |
| Use of proceeds mentioned | Indicator | The issuer provides a rough breakdown for the use of funds raised during the ICO (e.g. 40% product development, 10% legal, 50% marketing). | Company website, ICO documentation, social media |
| Utility token enables decentralization | Indicator | The funds raised in the ICO are used to develop a decentralized platform on which buyers and sellers of a particular service or product engage in market based interaction, as opposed to the company conducting the ICO being or becoming the sole provider of the service or product. | Company website, ICO documentation, social media |
| Whitepaper page count | Discrete | Number of pages in the white paper document. | ICO documentation |
| Years since foundation | Discrete | Years since the founding team started working on the project for which the ICO is being conducted. Where unavailable, the date of incorporation from the commercial register is used. Rounded to the nearest integer. | Company website, ICO documentation, social media, commercial registers |

## B.3 Additional descriptive statistics

Table B.3.3 – **Additional descriptive statistics**

The table shows additional summary statistics for a hand-collected sample of 306 ICOs that took place between March 2016 and March 2018. All variables are defined in Appendix B.2.

| Panel A: ICO attributes | | | | | | |
|---|---|---|---|---|---|---|
| | Mean | Median | Min | Max | SD | N |
| Is currency or general purpose blockchain | 0.16 | 0.00 | 0.00 | 1.00 | 0.36 | 306 |
| Is a utility token | 0.61 | 1.00 | 0.00 | 1.00 | 0.49 | 306 |
| Length of crowdsale (calendar days, actual) | 28.45 | 29.50 | 1.00 | 148.00 | 22.43 | 306 |
| Length of crowdsale (calendar days, planned) | 31.92 | 31.00 | 1.00 | 148.00 | 21.66 | 303 |
| Time to listing (calendar days) | 17.93 | 13.00 | -517.00 | 222.00 | 51.26 | 275 |
| Crowdsale is auction | 0.11 | 0.00 | 0.00 | 1.00 | 0.32 | 306 |
| Token supply is fixed | 0.89 | 1.00 | 0.00 | 1.00 | 0.31 | 306 |
| Token share crowdsale investors (ex post) | 0.42 | 0.40 | 0.00 | 1.00 | 0.27 | 227 |
| Unsold tokens kept by issuer | 0.45 | 0.00 | 0.00 | 1.00 | 0.50 | 306 |
| Lock up period unsold tokens (years) | 0.39 | 0.00 | 0.00 | 10.00 | 1.22 | 138 |
| Smart contract code available | 0.67 | 1.00 | 0.00 | 1.00 | 0.47 | 278 |
| Utility token enables decentralization | 0.75 | 1.00 | 0.00 | 1.00 | 0.44 | 186 |
| Financial advisor disclosed | 0.19 | 0.00 | 0.00 | 1.00 | 0.39 | 306 |
| Simple agreement for future tokens (SAFT) | 0.03 | 0.00 | 0.00 | 1.00 | 0.16 | 306 |
| **Panel B: Company attributes** | | | | | | |
| | Mean | Median | Min | Max | SD | N |
| Whitepaper page count | 30.54 | 27.00 | 0.00 | 127.00 | 17.40 | 302 |
| Business model available | 0.67 | 1.00 | 0.00 | 1.00 | 0.47 | 306 |
| Project code available | 0.29 | 0.00 | 0.00 | 1.00 | 0.45 | 306 |
| Development road map available | 0.79 | 1.00 | 0.00 | 1.00 | 0.41 | 306 |
| Issuer has customers for product | 0.20 | 0.00 | 0.00 | 1.00 | 0.40 | 306 |
| Use of proceeds disclosed in detail | 0.05 | 0.00 | 0.00 | 1.00 | 0.22 | 306 |
| Experienced team | 0.58 | 1.00 | 0.00 | 1.00 | 0.50 | 306 |
| Product can be tried out | 0.40 | 0.00 | 0.00 | 1.00 | 0.49 | 306 |
| Team size | 11.46 | 9.00 | 2.00 | 80.00 | 9.20 | 282 |
| Team member with business background | 0.56 | 1.00 | 0.00 | 1.00 | 0.50 | 306 |
| Years since foundation | 1.60 | 1.00 | 0.00 | 16.00 | 2.06 | 306 |
| Unknown or low quality advisors | 0.32 | 0.00 | 0.00 | 1.00 | 0.47 | 306 |
| Celebrity endorsement | 0.04 | 0.00 | 0.00 | 1.00 | 0.19 | 306 |
| Postal address known | 0.70 | 1.00 | 0.00 | 1.00 | 0.46 | 306 |
| Legal entity is foundation | 0.07 | 0.00 | 0.00 | 1.00 | 0.26 | 306 |

# C Appendix to "Information Intermediaries: How Commercial Bankers Facilitate Strategic Alliances"

## C.1 Anecdotal evidence on the role of bankers in brokering collaborations

We spoke to three individuals with first hand experience in how bankers broker strategic alliances. Our first interview was with a current commercial banker with more than 20 years of experience, who is employed at a major national lender on the U.S. West Coast and is also part of our dataset. This banker told us that miscellaneous consulting services to borrowers, such as pitching them potential collaboration partners, was an important part of building relationships with borrowers. She explained that her edge in brokering these alliances was two-fold. First, she had direct lines of communications to senior management at various companies. Approaching a potential collaboration partner is significantly harder when “cold calling” and an introduction through a common lender can significantly ease the process. Second, the banker explained that customers often needed very specific capabilities in collaboration partners, and it was not necessarily public knowledge which firms had them. Connections to a large number of firms allow bankers to directly point borrowers to a good fit, reducing the need to search for a suitable partner.

Our first interview partner then set us up to talk to one of her clients, the CFO of a medium sized U.S. corporation on the West Coast. This second interview partner stressed that brokering collaborations was an important aspect of relationship building with his banker, and that these types of consulting services were a precondition for a banker receiving lucrative mandates. He stressed the costs of finding collaboration partners in the presence of asymmetric information and how bankers can overcome these frictions.

Our third contact used to work as a banker in a large developing economy for a globally operating U.S. bank. This former banker told us that brokering relationship among clients was an important part of relationship building. He specifically mentioned a collaboration between his country’s railway operator and two major heavy industry corporations that he was involved in as an example of how bankers can broker collaboration even among large borrowers.

Additional evidence on the role of bankers in matchmaking can be found in the press. In 2016, PricewaterhouseCoopers (PwC) interviewed the CEO of Silicon Valley Bank, Greg Becker, for their CEO survey (PwC US, 2016). Becker stressed the matchmaking role as part of the value added his bank can provide to customers: “We are so concentrated in the target market we go after, our ability to make an introduction to another CEO that’s going through the same sort of challenges is higher than that of any other institution. Our ability to make introduction to a potential partnership–because we understand that business better than maybe one of our competitors would. The value added we give to our clients, whether it is making an introduction to a potential client or making an introduction to a potential partnership [...] Why is that so important for technology companies? The most important thing for technology companies is speed and execution.” An example of such matchmaking, in this case between customers and producers, is Silicon Valley bank’s brokering of both sales and takeovers between tech investors and Napa Valley wine makers (The Street, 2015). These public statements confirm similar information we received from market participants during our private conversations.

A 2016 article highlights the role bank matchmaking plays for connections across borders (China Daily, 2016). Chen Siqing, president of Bank of China (BOC, a commercial bank) argues that “pushing forward cooperation among Chinese and Central and Eastern European companies is a crucial step in BOC’s program”. One client interviewed for the article argued that “BOC helped us make a breakthrough by introducing us to our first overseas client.”

Finally, banks can also act as matchmakers between borrowers and strategic investors. In May 2019, Bank of America CEO Brian Moynihan made a contact between their borrower Occidental Petroleum Corp., Berkshire Hathaway Inc., and oil company Anadarko Petroleum Corp (Bloomberg, 2019). According to Buffet, the bank was crucial in making the introduction: “Last Friday, I got a call in the middle of the afternoon from Brian Moynihan, the CEO of Bank of America, and he said that they were involved in financing the Occidental deal and that the Occidental people would like to talk to me.”

## C.2 Additional description of the data

Table C.2.1 – Variable descriptions

| Variable name | Description |
|---|---|
| *Firm-pair characteristics* | |
| Previous alliances | Number of alliances the two firms have entered into collectively between the beginning of the sample period and the time of observation. |
| Same state | The headquarters of the two firms are located in the same state. |
| One unrated | Either one or both parties do not have a long-term issuer credit rating from S&P's, Moody's or Fitch. |
| One high intangibles | Either one or both parties to a strategic alliance have an intangibles-to-assets ratio in the top quintile. |
| *Bank loan related characteristics* | |
| Banker network distance | Minimum distance between the two firms' loan officers through the network, zero meaning both have the same loan officer. The measure has been winsorized from above at three. |
| Same bank | Both firms have taken out at least one loan from the same lead arranger/lead agent. |
| Same banker | Both firms have taken out a loan from the same banker. |
| Banker connection | The two firms are connected through the banker network (regardless of distance). |
| One has a syndicated loan | At least one party to a strategic alliance has borrowed in the syndicated loan market since the inception of electronic filing. |
| Both have a syndicated loan | Both parties to an alliance have borrowed in the syndicated loan market since the inception of electronic filing. |

## C.3 Additional results

Table C.3.1 – First difference model
The table displays estimates for firms' likelihood of entering a strategic alliance given whether and how closely they are connected through the network of bankers based on a first difference model. The unit of observation is a firm-pair-year and the dependent variable is the first difference in alliance status, i.e. an indicator variable equal to one if a certain firm-pair enters a strategic alliance during the year of observation. The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. *Same banker* is equal to one if the firm-pair has a banker in common. *Banker network distance* measures how many banker to banker connections are required to establish a connection between the two firms, zero indicating none. *Banker connection* is an indicator equal to one if the two firms are connected through the network of bankers. Parentheses contain t-statistics calculated from standard errors double clustered by firm one and firm two. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Δ Same banker | 0.0015*** | | | |
| | (2.74) | | | |
| Δ Banker network connection | | 0.0005*** | | |
| | | (2.58) | | |
| Δ Banker network distance | | | 0.0000 | |
| | | | (0.24) | |
| Δ (Distance = 0) | | | | 0.0018*** |
| | | | | (3.17) |
| Δ (Distance = 1) | | | | 0.0005** |
| | | | | (1.98) |
| Δ (Distance = 2) | | | | 0.0003** |
| | | | | (2.14) |
| Δ (Distance > 2) | | | | 0.0003 |
| | | | | (1.00) |
| Δ Same bank | 0.0003*** | 0.0003*** | 0.0008** | 0.0003*** |
| | (3.00) | (2.94) | (2.19) | (2.85) |
| Industry 1-year FE | Yes | Yes | Yes | Yes |
| Industry 2-year FE | Yes | Yes | Yes | Yes |
| N | 5,533,280 | 5,533,280 | 309,532 | 5,533,280 |
| $R^2$ | 0.0006 | 0.0006 | 0.0036 | 0.0006 |

Table C.3.2 – Linear probability model with time-phased network connections
The table displays estimates for firms' likelihood of entering a strategic alliance given whether and how closely they are connected through the network of bankers. The tests follow Table 18.2 but banker-to-firm, bank-to-firm and banker-to-banker connections require that at least one interaction between the parties took place *within the last five years.* The unit of observation is a firm-pair-year and the dependent variable is an indicator variable equal to one if a certain firm-pair has entered a strategic alliance before or during the year of observation. The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. *Same banker* is equal to one if the firm-pair has a banker in common. *Banker network distance* measures how many banker to banker connections are required to establish a connection between the two firms, zero indicating none. *Banker connection* is an indicator equal to one if the two firms are connected through the network of bankers. Parentheses contain t-statistics calculated from standard errors double clustered by firm one and firm two. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Same banker | 0.0017* | | | |
| | (1.76) | | | |
| Banker network connection | | 0.0012*** | | |
| | | (2.81) | | |
| Banker network distance | | | 0.0001 | |
| | | | (0.49) | |
| Distance = 0 | | | | 0.0022** |
| | | | | (2.10) |
| Distance = 1 | | | | 0.0015*** |
| | | | | (2.76) |
| Distance = 2 | | | | 0.0007** |
| | | | | (2.00) |
| Distance > 2 | | | | 0.0005 |
| | | | | (1.35) |
| Same bank | 0.0002 | 0.0001 | 0.0006 | 0.0001 |
| | (0.68) | (0.51) | (1.47) | (0.49) |
| Firm-pair FE | Yes | Yes | Yes | Yes |
| Industry 1-year FE | Yes | Yes | Yes | Yes |
| Industry 2-year FE | Yes | Yes | Yes | Yes |
| N | 6,370,758 | 6,370,758 | 189,307 | 6,370,758 |
| $R^2$ | 0.7443 | 0.7443 | 0.8684 | 0.7443 |

Table C.3.3 – Influence of banker networks on the formation of strategic alliances: matched-pairs OLS regression results

The table displays estimates for firms' likelihood of entering a strategic alliance given whether and how closely they are connected through the network of bankers. The unit of observation is a firm-pair-year and the dependent variable is an indicator variable equal to one if a certain firm-pair has entered a strategic alliance during the year of observation or earlier during the sample period. For each firm-pair that ever enters a strategic alliance, a pair of control firms is chosen and added to the sample. Control firms are selected by choosing the firm in the same industry group that, during the year in which the alliance is observed, minimizes the Mahalanobis-distance for the natural logarithm of sales, the natural logarithm of age, the ratio of intangibles to total assets and the market-to-book ratio between the original and the matched firm and that is not a member of the original firm-pair entering the alliance. The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. *Same banker* is equal to one if the firm-pair has a banker in common. *Banker network distance* measures how many banker to banker connections are required to establish a connection between the two firms, zero indicating none. *Banker connection* is an indicator equal to one if the two firms are connected through the network of bankers. Parentheses contain t-statistics calculated from standard errors clustered by firm one and firm two. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Same banker | 0.0373 | | | |
| | (1.52) | | | |
| Banker network connection | | 0.0965*** | | |
| | | (4.65) | | |
| Banker network distance | | | 0.0340 | |
| | | | (1.36) | |
| Distance = 0 | | | | 0.0887*** |
| | | | | (3.04) |
| Distance = 1 | | | | 0.0948*** |
| | | | | (4.05) |
| Distance = 2 | | | | 0.1086*** |
| | | | | (3.76) |
| Distance > 2 | | | | 0.0942 |
| | | | | (1.47) |
| Same bank | 0.0011 | -0.0053 | 0.0674** | -0.0051 |
| | (0.07) | (-0.32) | (2.15) | (-0.30) |
| Firm-pair FE | Yes | Yes | Yes | Yes |
| Industry 1-year FE | Yes | Yes | Yes | Yes |
| Industry 2-year FE | Yes | Yes | Yes | Yes |
| N | 43,946 | 43,946 | 5,605 | 43,946 |
| $R^2$ | 0.7073 | 0.7083 | 0.7971 | 0.7083 |

Table C.3.4 – Variable capacity sequenced conditional logit model with additional control variables
The table displays results from a maximum likelihood estimation of the variable capacity sequenced conditional logit model as the one displayed in Table 18.4 but controlling for additional firm-pair characteristics. The sample consists of all publicly listed non-financial US firms in Compustat that enter at least one strategic alliance between 2002 and 2013. The unit of observation is a firm-pair-year and the dependent variable is an indicator variable equal to one if a certain firm-pair has entered a strategic alliance during the year of observation. A firm's maximum alliance capacity is assumed to be unlimited. *Same banker* is equal to one if the firm-pair has a banker in common. *Banker network distance* measures how many banker to banker connections are required to establish a connection between the two firms, zero indicating none (i.e. the firms share the same banker). *Banker connection* is an indicator equal to one if the two firms are connected through the network of bankers. The (omitted) base category for the indicator variables in Column 4 is two firms *not* being connected through the network (i.e. infinite distance). Financial characteristics have been winsorized at the 2 and 98% level. Parentheses contain z-statistics. Industry-pair-year fixed effects are implicitly embedded in the conditional logit estimation procedure. *, ** and *** indicate statistical significance at the ten, five and one percent level, respectively.

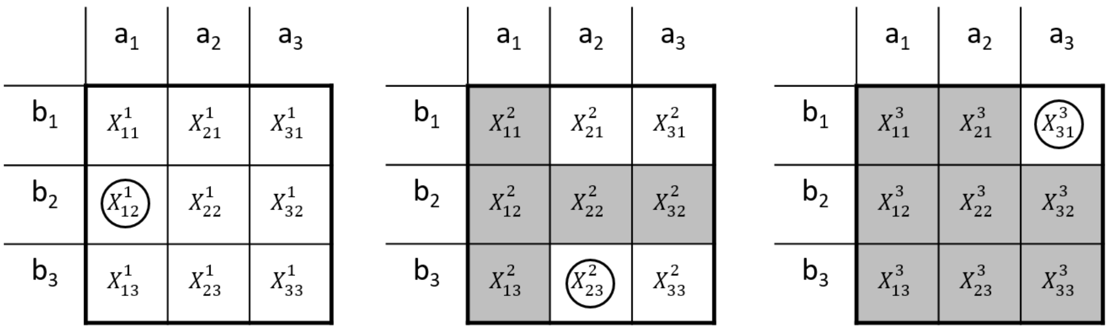
| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Same banker | 0.291** | | | |
| | (2.44) | | | |
| Banker network connection | | 0.238*** | | |
| | | (3.39) | | |
| Banker network distance | | | -0.052 | |
| | | | (-0.53) | |
| Distance = 0 | | | | 0.333*** |
| | | | | (2.77) |
| Distance = 1 | | | | 0.197** |
| | | | | (2.12) |
| Distance = 2 | | | | 0.253* |
| | | | | (1.69) |
| Distance > 2 | | | | 0.083 |
| | | | | (0.27) |
| Ln(total sales) | 0.296*** | 0.295*** | 0.284*** | 0.295*** |
| | (20.06) | (19.94) | (3.46) | (19.93) |
| Avg. tangibility ratio | 0.152 | 0.189 | -0.826 | 0.191 |
| | (0.90) | (1.12) | (-1.52) | (1.13) |
| Avg. market leverage | -1.019*** | -1.044*** | -0.981 | -1.042*** |
| | (-4.83) | (-4.94) | (-1.50) | (-4.93) |
| Same bank | -0.051 | -0.066 | -0.022 | -0.073 |
| | (-0.81) | (-1.06) | (-0.15) | (-1.15) |
| Same state | 0.371*** | 0.376*** | 0.450*** | 0.374*** |
| | (6.57) | (6.67) | (2.59) | (6.62) |
| Previous alliances | 0.014*** | 0.014*** | 0.013*** | 0.014*** |
| | (13.25) | (13.35) | (4.28) | (13.35) |
| N | 414,409 | 414,409 | 22,846 | 414,409 |
| Prob > $\chi^2$ | 0.000 | 0.000 | 0.000 | 0.000 |

## C.4 Sequenced conditional logit estimation example

This section illustrates the sequenced conditional logit model developed by Lindsey (2008) on an example. Substantial parts of this example are reproduced from the same source. In practice, the sequential structure is accounted for when forming the data panel and the same maximum likelihood estimation procedure as for a standard conditional logit model can be applied.

Assume there are two industries, $a$ and $b$, consisting of three firms ($a_i$ and $b_j$, where $i, j \in \{1,2,3\}$) each. Further, denote the firm-pair characteristics at time $t$ by $X_{ij}^t$ and assume we observe three alliances: $\{a_1, b_2\}$ at $t = 1$, $\{a_2, b_3\}$ at $t = 2$, and $\{a_3, b_1\}$ at $t = 3$.

The fixed capacity model assumes that firms could not have entered more alliances than we observe in the data. Figure C.4.1 illustrates the set of conditioning outcomes at each point in time for the fixed capacity model.



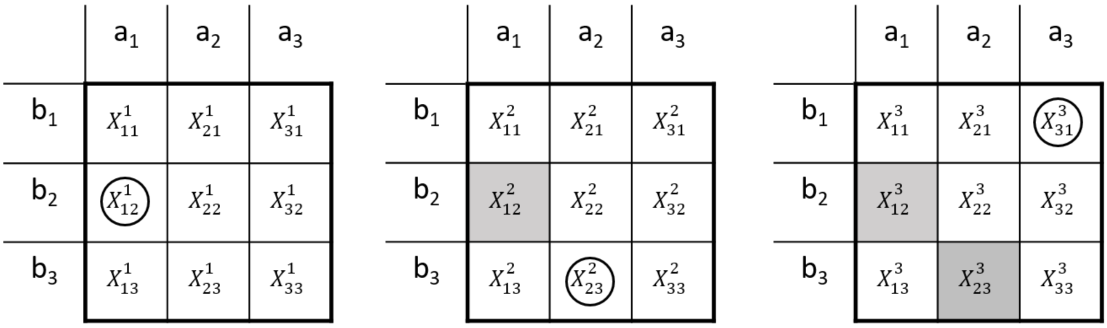

(a) $t = 1$ (b) $t = 2$ (c) $t = 3$

Figure C.4.1 – Fixed capacity model

The figure below illustrates the fixed capacity version of the sequenced conditional logit model developed by Lindsey (2008). Circles indicate realized alliances. Gray fields do not enter the estimation.

At $t = 1$, there are nine different alliances to choose from. The probability of observing $\{a_1, b_2\}$ is $\frac{e^{X_{12}^1\beta}}{\sum_{i=1}^3\sum_{j=1}^3 e^{X_{ij}^1\beta}}$. Because both $a_1$ and $b_2$ only enter one alliance each, both have reached their alliance capacity and are removed from the set of possible alliances at $t = 2$ and $t = 3$. Thus the probability of the observed combination $\{a_2, b_3\}$ at $t = 2$ is given by $\frac{e^{X_{23}^2\beta}}{e^{X_{21}^2\beta}+e^{X_{23}^2\beta}+e^{X_{31}^2\beta}+e^{X_{33}^2\beta}}$. Because $a_2$ and $b_3$ too have reached their alliance capacity, they are excluded from the set of possible alliances. At $t = 3$, only one possible alliance is left; its probability is equal to one regardless of the parameter vector $\beta$ and it does therefore not enter the estimation. The likelihood function $L^{ab}$ for industry-pair $\{a, b\}$ in the fixed capacity model is therefore given by

$$L^{ab} = \left(\frac{e^{X_{12}^1\beta}}{\sum_{i=1}^3\sum_{j=1}^3 e^{X_{ij}^1\beta}}\right)\left(\frac{e^{X_{23}^2\beta}}{e^{X_{21}^2\beta}+e^{X_{23}^2\beta}+e^{X_{31}^2\beta}+e^{X_{33}^2\beta}}\right) \tag{C.1}$$




(a) $t = 1$ (b) $t = 2$ (c) $t = 3$

Figure C.4.2 – Variable capacity model

The figure below illustrates the fixvariableed capacity version of the sequenced conditional logit model developed by Lindsey (2008). Circles indicate realized alliances. Gray fields do not enter the estimation.

In the variable capacity model, it is assumed that firms can enter any number of alliances. Hence only firm-pairs that have realized as alliances are removed from the estimation in subsequent periods. Figure C.4.2 illustrates the set of conditioning outcomes at each point in time for the variable capacity model on the same two-industry, six-firm example as above. This time, the likelihood function $L^{ab}$ for industry-pair $\{a, b\}$ is given by

$$L^{ab} = \left(\frac{e^{X_{12}^1\beta}}{\sum_{i=1}^3\sum_{j=1}^3 e^{X_{ij}^1\beta}}\right)\left(\frac{e^{X_{23}^2\beta}}{\sum_{i=1}^3\sum_{j=1}^3 e^{X_{ij}^2\beta} - e^{X_{12}^2\beta}}\right)\left(\frac{e^{X_{31}^3\beta}}{\sum_{i=1}^3\sum_{j=1}^3 e^{X_{ij}^3\beta} - e^{X_{12}^3\beta} - e^{X_{23}^3\beta}}\right) \tag{C.2}$$

Now assume we add a second pair of industries $\{c, d\}$ to the estimation, and there are no alliances between firms in industries $a$ and $b$ and firms in either industry $c$ or $d$. In both the fixed and the variable capacity model, calculating the overall likelihood is then just a matter of multiplying the likelihood $L^{ab}$ for industry-pair $\{a, b\}$ with the likelihood $L^{cd}$ of industry-pair $\{c, d\}$.

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Zetzsche, D. A., Buckley, R. P., Arner, D. W., and Föhr, L. (2017). The ICO gold rush: it's a scam, it's a bubble, it's a super challenge for regulators. Working paper, University of Luxembourg.

# Curriculum Vitae

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## Education

2019 Ph.D. in Finance, Ecole Polytechnique Fédérale de Lausanne (EPFL) and Swiss Finance Institute, Switzerland

2015 M.Sc. in Finance and Investments, summa cum laude, Erasmus University Rotterdam, Rotterdam School of Management, Netherlands

2014 B.A. in Business Administration, University of St. Gallen, Switzerland

## Research Interests

Corporate governance, financial intermediation

## Publications

1. Frattaroli, Marc, 2017. Does protectionist anti-takeover legislation lead to managerial entrenchment? *Journal of Financial Economics, forthcoming.*

## Working Papers

2. Frattaroli, Marc and Christoph Herpfer, 2018. Information intermediaries: How commercial bankers facilitate strategic alliances.
3. Fahlenbrach, Rüdiger and Marc Frattaroli, 2019. ICO investors.

## Seminar and Conference Presentations

Paper presented in parentheses. Asterisks indicate presentations by coauthors. Includes scheduled presentations.

2019 Mid-Atlantic Research Conference in Finance (2*), SGF (2), MoFiR Workshop on Banking (2*), Annual Private Capital Conference (discussant), FMA (2*)

2018 SGF (1), SFI Research Days (2), Executive Compensation Conference at Erasmus University (1), FMA European Conference (1), Private Markets Research Conference Poster Session (3), Irish Academy of Finance (2*), UNIL Brown Bag Seminar (3), Meeting of the German Economists Abroad (2*)

2017 UNIL Brown Bag Seminar (1)

## Media Mentions

Harvard Law School Forum on Corporate Governance and Financial Regulation, March 27, 2019.

Machine Lawyering, Centre for Financial Regulation and Economic Development at CUHK, August 7, 2019.

## Teaching Experience

Teaching Assistant, Venture Capital, Master in Financial Engineering at EPFL, 2016 - present

## Honors, Awards and Grants

American Finance Association, Doctoral Student Travel Grant, 2019

Swiss Finance Institute, Graduate Student Fellowship, 2015-2016

## Selected Other Work Experience

Deloitte, Internship in Corporate Finance, 2014

Accenture, Internship in Technology Solutions, 2013

*AXA Winterthur*, Internship in Business Development P&C, 2011

*Switzerland Global Enterprise*, Internship, 2009-2010

## Other Information

Language skills: German (native), English (fluent), French (intermediate), Italian (basic)

Programming skills: Stata, Matlab, Python, SQL, VBA, R

Citizenship: Swiss

CFA Charterholder