



Essays on Empirical Financial Accounting

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To Ghislaine, the love of my life

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Résumé

Les dirigeants d'entreprise sont des acteurs cruciaux du système économique. En effet, les dirigeants d'entreprises cotées sur les marchés financiers prennent constamment des décisions. Leur rôle consiste à fixer la stratégie de leur entreprise : sur quels marchés l'entreprise doit-elle décider de se lancer ? Comment se différencier de son concurrent ? Quelles informations l'entreprise doit-elle divulguer aux marchés financiers ? Le dirigeant décide, fixe les budgets, alimente des partenariats stratégiques et embauche une équipe pour l'épauler dans ses missions. Les décisions prises par les entreprises cotées constituent un enjeu important puisqu'elles ont des répercussions sur les clients, les fournisseurs, les employés de l'entreprise, et plus généralement la société. Les choix effectués par les dirigeants s'expliquent en partie par les choix passés de l'entreprise et la culture qui y règne. Toutefois, des travaux de recherches récents ont démontré l'importance du rôle des dirigeants pour expliquer une part importante de la variation observée en termes d'investissement, de structure de capital et de choix organisationnels (Bertrand and Schoar, 2003).

Comment les managers prennent leurs décisions ? La théorie économique part du principe que les agents sont rationnels et agissent en fonction de leurs intérêts personnels. Plus précisément, ils prennent des décisions qui maximisent leur utilité, même si cela peut conduire à des situations sous-optimales pour d'autres agents économiques. Dans cette thèse, j'étudie les choix de dirigeants d'entreprises cotées dans plusieurs contextes : décisions d'investissement, choix de stratégie de divulgation d'information et enfin de stratégie de ventes d'actions en nom propre. En

particulier, je vise à améliorer notre connaissance des facteurs externes aux dirigeants, comme des changements de lois, qui modifient leur choix dans ces contextes différents.

Le problème majeur des entreprises cotées à l'heure actuelle vient de la séparation entre les dirigeants et l'actionnariat de l'entreprise. En effet, les dirigeants sont engagés par les actionnaires pour prendre des décisions dans leur intérêt en vue de maximiser la valeur de l'entreprise. Toutefois, les dirigeants ne détiennent en général qu'une très faible proportion du capital des entreprises qu'ils dirigent. De ce fait, ils peuvent être tentés de faire des choix qui leur bénéficient mais qui ne maximisent pas la valeur de l'entreprise. Par exemple, ils peuvent surinvestir dans des projets destructeurs de valeur pour accroître la taille de l'entreprise qu'ils dirigent. Ils peuvent aussi manipuler les dépenses discrétionnaires de l'entreprise pour accroître la rentabilité de court terme au détriment de la valeur de long terme. Enfin, les dirigeants peuvent vendre des actions pour accroître leur richesse en utilisant l'information privée qu'il possède au sujet de l'entreprise qu'ils dirigent. S'ils sont poursuivis en justice, cela réduira leur capacité à diriger. Tous ces exemples tirés des travaux de ma thèse sont situés dans un contexte théorique de la théorie de l'agence où il existe de l'asymétrie d'information entre des principaux et des agents. Ce genre de situation où les dirigeants peuvent se comporter de manière sous-optimale naît de l'absence d'alignement systématique entre les intérêts des dirigeants et ceux des actionnaires.

Un autre concept économique fondamental est celui d'incitations économiques auxquelles répondent les agents économiques. Plus précisément, les incitations correspondent aux manières utilisables pour motiver les agents à agir d'une manière souhaitée. Sans ces incitations, les dirigeants dirigeraient leur compagnie de manière à maximiser leur intérêt personnel et non nécessairement en maximisant la valeur de l'entreprise pour les actionnaires. Ce problème central en gouvernance a été étudié abondamment dans la littérature en sciences économiques. Shleifer and Vishny (1997) présente un résumé de ce champ de recherche.

Dans cette thèse, je ne prétends pas démontrer l'existence des problèmes d'agence. Je me

concentre sur les incitations qui affectent le processus de prise de décision des dirigeants concernant différentes situations. Plus précisément, j'étudie comment différentes formes d'institutions affectent la prise de décision des dirigeants, et si cela se fait au détriment ou au bénéfice des actionnaires ou du système économique en général. De manière générale, par institutions j'entends mécanismes permettant de faciliter les interactions entre les agents économiques. Dans ses travaux fondateurs, North (1991) définit les institutions comme des "contraintes créées par l'homme qui structurent les interactions politiques, économiques et sociales".

Les institutions sont généralement façonnées par les législateurs. Dans le cas des sociétés cotées, les institutions sous la forme de réglementation a considérablement augmenté ces dernières années en réponse à divers scandales financiers et aux lacunes perçues lors des crises financières à travers le monde. Par exemple, les entreprises doivent se conformer aux nombreuses exigences de divulgation financière périodiques et expliquer précisément comment elles rémunèrent leurs dirigeants. Ils doivent aussi obtenir l'approbation des organismes de réglementation dans les transactions qui peuvent affecter la compétitivité d'un secteur économique, et se conformer aux règles sur la façon de passer des contrats et traiter avec des agents tant au sein de leur entreprise (par exemple, les employés) qu'à l'extérieur de leur entreprise (par exemple, les fournisseurs).

Le choix premier auquel doit faire face un régulateur est la décision ou non de réglementer dans un contexte donné, afin de créer une institution qui affecte les incitations des dirigeants d'entreprise. Comme beaucoup l'ont souligné, le simple fait que certaines formes d'institutions peuvent avoir des avantages pour certaines catégories d'agents ne suffit pas à justifier l'existence de la réglementation. Par exemple, prenons le cas d'un règlement qui impose des coûts importants aux entreprises quand ils veulent licencier des employés. Cette réglementation sera bénéfique pour les employés actuels. Toutefois, cela peut aussi affecter les futures décisions d'embauche des entreprises et être préjudiciable à l'ensemble de la population active en général. En fait, l'histoire économique a démontré que les économies ont souvent des institutions

qui sont inefficaces, mais qui sont persistantes dans le temps (North, 1991). Dans cette thèse, composée de trois chapitres distincts, j'examine si et comment plusieurs formes d'institutions affectent la prise de décision des dirigeants d'entreprise. Je ne tente pas de fournir des conclusions normatives quant à savoir si certaines règles sont «bonnes» ou «mauvaises» et si elles doivent être promues ou supprimées. Je reconnais que des conclusions normatives ne peuvent être faites qu'une fois que les effets d'une institution donnée ont été quantifiés de manière exhaustive.

Pour évaluer scientifiquement l'impact des institutions sur les décisions des dirigeants, je positionne les travaux de ma thèse dans le cadre théorique du droit économique positif. L'approche de l'économie du droit se réfère à l'application de l'analyse microéconomique à des problèmes juridiques afin d'évaluer leur efficacité économique. Plus précisément, je cherche constamment à identifier comment des caractéristiques légale affectent les dirigeants en utilisant divers outils économétriques qui permettent d'évaluer des plausibles relations de causalité. Pour ce faire, je développe ou emprunte des instruments d'études précédentes qui correspondent à des sources plausibles de variation exogène relative à la force des institutions afin d'enquêter pour savoir si (1) les dirigeants réagissent à ces changements et (2) si cela a des conséquences positives ou négatives sur d'autres parties.

Dans cette thèse, je cherche à contribuer aux débats sur les diverses formes d'institutions en mettant en lumière des conséquences inattendues à ces réglementations sur la prise de décision de l'entreprise. Par exemple, j'examine si le droit qui régit les contrats de travail affecte également les gestionnaires des décisions de manipulations comptables. J'étudie également si le système de recours collectifs aux Etats-Unis, actuellement considéré comme un fardeau du système judiciaire pour les entreprises cotées peut, dans certaines circonstances, de réduire les problèmes d'agence liés aux décisions d'investissement. Dans l'ensemble, mes différents chapitres sont utiles pour évaluer les effets des institutions sur la prise de décision des dirigeants et potentiellement utile aux organismes de réglementation en identifiant les conséquences jusque-là inexplorés des

institutions qui doivent être intégrées pour évaluer l'effet global de ces institutions sur le système économique.

Chapitre 1 : Les recours collectifs sur les marchés financiers comme instrument de gouvernance

Le premier chapitre de cette thèse est le résultat d'une collaboration avec François Brochet et Sven Michael Spira. Dans cet essai, nous examinons si le risque de poursuites collectives sur les marchés financiers constitue un outil de gouvernance pour réduire les coûts de l'agence à l'égard des décisions d'investissement des dirigeants.

D'une manière générale, l'action de groupe représente un mécanisme juridique par lequel un groupe de plaignants poursuit une tierce partie. Dans le cas de poursuites judiciaires sur les marchés financiers, les investisseurs poursuivent l'entreprise et ses dirigeants pour des actes répréhensibles. En conséquence, il est possible que les actions de groupe constituent un mécanisme qui peut affecter la prise de décision des dirigeants et veiller à ce que leurs intérêts soient alignés sur ceux de leurs investisseurs. Ces poursuites sont un élément clé du système juridique des pays issue d'une tradition dite de «common law». Cependant, les critiques font valoir que les membres du groupe de plaignants ne reçoivent souvent peu ou pas d'avantages de ces actions en justice et alors que les avocats sont les seuls bénéficiaires du système juridique. Dans le contexte de marchés financiers, certains spécialistes affirment même que les actions collectives constituent un fardeau concernant l'attractivité des bourses américaines qui pourraient aller jusqu'à expliquer certains des choix de suppression de cotation par des entreprises étrangères dans les années 2000 aux Etats-Unis (Zingales, 2006). Bien que des études antérieures documentent que les poursuites collectives affectent les dirigeants et notamment leur réputation (Brochet and Srinivasan, 2014), notre étude est la première à essayer de quantifier l'effet général des risques de poursuites judiciaires sur les décisions d'investissement.

Fait intéressant, plusieurs pays européens avec une tradition de droit civil ont récemment

changé leur droit en vue d'introduire certaines formes de recours collectifs. Par exemple, la France a adopté une loi en 2012 qui a introduit des recours collectifs pour les actions liées à la consommation. Ainsi, il est important de comprendre si de tels recours peuvent aider à améliorer le comportement des agents économiques avant de promouvoir et/ou d'introduire d'autres formes supplémentaires de recours collectifs dans d'autres pays.

Dans cette étude, nous utilisons des recours collectifs relatifs à des actions de fusions-acquisitions comme source de variation dans le risque juridique perçu par les dirigeants d'entreprise dans le même secteur d'activité. Notre échantillon est composé de recours juridiques où l'entreprise et ses dirigeants sont poursuivis pour avoir mené des fusions destructrices de valeur et avoir menti de la performance de la nouvelle entité après l'acquisition.

Nous établissons trois séries de résultats. Tout d'abord, nous ne trouvons que les marchés financiers réagissent mieux aux annonces de fusions dans l'industrie après un recours collectif, ce qui se traduit par des rendements supérieurs. Ensuite, ils choisissent des méthodes de paiement plus appropriées compte tenu de la nature de leur cible. Enfin, ils se livrent à des acquisitions porteuses de caractéristiques moins destructrices de valeur.

Nos résultats contribuent à plusieurs champs de littérature. Tout d'abord, nous contribuons à la littérature en gouvernance d'entreprise en identifiant que les recours collectifs concernant les fusions et acquisitions incarnent un mécanisme qui peut contribuer à discipliner le comportement l'investissement des dirigeants (Shleifer and Vishny, 1997). Deuxièmement, nous mettons en avant un mécanisme par lequel les marchés financiers peuvent bénéficier des recours collectifs alors que la littérature juridique tend à mettre en avant les dysfonctionnement de ces recours (Rose, 2008; Coffee, 2006). Troisièmement, nous contribuons à la littérature sur les effets de contagion dans les industries en démontrant l'importance des recours collectifs pour expliquer les changements de comportement de la part des compétiteurs.

Chapitre 2 : Cout d'ajustement sur le marché du travail et manipulations comptables

Dans le deuxième chapitre de cette thèse, je me concentre sur une autre forme d'institutions, à savoir la réglementation du travail. La réglementation du travail représente un ensemble très complexe de règles qui régissent la relation entre les employeurs et les employés. Auparavant, la réglementation du travail a été essentiellement discutée et conçue afin d'offrir une certaine forme de sécurité pour les employés et améliorer leur capacité à retrouver un emploi. Bien qu'il existe une variation considérable dans la législation du travail à travers le monde, les crises financières récentes ont remis la protection de l'emploi au premier plan du débat politique. Autrement dit, les gouvernements tentent maintenant de déterminer si leur pays offre la réglementation du travail optimale à la fois pour aider les employés chemin sur le marché du travail et mais aussi pour promouvoir la compétitivité des entreprises dans une économie mondialisée.

Dans cette étude, je me concentre sur les coûts d'ajustement de l'emploi en tant que composante de la souplesse opérationnelle des entreprises et de leur interaction avec d'autres choix de manipulation comptable. Ces coûts d'ajustement du travail correspondent à tous les coûts liés aux licenciements d'employés. Plusieurs travaux de recherche ont mis en avant le fait que l'ajustement du nombre d'employés est un outil utilisé pour manipuler les résultats comptables (Dierynck et al., 2012). En outre, des études théoriques et empiriques montrent que les dirigeants étudient les couts et bénéfices des différentes options pour accroître le résultat comptable publié (Ewert and Wagenhofer, 2005; Demski, 2004; Zang, 2012). Je formule l'hypothèse que plus les couts de licenciements sont élevés, moins les dirigeants sont enclins à licencier des employés pour manipuler leur résultat, et en contrepartie ils vont réduire d'autres formes de dépenses discrétionnaires.

Pour établir un lien de causalité, j'utilise des changements de jurisprudence entre les différents états américains concernant la capacité des entreprises à licencier des employés. Ces changements

de jurisprudence ont été adoptés entre 1959 et 1998 et constituent des exceptions à la doctrine du «laissez-faire» qui prévaut ailleurs aux États-Unis. Plus précisément, ces lois augmentent les couts de licenciements en autorisant les employés à remettre en cause plus facilement leur renvoi en justice.

J'établis trois séries de résultats. Tout d'abord, je trouve que, après l'adoption des changements de jurisprudence, les entreprises réduisent les dépenses discrétionnaires anormales compare à un groupe d'entreprises qui ne sont pas touchées par un changement de réglementation. Deuxièmement, je trouve que les résultats sont plus prononcés pour les entreprises avec des incitations spécifiques pour accroître le bénéfice. Enfin, je trouve que de telles manipulations sont préjudiciables à la compétitivité des entreprises et se traduisent par une diminution de leur part de marchés dans les périodes subséquentes.

Mes résultats contribuent à plusieurs courants de recherche en comptabilité. Tout d'abord, mon papier est lié à la littérature qui examine le coût et les avantages des divers choix pour manipuler les résultats à la hausse. Ensuite, je contribue à des études sur les conséquences de la manipulation des revenus pour les entreprises dans le futur. Enfin, mes résultats sont liés à la littérature sur la réglementation et la comptabilité en fournissant les preuves d'une conséquence involontaire provoquée par un changement dans la réglementation du travail qui affectent les entreprises et se traduit par davantage d'agressivité à l'égard de dépenses discrétionnaires, ce qui affecte finalement leur compétitivité (Leuz, 2010).

Chapitre 3 : Connexions politiques et comportement sur les marchés financiers

Ce troisième chapitre est un travail conjoint avec Renaud Coulomb et Marc Sangnier. Tandis que les deux chapitres précédents étudient l'effet des différentes formes de réglementation sur les choix des entreprises, ce chapitre se concentre sur le comportement des individus.

Ce chapitre examine comment les connexions politiques affectent le comportement des agents. Nous plaçons notre étude directement dans les lignée de l'argument développé par Becker (1968).

Dans son article fondateur, la probabilité de violer la loi pour des agents dans un cadre rationnel dépend de savoir si les bénéfices attendus sont plus grands que les coûts prévus, qui dépendent eux-mêmes de la peine encourue et la probabilité de se faire poursuivre. Dans cet article, nous formulons l'hypothèse que les connexions politiques sont un mécanisme qui peut créer de l'hétérogénéité entre les agents dans leur perception du risque de se faire poursuivre en justice.

Nous appliquons notre hypothèse aux comportements de vente et d'achat d'actions sur les marchés financiers. A notre connaissance, notre étude est la première à examiner l'impact des connexions politiques sur le comportement des dirigeants. En effet, tous les travaux passés se sont intéressés aux conséquences des connexions politiques sur les entreprises. (Fisman, 2001) et (Faccio, 2006) montre l'existence d'une relation positive entre les connexions politiques et la valeur de l'entreprise. D'autres études montrent que les entreprises bénéficient de traitements préférentiels en raison de leurs connexions politiques en matière d'accès à des prêts par exemple (Khwaja and Mian, 2005) ou en termes de prosécution pour fraudes comptables.

Nous utilisons l'élection présidentielle française de 2007 comme une source d'augmentation de la valeur des connexions au futur président, Nicolas Sarkozy. Utilisant un modèle de double différences, nous comparons le comportement des dirigeants liés au président avant et après l'élection relatif au comportement des dirigeants qui ne possèdent pas de telles connexions.

Le contexte français est particulièrement approprié pour répondre à notre question de recherche. En effet, la France est un pays où le président dispose d'un important pouvoir. Par exemple, il nomme directement le responsable de l'agence chargée de superviser les marchés financiers. En outre, des études antérieures, telles que Bertrand et al. (2007) et Kramarz and Thesmar (2013) font également valoir que la France est un pays particulièrement bien adaptée pour enquêter sur les conséquences de liens sociaux que ses élites sont très concentrées et politiquement connectées.

Nous établissons trois séries de résultats. Tout d'abord, nous constatons que les achats

d'actions par des dirigeants politiquement connectés bénéficient de rendements anormaux plus importants après l'élection, suggérant que ces administrateurs sont plus susceptibles de procéder à des délits d'inités. Ensuite, nous trouvons aussi que les administrateurs politiquement connectés sont environ 20 % plus susceptibles de ne pas respecter le temps de divulgation de leurs achats ou ventes de titre après l'élection. Enfin, nous montrons que les administrateurs politiquement connectés sont plus susceptibles de vendre ou acheter des actions dans la période qui se situe entre la fin de l'exercice comptable et la publication des résultats, ce qui correspond à une période pendant laquelle les codes de gouvernance recommandent de ne pas effectuer de transaction. Dans l'ensemble, nos résultats sont cohérents avec notre hypothèse que le sentiment d'impunité se traduit par un changement de comportement sur les marchés financiers.

Introduction

Managers are key agents of the current economic system. Indeed, executives of listed companies constantly take decisions. Their role is to set the goals of the company they run. Which markets will the company enter? Against which competitors? With what product lines and which horizon? How will the company differentiate itself? What information should firms disclose to financial market participants? The CEO decides, sets budgets, forms partnerships, and hires a team to steer the company accordingly. Their decisions carry large implications for investors, employees, suppliers and potentially society as a whole. While part of the decisions made by managers is explained by the firm previous choices and culture, a significant extent of the heterogeneity in investment, financial, and organizational practices observed for publicly listed firms can be attributed directly to managers themselves (Bertrand and Schoar, 2003).

How do managers take their decisions? Economic theory relies on the assumption that agents act rationally and in their own interest. That is, they take decisions in order to maximize their utility, even if these choices might be sub-optimal for other agents. In this dissertation, I focus on listed corporations and their managers in the context of various forms of decision-making, including investment, reporting and individual trading choices. Specifically, I try to improve our understanding on how external forces that are beyond firms' control, such as regulations, shape managers' decision function.

The central challenge in modern listed corporations comes from the separation of ownership

and control. Indeed, managers are hired by shareholders to take decisions that are in the best interest of the company's owners. However, CEOs tend to only hold a small fraction of the firms they control and may therefore choose to pursue private goals instead of maximizing shareholder value. For example, they can undertake investment decisions in an "empire building" approach to maximize the size of the corporation they control, even if such decisions are made at the expense of shareholder value. Furthermore, they can set the amount of discretionary expenses in ways that maximize firms' current profitability. This short-term behavior might affect firms' long-term prospects. Finally, managers can maximize their wealth by trading on the private information they hold thanks to their executive positions. If they are prosecuted, this will affect their ability to properly run their company.

All these examples that I study in my thesis are grounded in the classical principal-agent problem where information asymmetry exists between managers and other parties. Such situations, where managers may or may not behave optimally arise partly from the misalignment of interests between investors and stakeholders.

Another fundamental economic principle is that agents, such as managers of listed firms, respond to economic incentives. That is, incentives represent all the different ways that can be used to motivate agents to take a particular course of action. Absent of specific incentives, managers will simply run their company in ways that maximize their utility and not necessarily maximize shareholders' value. This central governance problem has been extensively studied. Shleifer and Vishny (1997) provide a survey of the literature on firm governance, outlining the attempts of how to mitigate the problem of such agency costs.

In this dissertation, I do not attempt at documenting the well-known existence of agency problems. I focus on the sources of incentives that shape managers' decision-making process with respect to various outcomes. More specifically, I investigate whether various forms of institutional features affect managers' decisions and whether this is beneficial or detrimental

to shareholders or to the economic system in general. Broadly speaking, I refer to institutions as mechanisms that facilitate interactions between economic agents. In his work, North (1991) defines institutions as “humanly devised constraints that structure political, economic and social interactions”.

Institutions are usually shaped by the regulators. In the case of listed corporations, institutions in the form of regulation has substantially increased in recent years in response to various corporate scandals and perceived shortcomings during financial crises around the world. For example, firms have to comply with many periodic financial disclosure requirements and disclose specifically how they compensate their managers. They also have to seek approval of the regulators in transactions that may affect the competitiveness of an economic sector or comply with rules on how to contract and deal with agents both within their firm (e. g., employees) and outside their firm (e.g., suppliers).

The first choice that a regulator faces is the decision whether or not to regulate in a given context, in order to create an institution that affect managers incentives. As many have pointed out, the mere fact that some forms of institutions can have benefits to some parties, is not sufficient to justify the existence of the regulation. For example, consider the case of a regulation that imposes large costs to firms when they want to terminate employees. Such regulation will benefit current employees. However, this may also affect firms’ future hiring decisions and be overall detrimental to the labor force in general. Actually, economic history has documented that economies often have institutions that are inefficient but that are persistent over time (North, 1991). In this dissertation, comprised of three distinct chapters, I investigate whether several forms of institutions affect managers in their decision-making. I do not attempt at providing normative conclusions as to whether some regulations are “good” or “bad” and whether they should be promoted or suppressed. I recognize that normative conclusions can only be made once the effects of a given institution have been exhaustively quantified. That is, while a regulation

is usually primarily issued to govern specific situations, it may also have many unintended implications.

To scientifically evaluate the impact of institutions on managers' decisions, I overall position my dissertation in the positive law and economics theoretical framework. The law and economics approach refers to the application of microeconomic analysis to legal problems and evaluate their economic efficiency. Specifically, I constantly try to identify how legal features affect managers using various econometric tools that allow to assess plausibly causal relation. To do so, I develop or borrow to previous studies instruments that correspond to plausibly exogenous source of variations in the strength of institutions, and investigate (1) whether managers react to such changes and (2) whether this is plausibly positive or negative for other parties, such as investors.

In this dissertation, I aim at contributing to the debates on various forms of institutions by shedding light on unintended consequences of such regulations on corporate decision-making. For example, I examine whether law labors that govern labor contracts also affect managers discretionary decisions. I also investigate whether class-action, that are currently seen as a burden of the U.S. judicial system for listed firms can, under certain circumstances, reduce agency problems related to investment decisions. Overall, my different chapters are useful to assess the effects of institutions on managers' decision making and potentially useful to regulators by identifying previously unexplored consequences of institutions that need to be incorporated to evaluate the net effect of those institutions.

Chapter 1: Securities Lawsuits as a Disciplining Mechanism: Evidence from Mergers and Acquisitions

The first chapter of this thesis is the result of a collaboration with Francois Brochet and Sven Michael Spira. In this essay, we investigate whether securities lawsuits, a specific type of class-action lawsuits constitute a governance tool to reduce agency costs with respect to managers'

investment decision.

Broadly speaking, class action represents a legal mechanism by which a group sues another party. In the case of securities lawsuits, investors sue the firm and its managers for wrongdoing. As a result, class-action may constitute a mechanism that can affect managers' decision making and ensure that their interests are aligned to that of their investors. Class-actions are a key component of the legal system of common law countries. However, critics argue that class members often receive little or no benefits from such legal actions and that lawyers are the parties who benefit the most from the legal system. In the context of financial markets, some scholars even argue that class-actions constitute a burden for the attractiveness of the U.S. stock exchanges that may even explain some of the delisting choices by foreign firms in the 2000s (Zingales, 2006). While prior studies document that securities lawsuits affect individuals' wealth and labor market reputation (Brochet and Srinivasan, 2014), our study is the first to try to quantify the general effect of litigation risk with firms' investment decisions.

Interestingly, several European countries with civil law have recently changed their law to introduce some forms of private enforcement. For example, France adopted a law in 2012 that introduced class-action for consumer-related actions. Thus, it is important to understand whether private enforcement can help improving the behavior of economic agents before promoting and introducing additional form of institutions in other countries.

In this paper, we use securities lawsuits related to M&A as a positive source of variation in the perceived risk of being sued by industry peer firms. Our sample of lawsuits is composed of cases where firms engaged in a value-destroying M&A and then mislead investors about the poor post-acquisition performance.

We establish three sets of results. First, we do find that industry peers experience higher bidder announcement returns in the two years following the incidence of an M&A lawsuit in their industry. Next, they choose methods of payment associated with better acquisitions.

That is, they are more likely to pay for public targets in cash and for private targets in stocks. Finally, they engage in acquisitions carrying less value-destroying characteristics. Collectively, this evidence is consistent with post lawsuit deals being of higher quality. Our results are robust to endogeneity tests and various robustness tests.

Our results contribute to several stream of research. First, we add to the corporate governance literature by identifying that M&A-related lawsuits embody a mechanism that can contribute to disciplining managers' investment behavior (Shleifer and Vishny, 1997). Second, we shed light on a channel through which financial markets may benefit from private enforcements of securities law, in comparison to the legal literature, where Rose (2008) and Coffee (2006) argue that lawsuits target deep pocketed firms, and fail to deter fraudulent behavior and to compensate wronged investors. Third, we contribute to the literature on industry peer effects. While managers consider competitor's stock price movements (Foucault and Frésard, 2014), accounting restatements (Durnev and Mangen, 2009), hostile takeovers (Servaes and Tamayo, 2014) and securities lawsuits (Arena and Julio, 2014), we are the first to investigate whether the specific risk of M&A litigation has an intra-industry spillover effect, and whether it disciplines managers' investment behavior.

Chapter 2: Labor Adjustment Costs and Real Earnings Management

In the second chapter of this thesis, I focus on another form of institutions, namely labor regulation. Labor regulation represents a very complex set of rules that governs the relation between employers and employees. Previously, labor regulations was mostly discussed and designed in order to offer some form of security to employees and enhance their ability to find a job back when losing one. While there exist tremendous variation in labor laws around the world, the recent financial crises has brought employment protection back to the forefront of the policy debate using a new angle. That is, governments now try to assess whether their country offers the optimal labor regulation to both secure employees path in the labor market

and promote firms' competitiveness in a global economy.

In this study, I focus on labor adjustment costs as a component of firms' operating flexibility and their interaction with other reporting choices. Labor adjustment costs are all the costs associated with the termination of employees. Prior research documents that managers terminate employees to decrease expenses and cash outflows in order to boost firms' reported performance (Dierynck et al., 2012). Furthermore, theoretical and empirical studies document that managers trade-off the relative costs and benefits of the different options they can use to increase reported earnings (Ewert and Wagenhofer, 2005; Demski, 2004; Zang, 2012). I build on this literature and conjecture that when managers face higher adjustment costs, they are less likely to terminate employees and thus more likely to rely to other form of actions to increase earnings. Specifically, I conjecture that they will substitute cutting other discretionary expenses to firing employees. I do not label firing employees as a form of real-earnings management since I cannot assess whether such decisions were optimal or not for the firms. However, I do find that cutting discretionary expenses instead of employees constitute a second-best for investors since it affects firms' future competitiveness.

To establish causality, I use a difference-in-differences research design and exploit the adoption of wrongful discharge laws at the state level in the U.S. as a plausible source of exogenous variation in firing costs. Wrongful discharge laws were passed between 1959 and 1998 and constitute exceptions to the traditional fire-at-will doctrine that otherwise prevails in the United States. Wrongful discharge laws, and especially the good-faith exception in its broadest sense, protect employees from termination for any reason other than a *just* cause.

I establish three sets of results. First, I find that after the adoption of the good-faith exception, firms cut abnormal discretionary expenses more relative to a control group of unaffected firms. Second, I find that the results are more pronounced for firms with specific incentives to increase earnings. Lastly, I find that such manipulations are detrimental to firms' competitiveness

in subsequent periods.

My results contribute to several streams of accounting research. First, my paper is related to the literature that examines the cost and benefits of various choices to manipulate earnings upward. Next, I contribute to the studies on the consequences of real earnings management for firms subsequent performance. Finally, my results speak to the literature on regulations and accounting by providing evidence of an unintended consequence of a change in labor regulation that affect firms aggressiveness with respect to discretionary spending, which ultimately affects their competitiveness (Leuz, 2010).

Chapter 3: Political Connections and Insider Trading

This third chapter is a joint work with Renaud Coulomb and Marc Sangnier. While the two previous chapters investigate the effect of different forms of regulations on firms' investment and reporting choices, this chapter focuses on individuals. This paper investigates how political connections affect agents' behavior. We place our study directly in lines with the argument developed by Becker (1968). In his seminal paper, he models agents' likelihood to break the law in a rational framework in which individuals decide to break the law if the expected benefits from doing so are larger than the expected costs, which combine the incurred punishment and the probability of getting caught. In this paper, we conjecture that political connections is a mechanism that may create heterogeneity between agents in their perceived probability of getting caught and the penalty, if prosecuted. The intuition is that individuals may benefit from their political connections through lower enforcement, which ultimately increases their willingness to break the law.

We apply our conjecture to individuals trading behavior in financial markets. To our knowledge, our paper is the first one to consider the impact of political connections on individuals. Indeed, existing studies in accounting, finance and political economics examine the consequences of political connections at the firm level. (Fisman, 2001) and (Faccio, 2006) document a positive

impact of political connections on firm value. Recent studies also show that such connections lead to other types of benefits for firms. For instance, Khwaja and Mian (2005) and Claessens et al. (2008) report preferential access to finance and banks loans for connected firms, while Correia (2014) shows that they incur lower costs from public enforcement actions. Goldman et al. (2013), Tahoun and Van Lent (2013), and Tahoun (2014) provide evidence that such firms have a higher probability of obtaining government contracts or to be bailed out.

We use the French 2007 presidential election as a plausibly exogenous increase in the value of connections to the future President, Nicolas Sarkozy. A difference-in-differences framework allows us to capture the extent to which directors connected to Nicolas Sarkozy change their trading behavior from the pre- to the post-election period relatively to non-connected directors. This framework allows us to plausibly isolate how a shift in the value of the connection between directors and politicians affects directors' behavior in financial markets.

The French setting is particularly appropriate to answer our research question. France is a country where the President has major political power. For instance, he directly names the Head for the French SEC (AMF). In addition, prior studies, such as Bertrand et al. (2007) and Kramarz and Thesmar (2013) also argue that France is a particularly well-suited country to investigate the consequences of social ties of directors because its elites are highly concentrated and politically-connected.

We establish three sets of results. First, we find that purchases by politically-connected directors exhibit larger abnormal returns after the election relatively to our control-group of unconnected directors, suggesting that such directors are more likely to trade on private material information. Next, we also find evidence that politically-connected directors became about 20% more likely to break the legal disclosure time limit in response to the shift in power of Sarkozy. Lastly, we show that politically-connected directors are more likely to trade in the period between the end of the fiscal year and the disclosure of annual earnings after in the

post-election period. Soft law prescribe to trade in this period where managers likely possess superior private information compared to other market participants. Overall, our results are consistent with our hypothesis that the feeling of impunity translates in a lower enforcement probability that leads to connected directors to trade illegally in financial markets.

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Chapter 1

Securities Lawsuits as a Disciplining Mechanism: Evidence from Mergers and Acquisitions

joint with Francois Brochet and Sven Michael Spira

1.1 Introduction

Under the traditional principal-agent framework, corporate managers make investment and reporting decisions that are not always in the best interest of their shareholders. Poor investment and reporting decisions are co-mingled. That is, managers are likely to resort to financial reporting manipulation to disguise suboptimal investment decisions, such as perceived value-destroying acquisitions (Bens et al., 2012). Shareholders have access to a variety of mechanisms to either prevent or punish managers who engage in bad investment and/or reporting decisions. In the U.S., one of those mechanisms is the use of class-action securities lawsuits. Shareholders can resort to those lawsuits to seek compensation for damage when managers allegedly mislead them through improper disclosure. If those misleading disclosures are motivated by the need to cover up bad investment decisions, their detection and punishment signals a higher cost of engaging in suboptimal investments. Accordingly, we conjecture that, upon observing the litigation of an industry peer who is blamed for hiding poor post-acquisition performance, managers update their subjective probability of being brought into litigation themselves for a similar motive (Arena and Julio, 2014). Thus, because hiding poor acquisition performance is perceived as more costly by managers when observing a lawsuit, we examine the hypothesis that the perceived risk of litigation acts as an external governance mechanism and helps discipline opportunistic managers in their investment decisions.

Securities lawsuits have been identified as major corporate events with severe consequences for executives and directors of sued firms (e.g., Romano, 1991; Fich and Shivdasani, 2007). However, their role in promoting efficient resource allocation across firms has been debated in the law and financial economics literature. On the one hand, prior studies find that shareholder lawsuits can lead to desirable outcomes such as management accountability (Romano, 1991) and improved governance (Cheng et al., 2010). On the other hand, critics argue that shareholder litigation may harm the attractiveness of U.S. financial markets and fail to deter fraudulent

behavior (Bondi, 2010; Zingales, 2006; Coffee, 2006).

We investigate the governance role of securities litigation by innovating along two important dimensions. First, while Rule 10b-5 or Section 11 lawsuits are filed in response to allegedly misleading disclosures, we use a sample of lawsuits where the plaintiffs specifically claim that managers overpromised and hid poor performance after acquisitions (thereafter, “ex post M&A lawsuits”) to examine the hypothesis that M&A lawsuits discipline manager’s investment behavior. Second, instead of focusing on the investment behavior of litigated firms (McTier and Wald, 2011), we use ex post M&A lawsuits as a shock to the industry, and conjecture that the incidence of a lawsuit increases the (perceived) risk in the industry for also being targeted by an M&A lawsuit. This is likely to arise, in part, because lawsuits tend to cluster by industry (Kim and Skinner, 2012), and the sued firm’s misleading disclosures may have influenced peers’ investment behavior during the class period (Beatty et al., 2013). Our paper builds on a recent literature that highlights the importance of intra-industry spillovers and learning effects (e.g., Servaes and Tamayo, 2014; Foucault and Frésard, 2014). While peer effects have been documented for lawsuits, no study has investigated their effect on the moral hazard problem of the principal-agent relationship outside of the sued firm.¹

We obtain detailed information on federal securities class-action lawsuits in the U.S. from the Institutional Investor Services (ISS) Securities Class Action database, and read plaintiffs’ allegations to identify ex post lawsuits where the allegations claim that managers overpromised and hid poor performance related to a past merger or acquisition.² In our sample period 1996-2011, we match 89 relevant cases with the COMPUSTAT firms, which correspond to 79 industry-years with at least one filing of an ex post M&A lawsuit. Two patterns emerge from that sample. First, following a lawsuit, the sued firms, which tend to be large firms that over-

¹For instance, Gande and Lewis (2009) show that peer firms’ stock prices react to the filing of a securities lawsuit. Moreover, according to Arena and Julio (2014), competitors hold more cash in anticipation of future litigation costs.

²For brevity, we refer to “ex post M&A lawsuits” as “M&A lawsuits” or “lawsuits” interchangeably in this paper. Otherwise, we specifically refer to “imminent M&A lawsuits” that occur during the acquisition.

invest, decrease their investment level. Second, we observe that, at the industry level, M&A lawsuits are a positive predictor of M&A lawsuits in the following year, controlling for year and industry effects, and for the number of acquisitions in the industry. This suggests a genuine increase in litigation risk at the industry level.³ Hence, these results validate our assumptions that the lawsuits are economically meaningful, and a credible shock at the industry-level. In order to examine the industry-wide governance role of investment-related lawsuits, we examine peer firms' total investment, as well as specific M&A deals in the two-year period after an M&A lawsuit is filed in the industry, which we define at the 2-digit SIC (hereafter SIC2) level.

Our main empirical tests investigate the quality of M&A deals after a peer firm is subject to an M&A lawsuit, using a difference-in-differences regression model. We use the bidder's cumulative abnormal return (CARs) around the deal announcement as a proxy for the quality of the investment decision, which is, on average, also reflective of the acquisition's long-run performance (Bens et al., 2012; Sirower and Shani, 2006). Throughout our analysis, we control for industry and year effects, as well as acquirer, target and deal characteristics that have been shown to affect deal quality. Our regression results consistently show that in the two-year period after a lawsuit, bidders' announcement CARs are significantly higher for industry peers of litigated firms. Peers' announcement seven-day CARs in the period following an M&A lawsuit are, *ceteris paribus*, 0.70% higher than the sample average, which is economically significant. This finding supports the hypothesis that post lawsuit deals are of higher quality.

We further investigate how deal characteristics change in reaction to peer firms' lawsuits. We first examine payment methods. The optimal payment method depends on the target type, due to an asymmetric information problem (e.g., Eckbo and Thorburn, 2000; Hege et al., 2009). Theoretical and empirical research shows that when the target is a public firm, cash acquisitions are a positive signal to the market. For private firms and subsidiaries, the uncertainty about

³We test and find that the amount of M&A activity in the industry does not correlate with the occurrence of lawsuits. Thus, lawsuits do not seem to coincide with merger waves.

the true value of the target is higher, so that stock financing is preferred. We find that after an M&A lawsuit in the industry, acquisitions of public targets are more likely to be paid for in cash, and acquisitions of private targets and subsidiaries are more likely to be paid for in stock. Second, we find evidence that deals after an M&A lawsuit are less likely to be diversifying, large, or of an accretive nature. All three characteristics have been associated with value-destroying acquisitions. Thus, overall, these findings consistently suggest that post M&A lawsuit deals are of better quality.

We perform a number of robustness tests and extensions to validate our main assumptions. First, managers may change their behavior not because of a change in litigation risk, but because they observe a value-destroying takeover within their industry. Thus, we introduce various controls for poor acquisitions undertaken by industry peer firms in recent years. Our main results continue to hold after we control for the learning effect. Second, following Bertrand and Mullainathan (2003a) and Atanasov (2013), we conduct an additional test to examine the potential endogeneity of M&A lawsuits. We find that M&A lawsuits bear no significant association with bidder announcement CARs one or two years before their filings, which rules out reverse causality. Third, we test and find that the documented changes in investment behavior are driven by the threat of acquisition-related lawsuits, instead of an increase in overall securities litigation risk, which can be caused by allegations of, for example, improper accounting, insider trading, options backdating, etc. Finally, we rule out alternative explanations that our results are driven by changes in disclosure or by frivolous cases.

This paper makes several contributions. First, the corporate governance literature has identified a variety of mechanisms, such as the threat of takeovers, board composition, executive compensation and large shareholders, that can contribute to disciplining managers (Shleifer and Vishny, 1997). We highlight a new channel of corporate governance by examining whether firms adjust their investment behavior in response to the threat of M&A-related lawsuits. While

McTier and Wald (2011) document a decrease in overinvestment by firms subject to securities lawsuits, we find that a specific set of lawsuits - those where plaintiffs allege that a firm covered up poor performance following prior acquisitions - lead industry peers to engage in less value-destroying mergers and acquisitions. This suggests that securities lawsuit may contribute to the reduction of agency problems.

Second, the role of securities lawsuits vis-à-vis the attractiveness of the U.S. financial market has been subject to debate among scholars. In the legal literature, Rose (2008) and Coffee (2006) argue that lawsuits target deep-pocketed firms, while failing to deter fraudulent behavior and to compensate wronged investors. By looking at the effect of a previously unexplored subset of securities lawsuits, i.e., ex post M&A lawsuits, we shed light on a vehicle through which financial markets can possibly benefit from private enforcement of securities law.

Third, and finally, a recent literature stresses the importance of industry peer effects. For instance, managers learn from competitor's stock price movements (Foucault and Frésard, 2014), accounting restatements (Durnev and Mangen, 2009), activist campaigns Gantchev et al. (2014) and securities lawsuits (Arena and Julio, 2014). Our paper is closely related to Servaes and Tamayo (2014), who find that firms make investment and governance changes when an industry peer is targeted in a hostile takeover attempt. However, over the last decade, the number of hostile takeovers has been decreasing due to stronger antitakeover provisions, whereas the number of securities lawsuits has remained steady. To our knowledge, ours is the first paper to investigate whether the specific risk of M&A litigation has an intra-industry spillover effect, and whether it disciplines managers' investment behavior.

The remainder of this paper is structured as follows. Section 1.2 briefly summarizes the related literature develop our hypothesis. Section 1.3 describes the data and our empirical strategy. Section 1.4 presents the empirical results. Section 1.5 concludes.

1.2 Related Literature and Hypothesis Development

The classic principal agent problem that arises from the separation of ownership and control in the modern corporation is central to financial economic research. There are numerous factors, internal and external to the firm, that shape the severity of those agency costs across companies and jurisdictions. Shleifer and Vishny (1997) provide a survey of the literature. The most common mechanisms that may mitigate this moral hazard problem include takeover threats, concentrated ownership, large creditors, shareholder activism and the quality of boards of directors.⁴

Securities class-action lawsuits constitute one additional specific external corporate governance mechanism that shareholders can resort to in order to discipline managers and reduce agency costs. A class-action is a legal mechanism by which a group of plaintiffs collectively bring a claim to court. In the case of securities lawsuits under Rule 10b-5 of the Securities Exchange Act of 1934 and Rule 11 of the 1933 Act, investors can sue the firm, its executives, its directors and third parties for fraudulent behavior such as accounting manipulation, illegal insider trading, or questionable practices during an IPO. These lawsuits have consequences for the targeted firms. For example, McTier and Wald (2011) find that sued firms respond to a lawsuit by reducing investment and dividends while increasing leverage and cash holdings. Besides, Rogers and Van Buskirk (2009) document that shareholder lawsuits cause sued firms to reduce their amount of voluntary disclosure provided post-litigation. Finally, other studies also show that lawsuits can impose reputational costs to sued firms (Karpoff and Lott, 1993), executives (Romano, 1991) as well as independent directors (Brochet and Srinivasan, 2014). Overall, shareholder litigation can be used as a tool to discipline managers and lead to improvements in board independence, especially when lead plaintiffs are institutional owners (Cheng et al.,

⁴For example, Bertrand and Mullainathan (2003a) find that managers appear to “enjoy the quiet life” when anti-takeover laws are introduced, and Gompers et al. (2003) document that firms with stronger corporate governance provisions (as captured by their “G-index”) outperform their peers. Bebchuk et al. (2009) use a refinement of the G-index and find that the entrenchment index “E-index”) drives this superior performance by firms with better corporate governance.

2010).

Corporate governance needs not apply to firms in isolation. Indeed, an emerging field of research examines industry spillovers and intra-industry learning effects. For example, Foucault and Frésard (2014) document that firms take industry peers' valuation into account when making investment decisions, while Beatty et al. (2013) find that industry leaders that are found to have engaged in fraudulent accounting practices mislead their peers into increasing their investments during the fraud period. Our paper is closely related to two recent studies. First, Gantchev et al. (2014) document that industry peers change their dividend and investment policies in response to an increase in the threat of activist campaigns. Second, Servaes and Tamayo (2014) show that industry peers react when a firm in the industry is the target of a hostile takeover attempt. Inter alia, rival firms cut capital spending, free cash flows and cash holdings. That is, the threat of hostile takeovers discipline managers' investment and payout choices. The authors argue that such behavior arises because the agency costs of free cash flow are not specific to a particular firm, but generally affect an entire industry (Jensen, 1986, 1993; Shleifer and Vishny, 1988). Thus, as expected, firms take into account governance-related news about their competitors in their own decision-making.

In this paper, we conjecture that securities lawsuits might serve as another industry-wide governance mechanism to attenuate agency problems related to investment. Absent these spillovers, the literature does not fully capture the impact of securities litigation on investment. From a theoretical perspective, our approach is related to the model of crime deterrence developed by Becker (1968). In his seminal work, he models the deterrence effect of crime punishment as a function of the probability of being discovered and the severity of the punishment, if convicted. As noticed above, securities lawsuits can impose severe reputational costs on the firm and its executives. Thus, the ability of lawsuits to act as a governance mechanism with respect to investment depends on managers' perception of the risk of being sued.

The efficacy of those lawsuits as a deterrent of corporate misconduct has been debated for decades. On the one hand, securities class action lawsuits are often criticized for being frivolous (Langevoort, 1996). This has led some critics to suggest that the primary beneficiaries of litigation are lawyers rather than corporations or shareholders. Indeed, Rose (2008) observes that relatively cash-abundant firms are selectively targeted by securities lawsuits, and Coffee (2006) argues that securities lawsuits fail to deter fraudulent behavior, possibly due to the limited financial liability of directors and officers through the D&O insurance (Black et al., 2006; Klausner, 2009). Moreover, cases almost never go to trial. The settlement amounts do not compensate violated investors, and the costly process for firms in terms of legal and expert fees, are usually listed by critics in the legal literature against securities lawsuits, which are often seen as a burden for the attractiveness of the U.S. financial market. On the other hand, the risk of shareholder litigation seems to limit managers' proclivity to make opportunistic financial reporting choices (Hopkins, 2014). In addition, Donelson and Yust (2014) document a positive impact of officer and director litigation risk on firm value, pay-for-performance sensitivity and accounting quality. Thus, the ability of the risk of lawsuit in disciplining firms' investment behavior remains an empirical question.

Managers' decisions to manipulate reported financial information has received extensive attention from accounting scholars.⁵ Recently, Bens et al. (2012) document that executives are more likely to misreport after a poorly perceived acquisition, in an attempt to hide poor performance. This behavior stems from the fact that poor post-acquisition performance leads to severe consequences for the CEO in terms of pay and career trajectories (Lehn and Zhao, 2006). Consistent with the framework of Becker (1968), we conjecture that the threat of securities lawsuits can partly mitigate this behavior. Specifically, we expect that the incidence of a lawsuit related to past investment decisions in a given industry will increase managers' perceived

⁵See for instance Dechow et al. (2010b), Healy and Wahlen (1999a) or McNichols (2000) for thorough reviews of this strand of literature.

litigation threat and hence, discipline their investment behavior.

Our hypothesis is based upon two key assumptions. First, the credibility of the threat of securities lawsuits should determine whether it functions as an effective disciplining device. In this paper, we assume that an industry peer’s actual lawsuit can serve as a threat of lawsuit. This assumption is likely to be valid insofar as securities lawsuits tend to be clustered by industries and time (Kim and Skinner, 2012). In addition, prior studies document spillover effects of securities lawsuits on firms’ stock prices (Gande and Lewis, 2009) and cash holdings (Arena and Julio, 2014), consistent with lawsuits conveying news at the industry level.

Second, we also assume that lawsuits that allege misrepresentation related to M&A will discipline peers’ M&A activity. As explained above, this critically hinges on the notion that firms are inclined to disguise poor performance following acquisitions (Bens et al., 2012), as our cases of ex post securities lawsuits do not solely arise because of poor M&A, but the attempt to disguise them.⁶ Hence, our hypothesis is that, upon observing an M&A lawsuit in the industry, competitors update their perceived risk of being sued if they were to hide poor post-acquisition performance, making this behavior more costly. Consequently, managers will attempt to undertake acquisitions of better quality.

The tension in the hypothesis comes from several sources. First, as previously mentioned, the effectiveness of securities litigation risk as a deterrent of corporate fraud remains a strongly debated topic in academic and practitioner circles. Second, because the risk of ex post M&A litigation is a function of firms’ acquisitions and disclosure, firms may respond by changing their disclosure strategy (Rogers and Van Buskirk, 2009), without changing their acquisition behavior per se. Third, if ex post M&A lawsuits are in fact an attempt by plaintiff lawyers to extract rents from defendants, the market may perceive their effect as negative on subsequent

⁶In recent years, M&A deals have been targeted by plaintiff lawyers, often on behalf of target shareholders. We refer to those lawsuits (which are not always filed in a federal court) as “ex ante M&A lawsuits”. Indeed, Cornerstone reports that over the last 5 years almost all deals in excess of \$500m have been litigated (Cornerstone, 2013, 2014). See Krishnan et al. (2012) and Krishnan and Masulis (2013) for more details.

M&A and investment in the industry. However, while this may have a “chilling” effect on firms’ investment behavior, it is not clear whether that effect would manifest itself in market reactions to acquisition announcements.

1.3 Identification Strategy and Variables

1.3.1 Lawsuit Data Collection

The identification strategy in this paper comes from the incidence of ex post M&A lawsuits in given industries. To construct our sample, we use data on securities class-action lawsuits in the U.S. over the period 1996-2011 from the ISS Securities Class Action database. The ISS database includes all federal securities lawsuits filed against publicly-listed companies in the U.S. It does not differ from the Stanford Securities Class Action Clearinghouse’s database in scope, but it provides many lawsuit characteristics, including plaintiff allegations, in machine readable format. We first filter out all lawsuits not related to M&A activities through keyword searches in the plaintiffs’ allegations and obtain a sample of 588 observations. Many of those remaining lawsuits are filed during the takeover period in order to receive additional information from the acquirer or the target firms based on concerns that directors broke their fiduciary duties (Krishnan et al., 2012; Krishnan and Masulis, 2013). Since we are interested in lawsuits that occur after the completion of the deal and that accuse acquirers of overpromising, we perform a detailed lexicographic analysis of the lawsuit allegations to further identify those lawsuits.

We generate a score based on whether the allegations contain the keywords “synergies”, “integration”, “inflate”, “goodwill”, “write-off”, “deceive”, or variations of these words. Next, we verify by hand whether the identified cases are indeed related to ex post M&A allegations, and whether cases with a zero score are irrelevant (indeed, none is). We provide four examples of allegations in Appendix A, which received median lexicographic scores and were finally coded

as relevant because they relate to ex post integration issues that were initially hidden by the firm. As illustrated by the examples in Appendix A, the lawsuits in our sample follow a specific sequence of events. First, a firm needs to engage in a transaction to buy another company, and communicate projections to shareholders about the expected synergies to be generated by the deal in order to justify the acquisition. Then, while the post-acquisition performance falls below the expectations set up by the initial projections, the firm has to hide its true performance either through accounting manipulations or disclosures that fail to update the market in a timely manner about the disappointing realizations of the acquisition-related cash flows. Eventually, a triggering event, such as a restatement, a write-off of the goodwill or the release of earnings news far below the analysts' earnings consensus will reveal the true post-acquisition performance of the deal, leading investors to file a securities lawsuit on the ground that they were misled.

Overall, we identify 132 different lawsuits, of which we can match 116 acquirers by hand with their names and identifiers in COMPUSTAT. We drop regulated industries, eliminating 27 lawsuits, leading to 89 relevant lawsuits, and 79 industry-years with at least one filing of an ex post M&A lawsuit.

1.3.2 Sample construction and summary statistics

Panel A in Table 1.1 reports the distribution of lawsuits per year. On average, there are 5.5 lawsuits per year. Except in 1998, the yearly number of lawsuits is in the single digits, with no clear time-series trend. Panel B in Table 1.1 indicates that most lawsuits occur in the manufacturing industry, followed by the service sector. Thus, these two industries account for approximately two thirds of the sued cases. We define a lawsuit shock at the SIC2 industry-level if an acquirer from the industry was sued in the preceding 2 year period. This approach is similar to Servaes and Tamayo (2014) who use the 48 Fama– French industries to define industry fixed effects. Out of the 60 unique industries in our sample, 29 industries are targeted

at least once by an ex post M&A lawsuit.⁷

[Insert Table 1.1 about here]

In addition, we create a second dataset of M&A deals extracted from SDC Platinum. A detailed description of all the main dependent and independent variables is provided in Appendix B. Table 1.2 presents the descriptive statistics. The sample includes 11,373 deal-level observations.

[Insert Table 1.2 about here]

1.3.3 Validity of the identification strategy

The perceived threat of litigation is a latent variable. Our identification strategy relies on capturing variation in this unobservable variable, using the incidence of ex post M&A lawsuits as a plausible exogenous source of increase in litigation risk at the industry level. In this section, we perform several tests and discuss the validity of this strategy.

First, in order for M&A lawsuits to have an industry-wide (disciplining) effect, they should have a first-order effect on the behavior of the sued firms themselves. We examine the company characteristics of firms that are alleged of hiding poor post acquisition performance in our sample. Compared to non-sued industry peers, sued firms are significantly bigger and, as expected, tend to invest more than firms that do not get sued. Moreover, sued firms pay less dividends to their shareholders than non-sued firms. Importantly, intertemporally, following a lawsuit, sued firms decrease their investment level. Moreover, they decrease dividends while increasing their leverage.⁸ These findings are in line with those documented by McTier and Wald (2011) who, in contrast to our paper, consider all types of securities class action lawsuits

⁷In 1.4.4, we examine and rule out the concern that our results are driven by a single industry that could be affected by an unobserved confounded event unrelated with our ex post M&A lawsuit

⁸Specifically, sued firms are, on average, 25% larger than non-sued peer firms in terms of total assets. In the year before the occurrence of the lawsuit, sued firms invested twice as much as their peer. After the lawsuit, they reduce their investment to a level comparable to the one of their peers. Finally, sued firms do not seem to differ from their peers in terms of governance (as measured by the G-Index).

(i.e., not just those related to M&A). In addition, prior studies suggest that after the enactment of the Private Securities Litigation Reform Act in 1995, the outcome of the securities lawsuits (settlement versus dismissal) are more likely to reflect the relative merits of the case (Johnson et al., 2007). We find that 58.2% of our sued cases are settled, which is similar to the 61.87% of settled cases for the total sample of lawsuits from the ISS database reported by Brochet and Srinivasan (2014). We also find that the settlement amounts (expressed either in raw amounts or as a percentage of total assets) are similar between the two groups. This suggests that our sub-sample of M&A lawsuits consists of cases with significant consequences for the companies that are subject to a lawsuit.

However, the reaction of sued firms to a lawsuit does not necessarily imply that the announcement of a lawsuit conveys information for industry peer firms. One concern is that the reasons behind our M&A lawsuit cases could be unrelated to peer firms' behavior, providing managers no material reason to update their assessment of the risk to be sued for investment-related decisions. In Panel A of Table 1.3, we document that for an industry, M&A lawsuits are a positive predictor of M&A lawsuits in the following year, controlling for year and industry effects. This result is robust to also controlling for the number of deals per year and industry (column 2). Moreover, the takeovers that are subject to a lawsuit do not differ in their announcement abnormal returns from the other takeovers in the industry. Thus, M&A lawsuits do not seem to be anticipated by the market. This suggests that managers have objective reasons to update the probability of their firm of being sued as well.

[Insert Table 1.3 about here]

As a second test, we examine whether the occurrence of a lawsuit affects the M&A activity in the industry. If managers' perceived litigation risk increases, the industry as a whole may be less active in the M&A market. We define deal volume as the logarithm of the total transaction value per year, aggregated at the acquirers' SIC 2 industry levels. Then we test whether the

number of lawsuits in a given year and industry decreases the deal volume in the subsequent year in the industry. As reported in Panel B of Table 1.3, lawsuits lead to a reduction in deal volume in an industry. We obtain similar results when we replace the continuous variable of lawsuits with a lawsuit indicator in column (2).⁹

1.4 Results

To test our main hypothesis that an increase in the risk of lawsuits disciplines managers' investment behavior, we perform three different analyses. First, we focus on the immediate market reaction around M&A deals announced by sample firms as a proxy for their quality. Second, we examine the methods of payment used in the transaction. Lastly, we study other characteristics of those deals.

1.4.1 Market reaction around transactions by industry peers

Prior research uses bidders' announcement returns surrounding an M&A transaction as a signal about the perceived quality of this investment decision (Morck et al., 1990; Moeller et al., 2004; Bens et al., 2012; Kempf et al., 2015). In this context, we examine whether the market perceives M&A transactions more positively in a given industry following an ex post M&A related lawsuit.

We measure bidder announcement effects using cumulative abnormal returns (CARs) around the initial acquisition announcement dates. We obtain the transaction announcement dates from the SDC U.S. Mergers and Acquisitions database. We report our results using 7-day CARs (-3,+3) windows where event day 0 is the announcement date.¹⁰ The CARs are estimated with a market model using the CRSP equal-weighted return as the market return. To examine the

⁹A potential concern would be that the sued cases simply capture the end of industry mergers waves. However, when we examine the number of deals we find that lawsuits do not have a negative effect on the subsequent number of acquisitions in an industry.

¹⁰However, in untabulated analyses, we find that the effects are qualitatively similar for the 3-day announcement CARs (-1,+1).

impact of investment-related lawsuits on industry peers, we estimate the following difference-in-differences regression model:

$$CARs_{ijt} = \beta_0 + \beta_1 Post\ Lawsuit_{ijt} + Controls_{ijt} + \alpha_j + \alpha_t + \epsilon_{ijt} \quad (1.1)$$

In this model, we include two sets of control variables. The first set includes the following bidder’s characteristics: firm size, market-to-book ratio, and leverage. The second set includes the following deal characteristics: private target, diversifying deal, merger of equals, relative deal size, cash financing, cross-border deal, divestiture and tender offer. All these characteristics are associated with the immediate market reaction around the announcement of an M&A transaction (e.g., Masulis et al., 2007, Fuller et al., 2002). All variables are defined in greater detail in Appendix B. Moreover, α_j indicates industry fixed effects and α_t year fixed effects. The coefficient of interest, β_1 , captures the within industry change in market reaction before and after the occurrence of a lawsuit, using industries that are not affected at the same time as a benchmark group while controlling from economy-wide trends through the year fixed effects. Since the change in the perceived risk of lawsuit is defined at the industry level, we cluster standard errors by industry to account for potential serial correlation issues (Bertrand and Mullainathan, 2003a; Bertrand et al., 2004a).

Table 1.4 reports the results of estimating the regression model using OLS. In column (1), we report a baseline model with no control variables, except for industry and year fixed effects. In column (2), we add firm-level controls, and deal-level controls in column (3). In column (4), we replace the industry fixed effects with firm fixed effects. We systematically find a positive and statistically significant coefficient for the *Post Lawsuit* indicator, ranging from 0.66 to 0.88 percentage points, an economically significant effect. This suggests that the market assessment of deal quality is higher in the years following a lawsuit in a given industry, and is consistent with managers engaging in better M&A as a response to an increase in their perceived litigation

risk.

[Insert Table 1.4 about here]

1.4.2 Method of Payment

Myers and Majluf (1984) show that firms are more likely to acquire targets by stock if they believe that their company is overvalued. Since target shareholders can anticipate this behavior, bidders of higher value can choose cash payment in order to reveal their value to the market (e.g., Fishman, 1989; Berkovitch and Narayanan, 1990; Eckbo et al., 1990). Accordingly, Brown and Ryngaert (1991), and Martin (1996) empirically document that acquisitions of public targets paid for in stock are perceived as negative signals.

However, with increasing uncertainty about the target's value, cash offers become less appealing because targets will only accept offers that exceed their true value (Hansen, 1987; Eckbo and Thorburn, 2000; Hege et al., 2009). Moreover, other studies document that for private targets, for which uncertainty is *ceteris paribus* higher, stock acquisitions are viewed more positively (Martin, 1996; Chang, 1998; Fuller et al., 2002; Masulis et al., 2007). Besides, Masulis et al. (2007) show empirically that for subsidiary acquisitions, bidder announcements returns tend to be more positive for stock deals as well. Overall, our intuition is that if managers behave more in the interests of their shareholders, they should acquire target using the most appropriate method of payment, which reduces the risk of over-paying for a target as in agency costs.

Thus, we test whether the method of payment changes in the post-lawsuit period depending on the target type. In our model, the dependent variable equals one if at least 50% of the transaction value was paid for in cash, and zero otherwise. As we categorize three types of targets (subsidiaries, private and public), we follow Masulis et al. (2007) and choose subsidiaries as the baseline scenario and control for the target type by adding an indicator for private and public deals, respectively. The change in cash payments for (i) subsidiary targets will be measured

by the post lawsuit dummy, (ii) for public targets by the interaction term of the post lawsuit dummy and public target indicator, and (iii) for private targets by the interaction of post lawsuit and private target. We predict that industry peers of firms targeted by an M&A lawsuit will choose more appropriate payment methods, i.e., a positive coefficient on *Post Lawsuit * Public Target*, and a negative coefficient on *Post Lawsuit * Private Target*.

Table 1.5 reports results for our analysis of payment methods, where the dependent variable indicates deals for which at least 50% of the payment is in cash, and the regression specification is logistic.¹¹ In column (1), the negative coefficient on *Post Lawsuit* is statistically significant. The marginal likelihood of paying for a target in cash, ceteris paribus, decreases by 3.36 percentage points after a lawsuit. Because the average transaction value corresponds to approximately \$260m in our sample, on average, a lawsuit leads to a 8.58 million decrease in cash payments for peers' M&A, which is economically not negligible. This average reduction in the use of cash as a method of payment is consistent with Bens et al. (2012), who document that the M&A-related pressure on the probability of financial statement misstatements correlates positively with payments in cash. Moreover, an increase in stock payments is in line with the result from Arena and Julio (2014), who document that firms that are more exposed to litigation risk hold more cash in anticipation of settlements. Firms preserve their cash reserves, and in our case, are less likely to use their cash to acquire targets. The coefficients of the control variables in Table 1.5 generally carry the expected signs.

[Insert Table 1.5 about here]

In column (2), we add the interaction terms of the post lawsuit indicator and target types. The interaction terms therefore compare changes in the likelihood of the method of payment in the post lawsuit period compared to the period when no lawsuit occurred in the industry, for each target type, respectively. Compared to when no lawsuit has occurred, the significantly

¹¹We report regression coefficients in the tables. When interpreting the size of the coefficients in the text, we report the corresponding marginal coefficients. The marginal coefficients of interaction terms are calculated as the cross partial derivatives, following Ai and Norton (2003).

positive coefficient on *Post Lawsuit * Public Target* indicates that public targets are more likely to be acquired using cash, while subsidiaries and private targets are more likely to be paid for with stock. The likelihood of paying for a public target with cash increases by 4.24 percentage points after a lawsuit. In turn, for a private target, the likelihood of cash payment decreases by 7.90 percentage points. While the interaction term *Post Lawsuit * Private Target* is statistically insignificant, the sum of the interaction term and the main effect is significant. Indeed, a χ^2 test confirms that the private target interaction term and post-lawsuit main effect are jointly statistically significant at the 1% level ($p=0.0042$). Finally, the likelihood of paying for a subsidiary with cash decreases by 3.50 percentage points. We test and find that our results are similar when using a continuous method of payment variable (not tabulated). Thus, upon observing a lawsuit in the industry, acquirers change the methods of payment for each target type in such a way that suggests more appropriate methods of payment consistent with the theoretical and empirical literature.

1.4.3 Deals Characteristics

In the empirical M&A literature, certain deal characteristics have been associated with poor acquisition performance. More specifically, we consider diversifying acquisitions, target size, hostile takeovers and price-to-earnings (P/E) ratio as deal features that may be affected by peers' M&A lawsuits. First, according to Morck et al. (1990), diversifying acquisitions generally destroy shareholder value. Similarly, when studying bidder announcement returns, Masulis et al. (2007) document that diversifying acquisitions tend to be perceived as negative news by the market, even though the effect is marginally significant. Second, Fuller et al. (2002) report a negative correlation between target size and acquisition performance. In addition, Krishnan et al. (2012) find that larger transactions are more likely to be sued in the context of imminent M&A lawsuits. Third, Betton et al. (2008) document that the number of hostile deals has significantly decreased since the 1980s, and only a small fraction of unfriendly deals remain.

For a sample from the 1980s, Servaes (1991) finds that hostile takeovers are perceived as bad news by the market for acquirers. Fourth and last, firms may decide to acquire firms and structure M&A deals in order to boost their earnings per share (EPS), even if it comes at the expense of value creation.¹² Accordingly, we test whether peers of sued firms engage in M&A deals that are less likely to be value destroying, i.e., whether they are less likely to undertake diversifying acquisitions, hostile takeovers, EPS accretive deals, and to acquire larger targets.

Table 1.6 reports the results of our deal characteristic analysis. In column (1), the dependent variable is an indicator variable equal to one if the acquirer operates in a different industry than the target's, and zero otherwise. The negative coefficient on *Post Lawsuit* shows that the likelihood of diversifying acquisitions decreases after a lawsuit. The predicted marginal decrease is equal to 2.73 percentage points. Since less than half of the takeovers in our sample are diversifying, this corresponds to a 5.94% relative decrease.

[Insert Table 1.6 about here]

Furthermore, we find evidence that after a lawsuit, targets tend to be smaller, as per the significantly negative coefficient on *Post Lawsuit* (Table 1.6, column 2). In column (3), the dependent variable is an indicator equal to one if the deal attitude is not categorized as friendly in SDC, and zero otherwise. While the coefficient on *Post Lawsuit* is negative it is statistically insignificant. This may be due to the very low frequency of hostile takeovers in recent years, i.e., a power issue. Finally, we investigate the target's P/E ratio relative to the one of the acquirer, using the difference in P/E ratios as a proxy for accretive acquisitions.

Indeed, the greater the acquirer's earnings per share, the more the acquirer's P/E ratio exceeds the P/E ratio of the target.¹³ Column (4) documents that the difference in P/E ratios

¹²Lys and Vincent (1995) analyze the characteristics of AT&T's acquisition of NCR in 1991. They conclude that AT&T was willing to pay an extra of \$500 million to acquire NCR using the pooling accounting method. This change in accounting treatment had no effect on cash flow but boosted EPS by around 17%.

¹³Appendix A provides examples of shareholders' allegations in cases of ex post M&A lawsuits. The second allegation states that increasing earnings reported by the company came from accretion of revenues from past acquisitions.

is significantly smaller after a lawsuit. In particular, the difference is reduced by 8.89 after a lawsuit. This corresponds to a large relative change, since the average difference in P/E ratios in our sample is -12.65. Overall, the results in Table 1.6 suggest that, after a peer’s lawsuit, managers are more likely to make acquisitions with characteristics that are typically associated with value-enhancing deals.

1.4.4 Robustness Tests

In this section, we perform a variety of robustness tests to further rule out alternative interpretations of our main results. We first consider reverse causality concerns and potentially correlated omitted variables.

Following Bertrand and Mullainathan (2003a) and Atanasov (2013), we conduct a test to examine the potential endogeneity of M&A lawsuits. Specifically, we repeat our analysis of bidders’ returns by applying a placebo treatment at the industry level one year and two years before the actual observation of a lawsuit. Table 1.7 shows that we do not observe an effect for the pre-lawsuit period, as the coefficients on *Placebo 1 Year* and *Placebo 2 Years* are not significant. This mitigates the concern of endogeneity driving our results and suggest that the response to the incidence of M&A lawsuits was not anticipated.

[Insert Table 1.7 about here]

Next, we acknowledge that managers may change their behavior not because of a change in litigation risk *per se*, but rather because they observe a value-destroying takeover within their industry, and respond by being more careful in their target selection and due diligence efforts. The occurrence of a lawsuit could therefore simply coincide with poor acquisitions as a signal to industry peers. We repeat our analysis of deal announcement returns by adding controls for industry performance and for the quality of recent acquisitions in the industry as reported in Table 1.8. Indeed, as per the significantly negative coefficient on Industry CARs in column (2),

we find evidence that when the average deal announcement CAR for deals in the industry during the past year is relatively low, in the subsequent year, the bidder's CARs increase significantly. Similarly, if a peer undertook one of the worst takeovers in a year, defined by belonging to the lowest deciles in terms of bidder's announcement CARs per year, deals in the industry tend to be of better quality in the following year, although this effect is not statistically significant (column (3)).

Another concern would be that peer firms react upon observing other ex post signals of poor investment quality rather than ex ante signals, like peer firms' announcement returns. Goodwill impairments constitute a clear ex post signal of poor quality, as it means that previously estimated future cash flows in the form of synergies were revised downwards in light of new information. Thus, we create a variable *Goodwill Impairment* equal to one if at least one firm in the industry recorded a large goodwill impairment in the previous year.¹⁴ In column (4) of Panel A, the coefficient on *Goodwill Impairment* is positive and significant, which suggest that peer firms react to observing ex post signals in their industry. Hence, we find that firms, to some extent, may react to peers' recent underperforming acquisitions. However, across all specifications, the ex post M&A litigation indicator carries a statistically significant sign. Thus, the risk of being sued matters in addition to a potential learning effect that arises from the mere observation of ex ante and ex post signals of poor acquisitions by peers.

[Insert Table 1.8 about here]

We focus on acquisition-related lawsuits in order to identify a direct link between litigation risk and investment decisions. However, firms may react to any kind of lawsuit, due to an overall increase in the litigation risk, instead of an increase in the acquisition-specific litigation risk. To rule out this alternative explanation, we introduce an additional control variable in our

¹⁴An impairment is considered as large if (1) its amount is greater than 75% of the firm's total goodwill, and (2) if the firm's goodwill represents at least 5% of the total assets and (3) if the firm's size fall below the first tercile of the industry. This ensures that the cases represent major impairments for firms that undertook significant acquisitions in the past.

test, which is the logarithm of the number of any Rule 10b-5 or Section 11 class-action securities lawsuits in the industry (as per the ISS database). Table 1.9 displays the results. We find that our main result on abnormal returns is robust to controlling for overall industry-level litigation. This suggests that our variable *Post Lawsuit* captures a change in the perceived risk of lawsuit for investment-related reasons, and not a general shift in litigation risk.

[Insert Table 1.9 about here]

1.5 Conclusion

In this paper, we show that, after observing an ex post M&A lawsuit in the industry, managers alter their investment behavior. This is in line with the hypothesis that the risk of a lawsuit increases the perceived litigation risk, which disciplines manager's investment decisions. Therefore, securities lawsuits can provide a channel of corporate governance enforcement, and may help solve the moral hazard problem.

In particular, we find that post-lawsuit acquisitions are perceived more positively by the market, indicating better deal quality. In line with the methods of payment that are associated with better acquisitions, public targets are more likely to be paid for in cash, whereas private targets are more likely acquired with stock, respectively. Moreover, there are fewer diversifying takeovers in the industry, while, on average, the relative deal-size decreases. This suggests that acquisitions are less likely to be value-destroying in the post-lawsuit period. Overall, these results provide empirical evidence on the importance and disciplining effect of securities lawsuits.

Appendix A

We provide below several examples of allegations of *ex post* M&A-related lawsuits extracted from the ISS Securities Class Action database.

1. First, an allegation against TIBCO Inc. in 2005 would state that:

Defendants' Class Period representations regarding TIBCO were materially false and misleading when made for the following reasons: (i) TIBCO's integration of the Staffware PLC ("Staffware") acquisition was not proceeding as well as Defendants represented; (ii) that Staffware was performing well below expectations; and (iii) TIBCO did not maintain an adequate system of internal financial, operational or disclosure controls so as to reasonably assure the accuracy, completeness and veracity of the Company's public statements and representations to investors. On March 1, 2005, Defendants announced that TIBCO's results for Q1:F05 were well below guidance. In fact, shares of TIBCO were halted in after-market trading after the Company revealed that preliminary data showed that Q1:F05 revenues would reach well below the FirstCall consensus mean estimates. While Defendants had previously stated that the Staffware acquisition was substantially completed and that the integration was processing as expected.

2. Second, an allegation against Razorfish, Inc. in 2000 would state that:

The Complaint alleges as follows: Defendants misled investors, in filings with the SEC, regarding Razorfish's success in integrating recent acquisitions, particularly International Integrated Incorporated ("I-Cube"); its achievements of sharp earnings and revenue growth due to internal growth when in fact it was due to accretion of revenues and earnings from recent acquisitions; [...].

3. Third, an allegation against The Cooper Companies Inc. in 2006 would state that:

The Complaint alleges that defendants violated federal securities laws by issuing a series of

materially false statements regarding Cooper's business condition. Specifically, defendants failed to disclose that: (i) Cooper improperly accounted for assets acquired in the Ocular Sciences, Inc. ("Ocular") merger, as reported in the Proxy Statement, by misclassifying intangible assets as tangible, which had the effect of lowering amortization expense; (ii) Cooper's aggressive earnings guidance reflected the improper accounting for intangible assets and was inflated by the amount of the understated amortization expense; (iii) the merger synergies touted by defendants were unrealistic; (iv) Ocular had stuffed the channel with its Biomedics products; [...].

4. Finally, an allegation against Honeywell International, Inc. in 2001 would state that:

Defendants knowingly or recklessly disseminated materially false and misleading statements and omissions regarding the success of the merger of Honeywell International, Inc. and Allied Signal, Inc. ("Allied") and the Company's financial projections and disclosures during the first half of the year 2000. Specifically, Defendants failed to disclose that the merger was problem-ridden and not yielding operational synergies and millions in cost savings, and that the new Honeywell's business was not nearly as strong as represented and did not have nearly as strong prospects as forecast by Defendants. Furthermore, the misrepresentations and omissions by Defendants influenced the views of securities analysts and fostered an unrealistically positive assessment of Honeywell and its business, prospects and operations. As a result of such misinformation, its stock traded at artificially inflated prices throughout the Class Period.

Appendix B

Variable	Definition	Source
CAR [-3;3]	Bidders CARs over seven days	CRSP
Cash	1 if Cash/Value of Transaction $\geq 50\%$	SDC Platinum
Diversify	1 if SIC2 target \neq SIC2 bidder	SDC Platinum
Hostile	1 if Deal Attitude \neq Friendly	SDC Platinum
$PE_{acq.} - PE_{target}$	Acquirer P/E ratio minus target P/E ratio	SDC Platinum
Relative Dealsize	Value of Transaction / Size Acquiror	Compustat SDC Platinum
Size	$\ln(\text{cshot} * \text{prcc}_f)$ lagged	Compustat
MtB	$(\text{cshot} * \text{prcc}_f / \text{ceq})$ lagged	Compustat
Leverage	(lt / at) lagged	Compustat
Sales Growth	$(\text{sale} - \text{sale lagged}) / \text{sale lagged}$	Compustat
Target Private	1 if Target Public Status = Private	SDC Platinum
Target Public	1 if Target Public Status = Public	SDC Platinum
Number of Bidders	Number of Bidders	SDC Platinum
Crossborder	1 if Crossborder = Yes	SDC Platinum
Divesture	1 if Divesture = Yes	SDC Platinum
Tender Offer	1 if Tender Offer = Yes	SDC Platinum
Toehold	1 if creeping acquisition = Yes	SDC Platinum
Past Merger Activity	Number of completed deals per year	SDC Platinum
Past M&A	1 if (aqc / at) lagged > 0.05	Compustat
ROA	$(\text{oibdp} / \text{at})$ lagged	Compustat
Impaired Goodwill	Indicator if at least a firm in an industry recorded a large goodwill impairment in the previous year	Compustat
Industry ROA	Lagged mean of return on assets computed at the industry level	Compustat
Industry CARs	Lagged mean of bidders' announcement CARs computed at the industry level	CRSP
Industry low decile CARs	Lagged indicator variable equal to one if a peer firm bidder's CARs belonged to the lowest deciles of all announcement CARs in a given year	CRSP

This table defines the main dependent and control variables used in this study as well as the different data sources.

Table 1.1 – Distribution of Events per Year and Industry

Panel A: Distribution of Events per Year	
Year	No. of Lawsuits
1996	3
1997	8
1998	11
1999	3
2000	9
2001	6
2002	5
2003	5
2004	5
2005	8
2006	5
2007	2
2008	3
2009	4
2010	3
2011	9
Total	89

Table 1.1 – Distribution of Events per Year and Industry (continued)

Panel B: Distribution of Events per Industry	
Industry	No. of Lawsuits
Agriculture, Forestry, and Fishing (01-09)	2
Mining and Construction (10-19)	5
Manufacturing (20-39)	34
Transportation and Utilities (40-49)	9
Trade (50-59)	12
Services (70-89)	27
Total	89

This table presents the distribution of industries targeted by at least one ex post M&A lawsuits in a given year. Panel A shows the distribution of lawsuits per year. Panel B reports the number of lawsuits per industry. The data come from the ISS Securities Class Action database, and the cases are identified when allegations claim that managers overpromised and hid poor performance.

Table 1.2 – Descriptive statistics

Variable	N	Mean	P25	P50	P75
CAR [-3;3]	7,840	0.01	-0.03	0.00	0.04
Cash	6,246	0.86	1.00	1.00	1.00
Diversify	11,373	0.46	0.00	0.00	1.00
Hostile	11,338	0.07	0.00	0.00	0.00
$PE_{acq.} - PE_{target}$	1,603	-15.67	-30.45	-2.83	17.36
Size (\$bil)	11,373	5.12	3.42	5.04	6.77
MtB	11,373	2.49	1.18	1.93	2.94
Leverage	11,373	0.37	0.29	0.48	0.64
Sales Growth	11,373	0.12	-0.05	0.07	0.22
Relative Dealsize	11,373	0.25	0.04	0.09	0.27
Target Private	11,373	0.35	0.00	0.00	1.00
Target Public	11,373	0.19	0.00	0.00	0.00
Number of Bidders	11,373	1.01	1.00	1.00	1.00
Crossborder	11,373	0.05	0.00	0.00	0.00
Divesture	11,373	0.35	0.00	0.00	1.00
Tender Offer	11,373	0.05	0.00	0.00	0.00
Toehold	11,373	0.00	0.00	0.00	0.00
ROA	11,373	0.01	0.00	0.10	0.16

This table reports the summary statistics of the main variables used in the analysis. For each variable we report the mean, number of non-missing observations, and for the continuous variables the median, 25th and 75th percentiles. All variables are defined in Appendix B. In the upper panel, we present the statistics for the dependent variables used in this study. In the lower panel, we present the statistics for the control variables. The sample period is from 1996 to 2011.

Table 1.3 – Peer M&A Lawsuits and M&A Industry Activity

Panel A: <i>Ex Post</i> M&A Lawsuit Occurrences		
	No. of Lawsuits	No. of Lawsuits
Number of Lawsuits _{<i>t</i>-1}	0.1094*** (0.0237)	0.0512** (0.0223)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Deal Activity Controls	No	Yes
Observations	1,865	1,865
Adjusted- R^2	0.2260	0.3373

Table 1.3 – Peer M&A Lawsuits and M&A Industry Activity (continued)

Panel B: M&A Activity		
	Deal Volume	Deal Volume
Number of Lawsuits _{<i>t</i>-1}	-0.0831* (0.0482)	
Lawsuit Indicator _{<i>t</i>-1}		-0.1093* (0.0643)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Observations	1,865	1,865
Adjusted- R^2	0.6739	0.6740

This table presents regression results for occurrence of lawsuits and M&A deals. In Panel A, the number of lawsuits in an industry are estimated by the lagged number of lawsuits. In Panel B, the deal volume in an industry is the logarithm of the total transaction value of deals computed by the industry of the acquirer. Industry peers are defined at the 2-digit SIC level. All other variables are defined in Appendix B. All models are estimated using OLS. ***, **, and * indicate significance level at the 1%, 5%, and 10% level, respectively.

Table 1.4 – Peer M&A Lawsuit and Deal Announcements Returns

	CAR [-3;3]	CAR [-3;3]	CAR [-3;3]	CAR [-3;3]
Post Lawsuit	0.0066*** (0.0029)	0.0077*** (0.0030)	0.0067*** (0.028)	0.0088** (0.0035)
Size Acquiror		-0.0005 (0.0007)	0.0004 (0.0007)	0.0077*** (0.0025)
MtB Acquiror		0.1402*** (0.0772)	0.1631*** (0.0726)	0.1940* (0.1613)
Leverage Acquiror		0.0247*** (0.0065)	0.0208*** (0.0064)	0.0151 (0.0149)
Private Target			0.0107*** (0.0019)	0.0077*** (0.0024)
Diversifying			-0.0007 (0.0036)	-0.0035 (0.0044)
Merger of Equals			-0.0291 (0.0296)	-0.0023 (0.0472)
Relative Dealsize			-.0002*** (0.0000)	-0.0001*** (0.0000)
Cash Financing			0.0001*** (0.0000)	0.0001* (0.0000)
Crossborder			0.0018 (0.0115)	0.0629 (0.0455)
Divesture			0.0177*** (0.0028)	0.0119*** (0.0040)
Tender Offer			0.0262*** (0.0038)	0.0209*** (0.0061)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	No
Firm Fixed Effects	No	No	No	Yes
Observations	7,840	7,840	7,840	7,840
Adjusted- R^2	0.0167	0.0199	0.0304	0.1719

This table presents regression results for the analysis of bidder's announcement CARs. We compute 7-day CARs using the (-3,+3) window where the event day zero is the announcement date of the merger. We estimate abnormal returns with a market model using the CRSP equal-weighted return as the market return. Post Lawsuit is an indicator variable equal to one if a peer firm was subject to an ex post M&A lawsuit one or two years before the merger announcement. Industry peers are defined at the 2-digit SIC level. Standard errors are clustered at the industry level. All other variables are defined in Appendix B. All models are estimated using OLS. ***, **, and * indicate significance level at the 1%, 5%, and 10% level, respectively.

Table 1.5 – Peer M&A Lawsuits and Method of Payment for Acquisitions

	Cash	Cash
Post Lawsuit	-0.3080** (0.1192)	-0.3375** (0.1669)
Post Lawsuit X Target Public		0.5816* (0.3086)
Post Lawsuit X Target Private		-0.1218 (0.1433)
Target Public	-0.5595* (0.2976)	-0.7863*** (0.2570)
Target Private	-0.2683 (0.2635)	-0.1991 (0.2714)
Size Acquiror	0.2311*** (0.0530)	0.2319*** (0.0537)
Leverage Acquiror	0.8681** (0.4252)	0.8688** (0.4246)
MtB Acquiror	0.0002 (0.0029)	0.0004 (0.0029)
Relative Dealsize	-0.0018*** (0.0006)	-0.0018*** (0.0007)
Hostile Takeover	2.8413*** (0.3236)	2.9467*** (0.3442)
Divesture	0.7709*** (0.2622)	0.7790*** (0.2598)
Tender Offer	1.5621*** (0.2395)	1.6028*** (0.2515)
Additional Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Observations	6,246	6,246

This table presents logistic regression results for the method of payment. Post Lawsuit is an indicator variable equal to one if a peer firm was subject to an ex post M&A lawsuit one or two years before the merger announcement. Additional Controls include Sales Growth Acquiror, cross-border, diversifying and number of bidders. Industry peers are defined at the 2-digit SIC level. Standard errors are clustered at the industry level. All other variables are defined in Appendix B. ***, **, and * indicate significance level at the 1%, 5%, and 10% level, respectively.

Table 1.6 – Peer M&A Lawsuits and Deal Characteristics

	Diversify	Rel. Dealsize	Hostile	$PE_{acq.} - PE_{target}$
Post Lawsuit	-0.1383** (0.0595)	-6.9781*** (1.9334)	-0.0410 (0.1234)	-8.8936** (4.1834)
Size Acquiror	0.0710*** (0.0265)	7.6179*** (0.9305)	0.0661*** (0.0258)	2.4828** (1.1759)
Leverage Acquiror	-0.0309 (0.2557)	16.0129*** (2.6702)	-1.1726*** (0.3016)	-33.5198*** (12.7630)
MtB Acquiror	-0.0214 (0.0213)	-0.0470** (0.0220)	0.0176*** (0.0068)	16.6670 (14.4268)
Sales Growth Acquiror	0.0050 (0.0051)	0.0011 (0.0026)	-0.0055 (0.0053)	-0.2799 (0.4521)
Number of Bidders	-0.4661*** (0.1552)	27.7744** (10.5533)	0.1210 (0.2245)	0.5966 (5.9188)
Additional Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Observations	11,373	11,373	11,338	1,603

This table presents regression results for the analysis of acquisitions' methods of payment. Columns 1 and 3 have indicator variables as dependent variables and are logistic regression models, whereas columns 2 and 4 are estimated using OLS. Post Lawsuit is an indicator variables equal to one if a peer firm was subject to an ex post M&A lawsuit one or two years before the merger announcement. Other controls include target types, hostile takeover, divesture, tender offer, merger of equals, toehold and relative dealsize, when available. Industry peers are defined at the 2-digit SIC level. Standard errors are clustered at the industry level. All other variables are defined in Appendix B. ***, **, and * indicate significance level at the 1%, 5%, and 10% level, respectively.

Table 1.7 – Placebo Lawsuits and Deal Announcements Returns

	CAR [-3;3]	CAR [-3;3]	CAR [-3;3]	CAR [-3;3]
Placebo 1 Year	-0.0007 (0.0062)		-0.0062 (0.0081)	
Placebo 2 Years		-0.0047 (0.0055)		-0.0101 (0.0053)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No	No
Firm Fixed Effects	No	No	Yes	Yes
Observations	7,840	7,840	7,840	7,840
Adjusted- R^2	0.0328	0.0302	0.1892	0.1976

This table presents regression results for the analysis of bidder's announcement CARs. We compute 7-day CARs using the (-3,+3) window where the event day zero is the announcement date of the merger. We estimate abnormal returns with a market model using the CRSP equal-weighted return as the market return. Placebo 1 Year and Placebo 2 Years are indicator variables equal to one in the 1 year or 2 year period before a peer firm was subject to an ex post M&A lawsuit. Industry peers are defined at the 2-digit SIC level. All other variables are defined in Appendix B. All models are estimated using OLS and standard errors are clustered at the industry level. ***, **, and * indicate significance level at the 1%, 5%, and 10% level, respectively.

Table 1.8 – Confounding Events and Deal Announcements Returns

	CAR [-3;3]	CAR [-3;3]	CAR [-3;3]	CAR [-3;3]
Post Lawsuit	0.0077** (0.0034)	0.0069** (0.0037)	0.0067* (0.0037)	0.0072** (0.0032)
Industry ROA	0.0362 (0.0374)			
Industry CARs		-0.1276*** (0.0532)		
Industry low decile CARs			0.0022 (0.0045)	
Impaired Goodwill				0.0069** (0.0034)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Observations	7,840	7,840	7,840	7,840
Adjusted- R^2	0.0357	0.0389	0.0342	0.0394

This table presents regression results for the analysis of bidder's announcement CARs. We compute 7-day CARs using the (-3,+3) window where the event day zero is the announcement date of the merger. We estimate abnormal returns with a market model using the CRSP equal-weighted return as the market return. Post Lawsuit is an indicator variable equal to one if a peer firm was subject to an ex post M&A lawsuit one or two years before the merger announcement. Industry ROA is equal to the lagged mean ROA computed at the industry level. Industry CARs is equal to the lagged mean of bidders' announcement CARs computed at the industry level. Industry low decile CARs is a lagged indicator variable equal to one if a peer firm bidder's announcement CARs belonged to the lowest deciles of all announcement CARs in a given year. Impaired goodwill is equal to one if a firm in the industry recorded a large impairment in the previous year. Industry peers are defined at the 2-digit SIC level. All other variables are defined in Appendix B. All models are estimated using OLS and standard errors are clustered at the industry level. ***, **, and * indicate significance level at the 1%, 5%, and 10% level, respectively.

Table 1.9 – Industry-Level Litigation Risk and Deal Announcement Returns

	CAR [-3;3]	CAR [-3;3]
Post Lawsuit	0.0088** (0.0035)	0.0087** (0.0040)
Other Lawsuits	-0.0053 (0.0038)	-0.0054 (0.0035)
Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	No
Firm Fixed Effects	No	Yes
Observations	7,840	7,840
Adjusted- R^2	0.0318	0.1889

This table presents regression results for the analysis of bidder's announcement CARs. We compute 7-day CARs using the (-3,+3) window where the event day zero is the announcement date of the merger. We estimate abnormal returns with a market model using the CRSP equal-weighted return as the market return. Post Lawsuit is an indicator variable equal to one if a peer firm was subject to an ex post M&A lawsuit in the 2 years before the merger announcement. Other Lawsuits corresponds to the logarithm of the number of all 10b-5 securities lawsuits in the industry in the two year period before the merger announcement. Industry peers are defined at the 2-digit SIC level. All other variables are defined in the Appendix. All models are estimated using OLS and standard errors are clustered at the industry level. ***, **, and * indicate significance level at the 1%, 5%, and 10% level, respectively.

Chapter 2

Labor Adjustment Costs and Real Earnings Management

2.1 Introduction

Since the onset of the Great Recession, labor regulations have been brought back to the forefront of the policy debate. Labor market frictions have been blamed for firms' poor economic performance, especially in Europe. The main argument is that labor market frictions, including firing costs, deteriorate firms' competitiveness. This echoes the ongoing debate among economists on the relative costs and benefits of regulations governing dismissals.¹ This paper examines how labor market frictions in the form of dismissal costs affect managers' propensity to engage in real earnings management, and whether this has real consequences for firms.

The economics literature has recognized that the cost of establishing or severing an employment relationship is comparable to the costs associated with frictions in the physical capital market (Oi, 1962; Dixit, 1997). Dismissal costs arise from job security regulations that impose substantial constraints on firms by making it more difficult and costlier to discharge employees. I concentrate on frictions related to labor costs because such costs represent a major expense item.² From a cost accounting perspective, dismissal costs increase the rigidity of a firm's cost structure with respect to labor expenses.³

Prior literature has established that managers engage in income-increasing choices to meet some profitability targets (Degeorge et al., 1999). This behavior is motivated at least partly by some job security concerns (Fudenberg and Tirole, 1995).⁴ Theoretical studies document that managers trade-off the relative costs and benefits of the different options that they can rely on to increase earnings (Ewert and Wagenhofer, 2005; Demski, 2004).

Suppose that a manager observes that his firm's underlying economic performance is likely

¹See for example Topel (1983), Lazear (1990), and Nickell (1997).

²Labor expenses indeed range from 18% to 52% of total operating expenses across industries according to a 2013 PwC study. In comparison, capital expenditures represent, on average, 8% of total assets and 5% of total operating expenses for U.S. listed companies.

³In this paper, the term *rigidity* refers to a firm's general ability to quickly adjust the inputs of its production function, while in other recent studies it is defined as the mix of fixed and variable costs (Banker et al., 2014).

⁴Indeed, prior empirical research documents that poor corporate performance has a significant impact on executives' turnover (Warner et al., 1988; Murphy and Zimmerman, 1993)

to fall below investors' expectations. In this case, economic theory suggests that firms should terminate unproductive employees, if any.⁵ This will reduce cash outflows and increase earnings while ensuring that pay and employment levels correctly match with the firm's productivity. In lines with this argument, Dierynck et al. (2012) find that firms experiencing decline in sales terminate employees to restore firms' profitability and meet some binding legal thresholds in order to pay dividends.⁶ Next, assume that labor regulation introduces dismissal costs. Labor economics theory then predicts that firms will retain unproductive workers as long as the productivity shortfall is inferior to the present value of the adjustment costs. As a result, I first conjecture that managers will substitute cutting other discretionary costs to terminating unproductive employees to increase earnings when adjustment costs increase. Second, I predict that if managers are maximizing firms' value, cutting expenses to increase earnings instead of firing employees correspond to a move to a plausibly lower-best equilibrium, which is ultimately detrimental to the firm.

However, causality is difficult to discern. One concern is that managers' decision to terminate employees and cut expenses are simultaneously determined by managers' incentives to reach financial thresholds.⁷ To account for this endogenous relationship, I use a difference-in-differences research design and exploit state court rulings in favor of more stringent labor laws across U.S. states as a plausible source of exogenous variation in firing costs. A key advantage of this approach is that it requires only a reduced-form specification of the equilibrium level of firing costs, along with a conjecture about how this equilibrium level changes in the context of a plausibly exogenous shock in order to make a causal claim (Angrist and Krueger, 2001).

My identification strategy relies on the staggered adoption of wrongful discharge laws between

⁵This statement is valid absent of adjustment costs and assuming that firms cannot systematically adjust wages downward to match productivity levels.

⁶Similarly, IBM Inc. stated on April 28, 2013 that it will reduce its work force in some areas to meet its per-share earnings target due to sales decline. See <http://www.marketwatch.com/story/ibm-earnings-fall-as-hardware-service-sales-slip-2013-04-18>.

⁷Indeed, recent studies argue that managers deliberately lessen the rigidity in firms' cost structure to reach financial targets (Dierynck et al., 2012; Kama and Weiss, 2013).

1959 and 1998. These common laws arose from state court rulings and constitute exceptions to the traditional fire-at-will doctrine that otherwise prevails in the United States. Wrongful discharge laws, and especially the good-faith exception in its broadest sense, protect employees from termination for any reason other than a *just* cause.⁸ These laws increase managers' uncertainty about the legal outcome of the dismissals. Indeed, they generated a flood of lawsuits. For example, Dertouzos et al. (1988) find that discharge lawsuits are commonly filed in Californian state courts and that jury awards can cost firms hundreds of thousands of dollars per worker. In a more recent study, Boxold (2008) reaches similar conclusions. As a result, these laws have raised the expected legal and settlement fees associated with the termination of workers, which ultimately increase expected firing costs.⁹

To test my first hypothesis, I first estimate the amount of abnormal discretionary expenses, computed as the sum of R&D, advertising and SG&A expenses, following standard models in the real earnings management literature (Zang, 2012; Roychowdhury, 2006).¹⁰ I acknowledge that under U.S. GAAP discretionary expenses also incorporate some labor expenses. Thus, on the one hand, one might expect that labor expenses embedded in discretionary expense items will lead such expenses to display, on average, lower sensitivity to changes in economic fundamentals. On the other hand, I predict that managers will take advantage of their discretion over such expenses and cut discretionary expenses more to increase earnings after experiencing an increase in firing costs.

I find that after the adoption of the good-faith exception, firms cut abnormal discretionary expenses more relative to a control group of unaffected firms. The magnitude of the effect

⁸The reader can refer to Corbett (2005) who provides examples discusses the two well-known legal arguments (denial and pretext) used in courts to challenge termination that occurred during economic slowdown under the covenant of good-faith.

⁹Note that these laws have consequently been used as a plausible source of exogenous increase in firing costs to examine various corporate outcomes, including capital structure (Serfling, 2014) and corporate investment choices (Fairhurst and Serfling, 2014).

¹⁰I focus on real-earnings management rather than accruals for two reasons. First, recent studies argue that managers make sequential choices and first opt for manipulation of earnings through real choices (Kothari et al., 2015; Zang, 2012). The authors argue that real manipulations present a greater challenge for investors to uncover relative accruals management (I discuss this point in Section 3). Second, real choices allow managers to manage both earnings and cash flow levels.

corresponds to 1% of total assets, an economically significant amount. In my most stringent specifications, I introduce region-year or industry-year fixed effects, which enable me to account for any confounding linear and/or non-linear regional or industry trends in real-earnings management caused by unobservable factors. My results support my first hypothesis that managers substitute cutting abnormally more discretionary expenses to firing employees as a response to increase in firing costs.

To better explore the economic mechanisms behind my results, I follow Angrist and Krueger (2001) who argue that most exogenous shock settings will have a heterogeneous effect across cross-sectional tests. First, I find that the change in discretionary expenses after the adoption of the good-faith exception is concentrated among relatively low-unionized firms. This is consistent with studies in economics arguing that wrongful discharge laws do not pertain to employees already covered by collective agreements (Miles, 2000). Next, I conjecture that firms' reaction to the change in labor laws is conditioned by their incentives to report higher earnings. In lines with this argument, I find that my results are more pronounced for firms and managers that face capital market and/or compensation incentives to report higher earnings. Furthermore, I perform an endogeneity test and I fail to find that the additional cuts in discretionary expenses by firms in adopting states was anticipated.

Next, I find that firms that cut discretionary expenses below the predicted level in response to the adoption of the good-faith exception experience a decline in market shares in the next two years. This supports my second hypothesis that cutting discretionary expenses instead of terminating unproductive employees is detrimental to firms' competitiveness.

The central contribution of this paper is to provide empirical evidence that the lack of flexibility with respect to labor adjustment costs lead firms to cut other expenses. This paper contributes to the literature on the trade-off between various techniques of earnings manipulations (Zang, 2012; Ewert and Wagenhofer, 2005). To my knowledge, I provide the first empirical and

plausibly causal evidence that managers substitute various real choices to manipulate earnings based on their relative costs and benefits.

More broadly, my findings contribute to the vast literature examining the determinants of earnings management (Dechow et al., 2010a). In particular, my paper is related to the growing body of literature that investigates how labor characteristics affect firms' information environment.¹¹ These papers focus mostly on managers' choices to reduce employees' bargaining power. I depart from this literature and document that, likely unexpectedly, labor regulation shape reporting incentives when managers have incentives to increase reported earnings.

My results also contribute to the debate as to whether cutting discretionary expenses below the predicted level has negative (Kothari et al., 2015; Begley, 2014) or positive implications (Beneish et al., 2014; Gunny, 2010) for firms' future performance. I argue that prior mixed findings must be interpreted based on managers incentives to cut expenses and increase earnings in a given period (i.e., to mislead or inform investors). In lines with my prediction that labor regulation lead firms to a lower-best equilibrium, I find that firms' competitiveness is reduced when they substitute cutting expenses to firing employees.

The rest of the paper is organized as follows. Section 2 introduces the institutional background. Section 3 develops the hypotheses. Section 4 describes the sample and research design. Section 5 presents the results, and Section 6 concludes.

2.2 Institutional Framework

It is empirically challenging to test my main hypothesis that managers will substitute cutting other costs to terminating employees to increase reported earnings. Specifically, there is a plausible simultaneity concern. Indeed, prior research finds that firms experiencing decline in

¹¹See for example Aobdia and Cheng (2014); Dou et al. (2014); Bova et al. (2013); Matsa (2010); Hilary (2006); D'Souza et al. (2000); DeAngelo and DeAngelo (1991).

demand do terminate employees and that it helps them meet some earnings targets (Dierynck et al., 2012). Other studies also provide evidence that firms lessen the overall rigidity of their firms' cost structure to reach certain profitability thresholds (e.g., Kama and Weiss, 2013). As a result, firing decisions and costs structure presumably respond to the same set of underlying incentive and are thus, at least partially, simultaneously determined by managers. This would lead to plausibly spurious inferences absent a structural model.

To account for the endogenous relation between firing decisions and other features of firms' cost structure, I rely on the staggered adoption of state wrongful discharge laws in the United States over three decades, which unexpectedly increased firms' firing costs. A key advantage of this approach is that it requires only a reduced-form specification of the equilibrium level of firing costs, along with a conjecture about how this equilibrium level changes in the context of a particular exogenous shock (Angrist and Krueger, 2001).

Since the 19th century, the original common law rule for dismissal of employees is the fire-at-will doctrine. Under this type of contractual relationship, an employee can be dismissed by an employer for any reason, without warning and at no cost. However, since the 1970s, state courts have ruled in favor of so-called wrongful discharge laws that specify exceptions to the common law fire-at-will doctrine. There are three main exceptions: public-policy, implied-contract and good-faith.¹²

The public-policy exception was first recognized by the California Supreme Court in 1959. By 1999, forty-three states had adopted this widespread exception. It prevents termination for reasons that violate a given state's public policy, such as refusing to break the law upon the request of the employer. Autor et al. (2007) argue that courts recognize only certain violations of legal policies, thus limiting the constraints on employers and ultimately the scope of this exception.

¹²For more on wrongful discharge laws, see Miles (2000), Autor (2003), Kugler and Saint-Paul (2004), Autor et al. (2006) and MacLeod and Nakavachara (2007).

The implied-contract exception prevents workers' termination in cases of an implicit employment agreement between an employer and an employee. For instance, the implied-contract exception considers unwritten promises or the existence of an internal personnel policy handbook to assess the existence of an implicit contract. This exception has been extended to industry practices and the employee's tenure or promotion history within a company. By 1999, forty-one states had recognized this exception. However, legal scholars also argue that this exception is not particularly binding, as firms can include disclaimers in their handbook to preserve the use of the employment-at-will doctrine (Autor et al., 2007; Miles, 2000).

The good-faith exception was adopted by thirteen states between 1974 and 1999. It represents the most significant departure from the fire-at-will doctrine. It was first recognized to prohibit terminations made in bad faith. The case of *Fortune v. National Cash Register Co.* in the state of Massachusetts in 1977 constitutes an example in which a firm wrongfully terminated a salesman right before the payment of a substantial commission. However the jurisprudence evolved such that ultimately the scope of the exception is large, as it can be interpreted as preventing any termination without just cause (Kugler and Saint-Paul, 2004).

The court rulings in favor of the good-faith exception constitute a well-suited quasi-natural experiment setting to investigate my research question for several reasons. First, prior research in the field of corporate finance documented that the good-faith exception is the most influential of the three exceptions for U.S. listed firms (Acharya et al., 2014; Serfling, 2014).

Second, the precedent-setting court rulings were unexpected while exhibiting substantial variation in the timing of their implementation, which allows the implementation of a powerful identification strategy (Bertrand et al., 2004b). Contrary to laws that are passed by governments, court rulings are less likely to be subject to lobbying activities and firms less likely to anticipate such regulatory changes.

Third, as discussed in details in the next section, economic theory predicts that labor ad-

justment costs affect hiring and firing decisions, such that firms retain unproductive employees in bad times and require a higher productivity to hire in good times (Blanchard and Portugal, 2001). Empirically, Autor et al. (2007) document that the adoption of the good-faith exception leads to a reduction in annual employment fluctuations in adopting states. This confirms my assumption that firms terminate employees less after the adoption of the good-faith exception.

Fourth, Autor et al. (2007) note that the adoption of the good-faith exception generated a flood of lawsuits in adopting states and increased the uncertainty and expected costs of discharging workers. That is, the expected firing costs increase because firms anticipate that firing decisions are more likely to be challenged in courts, even for economic reason¹³, and/or because direct firing costs, such as the time spent to set-up the potential terminations increase. In lines with this argument, Dertouzos et al. (1988) examine the outcome of legal actions filed under the good-faith exception in California and find that plaintiffs (i.e., the employee) won in 68% of cases, with an average individual settlement amount of \$0.656 million. Similar conclusions are drawn by Boxold (2008), leading 46% of surveyed listed companies to express concerns regarding financial losses arising from such legal actions.¹⁴

2.3 Hypothesis Development

In this paper, I am interested in understanding some unintended implications of U.S. state labor regulations that hinder the ability of firms to flexibly adjust their labor levels. I develop my hypothesis using two streams of literature.

First, there is a body of theoretical research that documents how job security concerns and compensation incentives lead executives to manage earnings (Acharya and Lambrecht, 2015; Holmstrom, 1999; Demski, 1998; Fudenberg and Tirole, 1995; Lambert, 1984). That is, man-

¹³Corbett (2005) discuss the legal mechanisms under which terminations that occurred after economic slow-down can be challenged in courts under the good-faith exception in the case of Montana.

¹⁴This point is mentioned by Serfling (2014), who refers to the Chubb 2012 Public Company Risk Survey, entitled "U.S. Public Companies' Perceptions of Risk, and Their Risk Mitigation Strategies."

agers take costly actions to increase reported income when income is relatively low and to decrease income when income is relatively high compared to financial markets' expectations. Indeed, there is a large empirical literature that relates CEO turnover to poor corporate performance (Murphy and Zimmerman, 1993; Warner et al., 1988; Weisbach, 1988). In addition, prior studies also find that managers opt for income-increasing choices to meet earnings-based bonus thresholds (Healy, 1985).

Second, there is an important literature in labor economics that examines the role of labor adjustment costs. In a standard competitive model of the labor market, employment protection through the form of dismissal costs correspond to mandatory benefits that can be undone by Coasean bargaining such that employment levels are unchanged while wages fall to cover exactly the cost of the benefit (Lazear, 1990). However, when firing costs cannot be undone by Coasean bargaining, they function as a tax on separations. Specifically, firms will find it optimal to retain unproductive workers in the short-run as long as the present value of their productivity shortfall is smaller than the additional adjustment cost. As a result, larger adjustment costs lead to reduced employment volatility. That is, firms hire less in upturns and fire less in downturns (Lafontaine and Sivadasan, 2009; Autor et al., 2007). Consequently, firms that experience demand and/or productivity shocks will retain unproductive employees more when adjustment costs are larger, which ultimately increase costs and decrease earnings.

Next, I assume that managers privately observe the economic performance of their firm in a given period and adjust the features of their production function accordingly. Hence, absent adjustment costs and assuming that firms cannot fully adjust wages downward to individual productivity level, standard labor economics theory suggests that in a first-best equilibrium, firms will terminate unproductive employees. This allows firms to save on wages, which increases operating cash flows and earnings (Dierynck et al., 2012). This also ensures that wages correspond to marginal productivity levels.

If labor regulation imposes increases in adjustment costs to firms, they will depart from the optimal equilibrium and start retaining unproductive employees. As a result, managers will have to rely on other types of actions to increase reported earnings. Prior theoretical studies reveal that managers trade off the relative costs and benefits among second-best income-increasing options (Ewert and Wagenhofer, 2005; Demski, 2004).

Empirical studies confirm that earnings management can occur through two distinct channels: accruals management and real activities management. However, recent studies suggest that the timing of the different options is not independent and that managers sequentially first engage in real earnings manipulation and rely on accruals manipulation based on the realized amount of real manipulations (Zang, 2012). That is, managers preferred second-best option seems to be to alter firms' operations to improve earnings. This finding is confirmed by a recent paper by Kothari et al. (2015) in the context of seasoned equity offerings. The authors argue that real manipulations present a greater challenge for investors to uncover than that of accruals management. Furthermore, they argue that accruals manipulations can impose larger costs on firms, managers, and auditors via regulatory investigations, restatements, and personal penalties. In addition, in anonymous surveys (e.g., Graham et al., 2005), managers indicate a preference for real activities-based earnings management. Therefore, I state my first hypothesis as followed:

Hypothesis 1. *After an increase in labor adjustment costs, managers are more likely to cut other form of expenses to increase earnings.*

This first directional hypothesis does not come without tension. Indeed, an important channel through which labor market rigidity could affect firms would be by impeding reallocation of resources within firms. Prior literature documents that increases in labor adjustment costs are associated with increases in the persistence of labor decisions (e.g., Lafontaine and Sivadasan, 2009). That is, labor costs become more sticky. In addition, Banker et al. (2013) examine how

labor regulation affect costs features. They use a cross-country setting and find that stricter employment protection is, on average, associated with more sticky costs¹⁵. Under this framework, it is plausible that changes in adjustment costs across U.S. states would cause all costs to become more sticky. For instance, it could be that if firms are not able to fire employees, they are also not able to cut other expenses tied to labor costs, leading costs to be, on average, more sticky after the adoption of the good-faith exception. If so, firms should exhibit level of discretionary expenses that are above the predicted level relative to a control group of firms that do not experience an increase in adjustment costs. However, my prediction differs from that of Banker et al. (2013) because instead of looking at operating costs in general, I concentrate my analyses on discretionary expenses over which managers have plausibly greater discretion. As a result, my hypothesis predicts that managers will rely more on cutting discretionary expenses when labor adjustment costs increase.

The next natural question is to investigate whether changes in real manipulations caused by changes in labor regulations is detrimental or beneficial to firms. Recent empirical research in financial accounting has started to examine the consequence of real activities manipulations and find mixed evidence.

For instance, Gunny (2010) finds that firms that just meet earnings benchmarks by engaging in real activities manipulation have better operating performance in the subsequent three years than do firms that do not engage in real activities manipulation and miss or just meet earnings benchmarks. Similarly, Beneish et al. (2014) find that under certain circumstances, income-increasing manipulations is associated with higher future earnings, cash flows and stock returns. On the other hand, Bhojraj et al. (2009) show that firms that beat analyst forecasts by using real earnings management have worse operating performance and stock market performance

¹⁵It is not obvious that this result applies to my setting because their findings could be driven by unobservable, time-varying country characteristics. However, recall that prior labor studies find that employment level became less sensitive to changes in demand after the adoption of the good-faith exception (Autor et al., 2007). I discuss this point in greater detail in Section 5.4

in the subsequent three years. Recently, Kothari et al. (2015) document that real earnings management is closely associated with post-SEO stock market under-performance.

Prior mixed findings on the consequences of real earnings management can partly be explained by the heterogeneity of settings used by researchers. Indeed, the consequences of such manipulations are likely to depend on managers' incentives or intentions to inform or mislead market participants as discussed in Beneish et al. (2014). In my setting, I assume that in the pre-regulation equilibrium, managers will prefer to terminate unproductive employees and that they opt for cutting expenses over the predicted/optimal level as a response to increases in labor adjustment costs. As a result, cutting expenses might distort firms' investments in innovation (R&D) and organizational capital (SG&A), leading to negative consequences. Therefore, I state my second hypothesis as follows:

Hypothesis 2. *After an increase in labor adjustment costs, firms that cut their discretionary expenses over the predicted level experience decline in market shares in subsequent years.*

This prediction is similar to that in Begley (2014) who finds that firms cutting expenses for credit rating incentives experience negative performance and valuation consequences. However, this prediction assumes that firms exhibit optimal level of expenses in the pre-regulation period. An alternative prediction would be that cutting expenses will be followed by corporate improvements if this corresponds to cutting organizational slack that existed because of agency concerns.

2.4 Sample Selection and Identification Strategy

2.4.1 Sample Selection and Measures

Sample Selection

I use the coding of wrongful discharge laws from Autor et al. (2006) as in Serfling (2014). The authors provide a relevant coding as their data rely on the first major precedent-setting court decision in each state and for each exception separately. In my analysis, I also account for reversals in the jurisprudence. Accordingly, if the adoption of an exception is reversed in a higher court, this state is not coded as treated anymore. For example, the state of Oklahoma adopted the good-faith exception in 1985, but it was then reversed in 1989. In my sample, the *Post GF* variable is coded one only for the 1985 - 1988 period. Appendix A reports the year when each state passed the three exceptions and their reversals, if any.¹⁶

My sample encompasses the years 1967 - 1999. My sample starts five years before the ruling in 1972 by a court in California in favor of the implied-contract exception which corresponds the second-earliest adoption of a wrongful discharge law across U.S. states. This time period allows me to identify firms' behavior before and after the adoption of such exceptions. The sample ends in 1999 as in Autor et al. (2006) which is before the enactment of the Sarbanes-Oxley Act (SOX) in order to fix the legal environment with respect to accounting manipulation. Indeed, Cohen et al. (2008) documents that after SOX firms sharply reduced accrual manipulation and rely more on real actions to increase reported earnings. The last event in my sample occurred in 1998, when the state of Louisiana passed the good-faith exception. In my sample, 18.67% of the firm-year observations correspond to firms operating in states that have adopted the good-faith exception. This proportion is consistent with recent studies using wrongful discharge laws as an

¹⁶Since reversals only occur twice in states where a relatively small number of firms are headquarter, I am not able to exploit these reversals in my empirical analyses. However, my results are also robust to exclude states with reversals in labor laws from the sample.

identification strategy in corporate finance, including Acharya et al. (2014) (15.6%) and Serfling (2014) (17%).

Figure 1 presents the graph of the number of states with the good-faith exception in place in a given year. It confirms that the adoptions are staggered over time. This characteristics suggests that my results are unlikely to be driven by confounding events. Indeed, confounding events would need to affect states at different points in time while systematically introducing a bias in favor of more abnormally low discretionary expenses in regulated states.

I follow recent studies that investigate the effect of a change in labor laws on corporate outcomes and first assign firms to a state on the basis of the company's headquarters location according to the COMPUSTAT database (Serfling, 2014; Agrawal and Matsa, 2013). As discussed by Heider and Ljungqvist (2014), the COMPUSTAT database reports only firms' current location, creating a measurement error that is likely to attenuate the results. I modify my sample using their procedure for the 1988 - 1999 period.¹⁷

Measure of Earnings Manipulations

In this section, I describe the construction of my main left-hand side variable that captures earnings manipulations based on real actions. Specifically, I follow the seminal work by Roychowdhury (2006) and Zang (2012) and I estimate the normal level of discretionary expenditures using the following empirical model:

$$\frac{Discretionary\ Expenses_{i,t}}{Assets_{i,t-1}} = \alpha_0 + \alpha_1 \frac{1}{Assets_{i,t-1}} + \alpha_2 \frac{Sale_{i,t-1}}{Assets_{i,t-1}} + \alpha_3 ROA_{i,t-1} + \epsilon_{i,t} \quad (2.1)$$

In equation (2.1), *Discretionary Expenses* corresponds to discretionary expenditures com-

¹⁷Heider and Ljungqvist (2014) extract the historical headquarter's location for each fiscal year using information contained in firms' 10-K and 10-Q filings using the SEC's EDGAR service and Thomson Research. Thus, my sample does not take into account any change in the location of a firm's headquarters occurring before 1988. Such changes are still likely to generate an attenuation bias in my estimates.

puted as the sum of R&D, advertising, and SG&A expenditures in year t ; *Assets* is total assets; *Sale* is net sales; *ROA* is the return on assets computed as operating income before depreciation over total assets; and i and t index the firm and the year, respectively. I estimate equation (2.1) annually for each industry (defined using two-digit SIC codes) with at least twenty observations. I use the residuals from the estimation of equation (2.1) as a proxy for abnormal discretionary expenditures. I follow Zang (2012) and multiply the residuals by minus one. I also trim the residuals at the 1st and 99th percentiles to reduce the impact of outliers. Higher values indicate greater amounts of discretionary expenditures cut by firms, which ultimately increase reported earnings.

A key challenge in U.S. financial statements is that the amount of labor expenses cannot be observed independently. Indeed, labor expenses are embedded in various expense items in the income statement. In this paper, I do not examine abnormal production costs. Indeed, my hypothesis is built on the assumption that labor expenses are more fixed in the short-run in response to state adoptions of the good-faith exception. It causes firms to retain unproductive employees, leading ultimately to higher production costs. This has been confirmed by Autor et al. (2007) find that firms fire employees less after the adoption of the good-faith exception.

In this paper, I focus on abnormal level of discretionary expenses for several reasons. First, discretionary expenses represent expenses that are plausibly less labor intensive than COGS and thus less likely to be affected by stringent labor laws.¹⁸ For example, advertising expenses correspond to outsourced advertisement campaigns in which contracts are unlikely to be influenced by wrongful discharge laws. Thus, managers can presumably cut such expenses to meet earnings threshold if it is more costly to terminate unproductive employees.

Furthermore, prior studies document that managers can easily forego or postpone new R&D projects (Bens et al., 2002; Bushee, 1998). Next, prior research also suggest that SG&A expenses

¹⁸Recall that under my alternative hypothesis all costs should be less responsive to changes in fundamentals in the post regulation period. This could be the case because these discretionary expenses also carry some labor expenses and because some expenses might mechanically arise in the presence of employees

correspond to discretionary expenses (Roychowdhury, 2006) as well as to organizational capital (Begley, 2014) since they include expenses such as employees' training expenses. Thus, managers could plausibly and instantly substitute cutting employees' training expenses to firing employees when firing costs increase.¹⁹

Finally, I also concentrate on discretionary expenses rather than production costs or cash flows because managers seem to rely first on abnormal level of discretionary expenses relative to manipulating cash flows or production costs to increase reported earnings. Indeed, using a plausibly causal setting, Irani and Oesch (2014) find that managers respond to an increase in information asymmetry by manipulating earnings through real activities solely using discretionary expenses. Similarly, Zang (2012) only reports weak evidence that firms twist reported earnings through production costs manipulation to meet salient thresholds.

Additional Variables

I first build a set of firm control variables previously identified as determinants of real earnings manipulations. Specifically, following Zang (2012) I include firm size, market-to-book ratio, and financial distress. The vast literature on earnings management documents many additional determinants (Dechow et al., 2010a; Healy and Wahlen, 1999b). However, since my sample starts in 1967, several determinants computed using the statement of cash flows are not available before the enactment and implementation of SFAS 95 for fiscal year 1988 (e.g., proceeds from debt and equity issuance). Some other information, such as the name of the audit company, is missing in the COMPUSTAT database for the first decades of my sample period. I voluntarily do not include firms' profitability since lagged return on assets is already included in the first stage of the model when I estimate abnormal discretionary expenses.²⁰

¹⁹I acknowledge that managers may have less discretion over SG&A expenses (that are presumably more fixed) relative to R&D and advertising expenses. I explicitly account for that in the robustness test section. However, note that if managers have no discretion over SG&A, this empirically goes against finding support in favor of H1.

²⁰My results are robust to including contemporaneous return on assets in my specifications instead of lagged RROA.

One potential concern is that both the adoption of the good-faith exception and the change in reporting practices are driven by states' local economic situations. To rule out this omitted-variable concern, I further include the growth rate of states' GDP as a control variable. I obtain the data from the Bureau of Economic Analysis. I do not include the state unemployment rate in my main regressions as the data are only available since 1976 from the Bureau of Labor Studies. However, I find similar results if I include this variable in my models and backfilled the missing years with the most recent year available at the state level.

Table 2.1 displays the summary statistics for all dependent and independent variables used in this paper. I winsorize all variables at the 1st and 99th percentiles to eliminate the impact of outliers in my multivariate tests. Regarding the abnormal discretionary expenses, the sample mean is close to zero while the median is 0.0347 and the standard deviation 0.2803. The 25th and 75th values are respectively -0.0876 and 0.1587. Those figures are comparable to that of the main sample of recent studies, such as Zang (2012) and Huang et al. (2015).²¹ The independent variables in my sample are similar to that in Serfling (2014) who examines the impact of wrongful discharge laws on firms' capital structure over a similar time period.

[Insert Table 2.1 around here]

2.4.2 Empirical Specification

I use unexpected court rulings that impose more stringent labor laws as a source of plausibly exogenous variation in dismissal costs (Acharya et al., 2014).²² It allows me to identify the causal impact of increases in firing costs on firms' choices with respect to real earnings manipulations.

²¹Indeed, the sample median and standard deviation are, 0.0567 and 0.9324, respectively in the main sample of the Zang (2012) study. In Huang et al. (2015), these values are 0.031 and 0.284, respectively. The large standard deviation in Zang (2012) is due to the fact that she reports unwinsorized value for her main sample.

²²The staggered pattern of adoption is depicted in Figure 1.

Specifically I estimate the following model:

$$Abnormal\ Discr.\ Exp_{ijt} = \beta_0 + \beta_1 Post\ GF_{jt} + Controls_{ijt} + \alpha_i + \gamma_t + \epsilon_{ijt} \quad (2.2)$$

In this model, i indexes the firm, j indexes the state in which the firm's headquarter is located and t indexes time. The dependent variable, *Abnormal Discr. Exp*, corresponds to the residuals from the estimation of expected level of discretionary expenses. The independent variable *Post GF* takes the value of one if a given state j has a good-faith exception in place in year t , and zero otherwise.²³

Equation (2.2) essentially represents a difference-in-differences specification that is similar to the one in Bertrand and Mullainathan (2003b).²⁴ This research design is powerful for drawing causal inferences (Bertrand et al., 2004b). Consequently, the accounting literature has recently begun to make use of staggered changes in regulation, in both single-country (Armstrong et al., 2012; Dou et al., 2013; Burks et al., 2013) and cross-country studies (Christensen et al., 2013; Hail et al., 2014). In this baseline model, α_i denotes firm fixed effects and γ_t year fixed effects. Firm fixed effects deal with firm-level time-invariant omitted variables. Year fixed effects account for unobserved heterogeneity that varies across time (e.g., macroeconomic shocks).

In this model, the coefficient on β_1 is the difference-in-differences estimator. It captures the change in discretionary expenses for treated firms after the adoption of the good faith exception relative to a control group of firms located in states where courts did not rule in favor of more stringent labor laws in the same year. I expect β_1 to be positive so that firms facing more stringent labor exhibit relatively more abnormal cutting of discretionary expenses. As pointed by Gow et al. (2015), there is a need to further discuss the validity of quasi-natural experiment

²³Recall here that I multiplied the residuals by minus one to follow Zang (2012). As a result, according to my first hypothesis, I expect the coefficient on β_1 to be positive and statistically significant if firms cut discretionary expenses below predicted levels to increase earnings after increases in firing costs. Alternatively, I expect β_1 to be negative and statistically significant if firms

²⁴I do not include a *Treatment* variable in the model since the coefficient would be subsumed by the firm fixed effects due to collinearity. This approach is also known as the generalized difference-in-differences design.

settings in accounting research in order to make plausibly causal claims. A common challenge to difference-in-differences design that I cannot rule out is that an unobserved factor is affecting both the assignment to the treatment group and the change in real earnings manipulations. In this particular setting, the risk of confounding factors seems low since studies in labor economics could not predict the timing nor the reasons behind the ruling in favor of the good-faith exception by some state courts. As a result, the assignment to the treatment group seems fairly random. This supposes again that firms' location of headquarter was not driven by expected changes in labor regulation.

Since I include firm fixed effects in the model, the estimation of the coefficient on β_1 requires to have observations before and after the change in labor law for a given firm. It ensures that my results are not driven by firms who entered the sample after the change in law and that could decide where to locate their activities and headquarter while taking labor regulations into account. However, I acknowledge that firms may respond to more stringent labor regulations by altering their production function. For instance, they could rely more on outsourcing labor to avoid being tied by stringent regulations or choose to relocate their operations to other states.²⁵ As a result, I acknowledge that the coefficient on β_1 will capture the causal impact of labor laws on real earnings management under the assumption that the laws did not cause other unobservable changes to treated firms that would be associated with real earnings manipulations.

Since the change in labor laws is defined at the state level, I cluster standard errors by state.²⁶ This clustering method accounts for potential time-varying correlations in omitted variables that affect different firms within the same state (Bertrand et al., 2004b). I further follow Bertrand and Mullainathan (2003b) and replace the year fixed effects by either region-year or industry-year fixed effects to phase out the effects of regional or industry trends that are

²⁵However, note that Miles (2000) fails to find an effect of wrongful discharge laws on state employment level, suggesting that firms did not responded to the rulings by massively relocating their activities. One plausible explanation is that firms would incur large industrial, organizational and political costs to relocate their workforce in other states.

²⁶I find similar results if I cluster the standard errors both at the state and year levels, or at the firm and year levels (Petersen, 2009; Gow et al., 2010; Thompson, 2011).

contemporaneous with the passage of the wrongful discharge law itself.²⁷ For example, when I include industry-year fixed effects, I compare firms within the same industry at a given point in time, which rules out the effect of any other factors that do not vary within industry-year, such as investment opportunities or other types of regulation.

Prior studies in the earnings management literature usually follow two steps (e.g., Zang, 2012). First, the authors identified a sample of suspect firms. To do so, they rely on firms that suspiciously just beat or meet salient thresholds, such as analysts' forecasts or last year earnings. Next, such studies formally test whether firms in the *suspect* group exhibit larger amount of manipulations. In this paper, I rely on another approach and compare the average amount of real earnings manipulation pre- and post regulation compared to a control group. Since managers respond to specific incentives, I ensure the validity of my findings by showing that my results are pronounced when managers face capital markets or compensation incentives. My approach does not require to obtain data on analysts or management forecasts that are not available for my sample period. More importantly, it does not require to make an assumption about what managers' threshold actually is. Indeed, a recent study by Kim and Yang (2014) finds that using historical trends or previous year's performance targets to predict current year is inaccurate.²⁸

2.5 Results

2.5.1 Baseline Results

In this section, I apply my estimation strategy to test my main hypothesis that increases in the rigidity of labor costs will cause firms to rely more intensively on cutting discretionary expenses

²⁷I use the nine divisions as defined by the U.S. Census Bureau. I find similar results if I use the four U.S. regions instead. Industry is defined using two-digit SIC codes.

²⁸Specifically, they find distortions in earnings distribution when using the exact thresholds of targets set in managers' compensation contracts but failed to find any distortion when using predicted thresholds.

more to increase earnings. Table 2.2 displays the results of the baseline model.

[Insert Table 2.2 around here]

In column (1), the coefficient on *Post GF* is positive and statistically significant at the 1% level. This indicates that, on average, firms display lower level of abnormal discretionary expenses after the adoption of the good-faith exception and relative to a control group of firms that do not experience a change in labor laws. This also support my main hypothesis that firms are more likely to cut discretionary expenses below the predicted level as a response to more stringent labor laws. Recall that under the alternative hypothesis, more rigid labor costs would cause all other costs to be also more fixed. As a result, I should have observed a negative coefficient on *Post GF* indicating that, for a given level of past sales, firms exhibit higher level of discretionary expenses. On the contrary, I provide evidence that firms substitute cutting discretionary expenses to firing employees in order to increase earnings in the post-regulation period.

The magnitude of my effect seems economically significant. Indeed, the coefficient in column (1) suggest that firms cut discretionary expenses more by 1.5% of total assets after the change in labor laws relative to unaffected peers. Since the mean of discretionary expenses expressed as a fraction of total assets is equal to 0.31, this represents a 4.8% reduction in discretionary expenses.

In column (2), I add several firm-level and state-level controls as discussed in the previous section. The magnitude of the coefficients on *Post GF* remains stable and statistically significant at conventional levels. This suggests that the observed differences in abnormal discretionary expenses cannot be fully explained by a change in firms' or states' characteristics. Specifically, the coefficient on *Size* is positive and statistically significant in two out of three specifications and the coefficient on *Market-to-Book* is negative and statistically significant across all specifications. The signs of these coefficients are in lines with that of the main sample in Zang (2012) who

finds that larger firms exhibit lower level of abnormal discretionary expenses while firms with higher market-to-book ratio display, on average, higher level of discretionary expenses. The coefficient on *Z-score* is negative and statistically significant, suggesting that firms in better financial health display lower level of abnormal discretionary expenses.²⁹ The coefficient on *State GDP Growth* is not statistically significant across all specifications.³⁰

In columns (3) and (4), the results continue to hold when I replace the year fixed effects with industry-year and region-year fixed effects, respectively. This suggest that my results are not driven by unobservable trends at the industry or region level, such as other growth opportunities or other form of regulation, respectively.

2.5.2 Cross-Sectional Results

In this section, I perform several cross-sectional analyses to ensure the validity of my baseline results. Indeed, Angrist and Krueger (2001) argue that most exogenous shock settings will have a heterogeneous effect across affected subjects. That is, if my main hypothesis is true, it should affect firms differently in predictable ways.

[Insert Table 2.3 around here]

First, I explore the role of organized labor. The legal literature argues that wrongful discharge laws do not apply to unionized workers. Indeed, collective bargaining agreements usually contain provisions that are more binding than the jurisprudence developed around wrongful discharge laws. Thus, the adoption of the good-faith exception constitutes a “no event” for unionized firms

²⁹Note that this particular finding differs from that in Zang (2012) who finds a positive coefficient. This difference might be due to at least two factors. First, my research design varies since I include firm fixed effects in my model, so that the estimates capture the effect of a variable after demeaning both the left hand side and right hand side variables. Second, the sign on financial distress in the Zang (2012) paper is only displayed for the relatively small sample of firms that are suspected of earnings manipulations and not for the entire COMPUSTAT sample as for the effect of size or market-to-book.

³⁰In untabulated tests, I also interact the four control variables with the *Post GF* indicator variable. This allows for a non-linear relation between such controls and abnormal discretionary expenses pre and post regulation. My results remain unchanged. Econometrically, the benefits of this approach are not clear. On the one hand, it can take into account the fact that some observable variables affect discretionary expenses pre and post regulation. However, Gow et al. (2015) explains that if these covariates are affected by the treatment, it can bias the difference-in-difference estimator.

and I subsequently expect the change in accounting manipulation to be concentrated among non-unionized firms. To test this mechanism, I define *Union* as a dummy variable that equals one if a firm belongs to an industry that had at least 25% of its workforce covered by a collective agreement in 1983, as constructed in Matsa (2010).³¹ Furthermore, defining unionization at the industry level is appropriate since economic studies suggest that the threat of unionization at the industry level has an impact on firms' choices that is, on average, three times larger than the one caused by actual unionization rate (Bronars and Deere, 1991).

Table 2.3 presents the results. The coefficient on *Post GF X Union* is negative and statistically significant at the 10% level across all specifications. More importantly, the sum of the coefficient on *Post GF* and *Post GF X Union* is not statistically significant. For example, in column (2), this sum is equal to 0.0048 with a p-value of 0.583. This indicates that I fail to find a statistically significant response to the change in law for firms in unionized industries. Consequently, this suggests that my results are concentrated among firms with a relatively low union intensity. Note that I do not include the non-interacted *Union* variable since it is subsumed by the firm fixed-effects in the model.

Next, I examine several situations where firms/managers have specific incentives to manipulate earnings upward. If my story is true, then firms experiencing larger firing costs should rely more on cutting other discretionary expenses to increase reported earnings in those specific situations. Table 2.4 displays the results.

[Insert Table 2.4 around here]

I first consider the case of firms that are likely to issue debt in the following year. To identify such firms, I define *Financing* as a dummy variable that equals one if a firm's fraction of long-term debt due in one year over total debt and equity falls above the median of the year,

³¹Industry is defined using the two-digit SIC codes. I obtain the data in Table AI from the appendix of Matsa (2010). His data on union coverage is from Hirsch and Macpherson (2003). While this dataset is static, the year 1983 is a reasonable point in time for my study, as the adoption of the good-faith exception started in the late 1970s and continues mostly through the 1980s.

and zero otherwise. The intuition here is that debtholders willingness to lend, as well as the terms of the lending contract are a function of firms' economic performance and its ability to generate operating cash flows that will be used to repay the debt. Prior studies document a negative relation between loan interest rate spread and the financial performance of borrowers (Machauer and Weber, 1998). Thus, firms that are not able to reduce their workforce to reduce cash outflows before issuing debt may opt for cutting other discretionary expenses to increase reported income and operating cash flows. In column (1), the coefficient on *Post GF* is positive and statistically significant at the 10% level. Furthermore, the coefficient on *Post GF X Financing* is also positive and statistically significant at the 10% level. This indicates that, as expected, the results are more pronounced for firms that are about to issue long-term debt.

Next, I examine whether the change in cutting discretionary expenses below the predicted level is driven, at least partially, by compensation incentives. Indeed, when firms experience a small decline in sales, firms' economic performance is likely to fall below the profitability target used by investors to set up managers compensation. In lines with this argument, Healy (1985) documents that managers engage in income-increasing choices when their economic performance falls right below the target rate that triggers compensation bonuses. Recently, Dierynck et al. (2012) find that managers of private firms terminate employees more when they experience a small decline in sales while having a corporate performance below an important threshold.

Thus, I conjecture that managers have incentives to engage in income-increasing manipulation around small declines in sales and I predict that after the adoption of the good-faith exception managers will rely more on cutting discretionary expenses since it is more costly to fire employees to save on wages and increase income. I create a dummy variable, *Sales Decline*, that equals one if a firm experiences a yearly decline in sales of 5% or less, and zero otherwise. I restrict this variable to relatively small declines in sales as prior studies also document that when managers are too far away from their profitability target, they do not opt for income-

increasing discretionary choices (Healy, 1985). In column (2), the coefficient on *Post GF* is positive and statistically significant at the 5% level. Furthermore, the coefficient on *Post GF X Sales Decline* is also positive and statistically significant at the 5% level. This suggests that managers rely more intensively on cutting discretionary expenses below the predicted level to increase earnings after the adoption of the good-faith exception when they face compensation incentives.

Finally, I investigate the behavior of firms whose gross margin, defined as sales minus cogs, falls below that of industry peers. When investors decide on retaining managers and setting up their compensation they also benchmark firms' performance to that of its industry peers (Aggarwal and Samwick, 1999). Thus, I predict that when firing costs increase, managers will rely more on cutting other form of expenses to increase earnings when their gross margin falls below that of industry peers to maximize their probability to secure their job and maximize their compensation (Fudenberg and Tirole, 1995). I define *Gross Margin* as a dummy that equals one if a firm's gross margin falls below the median of the industry-year, and zero otherwise. In column (3), I find that the coefficient on *Post GF X Gross Margin* is positive and statistically significant at the 10% level. This confirms that firms whose performance falls below that of industry peers rely more on cutting discretionary expenses to increase earnings after the adoption of the good-faith exception.

2.5.3 Market Shares

In the two previous subsections, I performed several tests to provide convincing empirical evidence that increase in firing costs lead firms to cut discretionary expenses more and that the effect is more pronounced for firms with capital markets and/or compensation incentives to increase reported earnings or operating cash flows. In this section, I change the focus of my analysis and test my second hypothesis. Specifically, I estimate whether cutting more discre-

tionary expenses due to change in labor laws has real effects or not, and whether such effects are beneficial or detrimental to firms in the future.

As discussed in detail in Section 3, it is not clear *ex ante* whether cutting expenses over the predicted level is detrimental to the firm in the long-run. The real effects of such actions likely depend on the incentives behind these decisions and explain the mixed findings of prior studies. That is, if managers intend to increase earnings to mislead investors, the consequences are likely to be negative as reported in Kothari et al. (2015). However, if managers increase earnings by cutting expenses as a credible signal to investors, it is followed by improved subsequent performance (Beneish et al., 2014).

In the context of changes in labor laws, my second hypothesis predict that cutting more discretionary expenses will have negative consequences for firms. Indeed, according to economic theory, it is optimal to terminate employees if their productivity falls below their wages. However, the adoption of the good-faith exception lead firms to retain unproductive employees, leading to a second-best equilibrium where firms will cut other expenses to reduce cash outflows and increase earnings instead of saving on wages. As a result, I conjecture that cutting expenses correspond to a myopic short-term behavior used to fool investors while passing on positive NPV investments that will dampen the firms' competitiveness in subsequent periods. Such positive NPV investments can include the development of new products (R&D expenses), investing in developing employees' skills (SG&A expenses) or enhancing products' visibility (advertising expenses).

I concentrate on firms' market share.³² I do so because market shares allow to directly test the mechanism at play here. Indeed, if cutting expenses over the optimal point affects lead my sample of regulated firms to pass on positive NPV investments relative to a control group of firms located in states that do not experience an increase in firing costs, this should be reflected

³²One alternative approach would be to additionally examine subsequent stock returns, which I consider doing in the next version of this paper

in their future market position. To test this intuition, I construct a firm's market share as firms' sales over industry sales, defined at the three-digit SIC codes and test whether firms' market share increase or decrease after the adoption of the good-faith exception. Table 2.5 displays the result.

[Insert Table 2.5 around here]

I first estimate my model without interactions. In column (1) the coefficient on *Post GF* is negative and statistically significant at the 5% level. The effect seems economically significant. Indeed, it corresponds approximately to a 11% decrease in market share after the adoption of the good-faith exception relative to a control group of unaffected peers.³³ However, in column (2), the coefficient on *Post GF* is not significant anymore when I replace the year fixed effects with region-year fixed effects, suggesting that I was capturing a trend due to unobservable time-varying characteristics at the region level.³⁴

Next, I specifically examine the consequences of cutting discretionary expenses on future market shares. To do so, I first create a dummy variable, that equals one if the residuals in my sample are positive (i.e. if a firm exhibit a level of discretionary expenses that is below the predicted level). Then I use it to create two lagged indicator variables: Cut_{t-1} and Cut_{t-2} .

In columns (3) and (4), the coefficients on Cut_{t-1} and Cut_{t-2} are not statistically different from zero. This indicates that, on average, cutting discretionary expenses below the expected level does not affect firms' future competitiveness in one direction or the other. This is consistent with prior results suggesting that the effect of real earnings management on subsequent performance must be conditioned on managers' incentives behind such choices Kothari et al. (2015); Beneish et al. (2014). I find that the coefficients on the interaction terms between *Post GF* and Cut_{t-1} and Cut_{t-2} are negative and statistically significant at the 5% level and 10% level, respectively. This evidence supports my second hypothesis. This indicates that firms

³³Indeed, the mean of market shares is 0.0558.

³⁴As a result, I run my next analyses using region-year fixed effects exclusively.

that cut discretionary expenses to presumably increase earnings as a response to state court rulings in favor of the good-faith exception experience decline in market shares in the next two years. In untabulated analysis, I do not find a statistically significant decrease in the next third and fourth years, consistent with the results in Begley (2014) who find statistically significant decline in valuation in the next two years after that a firm cut discretionary expenses (defined as SG&A and R&D expenses) to meet some credit rating thresholds.

Overall, the results in Table 2.5 suggest that increases in firing costs lead firms to cut discretionary expenses below the optimal level, which ultimately affect firms' competitiveness. This is consistent with prior research indicating that reducing SG&A affect employees' human capital, a factor of growth and performance (Becker, 1962).

2.5.4 Additional Analysis

Endogeneity Analysis

Next, I evaluate the extent to which the adoption of a good-faith exception is exogenous. The validity of my research design relies on the assumption that the adoption of wrongful discharge laws is not driven by previous trends in earnings manipulations and that the change in discretionary expenses is not anticipated. To rule out a potential concern of reverse causality, I follow Bertrand and Mullainathan (2003b) and decompose the adoption of good-faith exceptions into separate time periods for each state. Specifically, I re-estimate my first model and replace the *Post GF* dummy by several indicator variables: *Post GF*⁻², *Post GF*⁻¹, *Post GF*⁰, *Post GF*¹ and *Post GF*²⁺. I then interact these dummy variables with the *Demand Shock* variable. For example, *Post GF*⁻¹ is a dummy that takes the value of one in the year before the adoption of a good-faith exception, and zero otherwise. The other dummy variables are defined similarly.

[Insert Table 2.6 around here]

Table 2.6 reports the results of three specifications with different sets of fixed effects to clearly separate the effect of labor laws from that of other trends. In all three specifications, the coefficients on $Post\ GF^{-2}$, $Post\ GF^{-1}$ and $Post\ GF^0$ are not statistically significant different from zero. This suggests that the effect of the ruling never preceded its adoption. Second, it also rules out an alternative behavioral explanation that firms reacted only around the passage of the good-faith exception since the coefficient on $Post\ GF^{1+}$ is not statistically significant either. On the contrary, the coefficient on $Post\ GF^{2+}$ is positive and statistically significant, indicating that the change in behavior occurred after two years.

Robustness Tests

Next, I conduct a series of additional tests to assess the robustness of my findings and rule out some alternative explanations. I also discuss some alternative research designs.

Employees' location The first concern is that I do not observe directly whether firms' employees (or what fraction of firms' employees) are located in the state where the firm is headquartered, which is likely to bring some noise into my estimations. To address this issue, I consider the case of firms whose workforce is likely geographically dispersed (Agrawal and Matsa, 2013).³⁵ The intuition is that a change in the labor adjustment costs in a given state only affects a limited fraction of such firms' workforce, which has two implications. First, it may not raise significantly firing costs at the firm level. Alternatively, such firms could potentially respond to an economic slowdown by terminating employees' located in unregulated states. Thus, I conjecture that they should be less likely to rely more intensively on abnormally low level of expenses to boost earnings in response to the adoption of the good-faith exception. Table 2.7 displays the results.

[Insert Table 2.7 around here]

³⁵Dispersed industries include retail, wholesale and transportation.

I find that the coefficient on *Post GF* is statistically significant across all specifications. This suggests that firms with relatively less dispersed workforce cut more discretionary expenses in response to the good-faith exception compared to a control group of unaffected firms. On the other hand, the coefficient on *Post GF X Dispersed* is negative and the sum of the coefficients on *Post GF* and *Post GF X Dispersed* is not statistically different from zero. As expected, it means that I fail to find a response to the change in labor laws for firms with a dispersed workforce. Overall, this result suggests that even if using firms' headquarter to measure the location of firms' activity is noisy, it does capture changes in labor adjustment costs in the predicted direction.

Balance of the sample One concern is related to the validity of the difference-in-differences methodology with respect to the staggered adoption of the labor laws. By 1999, thirteen state courts had ruled in favor of a good-faith exception. However, the number of firms headquartered within each of these thirteen states is largely unbalanced across states. Specifically, the state of California accounts for 51.4% of the number of firm-year observations with a good-faith exception in place. One issue is that my results may be driven solely by firms headquartered in California. This would be problematic as the change in behavior of Californian companies could be due to an unknown event that is unrelated to the change in labor laws. To rule out that concern, I re-estimate my models excluding the state of California from the sample. As shown in Table 2.8, my results continue to hold, albeit exhibiting a statistical significance only at the 5% or 10% levels.

[Insert Table 2.8 around here]

Alternative Specifications There are some discussions in the legal and labor economics literature about the coding of the adoption of the wrongful discharge laws. Specifically, some scholars argue that the court decisions in Utah and Florida do not correspond to a shift in

jurisprudence. To account for this debate and ensure that my results are not, on average, driven by such states, I re-estimate my baseline specification excluding observations in Florida and Utah, respectively. I report the estimations in columns (2) and (3) of Table 2.8. My results continue to hold. A final concern change is that the change in labor laws affected firms' mergers decisions and that my results are driven by firms who suddenly changed their structure. To rule out this concern, I re-estimate my baseline model by excluding firms who experience a yearly change in total assets of at least 50%. Again, as shown in Table 2.8 column (4), my results continue to hold.

Validity of the instrument One key underlying assumption for this paper is that U.S. listed firms are indeed affected by state court rulings in favor of the good-faith exception. Serfling (2014) documents that U.S. listed firms change their capital structure in response to these rulings as predicted by corporate finance theory. Nevertheless, it does not imply that his findings and mine are necessarily driven primarily by these rulings. I motivate my approach by relying on the work of Autor et al. (2007) that finds a reduction in employment volatility for firms in states that ruled in favor of the good-faith exception. However, they use U.S. Census Bureau data at the plant level. To ensure that U.S listed firms are affected, I compute the change in employment level using Compustat data on yearly number of employees.³⁶ On average, firms' employment level grew by 9% over the period and the growth rate is positive in more than 80% of the firm-year observations throughout my sample. This is consistent with the seminal paper by Gabaix and Landier (2008) that argues that increase in executive compensation is largely explained by increase in firms' size. Table 2.9 displays the results of my estimation. I find a negative and statistically significant on *Post GF* across the three specifications. This coefficient might be interpreted in various ways. First, it could mean that treated firms became smaller. However, in an untabulate test I fail to find an effect on firm size. Thus, it suggests

³⁶I acknowledge that this item may not be reliable in Compustat, especially in the pre 2000 years since it was not audited. Thus, findings of Table 2.9 must be interpreted with caution.

that treated firms became less labor intensive or, as suggested by the labor literature, relied more on outsourced labor, holding size growth constant. Overall, it does suggest that treated firms reacted to the adoption of the good-faith exception by changing their hiring decisions.

[Insert Table 2.9 around here]

Discretionary Expenses In my analysis, I estimate abnormal level of discretionary expenses, defined as the sum of R&D expenses, advertising expenses and SG&A expenses. This is in lines with the seminar real earnings management study by Roychowdhury (2006). One could argue that managers have more discretion over R&D expenses and advertising expenses compared to SG&A expenses and that SG&A expenses are relatively fixed over time.³⁷ I estimate abnormal level of R&D expenses and advertising expenses separately and then create a additional measure of abnormal discretionary expenses by musing the residuals of the two models. Next, I run my main test using this new left-hand side variable. Table 2.10 displays the results. The coefficient on Post GF remains statistically significant across all specifications, albeit significant only at the 10% level. Note that the decrease in sample size by approximately a third is due to missing data on both R&D and advertising expenses.

[Insert Table 2.10 around here]

Trade-off In this paper, I provide plausibly causal empirical evidence that managers substitute cutting discretionary expenses to terminating employees to increase reported earnings when firing costs increase, and that such behavior affects negatively firms' future market shares. As discussed in Section 3, I focus on real-earnings management choices because recent studies suggest that managers sequentially opt first for real manipulations to increase earnings and that such manipulations are less likely to be uncovered by investors (e.g., Kothari et al., 2015; Zang, 2012). An additional and complementary perspective would be to investigate accounting ma-

³⁷Note, however, that such statement this is inconsistent with the findings of a recent study in the real-earnings management literature using a plausibly causal setting (Begley, 2014).

nipulation using accruals. That is, I could also examine whether managers also rely on accrual manipulations to increase earnings after falling short of expenses to cut.

To do so, I would need to rely on a measure of accounting manipulation that captures the direction of the manipulation. Indeed, my main hypothesis states that managers substitute cutting expenses instead of firing employees to increase earnings. I can not rely on accrual-based models derived from the work of Dechow and Dichev (2002) and/or Hutton et al. (2009). Indeed, these models intend to capture the quality of financial statements and do not indicate the direction of the potential manipulation. As a result, the only set of models that I could concentrate on are the discretionary accruals models derived from the work of Dechow et al. (1995). I first compute total accruals using the balance sheet approach.³⁸ Next, I examine how total accruals are affected by changes in labor laws. Table 2.11 displays the results.

[Insert Table 2.11 around here]

The coefficient on *Post GF* is negative and statistically significant at conventional levels across the three specifications. It indicates that total accruals are, on average, systematically lower after the court rulings in favor of the good faith exception. This result raises several concerns. First, it suggests that firms' overall accounting quality improved after the change in regulation (Richardson et al., 2006), which goes against the prediction that would be that firms use discretion in accruals more intensively to report higher earnings in the post regulation period. More importantly, it indicates that the labor laws had an effect on the outcome of the accounting process. For example, it could be that booking larger provisions for the termination of employees lead to lower level of accruals.³⁹ Consequently, if total accruals are lower, holding sales constant, this will lead to more positive residuals in a standard discretionary accruals model (Dechow et al., 1995). However, it would be difficult to disentangle between the use of

³⁸While this approach has been questioned by prior work (Collins and Hribar, 2002), total accruals cannot be computed using the statement of cash flows since it was not available for U.S. listed firms before 1988.

³⁹Note that this concern does not apply to real earnings management. Indeed, if firms report higher expenses because of the laws, this goes empirically against me finding results in favor of larger cut in discretionary expenses, while it biases me in favor of finding higher discretionary accruals.

discretion and the mechanical effect induced by the effect of the laws on normal level of accruals.

2.6 Conclusion

I use the adoption of the good-faith exception by U.S. courts as a source of plausibly exogenous variation in dismissal costs at the state level to examine the causal relation between a firm's operating flexibility and its reporting choices. I find robust evidence that firms substitute abnormally low discretionary expenses to firing employees when it becomes more costly to cut labor expenses. Next, I provide evidence that such behavior lead to a decline in market shares in future periods.

My results contribute to several streams of accounting research. First, my paper is related to the literature that examines the cost and benefits of various choices to manipulate earnings upward. Next, I contribute to the studies on the consequences of real earnings management for firms subsequent performance. Finally, my results speak to the literature on regulations and accounting by providing evidence of an unintended consequence of a change in labor regulation affect firms aggressiveness with respect to discretionary spending, which ultimately affects their competitiveness (Leuz, 2010).

Appendix A - Wrongful Discharge Laws by State

State	Public-Policy Exception	Implied-Contract Exception	Good-Faith Exception
Alabama	-	1987	-
Alaska	1986	1983	1983
Arizona	1985	1983 (1984)	1985
Arkansas	1980	1984	-
California	1959	1972	1980
Colorado	1985	1983	-
Connecticut	1980	1985	1980
Delaware	1992	-	1992
Florida	-	-	1983
Georgia	-	-	-
Hawaii	1982	1986	-
Idaho	1977	1977	1989
Illinois	1978	1974	-
Indiana	1973	1987	-
Iowa	1985	1987	-
Kansas	1981	1984	-
Kentucky	1983	1983	-
Louisiana	-	-	1998
Maine	-	1977	-
Maryland	1981	1985	-
Massachusetts	1980	1988	1977
Michigan	1976	1980	-
Minnesota	1986	1983	-
Mississippi	1987	1992	-
Missouri	1985	1983 (1988)	-
Montana	1980	1987	1982
Nebraska	1987	1983	-
Nevada	1984	1983	1987
New Hampshire	1974	1988	1974 (1980)
New Jersey	1980	1985	-
New Mexico	1983	1980	-
New York	-	1982	-
North Carolina	1985	-	-
North Dakota	1987	1984	-
Ohio	1990	1982	-
Oklahoma	1989	1976	1985 (1989)
Oregon	1975	1978	-
Pennsylvania	1974	-	-
Rhode Island	-	-	-
South Carolina	1985	1987	-
South Dakota	1988	1983	-
Tennessee	1984	1981	-
Texas	1984	1984	-
Utah	1989	1986	1989
Vermont	1986	1985	-
Virginia	1985	1983	-
Washington	1984	1977	-
West Virginia	1978	1986	-
Wisconsin	1980	1985	-
Wyoming	1989	1985	1994

This table reports the year when each state passed the public-policy, implied-contract and good-faith exception. When there is a reversal by a higher court, the year of reversal is indicated in brackets.

Appendix B - Variable Definitions

Variable Name	Definition	Source
Discr. Exp.	Residuals from a discretionary expenses model	Compustat
Market Shares	Firms sales over industry sales (SIC-4)	Compustat
Post GF	Indicator that takes a value of one for firms headquartered in a state that adopted the good-faith exception	Autor et al. (2006)
Size	Logarithm of a firm's total assets	Compustat
Leverage	Long-term debt over book value of assets	Compustat
Market-to-Book Ratio	Market value of assets divided by book value of assets	Compustat
ROA	Operating income divided by total assets	Compustat
State GDP Growth	State GDP growth rates computed	BEA
Financing	Indicator that takes a value of one if the amount of long-term debt due in one year is over the median of the distribution in a given industry - year	Compustat
Union	Indicator that takes a value of one if a firm belongs to a industry with at least 25% of its workers covered by collective agreement in 1983, as in Matsa (2010)	Compustat
Dispersed	Indicator that takes a value of one if a firm belongs to an industry classified as dispersed in Agrawal and Matsa (2013)	Compustat
Sale Decline	Indicator that takes a value of one if the yearly change in sales is negative and below 5%	Compustat
Gross Margin	Indicator that takes a value of one if the gross margin falls below that of the industry year and zero otherwise	Compustat
Cutting	Indicator that takes a value of one is the abnormal discretionary expenses is below that of the state-year	Compustat

This table provides the definitions and sources used to measure all dependent, independent and partitioning variables used in the various models throughout this paper.

Figure 2.1 – Adoption of the good-faith exception by states over time

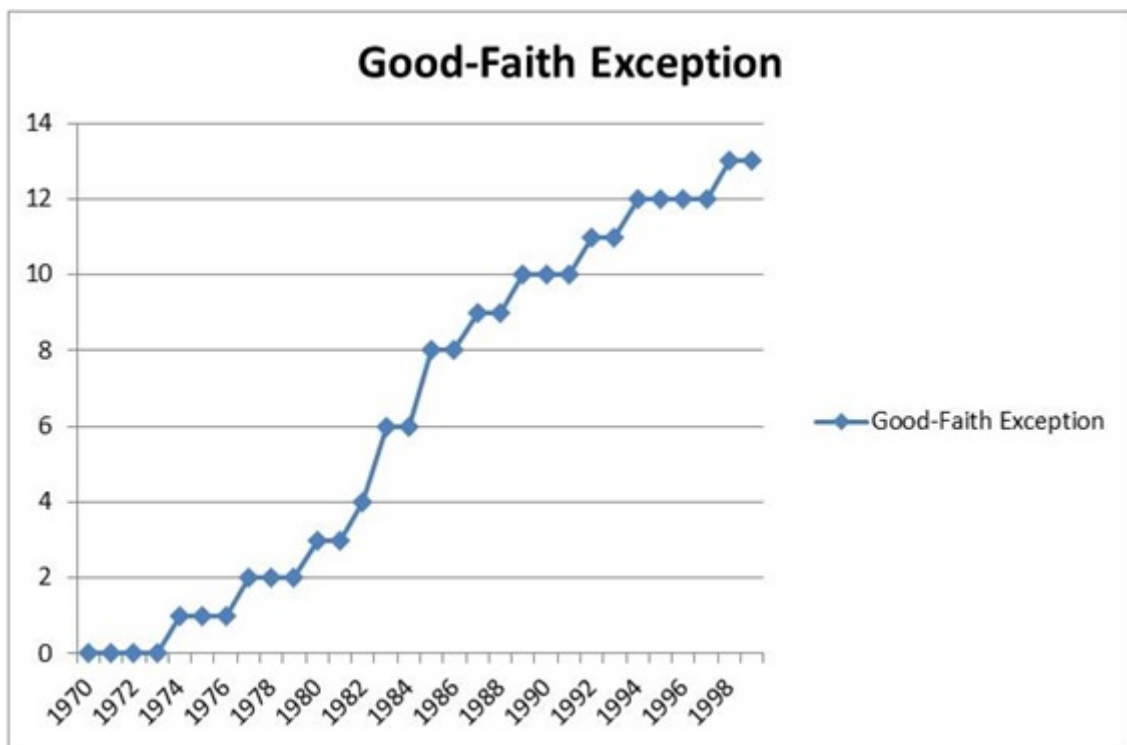


Table 2.1 – Descriptive Statistics

Variable Name	Observations	Mean	25th	Median	75th	Std. Deviation
Discr. Exp.	88,782	0.0004	-0.0876	0.0349	0.1587	0.2803
Market Shares	88,782	0.0558	0.0009	0.0073	0.0421	0.1258
ΔEmp_t	88,782	0.0924	0.0003	0.0504	0.1756	0.3651
Size	88,782	3.2869	4.9236	4.1127	6.0071	1.8603
ROA	88,782	0.0974	0.0586	0.1256	0.1922	0.1765
Market-to-Book	88,782	1.9331	0.9458	1.3436	2.3392	1.3467
State GDP Growth	88,782	0.0871	0.0052	0.0741	0.0957	0.0332
Union	88,782	0.3503	0.0000	0.000	1.0000	0.3226
Financing	88,782	0.4844	0.0000	0.0000	1.000	0.4997
Dispersed	88,782	0.1217	0.0000	0.0000	0.0000	0.3372
Sales Decline	88,782	0.2165	0.0000	0.0000	0.0000	0.1671
Gross Margin	88,782	0.4563	0.0000	0.0000	1.0000	0.4248

This table presents the descriptive statistics of all dependent, independent and partitioning variables used in the various models throughout this paper.

Table 2.2 – Baseline results

	(1) Disc. Exp.	(2) Disc. Exp.	(3) Disc. Exp.	(4) Disc. Exp.
Post GF	0.0150*** (0.005)	0.0117** (0.005)	0.0122* (0.007)	0.0105** (0.005)
Size		0.0081* (0.004)	0.0083* (0.004)	0.0042 (0.004)
Market-to-Book		-0.0199*** (0.002)	-0.0194*** (0.002)	-0.0242*** (0.003)
Z-Score		-0.0050*** (0.001)	-0.0052*** (0.001)	-0.0048*** (0.001)
State GDP Growth		0.0581 (0.039)	0.0166 (0.037)	0.0035 (0.034)
Constant	-0.0547*** (0.005)	-0.0279 (0.018)	-0.0182 (0.015)	-0.0171 (0.017)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Region-Year Fixed Effects	No	No	Yes	No
Industry-Year Fixed Effects	No	No	No	Yes
Observations	88,782	88,782	88,782	88,782
Adjusted- R^2	0.4732	0.4821	0.4815	0.5012

This table presents the results from OLS regressions relating abnormal discretionary expenses to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is a measure of discretionary expenses. All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.3 – Cross-Sectional Analysis with Organized Labor

	(1) Disc. Exp.	(2) Disc. Exp.	(3) Disc. Exp.	(4) Disc. Exp.
Post GF	0.0225*** (0.006)	0.0165** (0.006)	0.0173** (0.008)	0.0097* (0.007)
Post GF X Union	-0.0183* (0.010)	-0.0117* (0.007)	-0.0121* (0.007)	-0.0113* (0.006)
Size		0.0081* (0.004)	0.0082* (0.004)	0.0041 (0.004)
Market-to-Book		-0.0196*** (0.002)	-0.0196*** (0.002)	-0.0217*** (0.002)
Z-Score		-0.0052*** (0.001)	-0.0049*** (0.001)	-0.0038*** (0.001)
State GDP Growth		0.0584 (0.037)	0.0159 (0.032)	0.0030 (0.032)
Constant	-0.0536*** (0.005)	-0.0235 (0.017)	-0.0179 (0.015)	-0.0133 (0.016)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Region-Year Fixed Effects	No	No	Yes	No
Industry-Year Fixed Effects	No	No	No	Yes
Observations	88,782	88,782	88,782	88,782
Adjusted- R^2	0.4755	0.4867	0.4977	0.5027

This table presents the results from OLS regressions relating discretionary expenses to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is a measure of discretionary expenses. *Union* is a dummy variable that equals one if a firm belongs to an industry that had at least 25% of its workforce covered by a collective agreement in 1983, as constructed in Matsa (2010). All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.4 – Cross-Sectional Analysis with Specific Incentives

	(1) Disc. Exp.	(2) Disc. Exp.	(3) Disc. Exp.
Post GF	0.0080* (0.0050)	0.0125** (0.0065)	0.0072* (0.0044)
Financing	0.0016 (0.0018)		
Post GF X Financing	0.0071* (0.0037)		
Sales Decline		0.0121*** (0.0021)	
Post GF X Sales Decline		0.0141** (0.0070)	
Gross Margin			0.0075 (0.0070)
Post GF X Gross Margin			0.0112* (0.0057)
Constant	-0.0136 (0.017)	-0.0147 (0.017)	-0.0135 (0.016)
Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	88,782	88,782	88,782
Adjusted- R^2	0.4262	0.4051	0.4340

This table presents the results from OLS regressions relating discretionary expenses to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is a measure of discretionary expenses. *Financing* is a dummy variable that equals one if a firm's fraction of long-term debt due in one year over total debt and equity falls above the median of the year, and zero otherwise. *Sales Decline* is an indicator equal to one if a firm experiences a yearly decline in sales of 5% or less, and zero otherwise. *Gross Margin* is a dummy that equals one if a firm's gross margin falls below the median of the industry-year, and zero otherwise. All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.5 – Labor Laws, Market Shares and Discretionary Expenses

	(1)	(2)	(3)	(4)
	<i>MarketShares_t</i>	<i>MarketShares_t</i>	<i>MarketShares_t</i>	<i>MarketShares_t</i>
Post GF	-0.0061** (0.003)	-0.0033 (0.003)	-0.0023 (0.003)	-0.0027 (0.003)
<i>Cut_{t-1}</i>			0.0002 (0.000)	
<i>Cut_{t-2}</i>				-0.0001 (0.000)
Post GF X <i>Cut_{t-1}</i>			-0.0019** (0.001)	
Post GF X <i>Cut_{t-2}</i>				-0.0013* (0.006)
Constant	-0.0247** (0.010)	-0.0256** (0.010)	-0.0205** (0.009)	-0.0206** (0.008)
Controls	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	No
Region-Year Fixed Effects	No	Yes	Yes	Yes
Observations	88,782	88,782	84,752	79,547
Adjusted- <i>R</i> ²	0.6241	0.6601	0.6679	0.6366

This table presents the results from OLS regressions relating firms' market shares to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is a firms' market share defined as firms sales over industry sales, using four-digit SIC codes. All variables are defined in Appendix B. Control variables include Size, Market-to-Book and State GDP Growth. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.6 – Endogeneity Analysis

	(1) Disc. Exp.	(2) Disc. Exp.	(3) Disc. Exp.
Post GF -2	-0.0055 (0.006)	-0.0057 (0.006)	-0.0004 (0.009)
Post GF -1	-0.0039 (0.008)	-0.0042 (0.009)	0.0123 (0.007)
Post GF 0	-0.0081 (0.007)	-0.0084 (0.007)	0.0129 (0.009)
Post GF +1	-0.0156 (0.012)	-0.0181 (0.013)	0.0052 (0.014)
Post GF 2+	0.0221*** (0.006)	0.0172** (0.006)	0.0384** (0.017)
Size	0.0086** (0.004)	0.0084** (0.004)	0.0046 (0.004)
Market-to-Book	-0.0192*** (0.002)	-0.0200*** (0.002)	-0.0209*** (0.002)
Z-Score	-0.0050*** (0.001)	-0.0042*** (0.001)	-0.0039*** (0.001)
State GDP Growth	0.0574 (0.039)	-0.0339 (0.048)	0.0020 (0.032)
Constant	-0.0465* (0.024)	-0.0292 (0.018)	-0.0163 (0.015)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No
Region-Year Fixed Effects	No	Yes	No
Industry-Year Fixed Effects	No	No	Yes
Observations	88,782	88,782	88,782
Adjusted- R^2	0.4822	0.4838	0.5041

This table presents the results from OLS regressions relating discretionary expenses to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is a measure of discretionary expenses. All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.7 – Robustness Test - Dispersed Workforce

	(1) Disc. Exp.	(2) Disc. Exp.	(3) Disc. Exp.	(4) Disc. Exp.
Post GF	0.0136** (0.006)	0.0125** (0.005)	0.0198** (0.009)	0.0092* (0.005)
Post GF X Dispersed	-0.0087 (0.010)	-0.0079 (0.011)	-0.0102 (0.011)	-0.0139 (0.013)
Size		0.0081* (0.004)	0.0078* (0.004)	0.0042 (0.004)
Market-to-Book		-0.0195*** (0.002)	-0.0199*** (0.002)	-0.0212*** (0.002)
Z-Score		-0.0051*** (0.001)	-0.0047*** (0.001)	-0.0039*** (0.001)
State GDP Growth		0.0564 (0.039)	-0.0409 (0.047)	0.0036 (0.032)
Constant	-0.0535*** (0.005)	-0.0267 (0.017)	-0.0179 (0.015)	-0.0172 (0.017)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No
Region-Year Fixed Effects	No	No	Yes	No
Industry-Year Fixed Effects	No	No	No	Yes
Observations	88,782	88,782	88,782	88,782
Adjusted- R^2	0.4776	0.4811	0.4840	0.5042

This table presents the results from OLS regressions relating discretionary expenses to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is a measure of discretionary expenses. *Dispersed* is an indicator that equals one if a firm belongs to an industry with dispersed workforce as defined in Agrawal and Matsa (2013), and zero otherwise. All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.8 – Robustness Tests - Various Specifications

	(1)	(2)	(3)	(4)
	Disc. Exp.	Disc. Exp.	Disc. Exp.	Disc. Exp.
Post GF	0.0118** (0.006)	0.0092** (0.004)	0.0106** (0.005)	0.0087** (0.004)
Size	0.0050 (0.004)	0.0098** (0.004)	0.0079* (0.004)	0.0106*** (0.004)
Market-to-Book	-0.0181*** (0.001)	-0.0192*** (0.002)	-0.0192*** (0.002)	-0.0133*** (0.001)
Z-Score	-0.0047*** (0.001)	-0.0053*** (0.001)	-0.0052*** (0.001)	0.0007 (0.001)
State GDP Growth	0.0247 (0.037)	0.0636* (0.037)	0.0528 (0.038)	0.0088 (0.032)
Constant	0.0007 (0.017)	-0.0338* (0.018)	-0.0264 (0.018)	-0.0345** (0.016)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	76,370	85,257	88,068	79,081
Adjusted- R^2	0.4866	0.4837	0.4829	0.5422

This table presents the results from OLS regressions relating discretionary expenses to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is a measure of discretionary expenses. In column (1), I exclude observations from the state of California. In column (2), I exclude observations from the state of Florida. In column (3), I exclude observations from the state of Utah. In column (4), I exclude observations when the year change in total assets is superior to 50%. All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.9 – Robustness Tests - Employment Level

	(1) ΔEmp_t	(2) ΔEmp_t	(3) ΔEmp_t
Post GF	-0.0292*** (0.009)	-0.0205** (0.008)	-0.0237*** (0.010)
Size	0.0647*** (0.004)	0.0652*** (0.004)	0.0709*** (0.004)
Market-to-Book	0.0470*** (0.002)	0.0469*** (0.002)	0.0459*** (0.002)
State GDP Growth	0.1732*** (0.005)	0.2306*** (0.066)	0.1997*** (0.045)
Constant	-0.0621*** (0.023)	-0.0561*** (0.021)	-0.0723*** (0.022)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Region - Year Fixed Effects	No	Yes	No
Industry - Year Fixed Effects	No	No	Yes
Observations	88,782	88,782	88,782
Adjusted- R^2	0.2364	0.2372	0.2419

This table presents the results from OLS regressions relating change in employment level to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is a the yearly change in number of employees computed using the *emp* variable from Compustat. All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.10 – Robustness Tests - Excluding SG&A

	(1) Disc. Exp.	(2) Disc. Exp.	(3) Disc. Exp.
Post GF	0.0051* (0.002)	0.0041* (0.002)	0.0060* (0.003)
Constant	0.0390* (0.002)	0.0528*** (0.012)	0.0612*** (0.008)
Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Region - Year Fixed Effects	No	Yes	No
Industry - Year Fixed Effects	No	No	Yes
Observations	61,686	61,686	61,686
Adjusted- R^2	0.4091	0.4155	0.4178

This table presents the results from OLS regressions relating discretionary expenses to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is the sum of the residuals of abnormal level of advertising and R&D expenses. All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.11 – Robustness Tests - Total Accruals

	(1) Total Accruals	(2) Total Accruals	(3) Total Accruals
Post GF	-0.0068*** (0.002)	-0.0061** (0.003)	-0.0062** (0.003)
Constant	-0.0708*** (0.010)	-0.0946*** (0.009)	-0.0873*** (0.011)
Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Region - Year Fixed Effects	No	Yes	No
Industry - Year Fixed Effects	No	No	Yes
Observations	88,782	88,782	88,782
Adjusted- R^2	0.2340	0.2345	0.2367

This table presents the results from OLS regressions relating total accruals to the enactment of wrongful discharge laws for Compustat industrial firms from 1967 to 1999. The dependent variable is Total Accruals computed using the balance sheet data. Control variables include Size and Market to Book Ratio and State GDP Growth. All variables are defined in Appendix B. Standard errors are corrected for heteroskedasticity and clustered at the state level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Chapter 3

Political Connections and Insider Trading

joint with Renaud Coulomb and Marc Sangnier

3.1 Introduction

This paper investigates how politically-connected directors change their behavior in financial markets in response to a shift in power of the politician they are connected to. We use the French 2007 presidential election as a plausibly exogenous change in the value of political connections in a difference-in-differences research design. Our results suggest that politically-connected directors have a sense of impunity and engage in plausibly fraudulent behavior in financial markets, after the election of the politician they are connected to. They tend to trade more on private information on their company's stocks and are more likely not to comply with trades' legal reporting requirements

Existing studies in finance and political economics examine the consequences of political connections at the firm level. Fisman (2001) and Faccio (2006) document a positive impact of political connections on firm value. Recent studies also show that such connections lead to other types of benefits for firms. For instance, Khwaja and Mian (2005) and Claessens et al. (2008) report preferential access to finance and banks loans for connected firms, while Correia (2014) shows that they incur lower costs from public enforcement actions. Goldman et al. (2013), Tahoun and Van Lent (2013), and Tahoun (2014) provide evidence that such firms have a higher probability of obtaining government contracts or to be bailed out. To our knowledge, our paper is the first to depart from this literature by examining how political connections affect directors themselves.¹

Under the rational framework developed by Becker (1968), individuals decide to break the law if the expected benefits from doing so are larger than the expected costs, which combine the incurred punishment and the probability of getting caught. Under the assumption that political connections can alleviate their legal exposure to securities regulation, politically-connected directors might be more likely to engage in fraudulent behavior in financial markets. In other

¹Recent contributions by Hwang and Kim (2009) and Khanna et al. (2014) emphasize the role of social ties amongst directors in shaping their decisions but do not consider political connections.

terms, we conjecture that the sense of impunity due to political connections leads directors to act illegally in financial markets. Testing this conjecture is empirically challenging. Once political connections are established, it is particularly difficult to assess when a director feels that he is protected enough by his tie to a politician. It is also arduous to determine when a politician is exactly able to protect a director if he behaves fraudulently.

We use the French 2007 presidential election as a plausibly exogenous increase in the value of connections to the future President, Nicolas Sarkozy. A difference-in-differences framework allows us to capture the extent to which directors connected to Nicolas Sarkozy change their trading behavior from the pre- to the post-election period relatively to non-connected directors. This framework allows us to plausibly isolate how a shift in power of the politician directors are connected to affects directors' behavior in financial markets.

The French setting is particularly appropriate to answer our research question. France is a country where the President has major political power. He directly appoints the head of the “*Autorités des Marchés Financiers*”—the national agency which oversees French financial markets, including insider trading prosecution. Therefore, the election of Nicolas Sarkozy as President goes together with a tangible increase of his political power, and, as consequence, increases the value of connections to him. Prior studies, such as Bertrand et al. (2007) and Kramarz and Thesmar (2013) also argue that France is a particularly well-suited country to investigate the consequences of social ties of directors because its elites are highly concentrated and politically-connected.

We define political connections to Nicolas Sarkozy through two channels. The first one is the group of major contributors to Nicolas Sarkozy presidential campaign. We obtained the data from a major French information website, Mediapart, which leaked the list of major contributors to Sarkozy's 2007 political campaign online. The second group is composed of businessmen who are Sarkozy's friends as constructed by Coulomb and Sangnier (2014).

We study whether connected directors are more likely to trade their company's stocks on the basis of private information due to the election of Nicolas Sarkozy. Insider trading based on private information is prohibited under French law, as in most developed countries. According to Beaver (1968), Rozeff and Zaman (1988), and Kim and Verrechia (1991), changes in abnormal returns of a firm around the public disclosure of insider trades reveal insiders' private information concerning future profits of the firm. We find evidence that purchases by politically-connected directors exhibit larger abnormal returns after the election relatively to our control-group of unconnected directors. Our baseline estimation provides us with a difference-in-differences estimate around 0.7% around the disclosure date of purchases.

Our findings on stock returns might be due to the fact that politically-connected directors have superior information on Government's future decisions. To disentangle this interpretation from the one where directors break the law because of expected impunity, we next examine changes in reporting behavior. Since April 2006, executives and board members of French publicly listed companies are required to disclose their transactions on AMF's website within five business days. The difference-in-differences estimate we obtain suggests that politically-connected directors became about 20% more likely to break the law in response to the shift in power of Sarkozy. This supports our conjecture that the sense of impunity due to political connections leads directors to engage in fraudulent behavior in financial markets. However, we cannot exclude that, in addition, connected directors can have access to more non-public information about future laws or policies.

Next, we directly examine the trading behavior of connected directors during periods where they likely possess superior information compared to non-insiders. Ideally, we would like to observe their trading patterns before unexpected events, such as layoffs or mergers and acquisitions. However, such data is only available for the largest French listed firms on commercial databases (e.g., Capital IQ). As a result, we concentrate on the period between the end of

the fiscal year and the release of earnings to market participants. Under French soft law, it is recommended not to trade during this period exactly because of higher concerns of information asymmetry between insiders and outsiders. We find that connected directors are more likely to trade during this period after the 2007 elections.

We perform a variety of additional tests to ensure the robustness of our findings. First, we show that our results are robust to various estimation windows for the market model we use to construct abnormal returns. We also show that no effect is reported around the transaction date, as the information is not publicly available to market participants, and that our effects are exclusively driven by directors' purchases, which is consistent with findings by Lakonishok and Lee (2001), Jeng et al. (2003), and Cohen et al. (2012). Second, we perform several placebo tests around fictitious election dates and non-presidential elections in France between 2008 and 2011 that do not correspond to any shift in power of Nicolas Sarkozy. As expected, applying our identification strategy around these dates does not produce meaningful difference-in-differences estimates. Overall, our results suggest that connected directors behave plausibly fraudulently after a change in the value of their political connections.

The remainder of the paper is organized as follows. Section 3.2 presents the related literature and details our research hypothesis. Section 3.3 describes the institutional and political context. Section 3.4 lays out the data and our estimation strategy. Empirical results are presented in Section 3.5. Section 3.6 offers some concluding remarks.

3.2 Literature review and hypothesis development

In this paper we relate and build on the results of two research fields. First, the literature on political connections and firms' value. Second, the literature on social ties and directors' behaviors. We summarize their main findings and subsequently develop our hypothesis that political connections may induce directors to act plausibly fraudulently in financial markets.

Prior studies in finance and political economy examine the value of political connections for a firm. The literature defines politically-connected firms in various ways. For example, Jayachandran (2006), Claessens et al. (2008), Ferguson and Voth (2008), and Cooper et al. (2010) define politically-connected firms as firms that financially contributed to an electoral campaign, while Knight (2007) and Coulomb and Sangnier (2014) consider as politically-connected those that should benefit from political platforms. In the literature, connected firms can also be firms whose directors or shareholders are personally connected to a political party or a politician. In this case, different types of personal connections are considered. Executives' campaign contributions are used by Ferguson and Voth (2008), direct friendships by Johnson and Mitton (2003) and Coulomb and Sangnier (2014), common educational background or geographical locations by Bertrand et al. (2007), Faccio and Parsley (2009), and Cohen et al. (2010), while cases where investors and directors are politicians or government officials themselves are considered by Faccio (2006), Faccio et al. (2006), Goldman et al. (2009), Imai and Shelton (2011), Luechinger and Moser (2012), and Cingano and Pinotti (2013).

In this literature, most studies use abnormal stock returns surrounding events that change firms' connections to establish the effect of political ties on firms' value. Such events include elections (Ferguson and Voth 2008, Goldman et al. 2009, Cooper et al. 2010, Imai and Shelton 2011, Do et al. 2013, and Akey 2014), or non-electoral power shifts (Fisman 2001, Jayachandran 2006 and Acemoglu et al. 2014), appointments of politically-connected directors (Faccio 2006 and Fan et al. 2007), or appointments in local governments of former employees (Cingano and Pinotti 2013).

The literature examines how politically-connected firms receive preferential treatments from public institutions. Such studies are motivated by theoretical contributions of Stigler (1971) and Pelzman (1976) that suggest that agencies use public resources to improve the economic status of specific economic groups. Empirically, Gordon and Hafer (2005) report lower investigation

rates by the Nuclear Regulatory Commission for firms that contribute to political campaigns, while Correia (2014) finds that firms with long-term political connections incur lower costs from the enforcement actions by the Securities and Exchange Commission (SEC). Some studies, such as Khwaja and Mian (2005) and Claessens et al. (2008), argue that politically-connected firms have a preferential access to finance and banks loans, while Tahoun and Van Lent (2013) document that financial institutions in the portfolios of key committee members of the US Congress received higher and quicker bailouts during the financial crisis. Finally, Goldman et al. (2013), Boas et al. (2014), and Tahoun (2014) provide evidence that connected firms receive more government contracts. In lines with the previous findings, Amore and Bennedsen (2013) find that firms in industries relying heavily on public demand exhibit better operating returns if they are connected to local governments. Overall, these findings support the idea that political connections lead to favorable treatments by politicians in power.

Another stream of research emphasizes the role of social ties in shaping directors' decisions. For instance, Hwang and Kim (2009) report that CEOs that are socially-connected to independent directors enjoy higher level of compensation and lower turnover-performance sensitivity. In the same vein, Khanna et al. (2014) document that CEOs develop connections through the appointment of directors. They provide evidence that appointment-based CEO connectedness increases the likelihood of committing corporate fraud and decreases the likelihood of detection. Finally, Fracassi and Tate (2012) find that firms with more CEO-director ties engage more in value-destroying acquisitions.

In this paper, we link these two streams of literature by investigating how political connections shape the behavior of directors themselves. Specifically, we explore whether political connections induce managers to engage in fraudulent transactions in financial markets. Our intuition relies on the seminal work by Becker (1968) who considered criminality as a rational choice under uncertainty, where the offender decides to break the law if the expected benefits

from acting so exceed the expected costs. In this framework, the deterrence effect depends on the expected costs associated with the crime. Such costs are a function of two elements: the probability of being caught and the severity of the punishment, if convicted.

We conjecture that politically-connected directors should experience a decrease in the perceived probability of being targeted by an enforcement action whenever the politician they are connected to increases his political power. Such expectations seem obvious for directors tied with a politician by friendship connections. They are also plausible for contributors. Indeed, in a framework *à la* Stigler (1971) and Grossman and Helpman (1994), utility-maximizing politicians who want to increase their re-election probability have incentives to protect their contributors in response to donations, while financial support is supposed to be a function of the expected returns to politicians' constituencies. Consistently with our conjecture, Correia (2014) shows that firms' political contributions reduce the penalties prescribed by the SEC both to firms and their executives in cases of prosecutions for fraudulent accounting practices. Following our reasoning, connected directors should be more likely to engage in plausibly fraudulent behavior in financial markets among other activities.

We focus on the market returns around the reporting of insider trades to determine whether connected-directors trades contain more private information after their candidate won the election. This approach follows prior theoretical and empirical contributions by Beaver (1968), Rozeff and Zaman (1988), and Kim and Verrechia (1991) who emphasize the role of stock returns in measuring the information content of a public announcement. Larger abnormal returns around the disclosure of purchases are meant to capture illegal trading on material information by insiders.²

²It is beyond the scope of this paper to review the literature on insider trading regulation and its role on stock markets. These issues are detailed by Manne (1966), Manove (1989), Ausubel (1990), Leland (1992), Fernandes and Ferreira (2009), and Brochet (2014) among others.

3.3 Institutional context

This section presents the French insider trading regulation and describe the context of the 2007 presidential election.

3.3.1 Insider trading regulation

The “*Autorités des Marchés Financiers*” (AMF) oversees French financial markets and its court rules on penalties. Two important features characterize insider trading regulation: restrictions to trade on material and non-public information, and reporting requirements.

As most developed countries, France has laws that restrict trading on private information. Insider trading was initially recognized as a problem in France during the late 1960s. The first law was passed in 1970. The French Monetary and Financial Code prohibits insiders from carrying out or facilitating transactions before the public has knowledge of the information that is privileged. The 2005 version of this code lists a maximum penalty of two years of imprisonment and a fine of 1.5 million euro, which could be increased to up to ten times the amount of profit.³⁴ In addition, French listed companies usually prohibit directors transactions before major corporate events such as earnings releases.

Reporting requirements under French law are derived from the 2003 European Market Abuse Directive (2003/6/*EC*). This directive aims to harmonize disclosure requirements across European Union member states by mandating disclosure of transactions within five working days. In France, executives and other directors disclose their trades directly to the AMF since April 2006. The information is then posted on the AMF’s website. Before this date, trades were not systematically disclosed to market participants. Directors that fail to timely disclose their

³The 2010 Banking and Financial Regulation Act increased the maximum penalty up to 100 million euro and to twenty years of imprisonment.

⁴Recently, the court of the AMF pursued a case and ruled on a 14 million euro fine against Joseph Raad and Charles Rosier for illegal insider trading during the 2008 takeover bid of the SNCF on Geodis.

transactions incur financial penalties.⁵

One could argue that breaching the law with respect to the disclosure of transactions is not particularly costly for insiders. Indeed, the AMF rarely investigates a case simply for failing to disclose on time. However, anecdotal evidence suggests that the national agency uses disclosure delays as an aggravating factor when it pursues cases for suspicions of illegal insider trading.⁶

3.3.2 Political context

French citizens elect their president for a five-year term by direct universal suffrage. In the 2007 election, Nicolas Sarkozy was declared as the official candidate of the largest rightist party—the “*Union pour un Mouvement Populaire*” (UMP). His main competitor was Ségolène Royal, the official candidate of the largest leftist party—the “*Parti Socialiste*”. The 2007 French presidential election was held on April 22nd. As no candidate received a majority of votes, a run-off between the two top vote-getters was held on May 6th. Sarkozy won this second run-off against Royal, with 53.06% of the votes.

Nicolas Sarkozy was already a member of government before the 2007 presidential election.⁷ However, his election as President did change much his power and, consequently, the value of being connected to him for two reasons.

First, France is a semi-presidential republic where the President has a large power. The Prime Minister is chosen by the President and appoints the Government that must be validated by the President. The parliament votes laws that are *de facto* fostered by the President. Indeed, for the

⁵For instance, the AMF imposed a 30,000 euro fine on September 18th 2009 to an executive that did not timely disclosed the sale of 87,141 stocks of his company.

⁶On July 22nd 2014, the Commission of Sanctions of the AMF ruled on a case (SAN-2014-16) and charged several executives with fines ranging from 30,000 to 90,000 euro for trading on private material information while not complying with the disclosure rule.

⁷Under President Jacques Chirac’s second term, Sarkozy served as Minister of the Interior in Jean-Pierre Raffarin’s first governments from May 2002 to March 2004. He was appointed as Minister of Finances in Raffarin’s second government from March 2004 to May 2005. He was then appointed again as Minister of the Interior in Dominique de Villepin’s government from June 2005 to March 2007. Sarkozy left this position to run for the 2007 presidential election. He was also the leader of UMP party since November 2004.

past two terms, the presidential election has immediately preceded parliamentary elections, and the party of the elected President systematically obtained the majority. In addition, the French National Assembly can be dissolved by the President at any time. Furthermore, the President can also appoint the director of the AMF for a five-year term.⁸ Finally, Nicolas Sarkozy was known to have a strong conception of the President role as argued by Jan (2011).

Second, a well-documented animosity existed between Nicolas Sarkozy and former President Jacques Chirac, as well as between Nicolas Sarkozy and former Prime Minister, Dominique de Villepin, that was likely to limit Nicolas Sarkozy’s influence before his election as President. All in all, Sarkozy’s election goes together with a real additional power that could be used to benefit to individuals related to him.

3.4 Data and estimation strategy

This section first presents the data used in this paper. We then explain our estimation strategy.

3.4.1 Political connections

We use two sources to uncover politically-connected directors: directors that were major contributors to Sarkozy’s presidential campaign and those that are Sarkozy’s friends.

On September 25th 2012, the French information website, Mediapart, published a column about a list of individuals considered as “*grands donateurs*” (“large contributors”) of UMP, Sarkozy’s party. This list has been produced by the party’s administration. The existence of the list has never been contested nor denied by anybody. Furthermore, its accuracy has been publicly confirmed by some of the individuals it contains. The group of “*grands donateurs*” was a club: meetings and dinners were organized to gather all its members, and therefore the

⁸For example, President Sarkozy appointed Jean-Pierre Jouyet—who first served in the government following the 2007 presidential election—as head of the AMF on December 15th 2008.

composition of the group was known by all its members. The list contains first and last names of 584 distinct individuals. Individuals working in finance were overrepresented in this group (Mediapart, 2012), this strongly suggests that the information concerning the identity of UMP large contributors was known by market participants around the presidential election. In France, the maximum donation an individual can make to a political party was 7,500 euro in 2007. Individuals that appear on the list gave at least 3,000 euro to the party during the 2007 presidential campaign.

The second group is composed of businessmen who are friends of Nicolas Sarkozy. Around the 2007 presidential election, French media reported a number of connections between Nicolas Sarkozy and prominent businessmen, while no such connections were reported for the leftist candidate, Ségolène Royal. This group is made of 27 businessmen and has been constructed by Coulomb and Sangnier (2014) who used information from Chemin and Perrignon (2007) and Dély and Hassoux (2008)—books written by journalists and political pundits.⁹

3.4.2 Insider transactions

Data on insiders' trades contains all trades by board members of French listed companies since 2006. We obtain this database from Directors Deals, a data vendor that compiled data from the AMF's website.¹⁰ Each trade is registered in the dataset by the name of the trader and the company whose stocks are traded. The dataset also contains the position of the board member in the firm (e.g., non-executive director, executive etc.) the type of the transaction (e.g., sale, purchase), the number of shares traded and the total trade value, as well as the transaction date and the announcement date, i.e. the date at which the trade has been made public. The data set is exhaustive and contains 7,385 trades from mid-2006 to mid-2008—the time-window

⁹See Coulomb and Sangnier (2014) for more information on the construction of this group, evidence of these friendship connections, and measures of their visibility in the media.

¹⁰Other recent studies such as Fidrmuc et al. (2013) and Brochet (2014), focusing on non-US insider transactions used Directors Deals as a primary source of information.

that will be used in the empirical analysis. These trades have been operated by 1,643 distinct individuals.

We use four different dependent variables to capture two dimensions of the behavior of traders. As a first dependent variable, we compute the *two-day cumulative abnormal returns on purchases* of the traded stock at the announcement date. Under the efficient-market hypothesis, following Rozeff and Zaman (1988) and Brochet (2010), we consider changes in such returns as proxies for the private information embedded in insider trades. However, the literature makes an important distinction in the informativeness of sales and purchases with respect to illegal trading. Indeed, Lakonishok and Lee (2001) and Jeng et al. (2003) argue that open market sales by directors are driven by diversification motives while illegal insider trading is mostly embedded in open market insider purchases. This is the reason why our main analysis of abnormal returns focuses exclusively on purchases of a company’s stocks made by its directors.

We follow MacKinlay (1997) in constructing firms’ abnormal returns. For each purchase, we first estimate the relationship between a firm’s return and that of the market before the announcement date. We then predict firm’s returns from the market returns observed on the announcement day and the next two days. Specifically, we run the following regression for each stock i for which a purchase is announced on day t :

$$\mathbb{R}_{i\tau} = \alpha_{it} + \beta_{it} \times \bar{\mathbb{R}}_{\tau} + \varepsilon_{i\tau}, \text{ for } \tau \in [t - 30, t - 1],$$

where $\mathbb{R}_{i\tau}$ is firm i ’s stock return on day τ , $\bar{\mathbb{R}}_{\tau}$ is the market return on day τ , and $\varepsilon_{i\tau}$ is the error term. We obtain daily stock and market returns from Datastream. We use the SBF 120 return as market return. The SBF 120 is a reference index composed of the 120 most actively traded stocks on the Paris Stock Exchange. We estimate the above expression separately for each firm and each announcement date, which yields trade-level estimated parameters $\hat{\alpha}_{it}$ and $\hat{\beta}_{it}$. These are used to compute the abnormal returns of each purchase over the two following

business days using the following formula:

$$\tilde{\mathbb{R}}_{i\tau} = \mathbb{R}_{i\tau} - \left\{ \hat{\alpha}_{it} + \hat{\beta}_{it} \times \bar{\mathbb{R}}_{\tau} \right\}, \text{ for } \tau \in [t, t+1, t+2],$$

where $\tilde{\mathbb{R}}_{i\tau}$ is the abnormal return of stock i on day τ .

Finally, we compute the two-day cumulative abnormal return as:

$$\tilde{\mathbb{R}}_{it}^{\text{cum}} = \left(1 + \tilde{\mathbb{R}}_{i,t}\right) \times \left(1 + \tilde{\mathbb{R}}_{i,t+1}\right) \times \left(1 + \tilde{\mathbb{R}}_{i,t+2}\right) - 1.$$

We use two additional dependent variables related to the disclosure of the trades. We compute the *announcement delay* of each trade by taking the difference in business days between the transaction date and the announcement date. From this measure, we also construct our third dependent variable, a dummy variable, labeled *non-compliance with legal time limit* that is equal to one if the announcement delay is strictly larger than 5 business days, the legal time limit, and zero otherwise. These last two dependent variables capture the intensive and the extensive margins of traders' compliance with legal announcement requirements.

Finally, we construct a variable, *soft*, that equals one if a trade occurs in the first three months following the end of the fiscal year. This variable intends to capture transactions in a period where trading is usually prohibited because of insiders' private knowledge. Indeed, French listed companies usually adopt guidelines from soft law stating that managers and directors must abstain from any trading activities during a certain period before the publication of annual results.¹¹ Under French corporate law, listed companies must disclose their annual reports at last exactly four months after the end of the fiscal year. We retrieve fiscal year end information from Thomson and created our dummy variable using only the first three months to avoid capturing transactions that plausibly occur right after the release of information to investors.¹²

¹¹See for example: <http://uk.practicallaw.com/8-502-1296>

¹²Note that we adopt this strategy because we do not observe the exact release date of earnings for the majority

3.4.3 Estimation strategy

As reported by Coulomb and Sangnier (2014), the outcome of the 2007 French presidential election was anticipated in the weeks that preceded the vote itself. Thus, we use a large time-window of two years around the election event—from mid-2006 to mid-2008—to capture a change in directors’ behavior due to the shift in Sarkozy’s power.

We match data on insiders’ trades and the lists of connected businessmen in order to identify individuals that appear in both datasets. Out of 584 individuals that appear on the list of contributors, 28 could be matched to trades using their first and last names. So do 16 out of the 27 businessmen considered as friends of Nicolas Sarkozy in 2007. Only 2 traders are both friends of Nicolas Sarkozy and contributors of his campaign. We consider all these 42 individuals as *Sarkozy affiliates*. This group represents 2.5% of all traders that operated during our time-window. Yet, Sarkozy affiliates traded on average 15 times over these two years, against 4 times on average for other board members. As a consequence, 8.7% of all trades have been operated by Sarkozy affiliates. Table 3.1 displays summary statistics of dependent variables and descriptive statistics for the two groups of individuals.

[Insert Table 3.1 about here]

We estimate the change in behavior of politically-connected directors before and after Nicolas Sarkozy’s election thanks to a difference-in-differences approach. We implement this design by estimating the following expression:

$$\begin{aligned} y_{it} = & \beta \text{Sarkozy affiliate}_i \times \text{Post-election}_t \\ & + \gamma \text{Sarkozy affiliate}_i + \delta \text{Post-election}_t \\ & + \theta \text{Time}_t + \alpha + \varepsilon_{it}, \end{aligned} \tag{3.1}$$

where *Sarkozy affiliate_i* is a dummy variable equal to one if the trader is connected to Sarkozy,

of firms in our sample.

$Post-election_t$ is a dummy variable equal to one after May 6th 2007, $Time_t$ is a time trend, ε_{it} is the error term, and α is a constant. Our coefficient of interest, the difference-in-differences estimate β , captures the relative change in behavior of Sarkozy affiliates compared to other directors after the presidential election. Coefficients δ and θ capture the common change in the behavior of all directors after the election compared to before the election. The coefficient γ captures possible differences between the behavior of Sarkozy affiliates and other directors over the whole period.

3.5 Results

In this section, we present empirical evidence of the change of behavior of Sarkozy’s affiliates in financial markets after the 2007 presidential election.

3.5.1 Main results

We start our analysis by estimating expression (3.1) with abnormal returns on purchases as dependent variable. The first column of Table 3.2 presents estimated coefficients. The coefficient on *Sarkozy affiliates* \times *Post Election* is positive and statistically significant, suggesting that more private information is embedded in Sarkozy affiliates’ trades after the election, relatively to private information in other directors’ trades. The effect is economically significant as well. The difference-in-differences estimate corresponds to 70 additional basis points in returns.

[Insert Table 3.2 about here]

However, the estimated coefficient of the non-interacted *Sarkozy affiliate* variable is negative. This suggests that purchases by politically-connected directors produce lower abnormal returns on average. This is likely to be due to a composition effect related to the firms they are associated to. We tackle this issue in column 2 by adding firm fixed effects to our model. This

specification is more restrictive as it compares connected and non-connected traders within the same firm. This allows us to get rid of any effect that would be firm-specific, which encompasses composition effects due to firms' characteristics. Our estimate of interest is left unchanged both in terms of magnitude and statistical significance. The coefficient on *Sarkozy affiliate* is now close to zero and not statistically significant anymore. This indicates that trades by Sarkozy's affiliates contain on average the same level of private information as trades by non-connected directors before the election, conditional on firms' characteristics.

Other composition effects might be at play. Among them, it is likely that trades by executive directors contain more information than trades by other board members. Similarly, larger trades might be more informative than smaller ones. These are the reason why we introduce two trade-level control variables in the model estimated in column 3: *Trades' value* which corresponds to the logarithm of the total value of the transaction and *executive*, a dummy that equals one if the insider is an executive director at the trading date, and zero otherwise. The coefficient on *trades' value* is insignificant.¹³ As expected, the coefficient on *executive* is positive and statistically significant, indicating that more information is embedded in transactions by insiders who are involved in running the firm.¹⁴ We further exploit these trade-level variables in column 4 by interacting them with the *post-election* dummy variable. This helps us to alleviate the concern that our main results could be driven by changes in behaviors shared by all traders of a given type. Sarkozy affiliates are indeed more likely to be executives and tend to trade higher values as shown by Table 3.1. Our coefficient of interest would be biased if all executives or all directors that trade high values changed their behavior after Sarkozy's election. Our main estimate remains unaffected by this change of specification.

¹³Prior literature has found contradictory evidence that large trades by executives could contain more private information: Seyhun (1986) and Chang and Corbitt (2012) reported a positive relation between trade size and returns, contrary to Lin and Howe (1990).

¹⁴This result supports the information hierarchy hypothesis developed by Seyhun (1986) and Lin and Howe (1990).

All in all, estimates presented in Table 3.2 suggest that trades by Sarkozy affiliates contain more private information after than before the presidential election, relatively to non-connected board members. While this empirical finding is consistent with our conjecture that political connections lead managers to act fraudulently in financial markets, it does not prove it. Indeed, this higher content in information might be due to the fact that politically-connected directors have superior information on the government's future decisions that could impact their firms. Examining directors reporting behavior can help us to determine whether this is the correct interpretation of earlier findings or whether Sarkozy affiliates became more likely to break the law, both options being not exclusive of each other.

Figure 3.1 plots the disclosure patterns for all trades—i.e. sales and purchases together—and presents the means and confidence intervals for Sarkozy affiliates and other board members separately. While the overall trend seems to converge slowly toward the 5-day legal time limit since the law was enacted, a sizable difference in the behavior of both groups can be observed after the 2007 presidential election: the trade announcement delay—i.e. the number of business days between a trade and its disclosure—of Sarkozy affiliates increases significantly. This suggests that Sarkozy affiliates are more likely to break the law after than before the election, relatively to non-connected directors.

[Insert Figure 3.1 about here]

In Table 3.3, we formally estimate this change in behavior by estimating expression (3.1) with our two dependent variables that capture the extent to which traders comply with legal disclosure requirements. Estimated coefficients presented in the top part of Table 3.3 are those obtained when using the trade announcement delay as dependent variable. The dummy variable—that is equal to 1 whenever the 5-day legal time limit is broken—is used as dependent variable for estimates presented in the bottom part of the table. We replicate the different specifications used in Table 3.2 for both dependent variables. All reported difference-in-differences

estimates are positive and statistically significant. This indicates that Sarkozy affiliates became more likely to break the law after the election, relatively to other traders.

[Insert Table 3.3 about here]

Finally, we formally examine whether politically connected directors trade more in suspicious periods after a change in the value of their connections. Table 3.4 displays the results. In the first column, the difference-in-differences estimate is not statistically significant. However, once we add firm fixed-effects in the second specification, the effect is statistically significant at the 1% and suggests that politically connected directors are approximately 10% more likely to trade after the end of the fiscal year and the release of earnings in the post-election period. Adding controls does not alter our results. The change in significance with and without firm fixed effects suggest that connected directors are not randomly assigned to firms and that we need to account for firms' heterogeneity in our analysis. Recall that in our specification with firm fixed effects, the difference-in-differences coefficient is estimated by comparing the within firm change in behavior. That is, we compared the changes in trading patterns during suspicious periods for connected directors relative to unconnected directors in the same firms. Trades by directors of firms without any connected directors only increase the statistical power of the model and help estimating the coefficients on the control variables. In this table, we pooled both insiders purchases and sales transactions since we do not know the sign of the earnings surprise.¹⁵ However, note that in an untabulated analysis, we find the same results for purchases and sell separately.

[Insert Table 3.4 about here]

¹⁵Again, note that analyst data are only available for a small sub-sample of firms in our sample and that such data is required to estimate the sign of the surprise and condition the nature of the transaction (i.e., purchases versus sales) to the sign of the surprise.

3.5.2 Robustness checks

In Table 3.5, we perform additional analyses to ensure the robustness of our findings by varying the definition and the construction of abnormal returns. In column 1, we examine the market reaction over a single day after a trade’s disclosure to market participants. In columns 2–4, we test the sensitivity of our estimates by changing the pre-event period of the market model from 30 to 7, 60 and 120 days, respectively. Although slightly different, reported point estimates are of the same order of magnitude as those previously presented. The difference-in-differences estimate presented in column 5 illustrates that there is no market reaction at the transaction date as equity traders cannot react to a trade of which they are not aware. Finally, in column 6, we use cumulative abnormal returns on sales as a dependent variable. According to the literature, there is few information content in sales as such transactions are motivated by diversification purposes (Lakonishok and Lee 2001 Jeng et al. 2003). As expected, our difference-in-differences estimate is insignificant when focusing solely on sales.

[Insert Table 3.5 about here]

We also perform several placebo tests to ensure the robustness of our findings. In Table 3.6, we randomly select ten fictitious election dates and estimate expression (3.1) replacing the date of Sarkozy’s election by these dates.¹⁶ Two points are worth mentioning following these ten estimations. First, the only date for which we report difference-in-differences estimates that could be compared to those previously estimated—although smaller and not statistically significant—is August 20th 2007, a date that is relatively close from the actual election date. This illustrates the fact that our approach does not rely on a sharp discontinuity. Second, while we do find some statistically significant estimates for other fictitious dates, none of them provide us with estimates that are statistically significant for all of the three dependent variables.

¹⁶Each fictitious election date has been randomly drawn from the interval March 22nd 2007–June 28th 2012. The lower bound of this interval corresponds to the earliest date at which we observe a trade plus one year. The upper bound corresponds to the latest date at which we observe a trade minus one year.

Furthermore, none of these dates is located around an event we would be aware of and that would be relevant to explain a change in Sarkozy affiliates' trading behavior. Finally, we also estimate expression (3.1) around four non-presidential elections in France that took place between 2008 and 2011. These four elections are not supposed to change how directors could benefit from their political connections in a way that would lead to fraudulent behavior in financial markets. Thus, it helps us to disentangle between a potential effect of the election itself and the effect of a perceived change in law enforcement probability for connected directors due to the 2007 presidential election. As expected, reported difference-in-differences estimates around these elections are not systematically positive, nor statistically significant for all dependent variables.

[Insert Table 3.6 about here]

We next decompose the group of Sarkozy affiliates along the two sources we used to construct it. Table 3.7 presents the jointly estimated difference-in-differences coefficients for both groups. In the upper part of the table, the dependent variable is the two-day cumulated abnormal return on purchases. Estimates of our difference-in-differences model are similar to those of the baseline regression for both groups, but the one for Sarkozy's friends turns to be non-significant when firm fixed effects are added. This indicates that previous results were likely to be driven by UMP contributors solely. In the middle and bottom parts of the table, the dependent variables are the trade announcement delay and the dummy variable that indicates non-compliance with the legal time limit. Difference-in-differences estimates of both groups are positive and statistically significant. The one for Sarkozy's friends is significantly larger than the one for UMP contributors. This indicates that the election of Sarkozy increases the probability that both groups of connected directors breach the legal reporting-time limit, and that this effect is even larger for Sarkozy's friends compared to UMP contributors.

[Insert Table 3.7 about here]

3.6 Conclusion

In this paper, we examined the consequences of political connections on the insider trading behavior of directors. We use the victory of Nicolas Sarkozy at the 2007 presidential election in France as a plausibly exogenous source of variation in the perceived protection afforded to directors who are connected to him, either by friendship or through major contributions to his campaign.

First, we find larger two-day cumulative abnormal returns around the disclosure of stocks' purchases by politically-connected directors after the election compared to a control group of non-connected directors. This result indicates that connected directors are more likely to trade on private information, which is illegal under French business law. Second, we find that the probability to break the legal time limit in disclosing trades to the AMF increases significantly for politically-connected directors after the election. We ensure the robustness of our findings by using various specifications and by conducting placebo analyses. Overall, our results suggest that political-connected directors have a sense of impunity that leads to fraudulent behavior in financial markets.

Our findings contribute to the empirical literature on political connections. To our knowledge, this paper is the first to depart from this literature by focusing directly on directors' behavior rather than on firms' performance. Our results also speak to the literature on social ties by showing that links to politicians induce directors to plausibly engage in illegal insider trading. From a regulatory standpoint, this suggests that connected directors are more likely to exploit information asymmetries between themselves and less-well-informed market participants, including retail investors. This contributes to the lack of trust in stock markets, leading retail investors less willing to participate to financial stock markets (Guiso et al., 2008), which might hamper economic growth (Levine and Zervos, 1998).

Table 3.1 – Descriptive statistics

	Observations	Mean	Median	Standard dev.
Two-day cumulated ab. ret. on purchases	2,224	0.00	-0.00	0.03
Trade announcement delay	7,385	10.29	8.00	8.17
Non-compliance with legal time limit	7,385	0.73	1.00	0.44
Soft	4,940	0.19	0.00	0.23

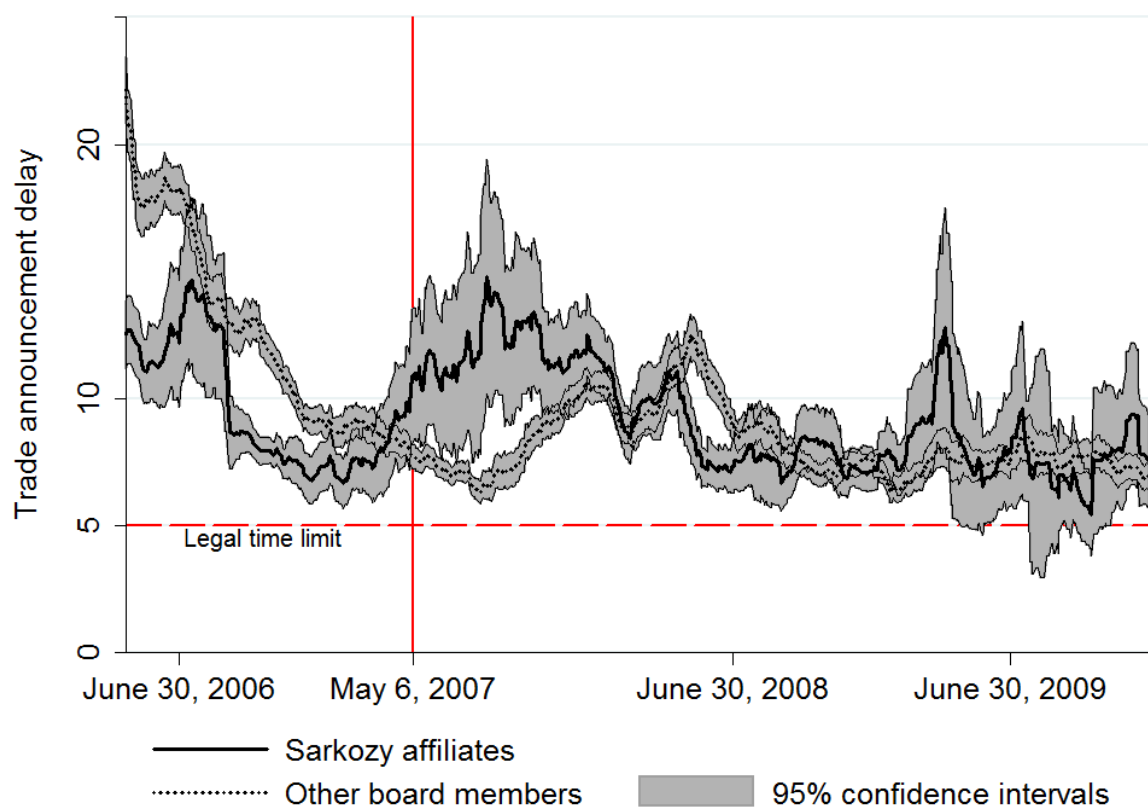
	Total	Sarkozy affiliates	Other board members
# of traders	1,643	42	1,601
# of trades	7,385	643	6,742
# of trades by trader	4.5	15.3	4.2
Trade's value (average, in thousand euro)	2,429	4,789	2,204
Executive	0.37	0.49	0.36

The time window is 365 days before and after May 6th 2007. The sample is made of all trades by board members of French listed firms during the time window. *Trade announcement delay* is the number of business days between a trade and its announcement.

Non-compliance with legal limit is a dummy variable equal to 1 if the trade announcement delay is strictly greater than 5 business days.

Two-day cumulated ab. ret. on purchases is the compound abnormal return (computed using a firm-specific 30-day market model) of the traded stock over the two days following the announcement of a purchase. *soft* is an indicator variable that equals one if a trade occurs in the first three months of the end of the fiscal year, and zero otherwise. *Sarkozy affiliates* are traders connected to Sarkozy. See the text for details about the construction of the group. *Executive* is a dummy equal to one for individuals that are members of the management board of the firm at the date of the trade. *Trade's value* is the trade's value in current euro.

Figure 3.1 – Trade announcement delay by Sarkozy affiliates and other board members.



Trade announcement delay is the number of business days between a trade and its official announcement. Means and confidence intervals have been estimated using a 30-day window before and after each date.

Table 3.2 – Difference-in-differences estimation of the change in the behavior of Sarkozy affiliates around Sarkozy's election: Abnormal returns on purchases

Dependent variable: Two-day cumulated abnormal return on purchases at announcement date				
	(1)	(2)	(3)	(4)
Sarkozy affiliate \times Post-election	0.007** (0.003)	0.007** (0.003)	0.007** (0.003)	0.007** (0.004)
Sarkozy affiliate	-0.008*** (0.002)	-0.001 (0.003)	-0.002 (0.003)	-0.002 (0.004)
Post-election	-0.004 (0.003)	-0.004 (0.004)	-0.004 (0.004)	-0.008 (0.009)
Executive			0.006* (0.003)	0.009*** (0.003)
Trade's value			0.000 (0.000)	-0.000 (0.001)
Executive \times Post-election				-0.006 (0.004)
Trade's value \times Post-election				0.001 (0.001)
Firm fixed effects		Yes	Yes	Yes
Observations	2,224	2,224	2,224	2,224

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors in parentheses. OLS regressions. Each column presents estimates from a separate regression. All regressions include a constant term and a time trend. The election date is May 6th 2007. The time window is 365 days before and after the election. The sample is made of all stock purchases by board members of French listed firms during the time window.

Post-election is a dummy variable equal to 1 for all trades that occurred after the election. The dependent variable is the compound abnormal return (computed using a firm-specific 30-day market model) of the traded stock over the two days following the announcement of a purchase. *Sarkozy affiliate* is a dummy variable equal to 1 if the trader is connected to Sarkozy. See the text for details about the construction of the group.

Executive is a dummy equal to one for individuals that are members of the management board of the firm at the date of the trade. *Trade's value* is the log of a trade's value in current euro.

Table 3.3 – Difference-in-differences estimation of the change in the behavior of Sarkozy affiliates around Sarkozy’s election: Compliance with legal requirements

Dependent variable: Trade announcement delay				
	(1)	(2)	(3)	(4)
Sarkozy affiliate \times Post-election	5.854*** (0.562)	4.428*** (0.549)	4.390*** (0.546)	4.263*** (0.563)
Sarkozy affiliate	-3.080*** (0.332)	-2.768*** (0.538)	-2.451*** (0.536)	-2.366*** (0.546)
Post-election	-0.038 (0.326)	0.376 (0.370)	0.337 (0.367)	-1.886* (1.074)
Executive			0.129 (0.260)	0.301 (0.342)
Trade’s value			-0.289*** (0.050)	-0.391*** (0.070)
Executive \times Post-election				-0.352 (0.387)
Trade’s value \times Post-election				0.194** (0.083)
Firm fixed effects		Yes	Yes	Yes
Observations	7,385	7,385	7,385	7,385
Dependent variable: Non-compliance with legal time limit				
	(5)	(6)	(7)	(8)
Sarkozy affiliate \times Post-election	0.188*** (0.035)	0.222*** (0.034)	0.220*** (0.033)	0.208*** (0.034)
Sarkozy affiliate	-0.056** (0.025)	-0.176*** (0.035)	-0.160*** (0.035)	-0.152*** (0.035)
Post-election	-0.100*** (0.022)	-0.117*** (0.024)	-0.118*** (0.024)	-0.260*** (0.064)
Executive			-0.004 (0.015)	-0.008 (0.018)
Trade’s value			-0.014*** (0.003)	-0.020*** (0.004)
Executive \times Post-election				0.007 (0.023)
Trade’s value \times Post-election				0.012** (0.005)
Firm fixed effects		Yes	Yes	Yes
	7,385	7,385	7,385	7,385

*** p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors in parentheses. OLS regressions. Each column presents estimates from a separate regression. All regressions include a constant term and a time trend. The election date is May 6th 2007. The time window is 365 days before and after the election. The sample is made of all trades by board members of French listed firms during the time window. *Post-election* is a dummy variable equal to 1 for all trades that occurred after the election. *Trade announcement delay* is the number of business days between a trade and its official announcement. *Non-compliance with legal limit* is a dummy variable equal to 1 if the trade announcement delay is strictly larger than 5 business days. *Sarkozy affiliate* is a dummy variable equal to 1 if the trader is connected to Sarkozy. See the text for details about the construction of the group. *Executive* is a dummy equal to one for individuals that are members of the management board of the firm at the date of the trade. *Trade’s value* is the log of a trade’s value in current euro.

Table 3.4 – Difference-in-differences estimation of the change in the behavior of Sarkozy affiliates around Sarkozy’s election: trading in suspicious periods

VARIABLES	(1) soft	(2) soft	(3) soft	(4) soft
Sarkozy affiliate \times Post-election	0.022 (0.032)	0.100*** (0.038)	0.100*** (0.038)	0.101*** (0.038)
Sarkozy affiliate	0.061*** (0.021)	0.019 (0.035)	0.019 (0.035)	0.019 (0.035)
Post-election	-0.318*** (0.017)	-0.316*** (0.019)	-0.317*** (0.019)	-0.326*** (0.055)
Time trend	0.027*** (0.001)	0.026*** (0.001)	0.026*** (0.001)	0.026*** (0.001)
Management board			0.001 (0.012)	0.010 (0.016)
Trade’s value			-0.001 (0.003)	-0.001 (0.003)
Management board \times Post-election				-0.017 (0.020)
Trade’s value \times Post-election				0.001 (0.004)
Observations	4,940	4,940	4,940	4,940
Firm fixed effects		Yes	Yes	Yes

p<0.01, ** p<0.05, * p<0.1. White heteroskedastic standard errors in parentheses. OLS regressions. Each cell presents an estimate from a separate regression. All regressions include a constant term, a time trend, a dummy variable equal to 1 for dates after the election, and the non-interacted *Sarkozy affiliate* dummy variable which is equal to 1 if the trader is connected to Sarkozy. See the text for details about the construction of the group. The election date is May 6th 2007. The time window is 365 days before and after the election. The sample is made of all stocks’ transactions (purchases and sales) by board members of French listed firms during the time window. Dependent variables are an indicator that equals one if the trade occur in the first three months following the end of the fiscal year.

Table 3.5 – Difference-in-differences estimation of the change in the behavior of Sarkozy affiliates around Sarkozy's election: Various types of abnormal returns

	(1)	(2)	(3)
Dependent variable :	One-day ab. ret. on purchases at announcement date	Two-day cumulated ab. ret. on purchases at announcement date (7-day market model)	Two-day cumulated ab. ret. on purchases at announcement date (60-day market model)
Sarkozy affiliate \times Post-election	0.008*** (0.002)	0.011*** (0.003)	0.006** (0.003)
	(4)	(5)	(6)
Dependent variable :	Two-day cumulated ab. ret. on purchases at announcement date (120-day market model)	Two-day cumulated ab. ret. on purchases at transaction date	Two-day cumulated ab. ret. on sales at announcement date
Sarkozy affiliate \times Post-election	0.007** (0.003)	-0.002 (0.004)	-0.001 (0.004)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. White heteroskedastic standard errors in parentheses. OLS regressions. Each cell presents an estimate from a separate regression. All regressions include a constant term, a time trend, a dummy variable equal to 1 for dates after the election, and the non-interacted *Sarkozy affiliate* dummy variable which is equal to 1 if the trader is connected to Sarkozy. See the text for details about the construction of the group. The election date is May 6th 2007. The time window is 365 days before and after the election. The sample is made of all stock purchases by board members of French listed firms during the time window, except in column 6 where sales are used. Dependent variables are abnormal returns (computed using a firm-specific market model of the traded stock) for different length and at different dates as specified in columns' heads.

Table 3.6 – Difference-in-differences estimation of the change in the behavior of Sarkozy affiliates around fictitious election dates and non-presidential elections

Dependent variable:	(1) Two-day cumulated abnormal return on purchases	(2) Trade announcement delay	(3) Non-compliance with legal time limit	(4) Two-day cumulated abnormal return on purchases	(5) Trade announcement delay	(6) Non-compliance with legal time limit
	Fictitious election on August 20, 2007			Fictitious election on October 21, 2007		
Sarkozy affiliate \times Post-election	0.001 (0.003)	0.911* (0.528)	0.042 (0.036)	0.000 (0.003)	-1.033* (0.559)	-0.005 (0.037)
	Fictitious election on September 8, 2008			Fictitious election on April 20, 2009		
Sarkozy affiliate \times Post-election	0.000 (0.005)	0.760 (0.507)	0.205*** (0.038)	-0.001 (0.009)	0.629 (0.633)	-0.051 (0.050)
	Fictitious election on June 21, 2009			Fictitious election on February 22, 2010		
Sarkozy affiliate \times Post-election	-0.011* (0.007)	0.762 (0.760)	-0.023 (0.052)	-0.010 (0.009)	1.315 (0.935)	-0.067 (0.060)
	Fictitious election on October 18, 2010			Fictitious election on March 15, 2011		
Sarkozy affiliate \times Post-election	0.007 (0.006)	-1.256 (0.971)	-0.129** (0.065)	0.006 (0.006)	-1.108 (1.001)	0.072 (0.069)
	Fictitious election on December 6, 2011			Fictitious election on December 28, 2011		
Sarkozy affiliate \times Post-election	0.003 (0.006)	-0.805 (0.865)	-0.016 (0.069)	0.003 (0.006)	-0.972 (0.871)	-0.033 (0.068)
	Municipal election on March 16, 2008			European election on June 7, 2009		
Sarkozy affiliate \times Post-election	0.001 (0.004)	-3.091*** (0.554)	-0.027 (0.036)	-0.011* (0.007)	0.985 (0.757)	-0.033 (0.051)
	Regional election on March 21, 2010			Partial gubernatorial election on September 25, 2011		
Sarkozy affiliate \times Post-election	0.002 (0.008)	1.463 (0.956)	-0.016 (0.061)	0.007 (0.006)	-0.786 (0.826)	0.102 (0.069)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. White heteroskedastic standard errors in parentheses. OLS regressions. Each cell presents an estimate from a separate regression. All regressions include a constant term, a time trend, a dummy variable equal to 1 for dates after the (fictitious or non-presidential) election, and the non-interacted *Sarkozy affiliate* dummy variable which is equal to 1 if the trader is connected to Sarkozy. See the text for details about the construction of the group. The true election date is May 6th 2007. The time window is 365 days before and after fictitious and non-presidential election dates. Each fictitious election date has been randomly drawn from the interval March 22nd 2007–June 28th 2012. *Trade announcement delay* is the number of business days between a trade and its announcement. *Non-compliance with legal limit* is a dummy variable equal to 1 if the trade announcement delay is strictly greater than 5 business days. *Two-day cumulated abnormal return on purchases* is the compound abnormal return (computed using a firm-specific 30-day market model) of the traded stock over the two days following the announcement of a purchase.

Table 3.7 – Difference-in-differences estimation of the change in the behavior of Sarkozy affiliates around Sarkozy’s election: Decomposition along the type of connection

Dependent variable: Two-day cumulated abnormal return on purchases at announcement date				
	(1)	(2)	(3)	(4)
UMP contributor \times Post-election	0.007* (0.004)	0.009** (0.005)	0.009** (0.005)	0.008* (0.005)
Sarkozy friend \times Post-election	0.006** (0.003)	0.004 (0.004)	0.004 (0.004)	0.005 (0.005)
Firm fixed effects		Yes	Yes	Yes
Trade-level variables			Yes	Yes
Interacted trade-level variables				Yes
P-value for equality of coefficients	0.843	0.357	0.366	0.610
Observations	2,224	2,224	2,224	2,224
Dependent variable: Trade announcement delay				
	(5)	(6)	(7)	(8)
UMP contributor \times Post-election	5.578*** (0.999)	6.022*** (1.024)	5.817*** (1.025)	5.845*** (1.028)
Sarkozy friend \times Post-election	4.515*** (0.485)	3.058*** (0.524)	3.165*** (0.521)	2.844*** (0.558)
Firm fixed effects		Yes	Yes	Yes
Trade-level variables			Yes	Yes
Interacted trade-level variables				Yes
P-value for equality of coefficients	0.330	0.00901	0.0196	0.0103
Observations	7,385	7,385	7,385	7,385
Dependent variable: Non-compliance with legal time limit				
	(9)	(10)	(11)	(12)
UMP contributor \times Post-election	0.091* (0.050)	0.141*** (0.047)	0.131*** (0.047)	0.133*** (0.048)
Sarkozy friend \times Post-election	0.243*** (0.045)	0.277*** (0.043)	0.282*** (0.043)	0.264*** (0.045)
Firm fixed effects		Yes	Yes	Yes
Trade-level variables			Yes	Yes
Interacted trade-level variables				Yes
P-value for equality of coefficients	0.0220	0.0279	0.0142	0.0432
Observations	7,385	7,385	7,385	7,385

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. White heteroskedastic standard errors in parentheses. OLS regressions. Each column presents estimates from a separate regression. All regressions include a constant term, a time trend, a dummy variable equal to 1 for all trades that occurred after the election, and the non-interacted group variables. The election date is May 6th 2007. The time window is 365 days before and after the election. The sample is made of all trades by board members of French listed firms during the time window. *Two-day cumulated abnormal return on purchases* is the compound abnormal return (computed using a firm-specific 30-day market model) of the traded stock over the two days following the announcement of a purchase. *Trade announcement delay* is the number of business days between a trade and its official announcement. *Non-compliance with legal limit* is a dummy variable equal to 1 if the trade announcement delay is strictly larger than 5 business days. *UMP contributor* is a dummy variable equal to 1 if the trader is connected to Sarkozy via the list of UMP contributors. *Sarkozy friend* is a dummy variable equal to 1 if the trader is a friend of Sarkozy. See the text for details about the construction of groups. *Trade-level variables* are *executive* and *trade’s value* as used in tables 3.2 and 3.3.

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Abstract

This dissertation is composed of three distinct chapters that empirically investigate various forms of decision-making by firms and/or managers in the field of empirical financial accounting. The first chapter presents a work joint with Francois Brochet and Sven Michael Spira, analyzing how the risk of securities lawsuit for investment-related reasons disciplines managers and reduce agency concerns with respect to investment. The second chapter examines how changes in labor regulation affect managers' incentives to manipulate earnings using other tools that are ultimately detrimental to firms. The third chapter, joint with Renaud Coulomb and Marc Sangnier, explores how political connections lead directors to engage in plausibly fraudulent insider trading in financial markets.

Keywords: Securities Lawsuits, Labor Laws, Earnings Manipulations, Political Connections

Résumé

Cette thèse est composée de trois chapitres distincts qui visent à analyser empiriquement la prise de décisions des entreprises et/ou des dirigeants dans le champ de la comptabilité financière. Le premier chapitre, co-écrit avec Francois Brochet et Sven Michael Spira, analyse comment le risque d'action collective sur les marchés financiers pour des raisons liées à des investissement discipline le comportement des dirigeants et réduit les coûts d'agence. Le deuxième chapitre étudie comment des changements de réglementation sur le marché du travail crée des incitations pour les dirigeants à manipuler leurs comptes autrement, ce qui est néfaste pour l'entreprise. Le troisième chapitre, co-écrit avec Renaud Coulomb et Marc Sangnier, étudie comment les connections politiques conduisent les dirigeants des entreprises à commettre des délits d'initiés présumés.

Mots-Clefs: Risque Juridique, Régulation du Marché du Travail, Manipulations Comptables, Connexions Politiques